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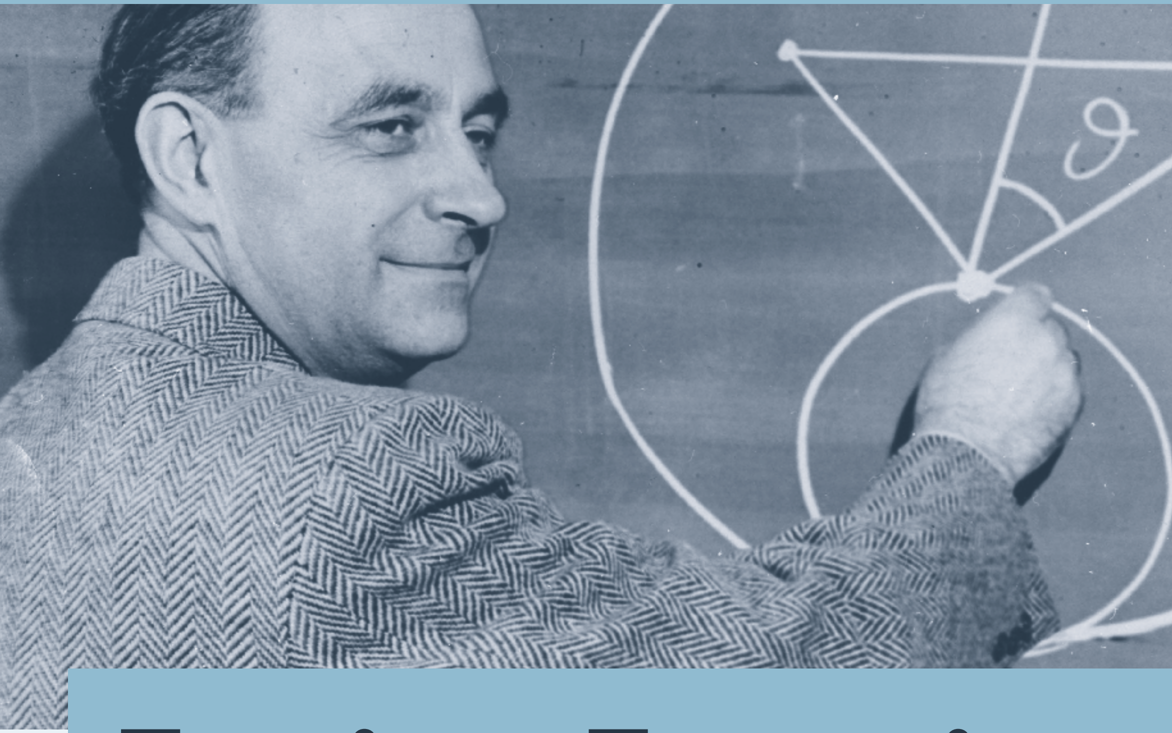

Enrico Fermi

The Obedient Genius

Translated by Ugo Bruzzo

GIUSEPPE BRUZZANITI

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Genova, Italy

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To my parents

Preface to the English Edition

I am honored that Springer is publishing my scientific biography of Enrico Fermi in English, eight years after the original Italian edition was released. The opinions I expressed eight years ago have not changed; should I write Fermi's biography now, I would write it exactly the same way.

I would like to thank Birkhäuser, in particular Chris Tominich, and Einaudi, the publisher of the Italian edition. A special thank you also goes to Allen Mann, who made this project possible.

It is difficult to find the right words for thanking Ugo Bruzzo, who not only performed this translation but also carried out an accurate scientific revision. (I, however, still bear responsibility for any potential inaccuracies that remain).

I would also like to once again thank those who made the original project possible in various ways, in particular Claudio Bartocci, whose support was invaluable.

And finally, thank you, Orietta.

Genova, Italy Giuseppe Bruzzaniti

Preface

Enrico Fermi was born in Rome in 1901; his scientific production started in 1921 and ended in 1954 with his death. At the beginning of his activity, only two fundamental forces of nature were known, gravitation and electromagnetism, and only two elementary particles, the hydrogen nuclei (protons) and electrons. In the mid 1950s, the fundamental forces, with the addition of the strong and weak nuclear interactions, were four, and over thirty elementary particles were known. In little less than thirty years, the conception of matter underwent such a radical and unprecedented change to make perhaps that period, for the amount and rapidity of the acquisition of new notions, a unique one in the history of the Western scientific thought.

Fermi's research deeply marked those thirty years, not only for the number and importance of his results but mostly for their historical role. It may happen indeed that enormously important scientific achievements are the result of long and tenacious researches and are the culmination of a carefully planned project. There are also discoveries that are perhaps less extraordinary, but lead to unexpected reorganizations of the acquired knowledge, dismantle the standard methodological principles and the commonly accepted notions, and point to new, unforeseen directions for the scientific enterprise. In his scientific itinerary, which we are going to revisit together, Fermi succeeded in both objectives.

The documents about Fermi's research depict a composite array of diverse scientific interests, crossing many areas of physics, both experimental and theoretical. However, Fermi's scientific biography is not just an ordered collection of documents. The specific result, the scientific paper, is not an inert object, well defined and limited by the objectives declared in the introduction and the results described in the conclusion. We must enter the document, clarify its structure, and section it to highlight its diverse causal connections with other documents, which not only delineate a more articulate research itinerary, but also anchor it to its scientific context.

However, this work of establishing the causal connections underlying a scientist's research itinerary raises subtle interpretative questions. The links indeed are not always explicit, and to unveil them one needs to examine other sources, such as personal reminiscences, letters, popular and review papers, and also completely external elements, such as the political and cultural events that took place in the relevant historical period. Sometimes one does not find direct connections to other documents, but rather links with elements that belong to what we could call the "global maps," that is, those networks of connections among the various elements of a certain discipline which the scientific community regards as well established.

The global maps, like the scientific itineraries, are deeply conditioned by some general regulating principles. Let us considerer, for instance, the postulate that the duration of a time interval does not depend on the reference frame where it is measured, which was at the basis of mechanics till the birth of the theory of relativity, or the idea that the elementary particles, such as the electrons, cannot be created or destroyed, which underlay all research on the nuclear structure until the early 1930s.

So, by means of "research itineraries" and "global maps," we shall analyze how Fermi was able to establish a number of concepts that turned out to be fundamental for the elementary particle physics. The structure of this text will reflect this twofold path. The first chapter has a biographical nature, while the second and fourth are devoted to the description of the global maps of nuclear physics before and after 1933, a date which is a kind of divide; Fermi's 1933 theory on β-decay decreed indeed the end of what we call the "nuclear protophysics" and opened the way to the construction of what still nowadays is called "nuclear physics." The third and fifth chapters are devoted to Fermi's research itineraries during those two periods.

I would like to end this brief introduction with a caveat. The book contains several notes and references to the appendixes, which have a didactic nature; they aim to help the reader to understand the content of the theories that we are describing. This book indeed is also addressed to those who, while not being specialists, are interested in Fermi's figure and want to understand his work in some detail.

Finally, I want to conclude by thanking three persons; without their unconditional personal and scientific help, this book would have not been written. I want to thank them in the simplest way, just with their names, in alphabetical order: thank you Claudio, thank you Ori, thank you Ugo.

Genova, Italy Giuseppe Bruzzaniti

Note on the sources

Fermi's works have been published in two volumes: E. Fermi, *Note e memorie (Collected papers),* Accademia Nazionale dei Lincei — The University of Chicago Press, Rome and Chicago 1962. The two volumes are here referred to as *CPF I* and *CPF II.*

The papers cited as "E. Fermi [number]" refer to the list of Fermi's works at pages 321–333.

The original version of this book was revised.
An erratum to this book can be found at 10.1007/978-1-4939-3533-8_7.

Contents

Chapter 1
The last Galilean

Galileo's investigation of nature, based on a deep interplay between "sensible experiences" and "certain proofs," has been a foundational passage in the history of modern science. The empirical and the rational aspects of the scientific investigation correspond to two different figures of researchers: the experimentalist and the theorist. Enrico Fermi belonged to the last generation of scientists in which the two attitudes could coexist. Nowadays, the higher mathematics needed in theoretical research and the more and more sophisticated instrumentation used in experiments require an extreme specialization and cannot be mastered by the same person.

In this chapter we go through the main events of Fermi's life, trying to reconstruct his human profile and profound intellect, abstract and concrete at the same time.

1.1 Elementorum physicae mathematicae

Alberto Fermi, a railway clerk, and Ida de Gattis, a primary school teacher, were 44 and 30 years old, respectively, when, 3 years after their marriage, Enrico Fermi was born. He was the third and last of their offspring; Maria was born in 1899, and Giulio in 1900. There was a deep tie between Enrico, a reserved boy of few words, and Giulio, extroverted and loving. They shared from their childhood inclinations and readings that already foretold the future prevailing interests in the scientist's life. Enrico and Giulio built an electric pile, some toys, and some small electrical engines; at age 10 — as told by Emilio Segrè, his friend and collaborator — Enrico

G. Bruzzaniti, *Enrico Fermi*, Springer Biographies,
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tried to understand the meaning of the sentence "the equation $x^2+y^2 = r^2$ represents a circle."[1]

Also young Enrico's readings reveal his interests and what he later chose to study. "Elementorum physicae mathematicae" is the title of a treatise on mathematical physics, written in Latin in 1840 by the Jesuit Andrea Caraffa. Fermi bought it when he was about 14 years old from a second-hand bookstall in Campo de' Fiori in Rome, a place he liked to haunt, and studied it carefully, as testified by the many annotations in the margins of the book.

In 1915, Enrico Fermi's life was deeply affected by the loss of his brother Giulio, who died after the anesthesia for a trivial operation to remove a throat abscess. Enrico's mother, as reported by his wife Laura,[2] never recovered from the loss; her character changed, and she had recurrent depressive crises that heavily affected the family's life. Enrico's unexpressed grief, his dismay, and sudden loneliness increased his already strong attitude to studying, and were somewhat relieved by a new friendship with Enrico Persico, a schoolmate of Giulio's. This friendship was destined to be a lifelong one.[3]

It was evident already from his early teenage years that Fermi had strong scientific interests, and these were further enhanced by his acquaintance with Adolfo Amidei,[4] his father's friend and colleague. A letter sent by Amidei to Segrè in 1958[5] is very useful to draw a picture of young Enrico's scientific education between 13 and 17 years of age. We learn that when he was 13 years old, Enrico had read and studied a treatise on projective geometry,[6] and Joseph A. Serret's trigonometry treatise;[7] at 14, Ernesto Cesaro's "Corso di analisi algebrica con introduzione al calcolo infinitesimale" [A course on algebraic analysis with an introduction to infinitesimal calculus] and Luigi Bianchi's notes for his courses on analytic geometry at the University of Pisa; at 15, Dini's "Lezioni di calcolo infinitesimale e integrale" [Lectures on infinitesimal and integral calculus] (again, notes for a course at the University of Pisa); at 16, Siméon D. Poisson's "Traité de mécanique" [A treatise on mechanics], and at 17, Hermann Grassmann's geometric calculus, preceded by Giuseppe Peano's operations for deductive logic.

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[1] E. Segrè, *Enrico Fermi Physicist,* The University of Chicago Press, Chicago 1970, p. 5. Translator's note: this book was first published in English and then in Italian. As a rule we quote from the English edition, and only seldom we translate from the Italian edition, when it is more complete. Unless otherwise stated, notes refer to the English edition.

[2] L. Fermi, *Atoms in the Family, my Life with Enrico Fermi,* The University of Chicago Press, Chicago 1961.

[3] The first meeting between Persico and Fermi is recounted in E. Persico, *Souvenir de Enrico Fermi,* Scientia 90 (1955), p. 316.

[4] L.G. Paldy, *A Master Teacher,* The Physics Teacher 8 (1969), p. 39.

[5] E. Segrè, *op. cit.,* p. 8.

[6] Th. Reye, *Geometria di posizione* [Position geometry], Tipografia Emiliana, Venezia 1884 (transl. from *Die Geometrie der Lage,* Baumgartner, Leipzig 1866).

[7] *Trattato di trigonometria piana e sferica* [A treatise on plane and spherical trigonometry], translated from *Traité de trigonometrie*, Mallet-Bachelier, Paris 1857.

But these books that Amidei gave to Fermi were not enough. From a postcard he sent to Persico on 7 September 1917, we learn that to prepare his admission exam to the Scuola Normale di Pisa (following Amidei's suggestion), Fermi also studied Chwolson's physics treatise[8] and other books.

Young Enrico, however, had more extended interests than just theoretical studies. He shared with Persico his enthusiasm and dedication to mathematics and physics; when he was between 14 and 17 years old they performed several experiments. They measured the value of the gravitational acceleration and of the terrestrial magnetic field in Rome, and the density of the water from Acqua Marcia.[9]

On entering his 18th year of life, Fermi was a true prodigy; he had a deep mathematical and physical education, great experimental skills, and most of all, a huge fascination for knowledge.

1.2 No man's land and the international milieu

"Distinctive features of sound and its causes" was the title of the dissertation Enrico Fermi was required to write to gain admission to Pisa's Scuola Normale (Figs. 1.1 and 1.2). What he produced is not the output of a brilliant high school student, but rather the work of somebody in possession of advanced notions in mathematics and physics. He started with a study of the partial differential equation governing the motion of a vibrating rod, which he solved using a Fourier series expansion. Even the members of the examining board were incredulous at the extent of Fermi's skills. One of them, Giulio Pittarelli, professor of descriptive geometry at the University of Rome, met Fermi personally, and at the moment of parting, he told him that "in his long career he had never seen anything like this, that Fermi was a most extraordinary person and was destined to become an important scientist."[10]

Enrico Fermi gained his admission to Scuola Normale, and in the fall of 1918 he enrolled at the University of Pisa. At first he chose Mathematics, but shortly afterwards he switched to Physics.

The years he spent in Pisa were very important for Fermi. He started a long and strong friendship with Franco Rasetti,[11] another physics student. During those years, he received many accolades, not only for his skills, but also for the enthusiasm and dedication he showed in deepening his (already remarkable) training

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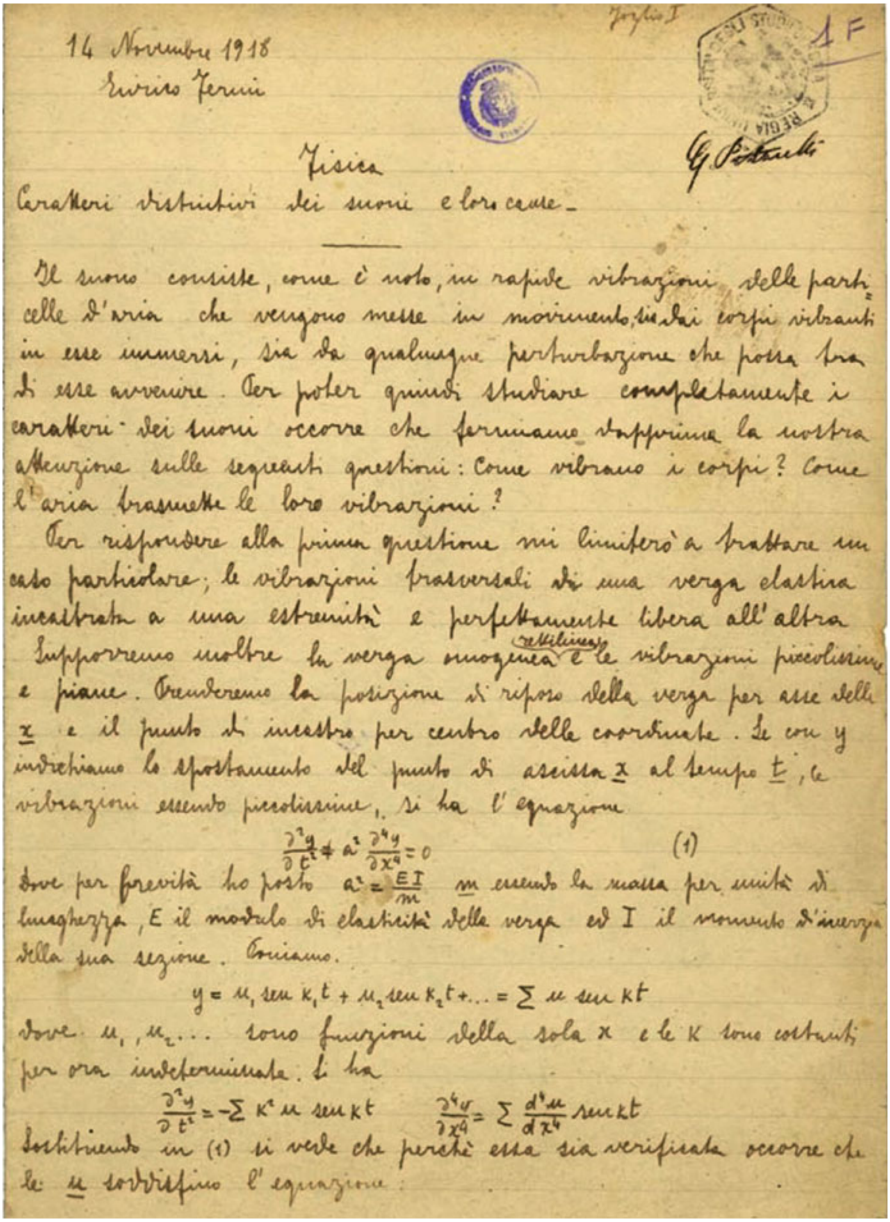
[8] Chwolson's treatise has nine volumes and basically includes all the physics that was known at the time, including the new revolutionary theories due to Einstein, Planck, Poincaré, Lorentz, and others.

[9] This is a (still functioning) Roman aqueduct built by the praetor Quintus Marcius Rex in 144 B.C.

[10] E. Segrè, *op. cit.,* p. 13.

[11] Franco Rasetti, close collaborator and a friend of Fermi's, went on cultivating his many interests in science, and became a recognized expert in entomology, paleontology, botanics, and embryology.



Foglio I

1 F

14 Novembre 1918
Enrico Fermi

G. Pittarelli

Fisica

Caratteri distintivi dei suoni e loro cause.

Il suono consiste, come è noto, in rapide vibrazioni delle particelle d'aria che vengono messe in movimento, sia dai corpi vibranti in esse immersi, sia da qualunque perturbazione che possa tra di esse avvenire. Per poter quindi studiare completamente i caratteri dei suoni occorre che fermiamo dapprima la nostra attenzione sulle seguenti questioni: Come vibrano i corpi? Come l'aria trasmette le loro vibrazioni?

Per rispondere alla prima questione mi limiterò a trattare un caso particolare; le vibrazioni trasversali di una verga elastica incastrata a una estremità e perfettamente libera all'altra. Supporremo inoltre la verga omogenea rettilinea e le vibrazioni piccolissime e piane. Prenderemo la posizione di riposo della verga per asse delle x e il punto di incastro per centro delle coordinate. Se con y indichiamo lo spostamento del punto di ascissa x al tempo t, le vibrazioni essendo piccolissime, si ha l'equazione

$$\frac{\partial^2 y}{\partial t^2} + a^2 \frac{\partial^4 y}{\partial x^4} = 0 \qquad (1)$$

dove per brevità ho posto $a^2 = \frac{EI}{m}$ m essendo la massa per unità di lunghezza, E il modulo di elasticità della verga ed I il momento d'inerzia della sua sezione. Poniamo.

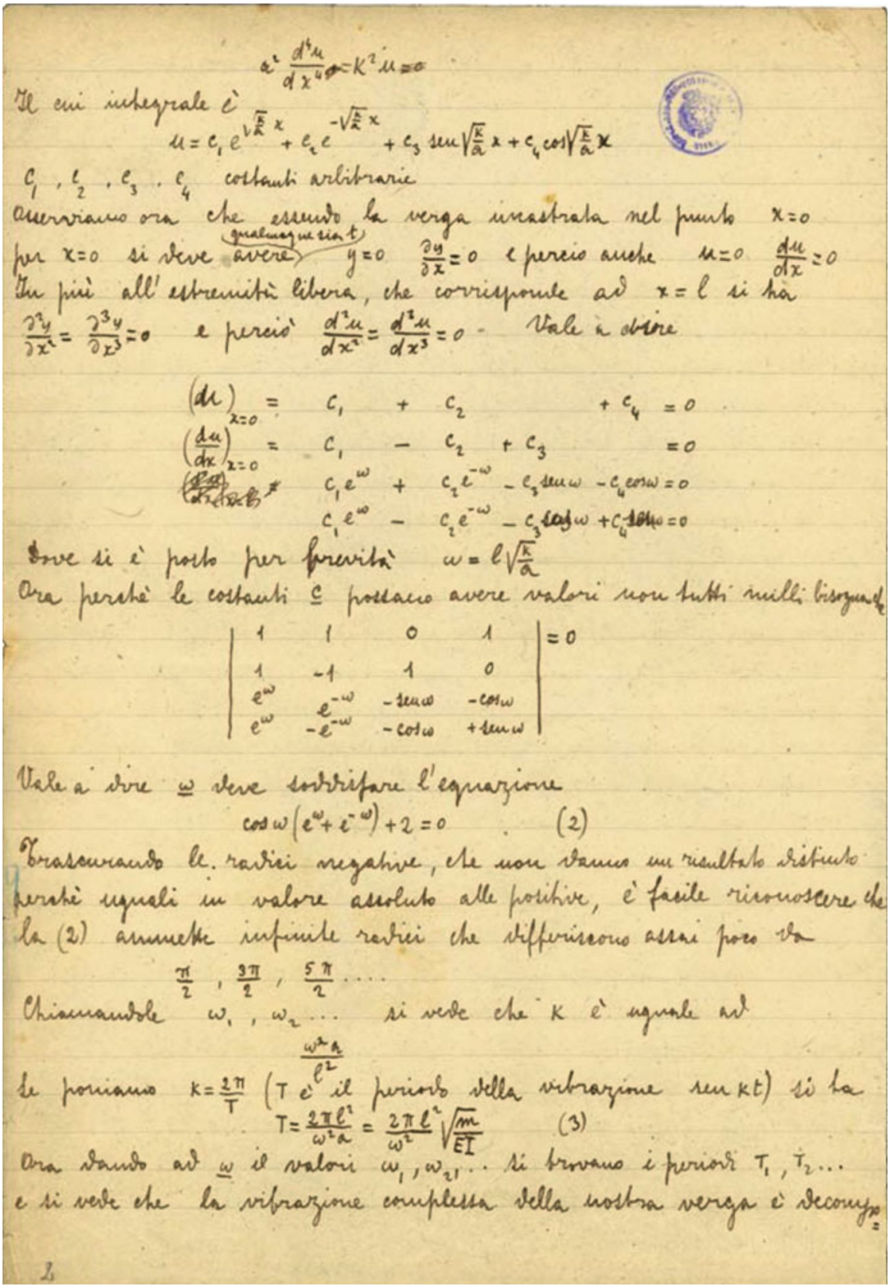
$$y = u_1 \operatorname{sen} k_1 t + u_2 \operatorname{sen} k_2 t + \ldots = \sum u \operatorname{sen} kt$$

dove $u_1, u_2 \ldots$ sono funzioni della sola x e le k sono costanti per ora indeterminate. Si ha

$$\frac{\partial^2 y}{\partial t^2} = -\sum k^2 u \operatorname{sen} kt \qquad \frac{\partial^4 y}{\partial x^4} = \sum \frac{d^4 u}{dx^4} \operatorname{sen} kt$$

Sostituendo in (1) si vede che perchè essa sia verificata occorre che le u soddisfino l'equazione:

Fig. 1.1 First page of Fermi's written exam to enter Scuola Normale Superiore



$$a^2\frac{d^4u}{dx^4} - K^2u = 0$$

Il cui integrale è

$$u = c_1 e^{\sqrt{\frac{k}{a}}\,x} + c_2 e^{-\sqrt{\frac{k}{a}}\,x} + c_3\,\mathrm{sen}\sqrt{\frac{k}{a}}\,x + c_4\cos\sqrt{\frac{k}{a}}\,x$$

c_1, c_2, c_3, c_4 costanti arbitrarie

Osserviamo ora che essendo la verga incastrata nel punto $x=0$ per $x=0$ si deve avere (qualunque sia t) $y=0$ $\frac{\partial y}{\partial x}=0$ e perciò anche $u=0$ $\frac{du}{dx}=0$

In più all'estremità libera, che corrisponde ad $x=l$ si ha $\frac{\partial^2 y}{\partial x^2} = \frac{\partial^3 y}{\partial x^3} = 0$ e perciò $\frac{d^2u}{dx^2} = \frac{d^3u}{dx^3} = 0$. Vale a dire

$$\begin{aligned}
(u)_{x=0} &= c_1 + c_2 + c_4 = 0\\
\left(\frac{du}{dx}\right)_{x=0} &= c_1 - c_2 + c_3 = 0\\
&\; c_1 e^{\omega} + c_2 e^{-\omega} - c_3\,\mathrm{sen}\,\omega - c_4\cos\omega = 0\\
&\; c_1 e^{\omega} - c_2 e^{-\omega} - c_3\cos\omega + c_4\,\mathrm{sen}\,\omega = 0
\end{aligned}$$

Dove si è posto per brevità $\omega = l\sqrt{\frac{k}{a}}$

Ora perchè le costanti c possano avere valori non tutti nulli bisogna che

$$\begin{vmatrix} 1 & 1 & 0 & 1\\ 1 & -1 & 1 & 0\\ e^{\omega} & e^{-\omega} & -\mathrm{sen}\,\omega & -\cos\omega\\ e^{\omega} & -e^{-\omega} & -\cos\omega & +\mathrm{sen}\,\omega \end{vmatrix} = 0$$

Vale a dire ω deve soddisfare l'equazione

$$\cos\omega\left(e^{\omega} + e^{-\omega}\right) + 2 = 0 \qquad (2)$$

Trascurando le radici negative, che non danno un risultato distinto perchè uguali in valore assoluto alle positive, è facile riconoscere che la (2) ammette infinite radici che differiscono assai poco da

$$\frac{\pi}{2},\ \frac{3\pi}{2},\ \frac{5\pi}{2}\dots$$

Chiamandole $\omega_1, \omega_2 \dots$ si vede che K è uguale ad

$$\frac{\omega^2 a}{l^2}$$

Se poniamo $k = \frac{2\pi}{T}$ (T è il periodo della vibrazione sen kt) si ha

$$T = \frac{2\pi l^2}{\omega^2 a} = \frac{2\pi l^2}{\omega^2}\sqrt{\frac{m}{EI}} \qquad (3)$$

Ora dando ad ω il valori $\omega_1, \omega_2, \dots$ si trovano i periodi $T_1, T_2 \dots$ e si vede che la vibrazione complessa della nostra verga è decompo=

Fig. 1.2 Second page of Fermi's written exam to enter Scuola Normale Superiore in mathematics and physics. From his correspondence with Persico and from his notes, meticulously written in a small booklet that was found in Chicago, we know that in those years he read several treatises: Poincaré's "Théorie des tourbillons" and "Leçons de mécanique celeste," Appell's "Traité de mécanique rationnelle," Planck's "Vorlesungen über Thermodynamik," and Richardson's "Electron Theory of Matter." He also studied — but it is impossible to ascertain the sources — Hamilton-Jacobi theory, Boltzmann's H theorem, Planck's black body radiation theory, and relativity theory. The booklet contains also a large collection of data about radioactive substances, taken from Rutherford's "Radioactive Substances and their Radiations" and two large reference lists from a book by Townsend on the electrical properties of gases. The last notes in the booklet, which has 102 pages in total, are dated 29 September 1919. All this shows the extent of Fermi's scientific education when he was just 18 years old. The tight interplay between theoretical and experimental work that will become the trademark of Fermi's approach to research clearly emerged during those years in Pisa: on one hand, he was the most authoritative member of the Physics Institute for what concerns relativity theory and the quanta, while on the other hand, together with Rasetti and Nello Carrara, another fellow student at Scuola Normale, he did experimental work on X-rays, and designed and built discharge tubes that were better suited to that purpose. They worked in the laboratory of the Physics Institute, made available to the three students by the director Luigi Puccianti, who recognized Fermi's extraordinary talent and often asked him to explain the new theories that were emerging during those years.

As Fermi writes to Persico, "In the physics department I am slowly becoming the most influential authority. In fact, one of these days I shall hold (in the presence of several magnates) a lecture on quantum theory, of which I'm always a great propagandist."[12] The new quantum theory and the theory of relativity were the two main theoretical issues that captivated the young scientist's interest. These ideas, which were not yet enjoying in Italy the attention they deserved, overturned many of the most rooted convictions in the physics of the early 20th century. The picture of nature they produced had some counterintuitive aspects that were founded on rather abstract argumentations, which were in turn based on complicated theoretical constructions. According to Rasetti, in Italy the new theories were "a no man's land between physics and mathematics" and "Fermi was the first in the country to fill the gap."[13]

Fermi's scientific production started with relativity theory. In 1921, one year before he graduated, the journal *Nuovo Cimento* published two papers on some relativistic issues,[14] and in 1922 the young scientist obtained his first remarkable result; he proved a theorem using a coordinate system that still bears his name today.[15]

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[12] Letter dated 30 January 1920, in Segrè, *op. cit.,* p. 194.

[13] F. Rasetti, *Introduzione alla formazione di immagini mediante raggi Röntgen* [Introduction to imaging by means of Röntgen rays], *CPF I,* p. 56.

[14] Fermi [1, 2].

[15] Fermi [3].

On the experimental side, he was busy with research on X-rays: the X-ray diffraction by curved crystals and the production of images with that method was the topic chosen by Fermi for his thesis. For Fermi, theoretical research and experimental work were not two unrelated skills or activities, as exemplified by what he wrote to Persico in 1920: "I have almost abandoned the idea to write my thesis on the photoelectric effect in gases. I might instead study the interesting phenomena concerning the diffraction of X-rays by crystals, also because I hope to relate them with statistical physics; I believe indeed that the properties of Röntgen rays should be markedly different from what is predicted by the usual wave theory."[16] This standpoint, already present when Fermi was 19 years old, will characterize his intellectual setting and attitude to research throughout his whole life, his "Galilean" approach: experience and theory are part of a close dialectic process that determines which scientific problems are interesting and deserve to be studied. Fermi defended his thesis on 7 July 1922, obtaining the highest grade (110/110 cum laude). After three days he also took the habilitation exam at the Scuola Normale, obtaining again the highest grade (50/50 cum laude).[17]

Orso Mario Corbino, senator, professor of experimental physics, authoritative researcher in the field of magneto-optics, and director of the Physical Institute of the University of Rome, was greatly influential in Fermi's life. The two scientists met at Fermi's initiative, when he returns to Rome after finishing his university studies in Pisa. Corbino immediately realized that Fermi showed great promise for Italian physics, and since that moment he always used his prestige and influence to support his research. On 30 October 1922 a committee made of two physicists, including Corbino, a chemist, and two mathematicians, awarded Enrico Fermi a grant for a visit abroad, writing a very appreciative report.[18] Fermi chose to go to Max Born's institute in Göttingen. He started his stay there in the winter of 1923.

During his days in Göttingen, Fermi got in touch with some of the main scientists working on the development of the emerging theory of quantum mechanics. In addition to Max Born, he got acquainted with Werner Heisenberg and Pascual Jordan. His stay in Germany did not turn out to be so fruitful as it could have

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[16] Letter dated 30 May 1920, in Segrè, *op. cit.,* p. 227.

[17] Fermi's habilitation thesis was entitled "Un teorema di calcolo delle probabilità e alcune sue applicazioni" [A theorem in probability and some applications], *CPF I,* p. 196. Giovanni Polvani introduces this work with an interesting comment: "Fermi, in addition to his degree in physics, in the same year obtained his habilitation from Scuola Normale Superiore in Pisa, with a thesis which treated the life of asteroids with probabilistic techniques, and was divided in two parts; the first was theoretical, and contained the proof of a theorem in probability theory, which was applied to the study of asteroids in the second part. During the defense of the thesis some mathematicians raised some objections about the solution of a certain equation. It is possible that that criticism is the reason why, contrary to the custom of publishing the habilitation theses in the journal *Annali della Scuola Normale Superiore di Pisa,* Fermi's thesis, while procuring him the full marks cum laude, was not published" (G. Polvani, in *CPF I,* p. 227).

[18] See E. Segrè, *op. cit.,* p. 32.

been,[19] but still allowed him to publish three important papers that drew the attention of another great physicist, Paul Ehrenfest,[20] who sent Fermi a letter via his student George Uhlenbeck. The acquaintance with the latter and the interaction with Ehrenfest were the reasons why Fermi decided to use a grant from the Rockefeller Foundation to visit Ehrenfest's institute in Leiden. Fermi obtained the grant thanks to Vito Volterra, a prominent mathematical analyst and a precursor of modern functional analysis.

Fermi left for Leiden on September 1st, 1924, after devoting the summer to studying the scattering between atoms and charged particles, obtaining some important results. He spent three months in Leiden, and contrary to what had happened in Göttingen, his stay was very fruitful, not only because of the scientific results he obtained, but especially for the stimulating and encouraging environment he found there. In addition to Ehrenfest, he got acquainted with several distinguished physicists, such as Hendrick Lorentz and Albert Einstein, and got in touch with some young researchers, for instance, Samuel A. Goudsmit and Ralph de Laer Kronig, who were to gain soon great prominence.[21] He established with Goudsmit and Kronig a friendship which lasted for his whole life.

Returning to Rome, Fermi faced the problem of finding a permanent job. The high regard he enjoyed in the more broad-minded scientific circles, and the interest of Antonio Garbasso, the director of the Physics Institute in Florence, allowed him to be charged with teaching a course in mathematical physics at the University of Florence, where he reunited with Franco Rasetti, then Garbasso's assistant.

During his stay in Florence, Fermi initiated several lines of research, both experimental and theoretical. The collaboration with Rasetti, an outstanding experimentalist, was very fruitful. In a short note in the journal *Nature,*[22] published in April 1925, they announced the launch of a research program aimed to study the effects of an alternating magnetic field on the polarization of the resonance light of

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[19] The reasons of this relative failure are not easily understood. In the third chapter we shall formulate a hypothesis. For the moment we only record Laura Fermi's comment (*op. cit.,* p. 40–41): "Still he could never shed the feeling that he was a foreigner and did not belong in the group of men around Professor Born. Born himself was kind and hospitable. But he did not guess that the young man from Rome, for all his apparent self-reliance, was the very moment going through that stage of life which most young people cannot avoid. Fermi was groping in uncertainty and seeking reassurance. He was hoping for a pat on the back from Professor Max Born." Nevertheless, according to what Bruno Pontecorvo wrote in his *Enrico Fermi* (Studio Tesi, Pordenone 1933), Max Born had a very high opinion of Enrico Fermi.

[20] The work that drew the attention of Ehrenfest — a theoretical physicist of great value and recognized prestige, especially in the area of statistical physics — was Fermi [13a].

[21] Fermi's stays in Göttingen and Leiden are reconstructed with much detail in F. Cordella, A. De Gregorio, and F. Sebastiani, *Enrico Fermi, gli anni italiani* [Enrico Fermi, his Italian years], Editori Riuniti, Roma 2001, pp. 117–125.

[22] Fermi [26].

mercury vapors. Here again, it is enough to read the introduction to a subsequent paper[23] to understand how the "Galilean method" underpinned Fermi's approach to research:

> We intend in this work to study the effect of a high-frequency alternating magnetic field on the polarization of resonance light; in the present note we treat the theory of the phenomenon, in a following one we shall report on some experiences that have confirmed the expected results.[24]

Not just a series of experiments for measuring an effect, but a close interplay between theory and experiment, with the purpose of building up a theoretical model able to explain a given physical phenomenon. This perfect symmetry and reciprocal necessity between theory and experiment is also witnessed by Fermi's standpoint with respect to Heisenberg's matrix formulation of quantum mechanics;[25] he judges it too abstract to allow for a real understanding of the physical phenomena.[26] In other words, mathematics was for Fermi always functional to the knowledge of the physical reality, while the empirical data are only meaningful within a suitable theoretical framework.[27]

In his time in Florence, Fermi, together with the experimental work with Rasetti, tackled a problem whose solution is one of his most important contributions to physics: the Fermi-Dirac statistics. The year 1925 was also full of events concerning the academic world. In March he obtained his "libera docenza,"[28] and applied to a professor position in Mathematical Physics at the University of Cagliari. The examining board, with a 3-to-2 vote, granted the position to Giovanni Giorgi.[29] The failure of that application embittered Enrico Fermi, who was just 24 years old,

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[23] Fermi [28(1)].

[24] *CPF I,* p. 161.

[25] In the summer of 1925 Werner Heisenberg, Max Born, and Pascual Jordan lay the bases of matrix mechanics, which later Jordan will show to be equivalent to Schrödinger's wave mechanics. We shall deal again with this topic in later chapters.

[26] Fermi indeed writes to Persico in a letter: "My feeling is that the progress during the last few years is not really substantial, in spite of the formal results in the zoology of spectral terms obtained by Heisenberg. According to my taste, they are going too far in giving up to understand things."

[27] According to Pontecorvo: "In all works of Fermi, the mathematical apparatus is strictly adequate to the problem under study. He has always been alien to excessive formalism, but, if necessary, he was ready to make use of the most sophisticated techniques [...] Concerning the relation between Fermi and mathematics, one should stress that during his years in Rome, he considered mathematics as a great science, which, however, was unable to reinvent itself. As he once said, "Today's mathematics is no longer at the cutting edge of science, as it used to be in Gauss' times; too often nowadays a mathematician or a mathematically oriented physicist invents a difficult problem and solves it, just to say "Look how smart I am!" (B. Pontecorvo, *op. cit.*, pp. 31–33).

[28] "Libera docenza," abolished in 1970, was a title, conferred after an examination, that allowed the holder to teach courses at a University. It was similar to Germany's *Privatdozent* title.

[29] Giovanni Giorgi, a student of the famous mathematicians Eugenio Beltrami and Luigi Cremona, graduated from the Engineering School of Rome in 1893. His interests were disparate, as shown by the courses he gave in many different topics. He is remembered especially because of his proposal in 1901 of the system of measurement that bears his name, based on meter, mass-kilogram, and

but was in a sense providential, since it was probably what convinced Orso Mario Corbino (who commented on the outcome of the opening in Cagliari by saying "the prevailing criterion has been the length of the beard") to push, supported by the great mathematicians Guido Castelnuovo, Federigo Enriques, and Tullio Levi-Civita, for the official recognition of theoretical physics as an academic discipline, with the creation of a dedicated chair.

1.3 Via Panisperna

> The committee, having examined Professor Fermi's large and complex scientific work, unanimously recognizes its exceptional level, and believes that, in spite of his young age and after very few years of scientific work, he highly honors Italian physics. He fully masters the subtlest mathematical techniques, but uses them in a sober and pragmatic way, without losing sight of the physical problem, of the interplay among the physical quantities that are involved, and their concrete value. The most delicate concepts of classical mechanics and mathematical physics are familiar to him; he moves with complete assurance in the most difficult questions of modern theoretical physics, so that he is the most qualified person for representing our Country in this field of research, which is internationally the object of an increasingly intense activity. This committee therefore unanimously declares that Prof. Fermi fully deserves to fill this chair in theoretical physics, and believes that he gives the best hopes for the future development of theoretical physics in Italy.[30]

This is the final report of the board formed by Michele Cantone, Orso Mario Corbino, Antonio Garbasso, Gian Antonio Maggi, and Quirino Majorana, that on 7 November 1926 assigned to Enrico Fermi the professorship in theoretical physics that had been opened at the University of Rome. According to the rules of the time, the board selected three winners, ranked according to their merits. In this case the second and third rank were given to Enrico Persico and Aldo Pontremoli,[31] who were then hired by the University of Florence and Milan, respectively.

Fermi's arrival in Rome was for Corbino the first step toward the realization of a plan that he had prepared with Fermi himself: to create, in Italy and in particular in Rome, a school of physics of international standing and up-to-date with the modern developments. To that end, Corbino called Franco Rasetti from Florence to be Fermi's assistant. Also Enrico Persico maintained a close collaboration with the Roman group and aimed at joining it as soon as possible. Rome and Florence became thus the radiating centers of the new physics in Italy.

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second, together with a fourth unit to be chosen among the electrotechnical practical units. For details about the awarding of the professorship in Cagliari, see E. Segrè, *op. cit.,* p. 41.

[30] These are the minutes of the meeting of the committee, translated from E. Segrè, *Enrico Fermi, fisico,* Zanichelli, Bologna 1987, p. 44.

[31] In 1928 Aldo Pontremoli was entrusted with part of the scientific program of Umberto Nobile's airship expedition to the North Pole. Pontremoli died on May 25 when the airship crashed on the pack.

University of Roma's Physics Institute was located at Via Panisperna 89a, and was reasonably equipped for researches in spectroscopy. Rasetti and Fermi resumed their collaboration and did some experimental work to verify the Boltzmann distribution for thallium atoms between the ground state and the first excited state. The results were published in 1927.[32]

On September 11 to 29, 1927, the hundredth anniversary of Alessandro Volta's death was commemorated in Como with an important international conference. Rasetti wrote:

> The entire Gotha of the world's physics was there, including a dozen Nobel laureates, and the great fathers of quantum physics: Bohr, Planck, Compton, Laue, Sommerfeld, Heisenberg and Pauli. Sommerfeld, the prestigious leader of the Munich school, presented some results in which he and his collaborators showed that all the strange phenomena concerning electrons in metals, that had no classical explanation, were easily interpreted in terms of Fermi's new statistics. It was a great triumph for him, and many Italian professors were very much surprised that young Fermi, hardly known in Italy, was already so famous in Germany.[33]

The Como conference was also attended by Emilio Segrè, an engineering student in Rome and a friend of Rasetti's. He was fascinated by the scientific environment, and decided to turn his love for physics (to which he had not indulged as at the time the best students were directed to engineering) into his profession. In the fall of that same year, he switched to the Physics school. Two more students followed his example: Ettore Majorana[34] and Edoardo Amaldi. This was the start of the "Via Panisperna boys," the first students of Enrico Fermi's.

Fermi's "Galilean dominant" emerged also in his relation with the students. As Segrè wrote: "We mainly received a theoretical instruction, and Fermi made no distinction between future theoreticians or experimentalists. There were no exercises of experimental physics. All experimental problems were research, more or less easy and more or less original."[35]

Parallel to the experimental work with Rasetti, his administrative duties, his activity as popularizer,[36] and the education of the students who had switched

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[32] Fermi [40b].

[33] F. Rasetti, *Enrico Fermi e la fisica in Italia,* [Enrico Fermi and Italian physics], in C. Bernardini and L. Bonolis (eds.), *Conoscere Fermi* [Knowing Fermi], Compositori, Bologna 2001, p. 49.

[34] Ettore Majorana was a nephew of Quirino Majorana, a professor of experimental physics at the University of Bologna. He somehow played a marginal role in the group of Via Panisperna. He did not attend the meetings and, due to his character, worked in isolation. He was a highly gifted researcher; as Segrè says, "Ettore Majorana greatly surpassed his new companions, and in some respects — for instance, as a pure mathematician — he was superior even to Fermi." (Segrè, *op. cit.*, p. 51). For a reconstruction of Majorana's figure see E. Recami, *Il caso Majorana. Epistolario, documenti, testimonianze,* Mondadori, Milano 1991; L. Bonolis, *Majorana: il genio scomparso,* Le Scienze, Milano 2002.

[35] Translated from E. Segrè, *Enrico Fermi, fisico,* p. 50.

[36] Fermi published several popular papers; he wanted to spread the basic ideas of the new physical theories. He mostly wrote in "Periodico di matematiche," a journal managed by Federigo Enriques and mainly directed to high school teachers. Fermi in the summer 1927 also wrote the first Italian

to physics,[37] Fermi continued his research in theoretical physics; unquestionably, the best result he got during these early Roman years was the application of his statistics to atomic electrons.[38] In his atomic model the electrons behave like a gas that surrounds the nucleus. The statistical treatment of atomic electrons, that was independently developed also by Thomas, and is nowadays known as the Thomas-Fermi method, was in those years central to Fermi's theoretical work: he applied it to many cases, obtaining important information about a number of atomic properties and on the structure of the periodic system of the elements.

These years were also full of events in Fermi's personal life. In 1928 he married Laura Capon and in 1929 he was appointed to the Royal Italian Academy.[39] This strongly improved his economic situation and allowed him to resign from other jobs, for instance, that of editor for *Enciclopedia Italiana*, that Fermi had accepted to improve his finances.

The international prestige enjoyed by the Roman group, and in particular by Enrico Fermi, can be seen in the many exchanges and study visits: Rasetti, funded by the Rockefeller Foundation, worked in Millikan's laboratory at the California Institute of Technology in Pasadena, where he performed experiments on the Raman effect; Segrè visited Zeeman's laboratory in Amsterdam; Amaldi studied the diffraction of X-rays in liquids with Debye in Leipzig. Most travels were to attend conferences: in June 1928 Fermi was in Leipzig, in April 1929 in Paris and Zurich, in 1930 in Bucharest, and again in 1930, during the summer holidays, he was invited for the first time to America at the University of Michigan in Ann Arbor. At the same time, many foreign physics were hosted in Rome: Hans Bethe, Felix Bloch, Sam Goudsmit, Christian Møller, Rudolf Peierls, George Placzek, Edward Teller, and many more.

The purpose of these visits abroad was to improve the technical skills of the "Via Panisperna boys." The young researchers also understood that the main area of their experimental work, spectroscopy and atomic physics, was losing momentum, and was no longer at the core of the experimental research in physics. Fermi's theoretical work also confirmed this fact. His activity during that period concerned

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book about modern physics: *Introduzione alla fisica atomica,* published by Zanichelli in 1928. In the same year he started writing a 2-volume textbook for high schools, which, according to his wife, "[...] although no masterpiece — it was mediocre prose and complied with unimaginative government programs — still served its purpose of bringing economics return for many years." (L. Fermi, *op. cit.*, p. 62).

[37] This activity took place on late afternoons in Fermi's office. It had mainly the form of informal meetings and casual conversations, often starting with a question by one of the students, and morphing into articulated lectures that appeared to have been carefully planned.

[38] Fermi [43].

[39] The "Reale Accademia d'Italia" was founded in 1929 on Benito Mussolini's initiative, with the aim of obscuring the Accademia dei Lincei, some members of which were definitely anti-fascist. Fermi was appointed directly by Mussolini, most likely after Corbino's suggestion; the latter was already a senator so he could not be appointed in the Academy. Until then Fermi had shown no particular support to fascism, but had rather kept a stance of political indifference.

the most advanced developments of the new quantum mechanics. In 1930 he was invited to participate in a summer school in theoretical physics in the United States. He gave a memorable course, whose topic was the quantum theory of radiation. The lectures notes were published in the journal *Review of Modern Physics* and became a standard reference in those years.[40]

So in 1930 there was marked contrast between the theoretical and experimental work of the Roman group: the first was very advanced, especially thanks to Fermi's research, while the activity in the second was hardly innovative. Fermi and his group knew the reason very well: experimental spectroscopy, and more general atomic physics, had by that time a well-defined theoretical status, and were not likely to give rise to new fundamental discoveries. This viewpoint was well summarized by Corbino in a speech delivered on 21 September 1929:

> One can therefore conclude that, while a great progress of experimental physics in its usual domain is quite unlikely, the assault of the atomic nucleus may offer many possibilities; this is the real field of tomorrow's physics. But to take part in the general movement, both nowadays and in the future, it is mandatory that our experimenters have a direct and certain access to the most recent results in theoretical physics, and that they can take advantage more and more liberally of the modern tools of investigation. To work in experimental physics without being daily informed of the progress in theoretical physics and without well equipped laboratories is like pretending to win a modern war without air force and artillery.[41]

Around 1930 Fermi and his group decided to become acquainted with the most sophisticated experimental techniques of the new nuclear physics, and to start a new line of research, which in a few years was to give rise to extraordinary discoveries. The first steps in the realization of this project were two: visits of some young researchers of the Roman group to some of the most advanced foreign laboratories, and the organization in Rome of the first international conference on nuclear physics. So Rasetti left for Lisa Meitner's laboratory at the Kaiser-Wilhelm-Institut in Berlin-Dahlem, and Segrè for Otto Stern's laboratory in Hamburg.[42] Also the

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[40] Fermi [67]. In 1955 Hans Bethe, during a symposium in Fermi's honor, said: "Many of you probably, like myself, have learned their first field theory from Fermi's wonderful article in the Reviews of Modern Physics of 1932. It is an example of simplicity in a difficult field which I think is unsurpassed." H. Bethe, *Memorial symposium in honor of E. Fermi at the Washington meeting of the American Physical Society,* April 29, 1955, in *Review of Modern Physics,* 27 (1955), p. 249.

[41] O. M. Corbino, *I nuovi compiti della fisica sperimentale* [The new objectives of experimental physics], *Atti della Società Italiana per il Progresso delle Scienze,* 18 (1929), p. 127.

[42] Lisa Meitner was a researcher of undisputed prestige in the field of radioactivity. She formulated one of the most known models for the nuclei of radioactive elements and made important discoveries, also thanks to the collaboration with Otto Hahn. Otto Stern, who in 1921, together with Walther Gerlach, performed the celebrated "Stern-Gerlach experiment," became the director of the Chemical-Physical Institute of the University of Hamburg in 1923; he created there an important research group on molecular beams.

group's readings changed course. Edoardo Amaldi started systematically reading the newly published book by Rutherford, Ellis, and Chadwick,[43] which became the standard text for nuclear physics in those years.

The 1931 conference in Rome was a very important event and can be considered as the birth of nuclear physics as an officially recognized discipline. The conference took place on 11 to 18 October, and hosted about 50 internationally renowned physicists, including Bohr, Compton, Curie, Millikan, Pauli, and many others. The agenda included the most urgent questions concerning the understanding of the nuclear structure, and there were opportunities for ample, usually informal, discussions. Often these produced bold new hypotheses, in the attempt to solve the serious anomalies which at that time hampered any comprehension of the structure of the atomic nucleus.

Many years later, recalling that delicate moment when the entire group reshaped its line of research, Rasetti wrote:

> The change required considerable effort, and was not done by chance or just to follow the fashion. It took place as a result of a conscious and well planned decision that came after a lot of discussion. The first step toward its realization was Rasetti's trip to Dahlem, to learn some techniques in nuclear physics. The 1931 Rome conference helped us to get acquainted with the most interesting current problems, and as a consequence, there was a change of topic of the assiduous readings that took place at the institute."[44]

The first nuclear physics conference preceded by one year the "annus mirabilis," the wonderful year, i.e., 1932, when three great discoveries were made in rapid succession: the neutron, deuterium (the mass 2 hydrogen isotope) , and the positron (the positive electron). In the next chapter we shall examine in some depth these three intricate stories, corresponding to extraordinary discoveries which quickly changed the landscape of the 20th century physics.

The 7th Solvay conference took place on October 22nd to 28th, 1933, and had nuclear physics as its central theme. The discoveries of the previous year on the one hand threw new light on the structure of the atomic nucleus, but on the other hand generated even more serious problems, due to the lack of a coherent theoretical framework. Fermi knew very well what were the basic questions that a theory of the atomic nucleus had to answer, and was ready to deal with the challenge offered by the discoveries of the previous year. Coming back from Brussels at the end of the Solvay conference, he solved in a few months one of the most taxing problems of the time, the interpretation of the energy spectrum of the β decay; more than that, he formulated a general theoretical framework for dealing with nuclear phenomena. The first version of the paper was submitted to the journal *Nature*. The paper was rejected as it "contained abstract conjectures that are too far from physical reality to be of some interest to the reader." Fermi did not give up, as he was conscious of the importance of his work. According to Segrè, "Fermi was fully aware of the

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[43] E. Rutherford, Ch. D. Ellis and J. Chadwick, *Radiations for Radioactive Substances,* Cambridge University Press, Cambridge 1930.

[44] F. Rasetti, in *CPF I,* p. XXXIV.

importance of his accomplishment and said that he thought he would be remembered for this paper, his best so far."[45] The paper was sent to *La Ricerca Scientifica;*[46] an ampler and more detailed version was published by the authoritative German journal *Zeitschrift für Physik,*[47] and immediately afterwards by *Il Nuovo Cimento* in Italian.[48]

1.4 The Pope, the Divine Providence, and the slow neutrons

As gold hunters abandon the almost exhausted veins when the news of a fresh deposit spreads, no matter if they need to open their way by brute force, so the Roman group launched into its most important intellectual adventure as soon as they were informed that in France the Joliot-Curies had been able to make some light elements — like aluminum — radioactive by irradiating them with α particles emitted by polonium.

Until then radioactivity was supposed to be a specific property of some heavy elements, such as uranium, thorium, actinium, radium, and polonium, and a property that cannot be modified, exactly as it is not possible to change the periods of revolution of the planets around the sun. The work of the two French scientists showed that an element which is usually stable, like aluminum, can become radioactive; a kind of microscopic equivalent to a change in the orbit of Jupiter.

The radioactivity obtained by the Joliot-Curies was very weak, and took place only in a few light elements. Fermi was the first to understand that this effect can be substantially amplified by using neutrons instead of α particles. Neutrons have no electric charge and therefore can penetrate the atomic nucleus without being repelled. In March 1934 he suggested Rasetti to try to irradiate some substances with neutrons produced by polonium and beryllium sources. These experiments did not succeed. Rasetti left for a short holiday in Morocco, but Fermi continued the experiment; he changed the sources, that were probably too weak, replacing polonium with radon, which makes for a much stronger source. He analyzed systematically all elements in the periodic system, starting with hydrogen. He detected some effects with aluminum and fluorine. The results were published in the paper "Radioattività indotta da bombardamento di neutroni I,"[49] submitted to *La Ricerca Scientifica* on 25 March 1934.[50]

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[45] E. Segrè, *op. cit.*, p. 75.

[46] Fermi [76].

[47] Fermi [80b].

[48] Fermi [80a].

[49] Neutron-bombarding induced radioactivity.

[50] Fermi [84a].

"The Pope" was right. This was Fermi's nickname in the Roman group, in view of his infallibility. By analogy, Rasetti was the "Cardinal Vicar"[51] or the "Venerable Master"; Professor Giulio Cesare Trabacchi, a chemist and director of the Physical Institute for Public Health, was the "Divine Providence," as he was in possession of a gram of radium, to be used for medical purposes. Indeed providentially, he was able to supply the radon necessary for the experiment. The "Cardinal de propaganda fide"[52] was Enrico Persico, who had moved from Florence to Torino, and turned the latter into a propagating center of the new physics. The "Abbots" were the young scientists in the group, Edoardo Amaldi and Emilio Segrè. Ettore Majorana had two nicknames, to be used according to necessity, "the Holy Ghost," or "the Great Inquisitor." Corbino was "God Almighty," of course.

The discovery of neutron-induced radioactivity triggered a period of intense activity in the Roman group. As Edoardo Amaldi remembers,

> A feverish activity started immediately at the Physics Institute of the University of Rome. The experimentation was organized on a large scale, so to make attempts in many directions and avoid the risk of missing some interesting phenomenon. Fermi not only supervised our work but also took part in all physical measurements and chemical manipulations, also making with his hands chemical glassware and mechanical parts."[53]

In four months of frenzied work the group produced more than 40 radioactive elements, some of an unknown chemical nature. This made the collaboration with a chemist necessary. After professor Trabacchi's suggestion, the group was joined by Oscar D'Agostino, a young employee of the Public Health Institute, who at the moment was working in the Joliot-Curies' laboratory in Paris supported by a grant.

During the systematic analysis of the effect produced by neutrons on all chemical elements, the group also irradiated specimens of thorium and uranium, the last elements in the periodic system, with atomic numbers 90 and 92, respectively. This experiment had an added complication: the two elements are naturally radioactive, and their emissions interfere with the effects of the neutron bombarding. A series of preliminary operations were needed to get rid of this problem. In the summer of 1934 the group obtained a surprising result: during the irradiation of uranium by neutrons, two new elements were produced, having atomic weights 93 and 94 — bigger than that of uranium. They were provisionally christened *hesperium* and *ausenium,* a choice which attests the exasperated nationalism prevailing in Italy at that time.[54] Unbeknown to Fermi, Corbino, in a speech held at

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[51] The Pope is also the bishop of Rome, but due to his many responsibilities, a "Cardinal Vicar" is appointed to help him with the spiritual administration of the diocese.

[52] "Congregatio de propaganda fide," now called "Congregation for the Evangelization of Peoples," is the congregation of the Catholic Church responsible for missionary work and related activities.

[53] E. Amaldi, *Commemorazione del socio Enrico Fermi,* Accademia Nazionale dei Lincei, quaderno n. 35 (1955), in Bernardini and Bonolis, *op. cit.,* p. 29.

[54] "Hesperia" was the ancient Greeks' name for the lands west to Greece, in particular Italy. "Ausoni" was the name of a pre-Roman population of Southern Italy.

Accademia dei Lincei in the presence of the King on the occasion of the closing of the academic year, announced: "I think I can safely conclude that a new element [number 93] has been discovered."[55]

Corbino's brag vexed Fermi, who was not sure about the interpretation of the experimental results. The triumphal overtones of the Italian press, which celebrated the supposed supremacy of the fascist culture, annoyed Fermi very much; the announcement was too hasty, and the interpretation of the results was uncertain and not well documented enough.

Indeed, the truth was different. Ida Noddack, a German chemist, sent Fermi an extract from a paper of hers, where she speculated that the experiments of the Roman group did not reveal the existence of new transuranic elements, but could be differently interpreted as due to a new nuclear reaction, the fission of the uranium nucleus.[56] Noddack's suggestion however was not taken into consideration, and Fermi missed the opportunity to tie up his name with the discovery of the nuclear fission. This was the only great blunder of his career.

In the late 1934 summer another young physicist, Bruno Pontecorvo, who had just graduated in Rome under Rasetti's supervision, joined the group, and was charged to perform with Amaldi more precise measurements of the neutron-induced radioactivity. The results they got were bewildering. In some cases, the response of the irradiated substances seemed to depend on the surface they lay on; the effects were very different whether the substances were set on a wooden table or a marble slab. To understand this odd behavior Fermi undertook a deeper analysis; among other things, he investigated the absorption of neutrons by a lead wedge interposed between the source and the substance to be irradiated.

In the morning of October 22 Fermi was alone in the laboratory; the other members of the group were busy with exams.[57] The "Pope" was starting the measurements when, on impulse, he decided to replace the lead wedge with a paraffin slab. The results were surprising and unexpected: the instruments recorded a sharp increase of the induced radioactivity with respect to the measurements taken in the absence of the paraffin slab. In the late morning Fermi called his collaborators to witness the strange phenomenon. As Segrè tells:

> At first I thought a counter had gone wrong, because such strong activities had not appeared before, but it was immediately demonstrated that the strong activation resulted from filtering by the paraffin of the radiation that produced the radioactivity.[58]

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[55] O. M. Corbino, *Risultati e prospettive della fisica moderna* [Results and perspectives of modern physics], in *Conferenze e discorsi,* Pinci, Roma 1937, p. 64.

[56] I. Noddack, *Über das Element 93,* in *Angewandte Chemie,* 47 (1934), p. 653.

[57] There is no consensus on the date. Recent investigations seem to prove that it should be anticipated by a few days; see A. De Gregorio, *Sulla scoperta della proprietà delle sostanze idrogenate di accrescere la radioattività indotta dai neutroni* [On the discovery of the property of hydrogenated substances of increasing the neutron-induced radioactivity], Il Nuovo Saggiatore, 3 (2003), p. 41.

[58] E. Segrè, *op. cit,* p. 80.

It was not difficult to check that the increase in the radiation was due to the paraffin: by replacing the latter with some other substances the effect disappeared. Everybody was bewildered, but Fermi maintained his aplomb, and uttered a sentence that became famous: “Let’s go to lunch.”

Again according to Segrè,

> By the time we went home for lunch and our usual siesta, we were still extremely puzzled by our observations. When we came back at about three in the afternoon, Fermi had found the explanation of the strange behavior of filtered neutrons. He hypothesized that neutrons could be slowed down by elastic collisions, and in this way become more effective — an idea that was contrary to our expectation.[59]

The hypothesis that the hydrogen nuclei in the paraffin decrease the speed of the neutrons, so that the nuclei in the target can more easily capture them, was again checked in the afternoon of the same day. A new experiment was performed, using the goldfish pool in the garden of the institute as filter for the neutrons. In the evening of that same day, Fermi, Amaldi, Pontecorvo, Rasetti, and Segrè signed a note for *La Ricerca Scientifica,* announcing their discovery.

It is quite reasonable to wonder what urged Fermi to replace the lead block with paraffin. This question is not easily answered. When the members of the group asked him this same question, Fermi smiled, and mockingly answered, “C.I.F. (Con Intuito Fenomenale, that is, with an extraordinary intuition).”[60] There is however a very important testimony by Subrahmanyan Chandrasekhar, a famous Indian theoretical astrophysicist. After telling him about Hadamard’s hypothesis on the psychology of the mathematical invention,[61] Chandrasekhar asked Fermi if he thought that that hypothesis is reasonable also in physics. Fermi’s answer was:

> I will tell you how I came to make the discovery which I suppose is the most important one I have made. And he continued: We were working very hard on the neutron induced radioactivity and the results we were obtaining made no sense. One day, as I came to the laboratory, it occurred to me that I should examine the effect of placing a piece of lead before the incident neutrons. And instead of my usual custom, I took great pains to have the piece of lead precisely machined. I was clearly dissatisfied with something: I tried every excuse to postpone putting the piece of lead in its place. When finally, with some reluctance, I was going to put it in its place, I said to myself: No! I do not want this piece of lead here; what I want is a piece of paraffin. It was just like that: with no advanced warning, no conscious, prior, reasoning. I immediately took some odd piece of paraffin I could put my hands on and placed it where the piece of lead was to have been.[62]

The October 22 discovery catalyzed the activity of the Roman group, which concentrated its research toward the comprehension of the properties of the slow

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[59] *Ibid.*

[60] B. Pontecorvo, *op. cit.*, p. 82.

[61] According to Hadamard there are four stages in the mathematical invention: the first is the conscious consideration of the problem, the second is a period of unconscious incubation, the third is the moment of revelation, when the solution that has been unconsciously elaborated emerges in the conscious stratum of the mind, and the fourth is the final, conscious dedication to the problem.

[62] *CPF II,* p. 927.

neutrons and in particular the features of their production. The group lived an extraordinary intellectual adventure and worked, as Segrè says, in an exciting atmosphere, characterized by a rapid succession of discoveries.

Orso Mario Corbino followed attentively the research of the group and foresaw the possibility that some of their results might have important practical applications. He suggested to patent the two discoveries: the radioactivity induced by irradiation by neutrons, and the increase in the effect of the neutrons obtained by slowing them down. The story of the patent No. 324458, owned by Fermi, Amaldi, Pontecorvo, Rasetti, and Segrè, started on 26 October 1935, in Rome, and ended in the summer of 1953 in Chicago, after a long controversy between Fermi and the US government. The contention took place in the immediate postwar period, and the tenacity of the American lawyers in finding all possible quibbles to reduce the due compensation embittered Fermi. At the end the US government decided to pay the Italian scientists a total sum of 400,000 dollars; after deducing the legal costs, that meant 24,000 dollars each.

1.5 The end of a small world

Starting in 1935, the Roman group began a process of slow dissolution, which ended in 1938 with Fermi's relocation to America. The process was triggered by many reasons. The deterioration of the European political situation and the colonial plans of the fascist regime, which was going to start the Ethiopian war, determined an increasing state of preoccupation that negatively affected the work of the group. The scantiness of the funding, certainly related to the growing financial commitment for the war, made the Roman group hardly competitive with respect to other foreign laboratories. Moreover, Rasetti, Segrè, Pontecorvo, and D'Agostino for diverse reasons left the group; Rasetti joined Columbia University in New York, Segrè obtained a professorship in experimental physics in Palermo, Pontecorvo joined the Joliot-Curie laboratory in Paris, and D'Agostino went back to his work of chemist at the Public Health Institute. Only Amaldi remained in Via Panisperna. In the fall of 1935 he and Fermi started a vigorous research program, aiming to clarify the anomalies in the absorption of slow neutrons that had emerged in some experiments in foreign laboratories. In 1936 the two scientists published some important papers.[63] It is quite easy to see in two of them that "Galilean dominant" we have already often mentioned, that is, an attitude of mind at the same time abstract and concrete, which resulted in a strict coordination between experimental and theoretical research. The first paper was authored by both of them; it dealt with the absorption of slow neutrons, and synthesized the experiments that the two scientists started in October 1935 and terminated in May 1936. These experiments

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[63] Fermi [118a, 119a].

had already been described in a series of letters to *La Ricerca Scientifica,*[64] which contained the results of many sophisticated measurements, their interpretation, and the theoretical questions they raised. The second paper, authored only by Fermi, was purely theoretical, and examined the properties of the slow neutrons, including those discovered in the experiments carried out with Amaldi. The latter reported that Fermi worked on the paper

> in the early hours of the morning before [he] arrived at the Institute. He was always there before nine and, at that time, often at eight. This paper contains the seeds of nearly all of the important ideas on neutrons that Fermi developed in succeeding years.[65]

The need to maintain the international leadership in the research on neutron physics, and a desire to be psychologically detached from the politics of the fascist regime, explains why in the period from October 1935 to May 1936 Fermi was particularly active. As Edoardo Amaldi recalled:

> We worked with incredible stubbornness. We would begin at eight in the morning and take measurements almost without a break, until six or seven in the evening, and often later. The measurements were taken with a chronometric schedule as we had studied the minimum time necessary for all the operations. They were repeated every three or four minutes, according to need, and for hours and hours for as many successive days as were necessary to reach a conclusion on a particular point. Having solved one problem, we immediately attacked another without a break or feelings of uncertainty: "Physics as soma" (this expression comes from Aldous Huxley novel "Brave New World" and refers to a pill with a sexual hormone base used by men in the year 2000 to combat spleen) was our description of the work we performed while the general situation in Italy grew more and more bleak, first as a result of the Ethiopian campaign and then as Italy took part in the Spanish Civil War.[66]

Fermi's efforts to maintain the leadership of the Roman school in neutron physics did not receive an adequate financial support. The most advanced American laboratories were building the first particle accelerators, the cyclotrons. Their use was opening entirely new perspectives, and stressed the inadequacy of the weak neutron sources available in Via Panisperna. The reports from Segrè in Pasadena and from Rasetti in Berkeley, in 1935 and 1936, respectively, dispelled every possible doubt: no advanced research in nuclear physics was possible without the new accelerators.

Fermi answered by devising a new project, whose implementation would have definitely changed the development of the Italian scientific and academic life. Certain of the impossibility to obtain the substantial funding necessary to build a cyclotron, he made an alternative plan. In late 1936, together with Domenico Marotta, the director of the National Health Institute, he proposed the construction of a more traditional accelerator, less powerful, but also less costly. He also relied on the fact that new accelerator could produce radioisotopes, that were more and more

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[64] Fermi [112–117].

[65] E. Amaldi, in *CPF I*, p. 810–811.

[66] *CPF I,* p. 811.

needed for medical applications. The project was approved and a first prototype was assembled in 1937 in the Institute of Physics built in the new campus of the University of Rome.[67] For Fermi this was just the first step of a more ambitious project. To be competitive on the international arena more investments, personnel, and equipment were necessary. For these reasons, Fermi applied to the National Research Council (CNR) for the creation of a "National Institute for Radioactivity." The financial request was 300,000 lire for the first two years, and then 230,000 lire per year.[68]

The application was sent to CNR on 29 January 1937. However, on 23 January Orso Mario Corbino had suddenly died of pneumonia. That was the end of the political protection for Enrico Fermi, his group, and his research. And indeed, Antonino Lo Surdo was appointed as the new director of the Institute of Physics instead of Fermi. The new director had already in the past shown a strong hostility toward Fermi. The situation was further aggravated by the death on 20 July 1937 of Guglielmo Marconi, president of CNR and of the Royal Italian Academy. In a few months the Roman group lost the support and protection of two important political figures.

The application for the creation of the Institute for Radioactivity was rejected in the spring of 1938, but in the summer of the same year Fermi was awarded a 150,000 lire grant to "start a series of experiments that this committee considers very interesting."[69]

The social and political situation in Europe, and especially in Italy, was steadily deteriorating. In March 1938 the Italian Government reacted to the Anschluss (the German annexation of Austria) with a silent acquiescence, showing a timorous acceptance of Hitler's hegemonic plans and foreshadowing Italy's ancillary role in the German expansionistic project. The Nazist dream of conquering the entire European continent, in the name of a supposed superiority of a nation, led to dramatic repressive measures. As one of the consequences, dozens of first-rate scientists emigrated. The fulcrum of the scientific research moved from Europe to America. Segrè recounted a touching anecdote about those events, concerning Erwin Schrödinger's escape from Austria:

> He escaped on foot with nothing but a rucksack and came to Rome. Immediately he went to Fermi and asked to be accompanied to the Vatican to seek protection. This event and other ominous daily portents deeply disturbed those physicists who were still in Rome.[70]

The political situation at this point was hopeless. Any serious research activity was impossible in Italy, and the many job offers coming from abroad were very

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[67] The accelerator was eventually built in the spring of 1939, when Fermi was already in the United States.

[68] The story is told in detail in M. De Maria, *Fermi: un fisico da via Panisperna all'America* [Fermi: a physicist from Via Panisperna to America], Le Scienze, Milano 1999, p. 51–53.

[69] *Ibid.*, p. 53.

[70] E. Segrè, *op. cit.*, p. 95.

appealing to Fermi. He was especially attracted by the American universities, which offered him state-of-the-art equipments and vast resources and the possibility to interact with scientists of international stature.

The decision to emigrate was sparked by the beginning of the Italian anti-semitic campaign: the *Manifesto of Race* was published on 14 July 1938. It declared that Italians are of pure Aryan race, and Jews do not belong to the Italian race. Within a few weeks, in August, the first racial laws were enacted. They only applied to foreign Jews, but soon, on September 2, they were extended to the Italian Jews. Laura Capon, Fermi's wife, was Jewish. Fermi's prestige and his position as a member of the Italian Academy could have mitigated the effects of the racial laws on his family, and his children, Nella and Giulio, were Aryans by law, but, as Laura said, "there is a limit to what one is willing to tolerate."[71]

It was not easy to expatriate during the fascist regime, but something extraordinary happened. The news spread in the fall of 1938 that Fermi had won the Nobel prize for his research on the neutron. The official news was given on November 10, but the Italian press was very lukewarm. Most likely, the regime had decided to downplay as much as possible the achievement of an intellectual who never took a critical stand toward fascism, but neither showed any enthusiasm for it. His marriage with a Jew raised further suspicions and the political police controlled and shadowed him.

The date of the ceremony in Stockholm was December 10. Fermi decided to take advantage of the trip to Stockholm to proceed for the United States, according to a carefully devised plan. In the summer 1938 Fermi wrote to four American universities; as recounted by his wife, to avoid the censorship, he just wrote that "his reasons for not accepting their previous offers had ceased to be."[72] All universities immediately replied and Fermi chose Columbia. In October he went to Copenhagen for a conference and on October 22 he wrote from Brussels, free from the fascist censorship. His letter to Columbia's professor Pegram clearly stated his intention to leave Italy.

> Dear Professor Pegram,
> I cabled to you yesterday as follows: "L.C. Pegram Columbia New York Accept Professorship writing Fermi."
> I should like to express to you again my really very sincere thanks for your generous offer; and please extend my thanks also to Professor Butler [Columbia's president].
> I should like to come to New York, if possible, for the beginning of the spring term which starts, so far as I remember, at the end of February.
> For reasons that you can easily understand however, I should like to leave Italy, without giving the feeling that this is due to political reasons. I could manage this much more easily if you could write me officially to teach at Columbia through the Italian Embassy in the U.S. Of course you need not mention, or stress, in this request, that it would be a permanent appointment.
> In order to get a non-quota visa for myself and my family, I should need besides an official letter from Columbia stating that I am appointed as professor and mentioning the salary.

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[71] L. Fermi, *op. cit.*, p. 120.

[72] L. Fermi, *op. cit.*, p. 139.

> In case that you cannot write me through the Embassy, please send me only this second letter. And in any case please do not give unnecessary publicity to this matter until the situation in Italy is finally settled.
> I shall take the opportunity that I am writing to you from Belgium, in order to give to you some information about the situation of the Italian physicists, that have lost their positions on account of racial reasons.
> (There follows two pages about Italian physicists who have been or are likely to be displaced.)
> Please write to my home address Via L. Magalotti 15, Roma, Italy.
> Looking forward to seeing you next winter, I am, with best greetings
> Signed: Enrico Fermi.[73]

On November 1st Columbia University answered with a letter complying with all Fermi's requests, and in middle November Fermi applied for the permission to go to Stockholm to receive the Nobel prize, and for a leave allowing him to accept Columbia's offer. Not without difficulties, the permissions was granted, and in the evening of November 6 the Fermi family and the nursemaid left Rome. Amaldi and Rasetti were at the railway station to see them off. It is hard to imagine the feelings of the Fermi and of their friends. We only know what Amaldi remembered of that moment.

> I knew, actually we knew, that that night a brief period of the Italian culture was getting to an end, a period that could have spread and developed and perhaps have a positive influence on the Italian academe, and who knows, over the years, even on the entire country. Our little world had been shattered, almost certainly destroyed, by forces and circumstances completely beyond our control. A keen observer might say we had been naive in thinking we could build such a fragile and delicate edifice on the slopes of a volcano that was clearly very close to erupting. But we were born and had grown up on those slopes, and were used to think that what we were doing could be much more long-lasting than the political phase we were going through.[74]

1.6 The Nobel prize

> Professor Fermi, the Royal Swedish Academy of Sciences has awarded you the Nobel Prize for Physics for 1938 for your discovery of new radioactive substances belonging to the entire field of the elements and for the discovery, which you made in the course of your studies, of the selective powers of the slow neutrons.
> We offer our congratulations and we express the most vivid admiration for your brilliant researches, which throw new light on the structure of atomic nuclei and which open up new horizons for the future development of atomic investigation.
> We ask you now to receive the Nobel Prize from the hands of His Majesty the King.[75]

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[73] R. Vergara Caffarelli, *Enrico Fermi. Immagini e documenti* [Enrico Fermi. Images and documents], La Limonaia, Pisa 2001, pp. 81–82.

[74] E. Amaldi, *Da via Panisperna all'America* [From Via Panisperna to America], Editori Riuniti, Roma 1997, p. 63.

[75] *Nobel Lectures, Physics 1922–1941,* Elsevier Publishing Company, Amsterdam 1965, p. 413.

When Fermi received the prize from the King of Sweden's hands he wore a tailcoat, instead of the uniform of the Italian Academy prescribed by the fascist regime, and shook hands with King Gustaf V instead of giving him the fascist salute. Fermi's behavior was harshly condemned by the Italian press; the suspicions that were lingering over him since a few years were indeed confirmed.

In his Nobel prize acceptance speech, Fermi resumed the results of his research, citing also the "discovery" of hesperium and ausenium. In the same days in Berlin Otto Hahn and Fritz Strassmann were obtaining barium atoms by irradiating uranium with neutrons; the fission of uranium had been discovered,[76] albeit only Lise Meitner and Otto Frisch were going to give after a few weeks the correct interpretation of the experiment.[77] The sensational news immediately reached all laboratories, but Fermi remained unaware for a while. On December 24 he and his family were already on the liner *Franconia,* bound to America.

1.7 Landing in the New World

"We have founded the American branch of the Fermi family."[78] According to his wife, these were Fermi's words upon his landing in New York.

The discovery of the fission of uranium immediately redirected the lines of research in many laboratories around the world, including those of Fermi at Columbia University. His first papers in the journal *Physical Reviews* are dated 16 February and 16 March 1939.[79] The nuclear fission had just been discovered, and Fermi felt the urge to continue his researches in that direction. In the first paper, with reference to the fission of the uranium nucleus, we can read: "the process should be accompanied by the delivery of an amount of energy of the order of 200 MeV"; and in the second: "one can think that the fission of the uranium nucleus may be associated with the production of neutrons." These two facts underline how the discovery was truly exceptional: if a nucleus of uranium, irradiated by neutrons, splits into two nuclei, releasing a conspicuous amount of energy, together with more neutrons, then the new neutrons can hit other nuclei of uranium, giving rise to a self-sustaining chain reaction, that can release a huge amount of energy. Suddenly, practical applications of extraordinary importance entered the rarefied atmospheres of the physics laboratories. Leo Szilard, a brilliant physicist and a refugee from

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[76] O. Hahn and F. Strassmann, *Über die Entstehung von Radiumisotopen aus Uran durch Bestrahlen mit schnellen und verlangsamten Neutronen,* in Naturwissenschaften 26 (1938), p. 755; *Id., Über den Nachweis und das Verhalten der bei Bestrahlung des Urans mittels Neutronen entstehenden Erdalkalimetalle,* Naturwissenschaften 27 (1939), p. 11.

[77] L. Meitner and O. R. Frisch, *Disintegration of Uranium by neutrons: A new type of nuclear reaction,* Nature 143 (1939), p. 239.

[78] L. Fermi, *op. cit.,* p. 139.

[79] Fermi [129, 130].

Hungary, worked at Columbia without a definite academic position. He was among the first to predict the huge potential of the nuclear fission, especially for military applications. Szilard was not an easy person, and his and Fermi's characters did not match; while having great esteem of each other, they had a very different approach to work and their direct collaboration was very limited (cf. Appendix B.4). In the subsequent years they worked in different groups (Szilard with Zinn and Fermi with Anderson) and signed only one joint experimental paper.[80] Only one, but of paramount importance. Fermi and Szilard in this 1939 paper showed that the number of neutrons released in a fission reaction is greater than the number of absorbed neutrons. This implied that a chain reaction is possible.

In the meantime the international political situation was steadily worsening, and the outbreak of a world war seemed imminent. There was a major worry that the Germans could use first the nuclear fission for military purposes, as witnessed by Szilard's proposal to the physicists working on it: to avoid passing on any information to the German scientists, they should not publish the results of their researches.

Informing the governmental authorities about the potentialities offered by the nuclear fission was for Fermi and Szilard not only a moral and civil obligation, but also the only way to get a financial support capable to boost their researches on the chain reaction. They followed two different routes, Fermi and Pegram on one side and Szilard on the other side. On 16 March 1939 Pegram wrote a letter to Admiral Hooper, technical assistant to the Chief of Naval Operations, informing him about the possibility to build a new type of bomb with a million times the explosive power of a conventional bomb. In the same time, on March 18th, Fermi went to Washington to give a talk at the U.S. Navy, where he illustrated the new possibilities offered by the fission of uranium to a small group of technicians. The young physicist Ross Gunn participated in the meeting and was impressed by Fermi's talk. He reported to Admiral Bowen, who immediately allocated 1500 dollars to Columbia University to continue the research. It was a small sum, by all means insufficient to support a research of that complexity, yet it was a first step toward an institutional commitment in the researches on the chain reaction. Szilard was firmly persuaded of its feasibility and decided to approach President Roosevelt directly.

The famous letter written on 2 August 1939 by Albert Einstein to the President of the United States (see Appendix B.1) was the result of an initiative of Szilard and Eugene Wigner, another great expatriated Hungarian physicist. The letter was delivered to Alexander Sachs on August 15,[81] but only on October 11 it reached the President. In the meantime, on September 1st, Germany had invaded Poland and World War II had begun. President Roosevelt's reaction to the letter was prompt: his aide General Edwin M. Watson was ordered to act immediately. The nuclear

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[80] Fermi [132].

[81] Russian-born Alexander Sachs, economist and biologist, was Lehman Corporation's vice-president. A staunch supporter of Roosevelt, was his economic consultant in the 1932 presidential campaign.

fission had become a question of national interest, and an “Advisory Committee on Uranium” was established, directed by Lymann Briggs, at the time the director of the National Bureau of Standards. The three Hungarian physicists Szilard, Wigner, and Teller were part of it and also Fermi was invited to its meetings. A first endowment of 6,000 dollars, and a second in June 1940 of 40,000 (but the request was of 140,000), started “Uranium Project,” destined to the realization of the first self-sustained chain reaction.

The advance of the German troops in Europe was fast and seemingly uncontrollable: on 10 May 1940 at 4 a.m. the German army crossed the borders with Belgium, Luxembourg, and the Netherlands; on June 11 France had already virtually lost the war. In those same days Mussolini’s ambitious plans led Italy into war, in the illusory hope to sit near Hitler at the peace talks, at the end of a conflict that seemed already won with very little sacrifice. At the same time the United States, thanks to a powerful technological development and the great advancement of its scientific research — due to Europe’s exasperated nationalism, that led to the dissipation of its enormous intellectual patrimony — were very quick to organize for war. In the summer of 1940 Roosevelt, after the suggestion of Vannevar Bush, professor of electrical engineering and first vice-president of the Massachusetts Institute of Technology, established the National Defense Research Committee (NDRC), at first chaired by Bush, and then by James B. Conant, president of Harvard University. NDRC’s aim was to promote war-related scientific research. The Advisory Committee on Uranium was absorbed into it, and all research was organized to comply with the maximum of secrecy; the ultimate scope was indeed that of building a bomb.

Scientifically, the project involved incredible difficulties. It was by then clear that the most abundant uranium isotope ^{238}U is not subject to the nuclear fission; only the much rarer ^{235}U is. The latter is present in the uranium minerals only in an extremely low proportion. To build a uranium bomb a huge amount of ^{235}U was needed, or as Fermi suggested, one could use plutonium (^{239}P), a new element, artificially created by Edwin McMillan and Philip Abelson in Berkeley. In both cases the availably of the substance was close to zero. However Fermi knew that the realization of an atomic pile, i.e., a chain reaction in natural uranium, has the secondary effect of producing considerable amounts of polonium. But then there was the problem of obtaining a great quantity of very pure graphite, needed for the construction of the pile. Thus, Project Uranium raised huge technological challenges, and the dynamics of the research and discovery process turned out to be very intricate, as we shall see in Chapter 5.

But all reluctance was abandoned when the Japanese air forces struck Pearl Harbor on 7 December 1941 and four days after, Hitler and Mussolini declared war to the United States. Project Uranium received a strong acceleration and was assigned an unprecedented amount of human and financial resources. The direction of the project was entrusted to Arthur Compton, the director of the Department of Physics of the University of Chicago. Under his coordination, in early 1942 the different groups that had till then worked on the project joined in Chicago, in the so-called Metallurgical Laboratory, whose fictitious name masked the true purpose of that huge concentration of resources: to build the atomic pile.

Fermi moved to Chicago without much enthusiasm. Segrè well described his feelings:

> In spite of his strong preference, however, Fermi had to "change his ways." More and more he had to attend meetings, make reports, give advice on technical matters, and tactfully guide engineers who were entirely new to the problem. Instead of performing the needed experiments as had been the case at Columbia, he now called on competent collaborators to make measurements for him. Although he always reserved for himself the analysis of the data, he regretted how little time he could spend in the laboratory. He told me once that he felt he was doing physics "by telephone."[82]

The construction of the pile began on 16 November 1942, in the squash court under the western stands of the University of Chicago's Stagg Field. There were two groups, coordinated by Zinn and Anderson, while Volney C. Wilson was in charge of the group dealing with the controls and instrumentation development. The groups worked almost uninterruptedly, and in less than a month, on December 2, the pile was completed. It was a highly dramatic moment, as very compellingly recounted by Albert Wattenberg:

> On December 2 we began by checking that the neutron intensity was the same as Herb Anderson had measured the previous night, when all except one of the cadmium rods in the pile had been removed. The rates on some of the other instruments were checked and some adjustments were made in anticipation of the neutron intensity's increasing as we proceeded in the morning. In the morning of that, around forty scientists are present during the procedures for turning on the pile. It is a very
>
> Fermi planned to use the last cadmium rod in the pile as a control rod. It would be set by hand at various positions so that we could measure neutron intensity for those positions. He had calculated in advance the intensity that he expected the pile to reach when it saturated at each of these various positions. George Weil was in the squash court in a position to be able to move the last rod. After the checks on the instrumentation were completed, Fermi instructed Weil to move the cadmium to a position which was about half-way out. It was well below the critical condition. The intensity rose, the scalers increased their rates of clicking for of a short while, and then the rate became steady, as it was supposed to.
>
> While it was rising, Fermi periodically read some numbers and did a quick calculation on its little slide rule of the exponential rate of rise of the neutron intensity in the pile. After the intensity had leveled off, he then told Weil to move the cadmium rod other six inches. The neutron intensity in the pile rose further and then again leveled off. The pile was still subcritical. Fermi had been busy noting the values on the back of its slide rule and calculating the rate of rise. After it had stabilized, Fermi told Weil to move the rod out another six inches. Again the neutron intensity increased and leveled off. The pile was still subcritical. Fermi had again been busy with his little slide rule and seemed very pleased with the results of his calculations. Every time the intensity leveled off, it was at the values he had anticipated for that position of the control rod. He moved the rod another six inches. After it has stabilized this time, the neutron intensity in the pile had reached an intensity that was too high for some of the instruments, and, as in other experiments, a few of the

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[82] E. Segrè, *op. cit.,* p. 121.

> instruments were no longer in their linear range. We wanted to take some time to rectify the situation and to modify the operating range of some of the instruments.
>
> After the instrumentation was reset Fermi told Weil to remove the rod another six inches. The pile was still subcritical. The intensity was increasing slowly — when suddenly there was a very loud crash! The safety rod, ZIP, had been automatically released. Its relay had been activated by an ionization chamber because the intensity had exceeded the arbitrary level at which it had been set. It was 11:30 am, and Fermi said, "I'm hungry. Let's go to lunch." The other rods were put in to the pile and locked.
>
> [...]
>
> Except of the one hand-controlled rod, all the other rods were again removed. Fermi asked for the last hand-controlled rod to be set at one of the positions where it had been in the morning. He checked the intensity and the rate of rise and the functioning of the instruments. The values were the same as they had been during the morning when the control rod was at the same positions. He then asked George Weil to set the rod where it had been before we went to lunch.
>
> The trace on the paper on which the neutron intensity was being recorded showed the intensity rising slowly, at the rate that Fermi expected. The intensity would have leveled off after an appreciable length of time. The pile was getting close to critical. Fermi measured the changes in the rate of rise for a while, then asked that ZIP be put in to bring down the intensity. He told George Weil, "This time, take the control rod out twelve inches." After the control rod was set, the ZIP rod was removed from the pile, and Fermi said to Compton, who was standing at his side, "This is going to do it. Now it will become self-sustaining. The trace will climb and continue to climb; it will not level off." Fermi computed the rate of rise after a minute. After another minute he computed it again. After three minutes, he calculated the rate of rise again, and it was staying the same. The pile was functioning exactly as he had expected. I have heard that at this point he broke into a big, cheerful smile. He put away his slide rule and announced, "The reaction is self-sustaining."
>
> Fermi let the activity of the pile increase and watched the pen. It continued to rise as it should, and the intensity was not leveling off. At 3:53, Fermi told Zinn to put ZIP in. The radiation and the neutron intensity and the counting rates all decreased almost instantaneously. We had built the pile, and Fermi had established that we could get a self-sustaining nuclear reaction that we could control in a very predictable manner.
>
> Eugene Wigner had a paper bag with him that I had not noticed. He took out a bottle of Chianti and presented it to Fermi. We each had a small amount in a paper cup and drank silently, looking at Fermi. Someone told Fermi to sign the wrapping on the bottle. After he did so, he passed it around, and we all signed it except Wigner.[83]

At the end of the day, while the scientists were leaving the Stagg Field and a group of technicians were tidying up, Arthur Compton called James Conant. For security reasons they spoke in code. They did not use a pre-established code, but rather used the words dictated by the feeling of the moment. "Jim, you might like to know that the Italian navigator has landed in the new world," said Compton. "How were the natives?" asks Conant. "Very friendly" was the answer.

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[83] A. Wattenberg, *December 2, 1942: the event and the people,* The Bulletin of the Atomic Scientists, 38 No. 10 (1982), pp. 30–32.

1.8 Power and sin: “Little Boy” and “Fat Man”

The history of the agencies that were in charge of the research on the military utilization of the uranium fission is very intricate. Their never-ending reshuffling demonstrates a strong effort to optimize their efficiency and to adapt their organization to the development of the scientific research, as well as to the wartime events.

NDRC’s director Bush strongly pushed President Roosevelt to establish in 1941 the Office of Scientific Research and Development (OSRD). It was an agency with a great autonomy, directly responsible to the President, whose aim was to focus all research made in the country toward the war effort. Bush passed to direct the OSRD, and Conant was appointed as the NDRC director. The Advisory Committee on Uranium became OSRD’s section S-1. In May 1942 also the S-1 section was replaced by a new committee chaired by Conant, and while in Chicago the construction of the atomic pile continued uninterruptedly, in June 1942 Bush communicated to Roosevelt the results of the feasibility studies, made in the previous months, about the possibility of building a new bomb. According to Bush’s report, there was a high probability that the bomb could be built, but to do that, it was also necessary to build industrial plants to produce enough fissile material to trigger an explosive chain reaction. Bush also estimated the cost: one hundred million dollars. In the summer the execution of this huge extraordinary enterprise was entrusted to the Army.

The project was directed by Colonel Leslie R. Groves (who after six days was promoted to general), and was given the code name “Manhattan Engineer District” (MED), but afterwards it would be more simply called “Manhattan Project.” The scientific directors were Harold Urey, Ernest Lawrence, and Arthur Compton. The first two were responsible for the separation of the fissile materials, the third for the theoretical study of the bomb and of the chain reaction. Fermi’s success in building the pile in Chicago was the main reason why Roosevelt decided to allocate 400 million dollars to support both the construction of the industrial plants necessary to the separation of ^{235}U and the nuclear reactors needed to produce the plutonium. General Groves bought vast extensions of land in the Argonne Forest near Chicago, in Los Alamos, New Mexico, and in Hanford, Washington, thus starting an operation of huge proportions, that in the next few months was to involve many industries and laboratories and thousands of men. And, most incredibly, all this happened in the most complete secrecy.

Even after the Manhattan Project had started, pure research was still the core of Fermi’s interests. While the plants for the uranium separation and the production of plutonium were built, Fermi used Chicago’s pile, that in meantime had been rebuilt in what was supposed to be its original location, the Argonne Forest, for sophisticated experiments. Thus in 1943 and 1944, in addition to many commitments with the Manhattan Project, he carried out important investigations in solid state physics and started a new research field, neutron optics.

MED's last phase was the construction of a laboratory where the bomb could be built. For reasons of security and secrecy, the choice fell on Los Alamos, located on a plateau at about 140 miles from Santa Fe. It was a very secluded place, which could be easily isolated from the rest of the world. The direction of the laboratory was assigned to Robert Oppenheimer, a professor of theoretical physics at the University of California at Berkeley and at the California Institute of Technology in Pasadena, a man with an ample culture: he was very knowledgeable in philosophy and literature, and read Sanskrit. In the past he had had a left-wing political orientation. According to Henry DeWolf Smyth (a physics professor and the physics department chair at Princeton and author of a report to the U.S. Government about the development of the atomic bomb),[84] Oppenheimer arrived in Los Alamos in March 1943 and started gathering there the scientists and technicians who were already working on Project Uranium in other research centers around the country. Within two years Los Alamos grew to an impressive size: more than two thousand scientists and technicians were working to the realization of the atomic bomb. Oppenheimer organized the laboratory in seven sections: theoretical physics, coordinated by Bethe; experimental nuclear physics, coordinated by Wilson; chemistry, coordinated by Kennedy and Smith; supplies, under Navy Captain Parsons; explosives, coordinated by Kistiakowsky; bomb physics, coordinated by Bacher; and finally the famous Section F, in charge of the "last touches," directed by Fermi. Every section was in turn divided into groups, each with a responsible person. The heads of the sections met weekly to plan the strategy of the laboratory. This structure was not stable and underwent several rearrangements between 1943 and 1945. The most prestigious researchers in the world worked in the laboratory: in the theoretical physics sections there were Donald A. Flanders, Robert Serber, Edward Teller, Victor Weisskopf, young Richard P. Feynman, and Geoffrey C. Chew. In other sections there were James Chadwick, Niels Bohr, Emilio Segrè, Bruno Rossi, and many other celebrities, including John von Neumann, who, together with Fermi, was considered to be the "oracle" of the laboratory.

In the late 1944 summer Fermi moved to Los Alamos and concentrated all his efforts on the atomic bomb. The realization of the bomb did not seem to be far away and this created hopes for a rapid end of the war. Indeed, little more than two months after the liberation of Rome by the Allied Forces, Fermi wrote to Amaldi:

> Judging from this side of the ocean, sometimes I hope that the reconstruction of Italy can be somehow easier than that of other European countries. Fascism has fallen in so miserably a way that possibly nobody can have regrets [...]. The turn of events seems to justify a hope that the end of the war is not so far away. Perhaps we shall meet reasonably soon.[85]

Fermi's hopes were most probably fuelled by the quick expansion of the Manhattan Project, made possible by the involvement of an unprecedented amount of human, industrial, and financial resources. Many big industries were involved:

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[84] H. DeWolf Smyth, *Atomic Energy for Military Purposes,* Princeton University Press, Princeton 1945.

[85] E. Amaldi, *op. cit.,* p. 148.

Westinghouse, General Electric, Allis-Chalmers, Stone and Webster, Du Pont, Union Carbide, and Eastman Kodak. Their plants and laboratories produced the materials that were sent to Los Alamos. On September 13 the first big power nuclear reactor, built by Du Pont in Hanford, on the Columbia river, was ready to work and produce plutonium. In case of unforeseen problems, Du Pont’s technicians asked Fermi to be present at the activation of the reactor, scheduled for September 27. And there was indeed a problem: after some hours of activity, the reactor stopped. Fermi hypothesized that the malfunction was due to some byproduct of the chain reaction that “poisoned” it. Fermi’s hypothesis was confirmed in a few days after accurate analyses made by the technicians: the pollution was due to an isotope of xenon. The problem took quite a lot of time to be fixed and the production of plutonium in Hanford could restart only in January 1945.

This episode highlights the role played by Fermi in Los Alamos: his section had no well-defined role, but rather dealt with all the problems that the other sections were unable to solve. As Segrè wrote, “Fermi was a sort of oracle to whom any physicist in trouble could appeal and more often than not come away with substantial help.”[86]

Fermi had to cope with manifold problems. The explosion of an atomic bomb is an extremely complex phenomenon, involving experimental and theoretical issues in nuclear physics, hydrodynamics, chemistry, and more. Fermi’s intellectual features allowed him to competently deal with such different questions. One should not neglect that the explosion of an atomic weapon is not a reproducible experiment, but is rather a unique event. Even if the different parts of the bomb were tested in many ways, a final experiment was needed, to check the good functioning and the effectiveness of the bomb before dropping it. The experiment, whose code name was Trinity, was made in a desert area in New Mexico, near Alamogordo. As recounted by Segrè, Fermi played a major role in the experiment.

> To my knowledge there are no written accounts of Fermi’s contribution to the testing problems, nor would it be easy to reconstruct them in detail. This, however, was one of those occasions in which Fermi’s dominion over all physics, one of his most startling characteristics, came into its own. The problems involved in the Trinity test ranged from hydrodynamics to nuclear physics, from optics to thermodynamics, from geophysics to nuclear chemistry. Often they were interrelated, and to solve one it was necessary to understand all the others. Even though the purpose was grim and terrifying, it was one of the greatest physics experiments of all time. Fermi completely immersed himself in the task. At the time of the test he was one of the very few persons (or perhaps the only one) who understood all the technical ramifications of the activities at Alamogordo.[87]

Two great events perturbed the activities at Los Alamos. On 12 April 1945 Roosevelt suddenly died and was immediately replaced by Truman, and on May 8 the war with Germany ended with Hitler’s suicide. The use of a mass destruction weapon such as the atomic bomb was now hardly justified. Many scientists felt “they

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[86] E. Segrè, *op. cit.,* p. 140.

[87] E. Segrè, *op. cit.,* p. 145.

had arrived too late" in an enterprise that was regarded as a fight between good and evil, embodied by Hitler. However, when Truman, who was unaware of MED's true activity and the results so far obtained, was informed, he immediately decided that the project had to be continued and carried to completion as soon as possible.

On May 17 a large explosive charge was detonated in Alamogordo to check the instrumentation that will be used in the final experiment. In the night of July 12 the installation of the bomb began and on July 16 everything was ready; Trinity could start. The 13 kiloton (i.e., having the equivalent power of 13,000 tons of TNT) plutonium bomb was installed on top of a 100 feet steel tower, to be detonated at 5:30 a.m. Fermi was with Segrè in an observation post 9 miles away from the bomb. They lay on the ground with their feet toward the bomb and wore black protective glasses. The explosion was terrifying, beyond any human expectation. Segrè told he thought "the atmosphere could catch fire and bring about the end of the world." The official declaration of the Department of War (see Appendix B.3) describes with much detail the countdown, the experiment, and the emotions of those who witnessed the explosion and its devastating effects.

According to Segrè, just after the explosion, when the shock wave was to arrive, Fermi stood up and let some tiny pieces of paper fall to the ground; he wanted to estimate the released energy by means of their deviation from the vertical. One hour after he went to the location of the explosion on a tank, suitably shielded with lead. The steel tower had disappeared, replaced by an enormous crater. The ground all around was vitrified.

The Trinity experiment reinforced the doubts that some scientists had about the deployment of the nuclear weapon. Men such as Leo Szilard, the leaders of the Manhattan Project, to which they had devoted all their energies, now promoted initiatives to avoid the use of the bomb. While it costed years of strenuous efforts and huge economic investments, its use against human beings prefigured an apocalyptical scenario. On the other hand, Japan was still at war; the end of the conflict did not seem to be near, but the bomb could quickly lead to an end of the hostilities: a public demonstration of its effects could induce Japan to surrender. According to the Franck Report:

> The prospect of nuclear warfare and the type of measures which have to be taken to protect a country from total destruction by nuclear bombing, must be as abhorrent to other nations as to the United States. England, France, and the smaller nations of the European continent, with their congeries of people and industries, are in an entirely hopeless situation in the face of such a threat. Russia and China are the only great nations which could survive a nuclear attack. However, even though these countries value human life less than the peoples of Western Europe and America, and even though Russia, in particular, has an immense space over which its vital industries could be dispersed and a government which can order this dispersion, the day it is convinced that such a measure is necessary — there is no doubt that Russia, too, will shudder at the possibility of a sudden disintegration of Moscow and Leningrad, almost miraculously preserved in the present war, and of its new industrial sites in the Urals and Siberia. Therefore, only lack of mutual trust, and not lack of desire for agreement, can stand in the path of an efficient agreement for the prevention of nuclear warfare.
>
> From this point of view, the way in which nuclear weapons, now secretly developed in this country, will first be revealed to the world appears of great, perhaps fateful importance.

> One possible way — which may particularly appeal to those who consider the nuclear bombs primarily as a secret weapon developed to help win the present war — is to use it without warning on an appropriately selected object in Japan. It is doubtful whether the first available bombs, of comparatively low efficiency and small size, will be sufficient to break the will or ability of Japan to resist, especially given the fact that the major cities like Tokyo, Nagoya, Osaka, and Kobe already will largely be reduced to ashes by the slower process of ordinary aerial bombing. Certain and perhaps important tactical results undoubtedly can be achieved, but we nevertheless think that the question of the use of the very first available atomic bombs in the Japanese war should be weighed very carefully, not only by military authority, but by the highest political leadership of this country. If we consider international agreement on total prevention of nuclear warfare as the paramount objective, and believe that it can be achieved, this kind of introduction of atomic weapons to the world may easily destroy all our chances of success. Russia, and even allied countries which bear less mistrust of our ways and intentions, as well as neutral countries, will be deeply shocked. It will be very difficult to persuade the world that a nation which was capable of secretly preparing and suddenly releasing a weapon, as indiscriminate as the rocket bomb and a thousand times more destructive, is to be trusted in its proclaimed desire of having such weapons abolished by international agreement. We have large accumulations of poison gas, but do not use them, and recent polls have shown that public opinion in this country would disapprove of such a use even if it would accelerate the winning of the Far Eastern war. It is true, that some irrational element in mass psychology makes gas poisoning more revolting that blasting by explosive, even though gas warfare is in no way more “inhuman” than the war of bombs and bullets. Nevertheless, it is not at all certain that the American public opinion, if it could be enlightened as to the effect of atomic explosives, would support the first introduction by our own country of such an indiscriminate method of wholesale destruction of civilian life. Thus, from the “optimistic” point of view — looking forward to an international agreement on prevention of nuclear warfare — the military advantages and the saving of American lives, achieved by the sudden use of atomic bombs against Japan, may be outweighed by the ensuing loss of confidence and wave of horror and repulsion, sweeping over the rest of the world, and perhaps dividing even the public opinion at home.
>
> From this point of view a demonstration of the new weapon may best be made before the eyes of representatives of all United Nations, on the desert or a barren island. The best possible atmosphere for the achievement of an international agreement could be achieved if America would be able to say to the world, “You see what weapon we had but did not use. We are ready to renounce its use in the future and to join other nations in working out adequate supervision of the use of this nuclear weapon.”
>
> [. . .]
>
> We believe that these considerations make the use of nuclear bombs for an early, unannounced attack against Japan inadvisable. If the United States would be the first to release this new means of indiscriminate destruction upon mankind, she would sacrifice public support throughout the world, precipitate the race of armaments, and prejudice the possibility of reaching an international agreement on the future control of such weapons.[88]

This report was commissioned by Compton to James Franck, a German refugee physicist, to analyze the implications and usefulness of the deployment of atomic weapons after Germany’s surrender. Franck chaired a committee which included Donald J. Hughes, James J. Nickson, Eugene Rabinowitch, Glenn Th. Seaborg, Joyce C. Stearns, and Szilard. The report is dated 11 June 1945, exactly one month after the “Target Committee,” in a meeting in Oppenheimer’s office, had spelled out

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[88] http://www.dannen.com/decision/franck.html.

the ideal features of the locations where the atomic bombs were to be dropped. The two bombs were christened *Little Boy* (the uranium one) and *Fat Man* (the plutonium one). The Franck report was also transmitted to the Scientific Panel of the Interim Committee on Nuclear Power, which had been appointed directly by Truman at the beginning of his term. The Interim Committee had the crucial task of advising the executive and legislative branches of the Government; it was chaired by Secretary of War Stimson and was formed by Bush, Conant, Compton, Deputy Secretary of State Clayton, President's representative Byrnes, and Assistant Secretary of the Navy Bard. The Scientific Panel was formed by some of MED's main scientists: Compton, Fermi, Lawrence, and Oppenheimer.

The Scientific Panel was not convinced by the report. On June 16 Compton, Fermi, Lawrence, and Oppenheimer handed in a report entitled "Recommendations on the immediate use of nuclear weapons":

> You have asked us to comment on the initial use of the new weapon. This use, in our opinion, should be such as to promote a satisfactory adjustment of our international relations. At the same time, we recognize our obligation to our nation to use the weapons to help save American lives in the Japanese war.
>
> (1) To accomplish these ends we recommend that before the weapons are used not only Britain, but also Russia, France, and China be advised that we have made considerable progress in our work on atomic weapons, that these may be ready to use during the present war, and that we would welcome suggestions as to how we can cooperate in making this development contribute to improved international relations.
> (2) The opinions of our scientific colleagues on the initial use of these weapons are not unanimous: they range from the proposal of a purely technical demonstration to that of the military application best designed to induce surrender. Those who advocate a purely technical demonstration would wish to outlaw the use of atomic weapons, and have feared that if we use the weapons now our position in future negotiations will be prejudiced. Others emphasize the opportunity of saving American lives by immediate military use, and believe that such use will improve the international prospects, in that they are more concerned with the prevention of war than with the elimination of this specific weapon. We find ourselves closer to these latter views; we can propose no technical demonstration likely to bring an end to the war; we see no acceptable alternative to direct military use.
> (3) With regard to these general aspects of the use of atomic energy, it is clear that we, as scientific men, have no proprietary rights. It is true that we are among the few citizens who have had occasion to give thoughtful consideration to these problems during the past few years. We have, however, no claim to special competence in solving the political, social, and military problems which are presented by the advent of atomic power.[89]

On June 27 Assistant Secretary of Navy Bard wrote:

> Ever since I have been in touch with this program I have had a feeling that before the bomb is actually used against Japan that Japan should have some preliminary warning for say two or three days in advance of use. The position of the United States as a great humanitarian nation and the fair play attitude of our people generally is responsible in the main for this feeling.

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[89] http://www.dannen.com/decision/scipanel.html.

> During recent weeks I have also had the feeling very definitely that the Japanese government may be searching for some opportunity which they could use as a medium of surrender.[90]

On July 3 Szilard and other 58 scientists signed a petition to the President of the United States which stressed that dropping an atomic bomb without giving Japan a chance to surrender, previously informing them of the power of the new weapon, would have been a mistake:

> [. . .] Atomic bombs are primarily a means for the ruthless annihilation of cities. Once they were introduced as an instrument of war it would be difficult to resist for long the temptation of putting them to such use.
>
> The last few years show a marked tendency toward increasing ruthlessness. At present our Air Forces, striking at the Japanese cities, are using the same methods of warfare which were condemned by American public opinion only a few years ago when applied by the Germans to the cities of England. Our use of atomic bombs in this war would carry the world a long way further on this path of ruthlessness.
>
> Atomic power will provide the nations with new means of destruction. The atomic bombs at our disposal represent only the first step in this direction and there is almost no limit to the destructive power which will become available in the course of this development. Thus a nation which sets the precedent of using these newly liberated forces of nature for purposes of destruction may have to bear the responsibility of opening the door to an era of devastation on an unimaginable scale.
>
> In view of the foregoing, we, the undersigned, respectfully petition that you exercise your power as Commander-in-Chief to rule that the United States shall not, in the present phase of the war, resort to the use of atomic bombs.
>
> Leo Szilard and 58 co-signers[91]

On July 4 Szilard started circulating his petition among his colleagues at Oak Ridge; and that same day Major General Groves wrote to Lord Cherwell[92] at the English War Cabinet, looking for evidence against Szilard:

> I wonder if it would be taxing your memory unduly if I were to ask you to write me briefly the subjects of your discussion in your meeting with Dr. Leo Szilard in May of 1943, when you were in this country. Dr. Szilard, as you will recall, worked in the Clarendon Laboratory during the years 1935 to 1938. Frankly, Dr. Szilard has not, in our opinion, evidenced wholehearted cooperation in the maintenance of security. In order to prevent any unjustified action, I am examining all of the facts which can be collected on Dr. Szilard and I am therefore seeking your assistance.[93]

On July 13 eighteen scientists working at the Oak Ridge Laboratories signed Szilard's petition, and soon after another petition was sent from Oak Ridge to President Truman, with 7 signatures, stressing that it was necessary to give Japan the possibility to ascertain the effects of the new weapon, thus inducing it to surrender. On July 17, after the Trinity experiment, Szilard sent a second petition to Truman,

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[90] http://www.dannen.com/decision/bardmemo.html.

[91] http://www.dannen.com/decision/45-07-03.html.

[92] Physicist Frederick A. Lindemann, first Viscount Cherwell, was Winston Churchill's scientific adviser.

[93] http://www.dannen.com/decision/lrg-fal.html.

this time with 69 signatures. On July 26 the Allied Forces allowed Japan a week's time to accept an unconditional surrender, but without mentioning the new nuclear weapons; the ultimatum was rejected.

In late July the cruiser *Indianapolis* carried *Little Boy* and *Fat Man* to Tinian, one of the Mariana Islands, where the 509th B-29 Superfortress Composite Group was stationing. The Group was led by Colonel Tibbets, and gathered the best bombardiers of the Air Force; their task was to hit a target from an altitude of 30,000 feet at a speed of 300 miles per hour. In the evening of August 5 Colonel Tibbets informed the Enola Gay's crew of their mission of the next morning: drop a high-potential bomb on one of the selected targets, Kokura, Yokohama, Nagasaki, and Hiroshima. The choice would have been made at the last moment, according to the weather conditions, by a surveillance aircraft that would fly ahead of them. On August 6 at 1:37 a.m. Major Eatherly took off on his *Straight Flush,* loaded with meteorological instruments. *Enola Gay* took off at 2:45, carrying *Little Boy* not yet activated. Tibbets was the pilot, Lewis the copilot, Stiborik the radar operator, Pearsons the weaponeer, Ferebee the bombardier, Van Kirk the navigator, Jeppson the electronics test officer, Beser the radar countermeasures officer, Nelson the radio operator, Shumard and Duzenbury the electricians, and Caron the tail gunner.

At 7:30 Pearsons activated the bomb and Major Ferebee made the last surveys. In the meantime Major Eatherly communicated to *Enola Gay* that the weather on Hiroshima was mostly clear, gave the coordinates, and flew away. At 8:14 *Enola Gay* was on Hiroshima. Ferebee pressed a button and dropped *Little Boy*. At 8:15, the first atomic bomb exploded about 2000 feet over Hiroshima. That same evening President Truman told the nation that "Sixteen hours ago an American airplane dropped one bomb on Hiroshima ... It is an atomic bomb. It is a harnessing of the basic power of the universe."[94]

Three days later *Fat Man* was dropped on Nagasaki and that evening President Truman announced:

> The world will note that the first atomic bomb was dropped on Hiroshima, a military base. That was because we wished in this first attack to avoid, insofar as possible, the killing of civilians. But that attack is only a warning of things to come. If Japan does not surrender, bombs will have to be dropped on her war industries and, unfortunately, thousands of civilian lives will be lost. I urge Japanese civilians to leave industrial cities immediately, and save themselves from destruction.[95]

The Emperor of Japan, through the Swiss Red Cross, informed the United States that Japan was willing to unconditionally surrender. The surrender was ratified on August 14 and on September 2 the battleship *Missouri* entered Tokyo's harbor. General MacArthur received the Japanese delegates. World War II was over. The last

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[94] http://www.pbs.org/wgbh/americanexperience/features/primary-resources/truman-hiroshima/.

[95] H.S. Truman, in *Public Papers of the Presidents of the United States: Harry S. Truman, Containing the Public Messages, Speeches and Statements of the President April 12 to December 31, 1945,* Washington DC, p. 212. The complete text was also published in the *New York Times,* 10 August 1945, p. 12.

war operation, the atomic bombing of Hiroshima and Nagasaki, caused 120,000 deaths and more than 110,000 wounded; many of them would have died on the next few days.

On August 28 Enrico Fermi from Los Alamos wrote to Amaldi in Rome:

> I guess you have understood from the news of a couple of weeks ago what kind of work we have done over the last few years. It has been a work of high scientific interest, and I am quite satisfied for having contributed to stop a war that could have gone on for months or years. We all hope that in the future these new inventions will be used in a reasonable way, and that they have some better result than worsening the international relations.[96]

On 25 November 25 1947 Oppenheimer gave a talk at the Massachusetts Institute of Technology:

> [. . .] the physicists have felt the peculiarly intimate responsibility for suggesting, for supporting, and in the end, in large measure, for achieving the realization of atomic weapons. Nor can we forget that these weapons as they were in fact used dramatized so mercilessly the inhumanity and evil of modern war. In some sort of crude sense which no vulgarity, no humor, no overstatement can quite extinguish, the physicists have known sin; and this is a knowledge which they cannot lose.[97]

1.9 Traveling again: from nuclei to elementary particles

In 1945 Arthur Compton established three new research institutes in Chicago, in nuclear physics, radiobiology, and metallurgy. Probably his aim was to perpetuate in peacetime the research style of Los Alamos during wartime, and most likely for this reason he offered the direction of the three institutes to three scientists who, while definitely being top authorities in their fields, played a key role in the Manhattan Project. They were Enrico Fermi for nuclear physics, Harold Urey for radiobiology, and Cyril Smith for metallurgy. Fermi wanted to entirely devote his time to research, without being bothered by paperwork, and declined; the direction of the institute was entrusted to Samuel Allison, actually after Fermi's keen suggestion.

Going back to a normal research activity in nuclear physics however was no easy matter. Researchers were still bound to secrecy, and while this was sensible in wartime, now seemed to be a hindrance to scientific progress. This happened on the basis of a precise plan: to bring nuclear research under the control of the military. Fermi assumed a definite standing about this problem when he wrote a letter in occasion of a meeting on the social and political implications of atomic energy that had been organized by Robert Hutchins, president of the University of Chicago:

> I believe that also the following points are true although the agreement as to them is perhaps less general at least in the non-scientific public:

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[96] E. Amaldi, *Da via Panisperna all'America, op. cit.*, p. 158.

[97] J. R. Oppenheimer, *Physics in the Contemporary World,* Anthoensen Press, Portland ME 1947, p. 11.

> That secrecy on the industrial aspects of the development would slow up a potential competing nation by only a few years.
> That secrecy on the scientific phases of the development not only would be of little effect but soon would hamper the progress of nuclear physics in this country to such an extent as to even make it exceedingly difficult to grasp the importance of new discoveries made elsewhere in the field.
> From these points one conclusion emerges. That it is imperative that this country not only should have but should put in operation in a very limited time a policy to face the new dangers.[98]

Thus in the immediate aftermath of the war Fermi was in the center of a heated debate between opposite standpoints about the use of the nuclear technology. On one side a group of scientists created the Federation of Atomic Scientists, with the aim of informing the public opinion of the dangers of the atomic energy and contrasting the attempt of the military to take full control of the nuclear technology. Fermi, while agreeing with its objectives, did not join the Federation. On the other side, after Truman's initiative, May and Johnson proposed a bill to transfer the full control of all nuclear matters to the military. This bill raised a spirited debate and Fermi participated by signing, with Oppenheimer and Lawrence, a cable sent to War Secretary Patterson to support the bill.

> We would most strongly urge the passage of the legislation now before Congress for the creation of an atomic energy commission. We know from our close association with the actual work in this field that delay will cost us heavily in efficiency, in accomplishment, and in spirit. We believe that with wisdom operations can be carried on within the framework of the proposed legislation safely, effectively, and in the best interests of this Nation. We believe that the broad powers granted the Commission by the legislation are justified by the importance and the perils of the subject. We think it necessary for the American people to understand in full the implications of the new technical situation, but we believe that the proposed legislation will make it possible for their desires and decisions to be responsibly and fully implemented. We assure you that in our opinion the legislation as presented represents the fruits of well-informed and experienced consideration.[99]

It is not easy to understand Fermi's ambiguous standpoint; he did not want to maintain secrecy, but was in favor of the May-Johnson bill. Probably he was thinking that, in view of the importance of its military applications, nuclear energy could only be managed under the control of the armed forces, who however were not to interfere with the free circulation of the scientific results. It seems that Fermi was convinced that the neutrality of science can be only be guaranteed by a strict control of its products.

The May-Johnson bill was not approved, due to the strong contrariety of many scientists and the disagreement of the public opinion. It was replaced by another proposal, by Connecticut Senator Brien MacMahon, which gave the control of nuclear energy to the civil authorities. Fermi took position against the MacMahon bill and the latter was amended many times before being approved. It was eventually

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[98] E. Segrè, *op. cit.,* p. 159.

[99] E. Segrè, *op. cit.,* p. 162.

promulgated by Truman on 1 August 1946. The control of nuclear energy was entrusted to the Atomic Energy Commission (AEC); this was formed by five members appointed by the President and included two sections, in charge of the military and scientific consultancy, respectively. The second was called General Advisory Committee (GAC), was chaired by Oppenheimer, and, between 1947 and 1950, included Fermi.

The most crucial problem Fermi had to deal with during his work at GAC was the construction of the first H bomb, namely, a bomb whose energy is released not by the fission of a nucleus of uranium or polonium, but by the fusion of two hydrogen nuclei. The study of the so-called “superbomb” had begun in Los Alamos already during the Manhattan Project, by a small group of scientists led by Edward Teller. It was actually Fermi who in 1941 conjectured that the explosion of a fission bomb, thanks to the very high temperature reached, could trigger the fusion of two nuclei of deuterium (an isotope of hydrogen), yielding a nucleus of helium, and releasing much more energy than a nuclear fission.[100]

Teller’s studies on the super-bomb began officially in 1942, but only after the explosion of the first Russian atomic bomb in 1949 the United States Government felt the urge to build an H bomb to maintain the American military supremacy. Here GAC played an important role. The committee was asked to give advice on the opportunity and feasibility of a thermonuclear bomb, having an unimaginable destruction power. In a meeting on 30 October 1949, the GAC expressed a negative opinion, however the decision was not unanimous. The AEC president David E. Lilienthal received two reports; the majority one, signed by James B. Conant, Hartley Rowe, Cyril Stanley Smith, Lee A. DuBridge, Olivier E. Buckley, and Oppenheimer, and the minority report, signed by Rabi and Fermi. As stressed by Oppenheimer, the committee had unanimously voted the suggestion not to build the H bomb, but the two reports differed about the conditions of this commitment; for the majority it had to be unconditional, while for the minority, it was to depend on the reaction of the Russian government to the proposal of not developing the thermonuclear weapons. From the Fermi-Rabi report:

> A decision on the proposal that an all-out effort be undertaken for the development of the “Super” cannot in our opinion be separated from considerations of broad national policy. A weapon like the “Super” is only an advantage when its energy release is from 100–1000 times greater than that of ordinary atomic bombs. The area of destruction therefore would run from 150 to approximately 1000 square miles or more.
>
> Necessarily such a weapon goes far beyond any military objective and enters the range of very great natural catastrophes. By its very nature it cannot be confined to a military objective but becomes a weapon which in practical effect is almost one of genocide.
>
> It is clear that the use of such a weapon cannot be justified on any ethical ground which gives a human being a certain individuality and dignity even if he happens to be a resident of an enemy country. It is evident to us that this would be the view of peoples in other countries. Its use would put the United States in a bad moral position relative to the peoples of the world.

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[100] R. Rhodes, *The Making of the Atomic Bomb,* Penguin Books, London 1986.

> Any postwar situation resulting from such a weapon would leave unresolvable enmities for generations. A desirable peace cannot come from such an inhuman application of force. The postwar problems would dwarf the problems which confront us at present.
>
> The application of this weapon with the consequent great release of radioactivity would have results unforeseeable at present, but would certainly render large areas unfit for habitation for long periods of time.
>
> The fact that no limits exist to the destructiveness of this weapon makes its very existence and the knowledge of its construction a danger to humanity as a whole. It is necessarily an evil thing considered in any light.
>
> For these reasons we believe it important for the President of the United States to tell the American public, and the world, that we think it wrong on fundamental ethical principles to initiate a program of development of such a weapon. At the same time it would be appropriate to invite the nations of the world to join us in a solemn pledge not to proceed in the development or construction of weapons of this category. If such a pledge were accepted even without control machinery, it appears highly probable that an advanced stage of development leading to a test by another power could be detected by available physical means. Furthermore, we have our possession, in our stockpile of atomic bombs, the means for adequate "military" retaliation for the production or use of a "Super."[101]

In spite of GAC's recommendation, on 31 January 1950 President Truman decided to give top priority to the construction of the H bomb; one of the reasons of this decision was most likely the arrest for spying of Klaus Fuchs, a physicist of German origin who had participated in the Manhattan Project and between 1942 and 1949 had passed much secret information to the Russians.

Despite his initial contrariety, Fermi actively collaborated to the making of the H bomb. In summer 1950 he was back in Los Alamos for almost three months and worked intensively with Ulam. On 8 May 1951, *George,* the first American H bomb, exploded on the Eniwetok atoll. It was a fission-fusion prototype. In November of the same year *Mike,* the first true H bomb, exploded, releasing an energy of 10 Megatons, almost 1000 times the energy released by the Hiroshima bomb. And, as it had been widely predicted, a nuclear arms race began: in August 1953 the first Russian super-bomb exploded, followed on 23 November 1955 by the first H bomb.

The American years, especially after the war, were for Fermi a period of intense activity. The picture we get of him during those years is somehow unusual; he was no longer totally absorbed by his research, but was active on several fronts and also played an official role. However, his research did not suffer from this and was still made at a very advanced level, always guided by that "Galilean component" which makes it impossible to classify his work as experimental or theoretical. In those years Fermi felt again, for the second time, the need to change the course of his research. As during the Roman years, with the passage from atomic to nuclear physics, Fermi now felt that nuclear research had lost its innovative drive and was becoming a field where the investigation, while still very important, was more and more routine.

"Innovate or perish" was the motto, ironically borrowed from Mussolini when together with the Via Panisperna boys Fermi has decided to change their research

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[101] *Bulletin of the Atomic Scientist* 32 no. 10 (1976), p. 58.

topic. This time the motto sounded like a writing of Gabriele D'Annunzio:[102] "It is never too late to go beyond; it is never too late to attempt unknown."[103] Thus Fermi, an indisputable authority in the field of nuclear physics, started another adventure; he wanted to master the new theories that were quickly changing the conceptual framework of theoretical physics and tackle a new leading-edge topic of physical research: elementary particle theory. Until 1951 he worked in quantum electrodynamics, in the new form proposed by Schwinger, Feynman, and Tomonaga, and in meson and cosmic ray physics. This research, with the experiment of Conversi, Pancini, and Piccioni (1944–1948), gave origin to the area of research nowadays called "high energy physics." In Chapter 5 we shall examine in detail Fermi's contributions to the physics of those years; for the moment we just stress that he got important and lasting results.

The synchrocyclotron, the most powerful accelerating machine at the time, started functioning in Chicago in the spring of 1951. Fermi had given important contributions to its construction. He started a period of intense experimental research. The discovery of the pion-nucleon resonance is a historical landmark. The elaboration of the experimental data was a task of great complexity, and, done by hand or with the help of the first computers, required a long time. Since Los Alamos Fermi had shared with von Neumann a keen interest for the electronic computing. Now, again in the Los Alamos laboratories, the mathematician Nicholas Metropolis was constructing MANIAC I (Mathematical Analyzer and Numerical Integrator and Computer). The computer started working on 15 March 1952 and Fermi spent a long period in Los Alamos, from July to September, to work with it. As Metropolis wrote:

> Fermi had early recognized the potential capabilities of electronics computers: his sustained interest was a source of stimulation to those working in the field; but it was his direct approach and complete participation that had the greatest effect on the new discipline. His curiosity extended beyond the calculation problem at hand; he raised questions about the general logical structure of computers and his remarks were always of a penetrating nature.[104]

Fermi's deep interest in computers, regarded not merely as tools for calculating, but rather as the basis of a "third way" simulation, in addition to theory and experiment, is witnessed by the work on the dynamics on nonlinear systems, done between 1952 and 1953 in Los Alamos, in collaboration with John R. Pasta and Stanislaw Ulam.[105] Fermi considered nonlinear dynamics as a very important problem for the future of physics. But his interest for computers and the pioneering work on the dynamics of nonlinear systems were not enough for Fermi's exuberant

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[102] Gabriele D'Annunzio (1863–1938) was one of the most celebrated Italian poets and writers in the early 20th century. He was a leading figure of the nationalistic fascist culture and was very well known, also for his military and patriotic deeds.

[103] G. D'Annunzio, *La Nave,* Fratelli Treves, Milano 1908. Third episode, p. 232.

[104] N. Metropolis, *Numerical Solution of a Minimum Problem,* (Introduction) in *CPF II*, p. 861.

[105] The paper was completed in 1955 after Fermi's death and was published as a Los Alamos report (Fermi [266]).

intellect. In the last year of his life he started a collaboration with the great astrophysicist Chandrasekhar, and obtained also in this field important theoretical results.[106]

The style of scientific research had changed a lot since Fermi's Italian years. The transition to *Big Science* was starting, and the Manhattan Project had been the first example. Advanced research in particle physics required huge governmental fundings, necessary for the realization of large accelerating machines. The aim was to perform experiments at higher and higher energies, and this also required powerful computers and large teams of researchers and technicians, with well-defined specializations. Fermi noticed the first signals of this trend already in 1946. In a letter to Amaldi and Gian Carlo Wick he wrote:

> [. . .] now that people are convinced that physics can be used to build atomic bombs, everybody speaks of fundings of million dollars with indifference. I am quite impressed by the fact that the biggest difficulty will be to figure out what to do with all that money.[107]

When there is a marked change in the style of research, or new scientific theories are created, or old theories are reformulated in more abstract and general terms, it happens that some scientists, who have already reached their full scientific maturity, withdraw into the stronghold of their knowledge, and take a critical stand toward the new theories. This was not the case with Fermi. His background and expertise and his interest for knowledge put him in the best conditions to take advantage of the new ideas. Chicago's Institute for Nuclear Physics thus became one of the leading international centers. Fermi's prestige attracted students and young researchers from all over the world. Geoffery F. Chew, Harnold Argo, Marvin Goldberger, Albert Wattenberg, Arthur H. Rosenfeld, Jay Orear, Darragh E. Nagle, Tsung Dao Lee, and Chen Ning Yang are only a few of the young scientists who formed the so-called "Chicago School."[108] Fermi is the "master" who reinitiated the old Roman habits: informal lectures in his office, which were not prepared but developed from a problem posed by the students or by Fermi (Appendixes B.6 and B.7 report some memories of his former students).

In 1953 Fermi was at the apex of his prestige and with his appointment as president of the American Physical Society the American scientific community endorsed his authoritativeness.

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[106] Fermi [261, 262].

[107] E. Amaldi, *op. cit.,* p. 166.

[108] A complete list can be found in C. N. Yang's introduction to *Are mesons elementary particles?*, in *CPF II,* p. 674, and at the URL www.lib.uchicago.edu/e/crerar/fermi/fermidiss.html.

1.10 The last trip

In the last years of his life Fermi intensified his relations with Italy, in particular with the old friends of the Roman school. In 1948 Amaldi asked him to write a letter to Alcide de Gasperi, Italy's Prime Minister, to recommend an increase of the funding for scientific research in Italy. The letter stressed the excellent level of the Italian scientific research and the surprise it evoked among the American scientists, who compared the quality of the results with the difficult conditions in which they were obtained. Fermi's plead had only a limited success: the funding was of 250 million lire instead of 500.

The first trip to Italy took place in 1949, eleven years after his departure. The opportunity was offered by a conference on cosmic rays organized in Como from September 11th to 16th. Giorgio Salvini gave a touching report of the meeting between Fermi and Heisenberg:

> Heisenberg and Fermi entered the hall from two opposite entrances, and greeted warmly after ten years of separation, during which they were engaged in competing, albeit similar programs. Historians, this is a meeting to remember, this cheerful meeting, in the presence of the best European physicists, winners or defeated. As if war had been eventually forgotten.[109]

After the Como conference, under the initiative of the Donegani Foundation and Accademia Nazionale dei Lincei, Fermi gave nine lectures,[110] six in Rome, on 3 to 14 October and three in Milan, on 18 to 21 October. The topics ranged from general theories to particular issues: from elementary particles to quantum electrodynamics, from the theory on the origin of chemical elements to neutron physics, from the optical analogies in the properties of the neutrons to the Dirac monopole. These lectures played an important role in orienting the new generation of Italian physicists and in directing their future research.

Fermi's last years were disturbed by an affair which deeply touched him. On 7 November 1953 the FBI director Edgard Hoover received a letter which, making reference to a secret file, informed him that Robert Oppenheimer was a spy who had worked for the Soviet Union, with the aim of hindering the realization of the American H bomb. The writer was William L. Borden, former director of the Joint Committee on Atomic Energy. President Dwight Eisenhower, who had been elected one year before, was immediately informed and after a meeting at the White House, decided to remove Oppenheimer from all governmental activities and to prepare a case against him. Lewis Strauss, AEC's new Chair, was in charge of handling the affair. He used as a basis a testimony given by Teller to the FBI. In late 1953, Oppenheimer was given two choices: resign or face a trial. He chose to

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[109] G. Salvini, *Enrico Fermi il maestro sperimentale e teorico del secolo ora trascorso. Alcuni ricordi personali* [Enrico Fermi, the experimental and theoretical master of the century which is just over. Some personal recollections], in "Celebrazioni del centenario della nascita di Enrico Fermi," *Il Nuovo Saggiatore,* 16 (2001), no. 5–6, p. 20.

[110] Fermi [240].

be tried. The preliminary investigation took place in April and May 1953; about 40 witnesses were examined, including Fermi. The testimonies were mainly in Oppenheimer's favor, as it was Fermi's; however, the scientists who had disagreed with him about the H bomb, in particular Teller, were against him. The prosecution and the defense debated harshly. Albeit the jury recognized Oppenheimer great loyalty to the Country, the final decision was to withdraw his security clearance. The decision was confirmed in the appeal trial. Fermi was deeply saddened by the outcome of the trial, to the point that a few weeks before dying he asked his friend Segrè to call Teller — whom he considered to be the main responsible of the jury's decision — for a clarification.

In the summer of 1954 Fermi made his second and last visit to Italy. He gave a course at the Varenna summer school on Lake Como, 16 lectures on pion and meson physics.[111] Bernard T. Feld so remembered him:

> Here was Fermi at the height of his powers, bringing order and simplicity out of confusion, finding connections between seemingly unrelated phenomena; wit and wisdom emerging from lips, white, as usual from contact with chalk.[112]

Fermi's health started deteriorating already during his trip to Italy. After he was back to Chicago, a first medical check did not reveal anything particular. However a subsequent exploratory surgery left no hope: inoperable stomach cancer. Enrico Fermi died at his Chicago home in the early morning of 28 November 1954.

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[111] Fermi [270].

[112] Introduction to *Lectures on pions and nucleons,* in *CPF II,* p. 1004.

Chapter 2
20th century physics: 1900–1933

The evening allocution of the Royal Institution of Great Britain on Friday April 27th, 1900, was entrusted to one of the most outstanding international scientific figures: Lord Kelvin. His speech was entitled "19th-century clouds over the dynamical theory of heat and light."[1] According to the celebrated physicist, there were two "clouds": the first was the failure of all attempts to detect the "ether wind," while the second was the energy equipartition theorem, whose applications to some physical phenomena were at variance with the empirical data. Kelvin's worries were fully justified, and stressed the importance and urgency of two basic theoretical issues. But these were also the consequences of the amazing scientific achievements of the century that was about to close: Maxwell's electromagnetic theory and Boltzmann's interpretation of thermodynamics in terms of the statistical analysis of molecular motion.

Kelvin's attempts to disperse the clouds originated the two different, albeit amply intersecting, main areas of physical research in the 20th century: relativity theory and quantum mechanics. These two fields of investigation basically correspond to two different scales of phenomena: large speeds and/or hugely massive stellar systems for the first, and the basic constituents of matter for the second. In this chapter we shall reconstruct the historical roots of our present-day scientific knowledge. As we already anticipated, we shall be concerned with global maps, made of shared knowledge and structured by some links that are different from the unstable local dynamics of the various specific investigations. We shall mainly pay attention to the researches on the structure of matter, whose main tool was quantum mechanics, just because this is the area of investigation where Fermi was most active.

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[1] W. Thomson (Lord Kelvin), *Nineteenth Century Clouds over the Dynamical Theory of Heat and Light,* Philosophical Magazine 2 (1901), p. 1.

G. Bruzzaniti, *Enrico Fermi*, Springer Biographies,
DOI 10.1007/978-1-4939-3533-8_2

2.1 The roots of the relativistic program

The theory of relativity is a consequence of the systematization of electromagnetism done by James Clerk Maxwell at the end of the 19th century. Maxwell's celebrated equations raised deep questions whose solution required a radical revision of the (often rather intuitive) concepts underlying the 19th century physics.

The term "Maxwell equations" refers to a set of four equations that resume the properties of the electric and magnetic field. They express the following facts:

a) the way the electric field depends on its sources (the electric charges), namely, that electric charges interact among them with a force which is inversely proportional to the square of their distance;
b) magnetic monopoles (magnetic charges) do not exist;
c) a varying magnetic field produces an electric field;
d) the way the magnetic field depends on its sources (the electric currents), and the fact that a varying electric field produces a magnetic field.

One of the main consequences of Maxwell equations is that they prove the existence of electromagnetic waves, i.e., the fact that electric and magnetic field can propagate in space. One can intuitively understand this considering an oscillating electric charge; it produces a varying electric field that, according to the fourth equation, generates a varying magnetic field, which, as postulated by the third equation, produces a varying electric field, and so on. The surprising fact is that Maxwell's equations allow one to compute the speed c with which electromagnetic waves propagate in space. It is a value close to 185,790 miles per second, and coincides with the experimentally measured value.

It is hard to overestimate the historical importance of Maxwell's work, which eventually clarified the physical nature of light. However one major issue was still quite unclear. Light is a wave propagating with a certain speed; since this statement makes sense only after specifying a reference frame, in which reference frame does light propagate with speed c? At the end of the 19th century this question had a natural answer: ether. To understand the meaning of this mysterious term, we can start from the entry "Ether" written by Maxwell for the 9th edition of the *Encyclopaedia Britannica.* There ether was defined as "a material substance of a more subtle kind than visible bodies, supposed to exist in those parts of space which are apparently empty."[2] It is a rather generic definition, but the ambiguity was unavoidable. Ether played different roles and designated substances whose ontological status would have changed very rapidly. According to Maxwell himself, "æthers were invented for the planets to swim in, to constitute electric atmospheres and magnetic effluvia, to convey sensations from one part of our bodies to another,

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[2] J. C. Maxwell, "Ether," *Enciclopedia Britannica*, 9th ed., VIII (1878). Reprinted in *The Scientific Papers of James Clerk Maxwell,* 2 vols., Dover, New York 1965, II, p. 763.

and so on, till all space had been filled three or four times over with æthers."[3] Some of these supposed features were gradually forgotten, but still the problem of the ether has been very important in the physics of the 19th century, as witnessed by the production of a large number of papers, especially in the years 1871–75 and 1891–95.[4]

Maxwell's impressive construction of the theory of the electromagnetic fields takes place within this scenario. Nowadays we know, as remarked by Feynman, that

> [. . .] what counts are the equations themselves and not the model used to get them. We may only question whether the equations are true or false. This is answered by experiments, and untold numbers of experiments have confirmed Maxwell's equations. If we take away the scaffolding he used to build it, we find that Maxwell's beautiful edifice stands on its own.[5]

Maxwell's scaffolding made reference to a mechanical model of the ether. While this was used more as an illustrative model than a factual explanation, yet one cannot underplay its basic role in Maxwell's research. As written in the above cited article in the *Encyclopaedia Britannica,*

> Whatever difficulties we may have in forming a consistent idea of the constitution of the aether, there can be no doubt that the interplanetary and interstellar spaces are not empty, but are occupied by a material substance or body, which is certainly the largest, and probably the most uniform body of which we have any knowledge.[6]

So the ether, this universal *plenum* which filled the empty space, air and everything else existing in the universe, was regarded as the absolute reference frame, a reference frame distinguished from all others, in which the electromagnetic waves, light, and the radiating heat propagate with the speed of 185,790 miles per second.

Maxwell's deduction of a finite speed of propagation of the electromagnetic waves, which seemed to imply the existence of an absolute reference frame, had a shattering effect on the theoretical framework of the 19th century's physics. It is a common experience that no mechanical experiment can detect a rectilinear uniform motion: if we are on a car or train and move on a straight path with constant speed, everything we do happens exactly the same way as when the car or train stands still. This easy but very important consideration bears the name of "Galilean relativity principle," as it was indeed Galilei who first mentioned it in a famous page, some parts of which deserve to be quoted here:

> Shut yourself up with some friend in the main cabin below decks on some large ship and have with you there some flies, butterflies, and other small flying animals. Have a large bowl of water with some fish in it; hang up a bottle that empties drop by drop into a vessel

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[3] *Ibid.*

[4] Cf. T. Hiroshige, *The ether problem, the mechanistic worldview and the origin of the theory of relativity,* Historical Studies in the Physical Science 8 (1976), p. 3.

[5] R. P. Feynman, R. B. Leighton and M. Sands, *The Feynman Lectures on Physics,* Addison Wesley, California Institute of Technology 1963. Ch. 18, p. 3.

[6] J. C. Maxwell, *op. cit.*

> [with a narrow opening][7] beneath it. With the ship standing still, observe carefully how the little animals fly with equal speed to all sides of the cabin. The fish swim indifferently in all directions; the drops fall into the vessel beneath; and, in throwing something to your friend, you need throw it no more strongly in one direction than another, the distances being equal; jumping with your feet together, you pass equal spaces in every direction. When you have observed all these things carefully (though there is no doubt that when the ship is standing still everything must happen in this way), have the ship proceed with any speed you like, so long as the motion is uniform and not fluctuating this way and that. You will discover not the least change in all the effects named, nor could you tell from any of them whether the ship was moving or standing still. In jumping, you will pass on the floor the same spaces as before, nor will you make larger jumps toward the stern than toward the prow even though the ship is moving quite rapidly, despite the fact that during the time that you are in the air the floor under you will be going in a direction opposite to your jump. In throwing something to your companion, you will need no more force to get it to him whether he is in the direction of the bow or the stern, with yourself situated opposite. The droplets will fall as before into the vessel beneath without dropping toward the stern, although while the drops are in the air the ship runs many spans. The fish in their water will swim toward the front of their bowl with no more effort than toward the back, and will go with equal ease to bait placed anywhere around the edges of the bowl. Finally the butterflies and flies will continue their flights indifferently toward every side, nor will it ever happen that they are concentrated toward the stern, as if tired out from keeping up with the course of the ship, from which they will have been separated during long intervals by keeping themselves in the air.
> [. . .]
> The cause of all these correspondences of effects is the fact that the ship's motion is common to all the things contained in it and to the air also.[8]

If Galilei had known Maxwell's surprising result and had agreed on the existence of ether as an absolute reference frame, his conclusions would have been very different. If we travel on a highway at 40 miles per hour and see a car that is overtaking us at 60 miles per hour, we see it traveling at 20 miles per hour; if the same principle applied to electromagnetic phenomena, and the two travelers on Galilei's ship had measured the speed of a light ray under different conditions of motion of the ship, they would have obtained different results.

In 1881 Albert Michelson made one of the most celebrated experiments in the history of physics, aiming at revealing the Earth's motion with respect to the ether. It was necessary to achieve a very high precision; the point was to measure the effect due to the different traveling times of two light rays, one propagating in an arm of the apparatus parallel to the speed of the Earth, and one in an arm perpendicular to the first. If we denote by v the speed of the Earth, and c the speed of light, both with respect to the ether, theoretical calculations show that the effects are of the second order, i.e., of the same order of magnitude as the ratio v^2/c^2. Since the value of c is about 185,790 miles per second, while v is about 18 miles per second, the precision of the measurement had to be of one part over a hundred millions. That is, the ratio

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[7] Drake translates the Italian "vaso di angusta bocca" into "wide vessel" instead of "vessel with a narrow opening," probably confusing "angusta" with "augusta."

[8] G. Galilei, *Dialogo sopra i due massimi sistemi del mondo.* Translated by S. Drake, *Dialogue Concerning the Two Chief World Systems,* University of California Press, Berkeley, 1953. p. 187.

between the weights of a piece of confetti and a car. The optical apparatus built by Michelson was capable of that performance, but the experiment gave a negative result: the speed in the two directions was the same. After the encouragement of two of the most reputed physicists of the time, Lord Kelvin and Lord Rayleigh, the measurement was repeated in 1887 by Michelson and Morley, but the result was once more negative.

The experiment was redone over and over till 1930, at least 13 times, and the result was always the same. Even with Michelson and Morley's apparatus, the two travelers on Galilei's ship could not have decided if the ship was standing still or was uniformly moving.

2.2 Special relativity

In 1905 Einstein provided his version of the story: the experiments gave a negative result for the simple reason that the ether does not exist. If so, what is the reference frame where the electromagnetic waves have speed c, as predicted by Maxwell's equations? Easy, said Einstein — that's the speed as measured in any inertial reference frame.[9] In other terms, a new law of nature was discovered: the speed of the electromagnetic waves, and so the speed of light, does not depend on the motion of the source.

Einstein's special relativity theory is based on the following postulates.

1. The Galilean relativity principle must hold for all physical phenomena, and not only for the mechanical ones, that is, *all* physical laws have the same form in all reference frames in relative uniform motion.
2. The speed of light in empty space does not depend on the motion of the source or of the receiver, in the sense that it is the same in all reference frames that move uniformly with respect to the source.

The second postulate is highly counterintuitive. We are accustomed to extend the explanation of everyday life's phenomena also to situations that are far away from it, and it seems natural to us that the same operation we make when we evaluate the speed of a car should apply also when something is moving at the speed of light. But this does not seem to be true.

Let us go back to the example with the cars. Let V be the speed of our car along a straight section of a highway, and let v' be the speed of a car that is overtaking us, with respect to us. What is the speed v of the car that is overtaking us? According to classical mechanics, the answer is very easy: $v = V + v' = 40 + 20 = 60$ miles per hour. According to Einstein's relativity theory, the formula we have used is just an approximation of the correct formula, which reads

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[9] An inertial frame is a reference frame where any small body which is far away from any other matter — and is thus free from interactions — moves in a uniform rectilinear motion. The relative motion of two inertial frames is a uniform rectilinear motion.

$$v = \frac{V + v'}{1 + \dfrac{Vv'}{c^2}},$$

where c is the speed of light. The approximation $v = V + v'$ is actually very good, since the correction term Vv'/c^2 is appreciable only for speeds close to the speed of light. In our case indeed its value is 0,000000000000003. If our speed is half the speed of light, and a car overtakes us with the same speed relative to us, then the classical formula gives for the speed of the second car $c/2 + c/2 = c$, while the (correct) relativistic formula yields 0,8 c.

One may wonder what is the rationale of this strange way of summing speeds. This funny formula actually follows from a radical revisitation of the concepts of space and time, which is necessary for the speed of light to be the same in all reference frames. Two other celebrated consequences of this revolutionary approach are the *contraction of lengths* and the *dilation of time.* From a classical viewpoint, the length of a segment and the duration of a time interval do not depend on the state of motion of the observer; according to the theory of relativity, on the contrary, they do depend. So, if ℓ_0 is the length of a segment as measured by an observer for which the segment does not move, and t_0 is the interval of time between two events that for that observer happen at the same position, for an observer in relative motion with speed v, the length will be $\ell = \ell_0/\gamma$, and the interval of time $t = \gamma t_0$, where

$$\gamma = \frac{1}{\sqrt{1 - \dfrac{v^2}{c^2}}}.$$

The correction factor γ is greater than or equal to 1, and increases for bigger values of v; thus the motion of the observer induces a contraction of the lengths and a dilation of the durations. Also these effects depend on the ratio v^2/c^2 and so we do not perceive them in everyday's experience, as the speeds are very small with respect to the speed of light.

Such a drastic change of the concepts of space and time that were at the basis of Newtonian physics required a radical reformulation of the basic laws of mechanics. According to the theory of relativity, also the mass cannot be regarded as an unchangeable characteristic of a body; it is rather a quantity that increases with speed, according to a formula analogous to the previous ones: if m_0 is the mass of a body at rest, when the body moves with speed v, we have $m = \gamma m_0$. This result is a particular case of a more general relation between mass and energy, which is most likely the most extraordinary feature of the theory of relativity: the two quantities, mass and energy, can transform one into the other. If a body absorbs an energy E, its mass increases by an amount m (and, *vice versa,* if the mass decreases by an amount m, an energy E is released) according to the celebrated formula $E = mc^2$. This effect is terribly evident in the atomic bombs. The 20 kiloton bomb dropped on Hiroshima released the same amount of energy as due to the explosion of 20,000 tons of TNT; to get that enormous amount of energy it has been enough to convert into energy just 1 gram of the mass of the fissile material.

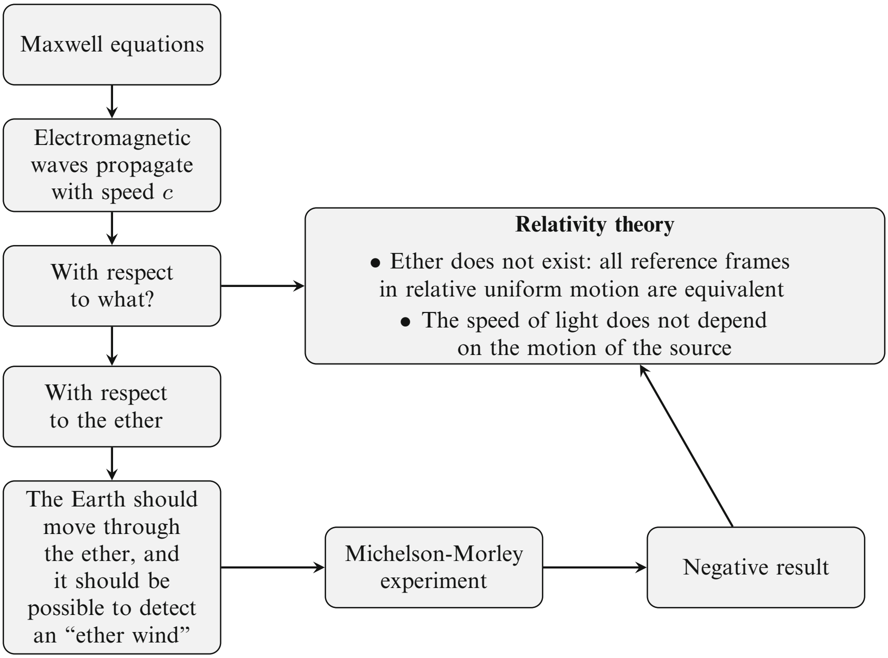

Fig. 2.1 The origins of the theory of relativity.

2.3 A note on global maps

As we have seen, the Michelson-Morley experiment played a central role in the birth of the theory of relativity, as a kind of logical premise to it. We can sketch the logical dependence among the various arguments as in Figure 2.1. One could ask if this is really the path followed by Einstein to formulate his theory; in particular, what was the role of Michelson-Morley's experiment? Specialists are still debating on this issue, and the arguments one can put forward are contradictory. In his original 1905 paper Einstein makes explicit reference — albeit in a few words, and without citing Michelson and Morley's papers — to 'the unsuccessful attempts to discover any motion of the earth relatively to the "light medium."'[10] In other circumstances, when he was interviewed and made some declarations, his attitude was more contradictory.[11] We do not mean here to enter into details about this issue; however, it is worth mentioning this question as it exemplifies very well the difference between the reconstruction of the global maps and of the local research

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[10] H. A. Lorentz, A. Einstein, H. Minkowski and H. Weyl, with notes by A. Sommerfeld, *The principle of relativity; a collection of original memoirs on the special and general theory of relativity*, Dover Publications, New York 1923, p. 37

[11] A. Pais, *Subtle is the Lord,* Clarendon Press — Oxford University Press, Oxford–New York 1982.

itineraries. It is only about the latter that the influence exerted by the Michelson and Morley experiment on Einstein's ideas is a meaningful problem. If we are interested to the reconstruction of the global maps, i.e., the network connecting the objective scientific problems and not the winding paths often followed by the scientists, then the question becomes utterly uninteresting.

2.4 General relativity

It is an extraordinary and fascinating aspect of the scientific enterprise that the most important mechanism of the scientific development is not the solution of existing problems, but rather the identification of new ones; this is the true engine of knowledge. Special relativity is no exception; if Einstein's 1905 theory solved the problems brought up by the Maxwellian synthesis of the electromagnetic theory, it also raised deep questions about one of the most successful theories of our time, Newtonian gravitation. This is expressed by the equation

$$F = G\frac{m_1 m_2}{d^2},$$

where F is the force between two bodies having masses m_1 and m_2 (mass, or to be more precise, gravitational mass, is an intrinsic property of bodies) at a distance d, and G is a constant. From the viewpoint of relativity theory, this equation has a problem: the gravitational force propagates instantaneously, that is, with infinite speed. Suppose that the bodies are located at a distance of several million miles, and that for some unknown reason, the mass m_1 doubles; according to Newton's law, the body with mass m_2 should immediately feel a force twice as big. How can this happen, if the special relativity theory singles out the speed of light c as an absolute upper bound for any speed, be it of a body, or of a signal?

This contradiction was solved by Albert Einstein in 1907. As he tells, while he was sitting at his desk at the Patent Office in Bern, he had the "happiest idea" of his life: he realized that "if a person falls freely he will not feel his own weight."[12] A seemingly simple idea, which however opens the way to the general theory of relativity. Einstein's "happiest idea" is fully formulated as the so-called "equivalence principle," which is the starting point of the general theory of relativity, and is empirically based on the proportionality between the inertial and the gravitational mass (see Appendix C.1). We can understand what that means from Einstein's words:

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[12] There are two sources for this anecdote. The first is an unpublished paper written in 1921, known as the "Morgan Manuscript," which is at the Pierpont Morgan Library in New York. The second is a talk given by Einstein at the University of Kyoto in 1922. The two sentences as reported here are from A. Pais, *op. cit.,* p. 179.

> Imagine a great lift at the top of a skyscraper much higher than any real one. Suddenly the cable supporting the lift breaks, and the lift falls freely toward the ground. Observers in the lift are performing experiments during the fall. In describing them, we need not bother about air resistance or friction, for we may disregard their existence under our idealized conditions. One of the observers takes a handkerchief and a watch from his pocket and drops them. What happens to these two bodies? For the outside observer, who is looking through the window of the lift, both handkerchief and watch fall toward the ground in exactly the same way, with the same acceleration. We remember that the acceleration of a falling body is quite independent of its mass and that it was this fact which revealed the equality of gravitational and inertial mass.
> [. . .]
> We also remember that the equality of the two masses, gravitational and inertial, was quite accidental from the point of view of classical mechanics and played no role in its structure. Here, however, this equality reflected in the equal acceleration of all falling bodies is essential and forms the basis of our whole argument. Let us return to our falling handkerchief and watch; for the outside observer they are both falling with the same acceleration. But so is the lift, with its walls, ceiling, and floor. Therefore: the distance between the two bodies and the floor will not change. For the inside observer the two bodies remain exactly where they were when he let them go. [13]

This thought experiment explains Einstein's idea very well: there is no difference between the results of an experiment made by an observer inside a freely falling elevator, and those obtained by a hypothetical observer located in an identical elevator that is standing still in some region of the intergalactic space, so far form any other body that it feels no gravitational force. Moreover, let us imagine that the elevator that is isolated in space is hauled by a rope attached to its ceiling, so that it moves with an acceleration of 32.2 feet per squared second, the same of a falling object near the Earth's surface. If the observer in the elevator stands on a scale, she would read the same weight as if the elevator was standing still on the Earth's surface, and she would see the objects fall with the same acceleration as objects fall in the elevator at rest on the Earth's surface. In other words, the observer has no way to know if she is under the action of the terrestrial gravitational field, or she is accelerated by a force.

One should remark that this reasoning would not be valid if the inertial mass was not the same as the gravitational mass (better said, if they were not proportional). If that was not the case, indeed, the observer could detect the presence of a gravitational field by noting that different bodies fall with different accelerations. Let us also remark that the size of the elevator cannot be too big. As Einstein suggests, let us think of an imaginary elevator which extends from the North Pole to the Equator. In this situation the handkerchief and the watch, if they were located one at the North Pole and the other at the Equator, would fall with different accelerations — since the gravitational fields at the North Pole and the Equator are different — allowing the observer to detect the presence of gravity.

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[13] A. Einstein, L. Infeld, *The Evolution of Physics,* Cambridge University Press, London 1938, pp. 226–227.

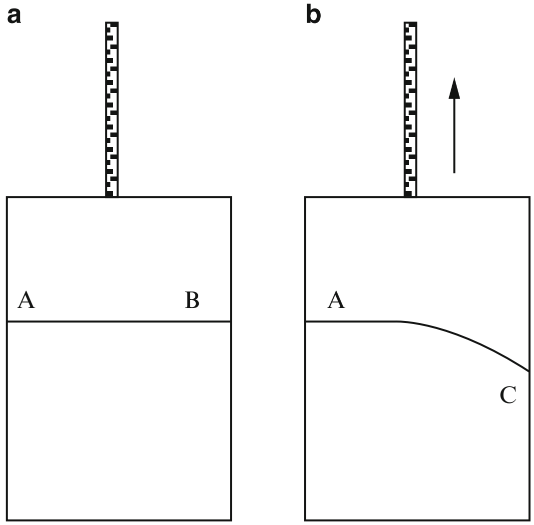

Fig. 2.2 The elevator in (**a**) is standing still in a gravitational field. The one in (**b**) is not subject to a gravitational field but is acted on by a force which accelerates it upward.

This "local equivalence principle" between gravitational fields and accelerated reference frames is the basis to extend the relativity principle: the physical law not only must have the same form in reference frames in relative uniform motion — they must be the same in *all* reference frames.

Let us analyze another thought experiment, and let us have a look at Figure 2.2. The elevator in (a) is at rest in a gravitational field, while in (b) the elevator is isolated in space and moves with an accelerate motion, dragged by a force applied to the ceiling by a rope. If the equivalence principle holds true, the observers inside the elevators cannot distinguish between the two situations. Think now that a light ray enters the elevator at the point A. How is this fact described in the two situations? At first it seems there is a violation of the equivalence principle. Indeed the observer in (a) sees a light ray moving along a straight line, entering the elevator in A and hitting the opposite wall in a point B exactly opposite to A, while (b) sees a curvilinear motion, since while the ray crosses the elevator, the latter has moved up.

But Einstein objected that this reasoning hides a serious mistake: also the observer (a) sees a light ray moving along a curvilinear path. Indeed light carries energy, hence has a mass, and is therefore subject to the action of the gravitational field, so that its trajectory curves toward the bottom of the elevator. There is no way to distinguish between the situations (a) and (b), and the equivalence principle holds.

This analysis is subject to experimental observation; it is enough to measure the position of the stars in the presence or the absence of the Sun. If the equivalence principle holds true, when the starlight passes near the Sun, it must be deflected; it should therefore be possible to detect a difference in the position of the stars according to whether their light grazes the Sun surface or not. In normal conditions this observation is impossible, as the strong solar light hides the stars, but it can be done during a total eclipse, when the Sun is present, but is obscured by

Moon, and the starlight which grazes the Sun surface is visible. On 29 May 1929 two British expeditions, one to Brazil led by Andrew Crommelin and one to St. Thomas and Prince, led by Arthur Eddington, made the measurement, during a total eclipse which was particularly suitable for the observations. The analysis of the photographic plates took a long time, but eventually on 6 November, an announcement was made at a common meeting of the Royal Society and the Royal Astronomical Society: “A very definite result has been obtained, that light is deflected in accordance with Einstein’s law of gravitation.”[14] The title at page 11 of the November 7 issue of *The Times* read “Revolution in science/New theory of the universe/Newtonian ideas overthrown.”[15] Einstein’s fame spread all over the world, and he became a myth and a legend.

Why all this fuss? Why Newton’s conceptions had been demolished? If light carries energy and therefore, in accordance with the special theory of relativity, it also carries mass, why the classical theory is not enough to explain that light is deflected by a gravitational field? Actually this is what happens, but the problem is that the Newtonian theory only accounts for exactly half the deflection predicted by Einstein’s theory. The plates made by the expeditions in Brazil and Guinea left no room for doubt: the measured deflection agrees with Einstein’s prevision. But what is the “revolution” announced by *The Times*? Special relativity had already radically changed the standard conceptions of space and time. General relativity gave another blow to common sense, showing that the space and time intervals not only depend on the motion of the observer, but also on the location where the measurement is made. General relativity shows that the physical space is not “flat,” i.e., it is not Euclidean, and that *the gravitational force is due to space-time curvature.*

The notion of space-time curvature is the true link between the equivalence principle and general relativity. To understand this point let us once more follow Einstein. Imagine a great disc (Fig. 2.3) on which two concentric circles have been drawn, one very small and the other very large. In our reference frame the disc is rapidly rotating. We use a rule to measure the lengths of the radii and the circumferences of the two circles, and find the well-known value 2π. What would be the result obtained by an observer on the disc? We assume she would be using the same rule. If classical mechanics — according to which the lengths and the time interval do not depend on the motion of the observer — was right, she would find exactly the same result. But as prescribed by the theory of relativity, there is a phenomenon of contraction of lengths. This effect depends on the speed, and to be more precise, only on the projection of the velocity along the direction of the length we measure. So the observer on the disc finds the same values of the radii, as they are orthogonal to the velocity of the rotating frame, while for the circumferences she finds different values, since the length of the rule has undergone a contraction. Actually for the small circumference she finds a value practically identical to ours, since the speed in this case is small (as the small circumference is not far for the

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[14] A. Pais, *op. cit.,* p. 305.

[15] A. Pais, *op. cit.,* p. 307.

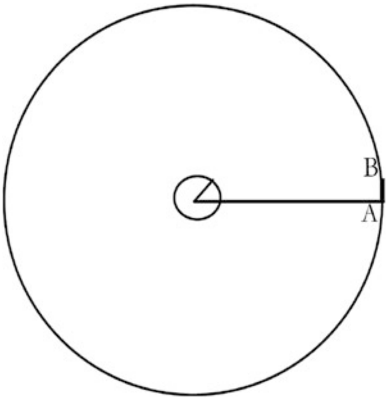

Fig. 2.3 The observer on the rotating disc uses the rule AB to measure the radii and the circumferences of the two circles.

center of rotation), while for the bigger circumference she finds, if the radius is big enough, an appreciably greater value. As a consequence, the ratio between the circumference and its radius, as measured by the observer on the disc, is no longer 2π, but rather a greater number, which increases with the value of the radius. This is a stunning result: all points in the bigger circle have the same distance from the center, as in any circle, yet the ratio between the length of the circumference and the distance of its points from the center is greater than 2π. One of the most celebrated and indisputable results of the Euclidean geometry seems to be no longer valid.

The remark that an accelerated observer has a different perception of the geometry of the physical space and of time, together with the equivalence principle, directly leads to general relativity. Indeed, if the equivalence principle holds, what happens in an accelerated frame of reference should also happen to an observer who is at rest but is acted upon by a gravitational field. Thus the presence of matter modifies the geometry of the space-time continuum; the distortions of the space and time intervals between two events depend not only on the state of motion of the latter, but also on their position with respect to matter. After three centuries the mechanism of gravity was understood: it is the curvature of space-time.

The mathematics of general relativity is very complicated, and it took many years for Einstein to reach a satisfactory formulation of the theory: from 1907's "happiest idea" to 1916, when the fundamental paper, containing the equations that relate the curvature of space-time to the matter distribution, was published.[16] It is not easy to talk about Einstein's equations without resorting to the mathematical formalism, whose conceptual content can be hardly visualized in an intuitive way. We are in a way like the inhabitants of Flatland (see Appendix C.2), who cannot conceive and describe an event involving a three-dimensional object (the arrival in

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[16] A. Einstein, *Die Grundlagen der allgemeine Relativitätstheorie,* Annalen der Physik 49 (1916), pp. 769–822. English translation: *The Foundation of the General Theory of Relativity,* in H. A. Lorentz, A. Einstein, H. Minkowski and H. Weyl, *The Principle of Relativity; a Collection of Original Memoirs on the Special and General Theory of Relativity,* Dover, New York 1952, p. 109.

their world of a sphere); in the same way, our intuition is unable to represent four-dimensional objects. We can however resort to some simplifications; for instance, we can neglect the time dimension, and consider a two-dimensional universe. In the absence of matter, there is no difference between the Newtonian and the Einsteinian pictures; in both cases, space is flat and its geometry is Euclidean. However, what happens if space contains a massive object? Using a very common imagery, it is like placing an iron ball on a stretched fishing net, which then undergoes a deformation; a heavier ball will produce a larger deformation. Thus, the relativistic picture of the phenomenon of gravitation is radically different from the Newtonian one. In Einstein's conception there is no room for the "gravitational force," which is replaced by the space-time curvature. Material bodies do not interact by means of a mysterious force, which is transmitted at infinite speed in a space which is just the infinite and inert container of all things existing, but rather by way of the reciprocal deformation of the space-time weave that each body generates in the point where the other is located.

Also the description of the way that matter is set in motion by gravitation is changed. For instance, what determines the trajectories of the planets in the solar system? According to both Newtonian mechanics and general relativity, in the absence of interactions the bodies move along straight lines. But in the presence of matter, and therefore in a space-time with a non-flat geometry, the straight lines are no longer the usual ones, but rather curves, called *geodesics.* The trajectories of the planets in the solar system are exactly the projections of these space-time curves onto the three-dimensional physical space.

The mass-induced space-time curvature and the motion of bodies along geodesics are two basic and tightly interrelated aspects of general relativity. As stated by the famous physicist John Wheeler, "mass grips space by telling it how to curve, space grips mass by telling it how to move."[17]

The imagery of the fishing net can also help us to deal with the contradiction between the instantaneous propagation of the gravitational force and the upper bound to the speed given by c. According to general relativity, the variation of a mass produces a perturbation which propagates in space exactly as the perturbation produced by throwing an iron ball on a fishing net propagates through it. And the speed of propagation of the perturbation, as computed by Einstein, is exactly c.

2.5 The quantum program

The history of quantum mechanics is long and tortuous. The "quantum of action" was introduced in 1900 by Max Planck, but it was not before 1925–27 that a rather heterogeneous complex of ideas and conjectures coalesced into a coherent formalism called "quantum mechanics." In contrast to the theory of relativity,

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[17] In B. Greene, *The Elegant Universe,* W. W. Norton & Company, New York–London 1999, p. 85.

quantum mechanics was not the result of the efforts of a single scientist, but rather the implementation of a program to which many researchers contributed, coming from different schools. It was actually the birth of a new research style.

In the early 20s Niels Bohr reunited in his famous Institute in Copenhagen the best talents of theoretical physics, coming from many countries: Wolfgang Pauli, Werner Heisenberg, Lev Davidovič Landau, Paul Ehrenfest, Oskar Klein, Hendrick Kramers, George Gamow, and many more. It was the first example of a new approach to theoretical research, which, from a solitary pursuit, became a collective enterprise. Quantum mechanics was the product of the common efforts of these scientists. Its birth did not take place in a precise moment, in a rigorous and definitive form, and was not the resolutive answer to a specific, well-defined problem. The formulation of the quantum theory was rather a complicated and complex process, and its interpretation has given rise to heated disputes. One should also remark that quantum mechanics was born and developed together with atomic and nuclear physics, providing the language and the concepts to be used to study the structure of matter. According to Victor Weisskopf, a most authoritative physicist of that period:

> [Quantum mechanics] has been a leap into the unknown. With it we enter a world of phenomena which cannot be described in terms of the physics of the previous century. To construct and develop it, it has been necessary to look for new formulations and ways of thinking. It has opened the way to the understanding of the world of atoms and molecules, with its discrete quanta of energy and its spectra and chemical bounds. One can say that the beginning of this century has seen a radical change in the nature of the physical theory, and this change took place with quantum mechanics.[18]

In a very schematic way, one can divide the birth of the quantum theory into three stages, that also correspond to three levels of the investigation on the structure of matter: the first stage *(Old Quantum Theory)* is the birth of atomic physics; the second (quantum mechanics) is the development of atomic physics and the birth of nuclear physics; and the third (quantum field theory) is the beginning of elementary particle physics. In this chapter we shall reconstruct the global maps corresponding to the first two stages of development of the quantum program.

2.6 From radiation physics to atomic physics

"The foundation stones of the material universe [. . .] continue this day as they were created — perfect in number and measure and weight."[19] James Clerk Maxwell wrote these words at the end of the 19th century, referring to the atoms, the elementary constituents of matter. However, this was just a rough image, still rooted

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[18] Translated from the French original, V. Weisskopf, *La révolution des quanta,* Hachette, Paris 1989. p. 20.

[19] J. C. Maxwell, *A Discourse on Molecules,* Philosophical Magazine, 46 (1873), p. 468.

in a naive interpretation of the physical experience. The term "atom" indeed referred to indivisible entities that displayed at the microscopical level the observed macroscopic properties of matter. This is proved, for instance, by the criterion used to order the elements in the periodic table; as in common experience weight is one of the main properties of the material bodies, in Mendeleev's table the chemical elements were ordered by increasing atomic weight.

However, starting in the first few years after 1895, the 19th century conception of matter began crumbling. The ontological status of the atom changed radically: from being an unchangeable entity, it became a complex structure that can be split into its elementary constituents. The starting point of this process can be traced to what was at the time a very active field of the research, namely, radiation physics, and in particular the discovery of the cathodic rays, the X-rays and radioactivity.

Figure 2.4 summarizes the historical process that led from radiation physics to the investigations on the structure of matter based on the conception of a nuclear atom. The discovery of cathodic rays in the mid 19th century set off a lively controversy about their nature. The strange rays emitted by the cathode of a discharge tube when a deep vacuum was created in it were waves or particles? This question was just one element in a complicated conceptual scheme, which also touched even deeper issues about the relations between electricity and matter and the concept of ether. However, before the controversy about the cathodic rays could be solved, the scientific community was struck by two surprising discoveries. In 1895 Wilhelm Röntgen announced that the glass wall of a discharge tube opposite to the cathode, when hit by the cathodic rays, emits other rays, of an unknown nature (and indeed called "X-rays").[20]

These rays were able to cross bodies that are opaque to ordinary light, and to impress a photographic plate. According to the apt definition by Louis Olivier,[21] the photography of the invisible was born.

In 1896 Henri Becquerel[22] was investigating the relations between the phosphoresce of the natural substances and the X-rays. This was motivated by the fact that the emission of the X-ray was localized exactly in the area of the discharge tube that became phosphorescent. He discovered that minerals containing uranium emit

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[20] W. Röntgen, *Über eine neue Art von Strahlen,* Sitzungberichte der physikalische-medikalische Gesellschaft Würzburg, December 1895, p. 132. This was followed by a second communication, published in 1896 in the same journal. The two papers were partially translated in English in *Nature* 53 (1896). The original text with a translation into English was also published in E. C. Watson, *The discovery of X-rays,* American Journal of Sciences 13 (1945), No. 5, pp. 281–291.

[21] L. Olivier, *La photographie de l'invisible,* Revue général de sciences pures et appliquées 7 (1896), No. 49, p. 2.

[22] H. Becquerel, *Sur quelques propriétés nouvelles des radiations invisibles émises par divers corps phosphorescents,* Comptes Rendus de l'Académie des Sciences 11 (1896), p. 559; *Sur les radiations invisibles émises par les corps phosphorescents,* ibid., p. 501; *Sur les propriétés différentes des radiations invisibles émises par les sels d'uranium, et du rayonnement de la paroi anticathodique d'un tube de Crookes,* ibid., p. 762.

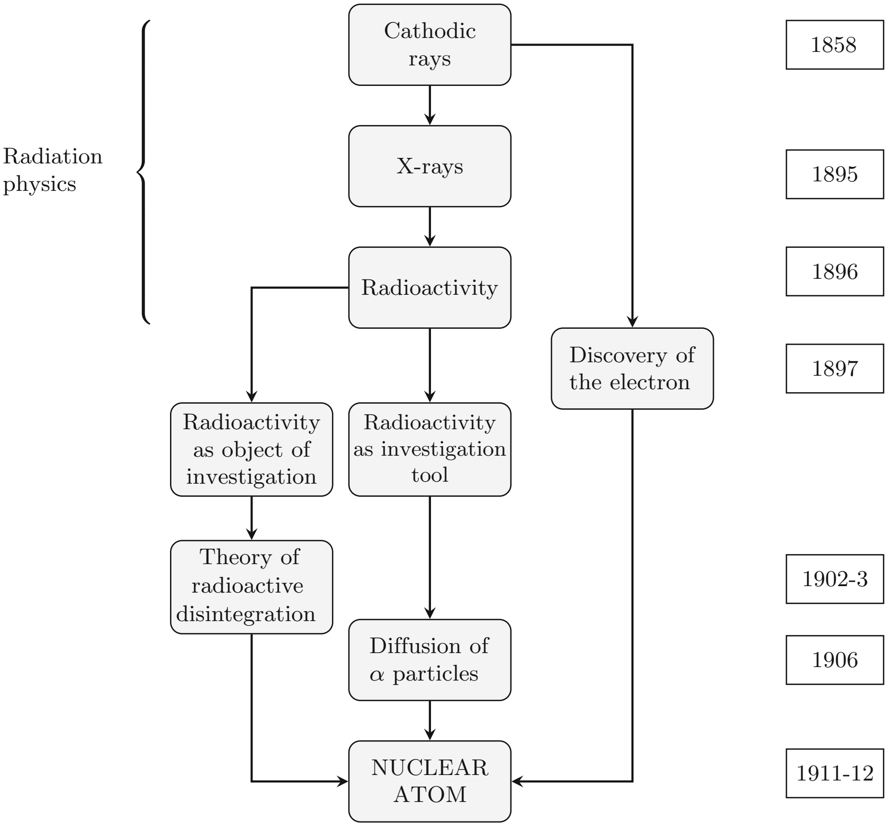

Fig. 2.4 A sketch of the line of research leading from radiation physics to the concept of nuclear atom.

penetrating radiations, having similar effects to the X-rays. Thus there were three different radiations of unknown nature: cathodic rays, X-ray, and uranium rays.[23]

The years between 1897 and 1903 were crucial. During that time span the mystery of the new radiations was deciphered; the nature of the cathodic and X-rays was understood, and to some extent, also that of the uranium rays. But more than that, during that period a process started, that would lead to a radical redefinition of the concept of atom. The main features of that process can be summarized in four stages:

1. In 1897 J. J. Thomson solved the problem of the cathodic rays by showing, beyond any doubt, that they are deflected by the electric and magnetic fields,

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[23] This historical period has been analyzed in detail in G. Bruzzaniti, *Dal segno al nucleo* [From sign to nucleus], Bollati Boringhieri, Torino 1993.

as only the electrically charged particles do. The most significant success of Thomson however was the measurement of the mass/charge ratio of the particles that form the cathodic rays.[24] The result was surprising. In Thomson's words: "its value 10^{-7} is very small compared with the value 10^{-4}, which is the smallest value of this quantity previously known, and which is the value for the hydrogen ion in electrolysis." This fact, supported by some results by Eduard Anton von Lenard (an important German experimental physicist), and most of all, by the measurements of John Sealy Edward Townsend (a bright student of Thomson's at the Cavendish Laboratory), which showed the equivalence between the charge carried by the cathodic rays and the hydrogen ions in electrolysis, allowed Thomson to assign to the particles forming the cathodic rays a mass of about 1/1700 of the mass of the hydrogen atom. The electron had been discovered.

2. In 1899 Marie Slodowska Curie discovered that thorium displays an activity similar to that of uranium.[25] In the same year, together with her husband, she isolated two new elements which were much more active of uranium and thorium: polonium and radium. This discovery[26] confirmed a very bold hypothesis they had made, namely, that radioactivity is the manifestation of a property of the atom.
3. Between 1898 and 1902 a series of investigations made by Ernest Rutherford,[27] and also by Becquerel and Villard, clarified the nature of the radiation emitted by the radioactive substances; they were of three kinds: α, β, and γ rays. The first were particles, with twice the electric charge of the electron, and four times the mass of the hydrogen atom. β rays were electrons, and the γ rays were electromagnetic radiation of very high frequency.
4. Between 1902 and 1903, Rutherford and Soddy advanced the first complete theory on the causes of the radioactivity, which was supposed to be a manifestation

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[24] J. J. Thomson, *Cathode Rays,* Philosophical Magazine, 44 (1897), p. 310.

[25] M. Slodowska Curie, *Rayons émis par les composés de l'uranium et du thorium,* Comptes Rendus de l'Académie des Sciences 126 (1896), p. 1101.

[26] P. Curie and M. Curie, *Sur un substance nouvelle radio-active contenue dans la pechblende,* Comptes Rendus de l'Académie des Sciences 128 (1898), p. 175; E. Demarçay, *Sur le spectre d'une substance radio-active, ibid.,* p. 128.

[27] E. Rutherford, *Uranium radiation and the electrical conduction produced by it,* Philosophical Magazine 47 (1899), p. 109. Reprinted in J. Chadwick (ed.), *The Collected Papers of Lord Rutherford of Nelson,* 3 vols., Allen & Unwin, London 1962–1965, vol. 1, p. 1169 (henceforth we shall refer to this work as *CPR*). Rutherford had the merit of detecting the α and β components of the uranium radiation, and discovering in 1903 the particle nature of the α rays (*The magnetic and electric deviation of the easily absorbed rays from radium,* Philosophical Magazine 5 (1903), p. 177). Henri Becquerel on the other hand was the first to identify the β rays with electrons (*Influence d'un champ magnétique sur le rayonnement des corps radio-actifs,* Comptes Rendus de l'Académie des Sciences 129 (1899), p. 996; *Sur le rayonnement des corps radio-actifs,* ibid., p. 1205). Paul Villard, finally, detected in the radiation emitted by radioactive bodies a component that is not deflected by the electromagnetic fields: the γ rays (*Sur le rayonnement du radium,* ibid., 130 (1900), p. 1178).

> of the disintegration of the atom.[28] In this connection the discovery of the corpuscular nature of the α radiation was of paramount importance. According to this theory, the atom of a radioactive substance that emits α and β rays undergoes a disintegration which transforms it into another chemical element. The latter can in turn disintegrate, emitting more α or β particles. One of the strongest tenets of the 19th century physics, the immutability of the chemical elements, was thus disproved.

Transformation theory — as the Rutherford-Soddy theory was called — and the discovery of the electron played a fundamental role in the investigations about the structure of matter, radically changing the image of the atom, which became a complex structure, formed by many components. This point deserves to be stressed. For the first time, albeit in an embryonal and quite implicit form, a regulating principle was introduced, that was to govern the first steps of nuclear physics: if a particle is emitted by some (nuclear or atomic) structure, then it is a constituent of that structure. This principle guided the investigations on nuclear physics and stood against the strongest anomalies, till it was demolished by Fermi's theory of the β decay.

Starting with 1903 research on radioactivity went along two different lines, which were correlated, but also independent as far as their methods and aims were concerned. On the one hand, there was radioactivity as an object of investigation *per se*, in relation to the changes of the concepts of atom and chemical elements that it induces. On the other hand, radioactivity played the role of a research tool; a radioactive substance was regarded just a source of rays. In this case the emission was not considered as a problem, and was studied only in relation to the interaction between radiation and matter. Within this second project, in 1906 the *scattering* of α particles was discovered: a very important phenomenon for the understanding of the structure of matter.

The first evidence of that phenomenon was observed by Becquerel, who noticed that a beam of α particles leaves different tracks on a photographic plate according to whether it is propagating in the vacuum or in the air. Rutherford[29] guessed that the α particles hit the atoms in the air and deviated from their rectilinear path. Rutherford's hypothesis was carefully checked by Geiger in 1908[30] by letting the α particles go through solid bodies, such as metals. The most surprising and unexpected effect however was obtained by Geiger and Marsden in 1909;[31] on average, one out of 8000 α particles hitting a gold plate was deflected by an angle bigger than 90°. This result could not be explained within the most popular

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[28] E. Rutherford and F. Soddy, *Radioactive change,* Philosophical Magazine 5 (1903), p. 576.

[29] E. Rutherford, *Some properties of the α rays from radium,* ibid., 11 (1906), p. 166; *Retardation of the α particle from radium in passing through matter,* ibid., 12 (1906), p. 134.

[30] H. Geiger, *On the scattering of the α particles by matter,* Proceedings of the Royal Society A 81 (1908), p. 174.

[31] H. Geiger and E. Marsden, *On a diffuse reflection of the α particles,* ibid., 82 (1909), p. 495.

atomic model of those years, proposed by Thomson in 1904, according to which the atom is formed by a uniformly charged sphere, with the electrons in its interior in some equilibrium configuration, like raisins in a cake.[32] The explanation of Geiger and Marsden's 1909 results is the content of two important papers published by Rutherford in 1911.[33]

As Rutherford wrote, "In order to explain these and other results, it is necessary to assume that the electrified particle passes through an intense electric field within the atom." Thus inside the atom there must be an electric charge, distributed over a very small volume, so that a very intense electric field is created. The atom became a structure formed by a central charge $\pm Ne$, where e is the charge of the electron, surrounded by an electric charge of the same amount but opposite sign, so that the whole atom is electrically neutral.

Rutherford's aim was to account for the electric — as opposed to the mechanical — structure of the atom. For this reason, it would not be correct to see his 1911 model as the birth of the nuclear atom, and in particular, of the concept of nucleus. Indeed, in his 1911 papers Rutherford never used the term "nucleus," but rather wrote "central charge"; moreover, in his model the central charge was surrounded by a spherical distribution of charge, uniformly spread over the entire volume of the atom.[34] While the importance of Rutherford's idea in this context is undeniable, however, the notion of nucleus originated from the confluence of various lines of research, as illustrated in Figure 2.4. Starting from very different considerations, around 1921 André Louis Debierne tackled the problem of the atomic structure from the viewpoint of the radioactive phenomena.[35] Every radioactive substance is characterized by a constant, usually denoted λ, which expresses the time that the substance takes to transform; it is in a way the substance's ID. The constant is unchangeable, even under extreme physical and chemical treatments, such as heating at very high temperatures; its value is independent of any external factor. As Debierne writes:

> One can however conclude that the infinitesimally small particle that we call atom is a very complex system. It is not only formed by electric charges moving in a more or less regular way. It must be formed by two quite distinct parts. The first region is the external part and manifests itself in many ways (electromagnetic radiation, molecular bounds, etc.); it is sensitive to the actions from the exterior (magnetic fields, electric discharges, etc).

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[32] J. J. Thomson, *On the structure of the atom: an investigation of the stability and periods of oscillation of a number of corpuscles arranged at equal intervals around the circumference of a circle; with application of the results to the theory of the atomic structure,* Philosophical Magazine 7 (1904), p. 237.

[33] E. Rutherford, *The scattering of the α and β rays and the structure of the atom,* Memoirs of the Literary and Philosophical Society of Manchester, 55 (1911) (*CPF II,* p. 212); *The scattering of α and β particles and the structure of the atom,* Philosophical Magazine, 21 (1911) p. 669 (*CPR II,* p. 238).

[34] For a detailed analysis of this issue see G. Bruzzaniti, *op. cit.*

[35] A. L. Debierne, *Sur les transformations radioactives,* in *Les idées modernes sur la constitution de la matière,* Gauthier-Villars, Paris 1913.

> The regular movements of the electric charges take place in this region. The second region is so to say inaccessible; thanks to an unknown process, it is shielded from the external physical agents; it must contain some elements in a steady state of disordered agitation, and should be responsible for the gravitational phenomena. The volume of this inner part is most likely very small with respect to the total volume of the atom, so that atoms can be hit by external bodies and also be crossed throughout by a projectile without any influence on the nucleus. The presence of the latter is revealed when, due to the disordered internal agitation, a violent explosion takes place. This picture of the atom is similar to that of a planet whose atmosphere occupies a much bigger volume than the solid or liquid mass. The atmosphere is sensitive to the outer agents and is where the phenomena that are perceptible from the outside take place. The internal mass manifests itself in a tangible way only in the occasion of a cataclysm or volcanic eruption.[36]

At the beginning of 1913 the image of the atom had been substantially reshaped. Now it referred to a region of space of the dimension of about 10^{-8} cm, having a central nucleus of a radius of about 10^{-10} cm. The latter is responsible for the radioactive phenomena and accounts for the whole mass of the atom. The electrons, necessary to make the whole structure electrically neutral, orbit around the nucleus. There is however a big, unresolved problem: the stability of this structure. Indeed, an oscillating charge emits electromagnetic waves; this is the mechanism at the basis of radio and TV broadcasts. The circuits and the antennas of the transmitter stations contain charges oscillating with a certain frequency. The charges radiate electromagnetic waves that carry the signal. From the point of view of classical electromagnetism, the electrons around the nucleus look like a small transmitter that radiates electromagnetic waves of very high frequency — the frequency of revolution of the electrons around the nucleus. How is it possible that the electrons orbit around the electrically charged neutron without collapsing on it, since during their motion they emit energy? According to classical physics, an atom of this kind would live for one hundred-millionth of a second.

2.7 Atomic models and "Old Quantum Theory"

The problem of the stability of the atom was solved in 1913 by Bohr, who applied the hypothesis of the quantum of action to the nuclear model. This is a typical "confluence process," in which different lines of research merge and give rise to a new theory.[37] Using Bohr's words:

> In an attempt to explain some of the properties of matter on the basis of this atom-model [the nuclear model] we meet, however, with difficulties of a serious nature arising from the apparent instability of the system of electrons [...] Whatever the alteration in the laws of motion of the electrons may be, it seems necessary to introduce in the laws in question a quantity foreign to the classical electrodynamics, i.e., Planck's constant, or as it often is called the elementary quantum of action [...] This paper is an attempt to show that the

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[36] *Ibid.,* p. 331.

[37] Cf. G. Bruzzaniti, *op. cit.*

> application of the above ideas to Rutherford's atom-model affords a basis for a theory of the constitution of atoms.[38]

The introduction of Planck's constant (see Appendix C.4) played here a fundamental role, analogous to that of the speed of light c in the theory of relativity.[39] The two constants set strict bounds on the validity of the 19th century theories, and delimited what nowadays is called "classical physics." Planck's original idea was that the process of emission and absorption of electromagnetic radiation takes place in a discrete way, by means of *quanta* of energy. However the electromagnetic radiation was still regarded to be continuous: only the emission and absorption processes were considered to have a new nature. George Gamow, a distinguished physicist and skilled popularizer of the last century, gave a nice picture of this state of affairs:

> Radiation is like butter, which can be bought or returned to the grocery store only in quarter-pound packages, although the butter as such can exist in any desired amount.[40]

It is Einstein's merit to have first used the quantum hypothesis to deduce the corpuscular nature of light. This first manifestation of the wave-particle duality appeared in 1905 in a paper devoted to the interpretation of the photoelectric effect (cf. Appendix C.4). Wave-particle duality for the electromagnetic radiation, i.e., the coexistence of antithetical properties in the same physical system, played an important role in the physics of that time. The research that Arthur Compton made in 1923[41] (see Appendix C.4) confirmed the idea that radiation has particle-like properties; that happened just before Louis de Broglie's contribution, which, by extending the wave-particle dualism to matter, opened the way to quantum mechanics.

Planck's hypothesis had a huge influence on the investigations on the structure of matter. If electromagnetic energy is quantized, why the same should not be true also for mechanical energy? With this ingenious idea Bohr solved the problem of stability. There are in the atom orbits where an electron can stay without irradiating energy; an electron radiates only when it "jumps" between two orbits, and then it emits a quantum of energy. Here is how Fermi described Bohr's idea in a talk, most likely addressed to high school teachers, which was published in 1925.

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[38] N. Bohr, *The constitution of atom and molecules,* Philosophical Magazine 26 (1913), p. 1.

[39] Bohr stressed that, by dimensional reasons, to give account of the existence of electronic orbits whose radii have the same order of magnitude as the atom, it was necessary to introduce a new constant: "By the introduction of this quantity the question of the stable configuration of the electrons in the atoms is essentially changed, as this constant is of such dimensions and magnitude that it, together with the mass and charge of the particles, can determine a length of the order of magnitude required." *Ibid.*

[40] G. Gamow, *Thirty Years that Shook Physics,* Dover, New York 1966. pp. 22–23.

[41] A. H. Compton, *The spectrum of scattered X-rays,* Physical Review 22 (1923), p. 409.

> The two basic principles of quantum theory are the following:
>
> a) the motion of an atomic system is a mechanically possible motion (i.e., it can be computed by using only the three laws of mechanics and the Coulomb law); however, not all mechanically possible motions actually take place, but only a discrete sequence of them, called quantum or static motions; so that in particular the energy w of the system can only take a discrete sequence of values $w_1, \ldots, w_n$.
> b) As long as the system moves according to a static motion there is no irradiation of energy (contrary to the results of classical electrodynamics). The irradiation of energy is always due to the non-mechanical jump or an electron between two quantum motions. If w and w' are the energies of the two motions, the radiated energy will be $w - w'$, and one assumes that it will be radiated in only one quantum. So the energy will be radiated with the frequency $\nu = (w - w')/h$.
>
> These two principles can be by now considered as being experimentally proved.[42]

In the perspective of the reconstruction of a global map, the contributions by Planck, Einstein, and Bohr can be considered as the basic elements of what nowadays we call *Old Quantum Theory* (henceforth shortened into *OQT*). It can be dated between 1913 and 1924 and formed the ground over which quantum mechanics would have been built. The succession of events that led to *OQT* and quantum mechanics is sketched in Figure 2.5.[43]

As it happens with all theories that mark radical changes from the received view, also *OQT* at the beginning was tightly linked with the existing theory, that is, classical mechanics. The quantum aspects of the theory were the result of suitable quantization rules superimposed to a conceptual structure still based on classical physics. In an attempt to provide a sounder basis to the theory, several researchers, including Bohr and Einstein, looked for more general principles. In the talk cited above, Fermi expressed the following point of view.

> Obviously, [the two postulates] are not enough to solve all the problems of atomic physics; therefore they need to be integrated with the solutions of other problems, which however are for the moment incomplete and uncertain. In this talk I will make an attempt to explain how one can try to find a systematic solution to some of these problems. In particular we shall deal with the following questions:
>
> a) What are the rules for choosing the quantum motions among all mechanically possible motions. Evidently a complete solution to this problem would be of paramount importance, since it would allow one to use [a formula previously written] to compute all frequencies that the atom under consideration can emit, namely, to completely solve the problem of determining theoretically all spectral lines.
> b) What is the probability that, at a given temperature, an atom will be in the state corresponding to a specified mechanically possible motion.
> c) What is the probability that an atom in a given quantum state during a time interval dt will pass to another given state.

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[42] E. Fermi [22], *CPF I,* p. 138.

[43] From an idea of A. Pais, *op. cit.,* p. 385.

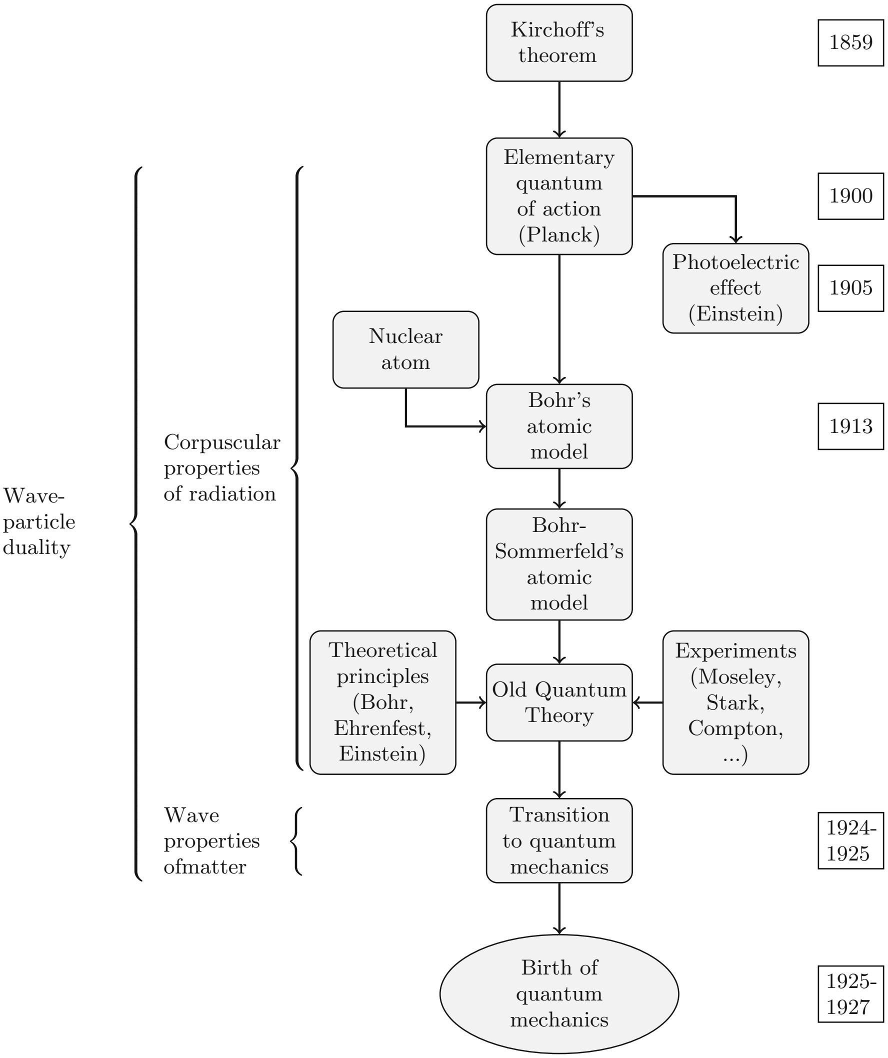

Fig. 2.5 Development of the *OQT* and main lines of influence in the birth of quantum mechanics.

> These three problems can be at least partially solved by resorting to some general principles, in particular to Ehrenfest's adiabatic principle and the correspondence principle.[44]

Bohr's correspondence principle, Ehrenfest adiabatic principle (see Appendix C.10), and Einstein's introduction of statistical elements were the main attempts to give *OQT* a sounder theoretical foundation, and this was accomplished, especially

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[44] E. Fermi [22], *CPF I*, p. 139.

in the first two cases, by anchoring it to the conceptual structure of classical physics. The correspondence principle, which was given this name only in 1920,[45] was introduced by Bohr in its primitive form in 1913, at the end of the third paper of his trilogy devoted to his celebrated atomic model.[46] It is a strong constraint on the new theory, which, in the limit of large orbit and large masses, must reproduce the results of classical mechanics. Ehrenfest's adiabatic principle was published in its definitive form in 1917.[47] It allowed the determination of the quantities of a physical system that are quantized, i.e., those that can only assume a discrete series of values (more details can be found in Appendix C.10). Einstein's attempt to strengthen the theoretical foundations of the *OQT* started in 1916, as shown by a letter written to his dear friend Michele Besso on November 18: "A splendid light has dawned on me about the absorption and emission of radiation."[48] Einstein hypothesis, published in 1917 after a preliminary version of the previous year,[49] establishes a deeper connection between Bohr's and Planck's hypotheses.[50]

OQT's experimental foundations were provided by a wealth of data about atomic spectra. Every atom, if heated at a certain temperature, emits a characteristic radiation. A standard example is provided by the light emitted by table salt (which contains sodium) or by a calcium salt: yellow for sodium, brick red for calcium. Every element has an ID provided by the wavelength of the emitted radiation. The atomic spectra, which can be measured by means of optical instruments called spectrometers, are formed by a set of lines, each corresponding to a certain frequency or wavelength. Figure 2.6 shows the hydrogen spectrum, with wavelengths expressed in Ångströms (one Ångström equals 10^{-10} m).

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[45] Cf. A. Pais, *Niels Bohr's Times, in Physics, Philosophy, and Polity,* Clarendon Press, Oxford 1991.

[46] N. Bohr, *The constitution of atom and molecules, op. cit.*

[47] P. Ehrenfest, *Adiabatic invariants and the theory of quanta,* Philosophical Magazine 33 (1917), p. 500.

[48] A. Pais, *Subtle is the Lord, op. cit.,* p. 405.

[49] A. Einstein, *Zur Quantentheorie der Strahlung,* Physikalische Zeitschrift 18 (1917), pp. 121–128. English translation *On the quantum theory of radiation,* in B. L. van der Waerden, *Sources of Quantum Mechanics,* North-Holland Publ. Co., Amsterdam 1967. p. 63.

[50] To understand Einstein's idea, let us consider a gas in thermal equilibrium in an electromagnetic radiation field. Einstein conjectured that the probability that a molecule of the gas absorbs energy to pass between two energy levels is proportional to the energy of the electromagnetic field, while the probability to release energy to move between two energy levels is the sum of two terms, one independent of the radiation density (spontaneous emission) and one proportional to it. From this hypothesis Einstein obtained an important result, namely, that a necessary condition for Planck's law to hold is that during the transitions between the energy levels a single quantum of energy is absorbed or emitted, with energy given by (and frequency proportional to) the difference between the two energy levels — exactly Bohr's hypothesis.

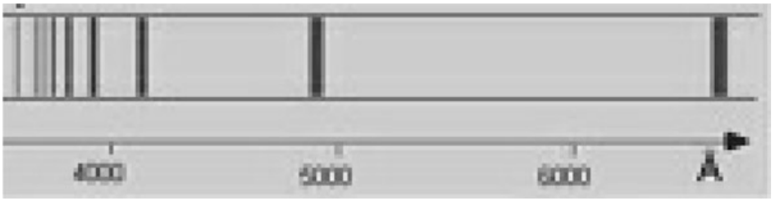

Fig. 2.6 Wavelengths of the light emitted by the hydrogen atom. The first line on the right is very strong; moving to the left the lines are more densely distributed and less intense.

The investigation of atomic spectra was one of the main experimental research areas in the 19th century physics. In an interview cited by Pais,[51] Bohr, referring to the situation of spectroscopy during those years, expressed the following viewpoint.

> One thought [spectra are] marvelous, but it is not possible to make progress there. Just as if you have the wing of a butterfly then certainly it is very regular with the colors and so on, but nobody thought that one could get the basis of biology from the coloring of the wing of a butterfly.[52]

But one of the first successes of Bohr’s model was indeed about the atomic spectra: the theoretical calculation of the constant called R in Balmer’s formula (the empirical rule that relates the spectral lines with their frequencies). More than that, the basic force behind the development of *OQT* was the comparison with the atomic spectra. In this connection, Arnold Sommerfeld’s words in his celebrated treatise *Atombau und Spektrallinien* are revealing.

> What we are nowadays hearing of the language of spectra is a true “music of the spheres” in order and harmony that becomes ever more perfect in spite of the manifold variety. The theory of spectral lines will bear the name of Bohr for all time. But yet another name will be permanently associated with it, that of Planck. All integral laws of spectral lines and of atomic theory spring originally from the quantum theory. It is the mysterious organon on which Nature plays her music of the spectra, and according to the rhythm of which she regulates the structure of the atoms and nuclei.[53]

Bohr’s model involves another assumption: the order of the chemical elements in the periodic table is given by the value of the charge of the atomic nucleus.[54] This was at variance with the principle that had underlain the structure

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[51] A. Pais, *Niels Bohr’s Times, op. cit.,* p. 146.

[52] *Ibid.,* p. 142.

[53] A. Sommerfeld, *Atombau und Spektrallinien,* English translation *Atomic structure and spectral lines,* Methuen & Co., London 1934.

[54] This idea is due Antonius van den Broek, a Dutch lawyer who studied the periodic system as a hobby. Between 1907 and 1914 he published, in some authoritative English and German journals, a number of papers about new interpretations of the periodic system. His most important papers are *Das Mendelejeffsche “kubische” periodische System der Elemente und die Ernondnung der Radioelemente in diesem System,* Physikalische Zeitschrift 12 (1911), p. 490; *The number of possible elements and Mendeléeff’s “cubic” periodic system,* Nature 92 (1911), p. 78; *Die*

of Mendeleev's table until 1913, namely, that elements are ordered by their atomic weight, as the chemical properties of the elements were expressed by their atomic weight. It was known however that this principle gave rise to three anomalies: in the three pairs argon-potassium, cobalt-nickel, and tellurium-iodine, the first elements had a bigger atomic weight than the second, however potassium, nickel, and iodine, in view of their chemical properties, were accommodated in the periodic table after argon, cobalt, and tellurium, thus violating the underlying principle of the periodic table. The idea of ordering the periodic table according to the electric charge revealed the need to base Mendeleev's systematics on a deeper and less intuitive notion than the atomic weight: the electronic structure. An idea of this significance required an experimental verification. Indeed in 1914 Moseley's results[55] confirmed that hypothesis: the atomic number, i.e., the number corresponding to the position of a chemical element in the periodic table, has a precise physical meaning: it is the value of the electrical charge of the nucleus.

Initially, Bohr, according to the idea we have just described, quantized the orbits of the hydrogen atom with a number n, which determines the energy of the state and therefore the radius of the corresponding circular orbit. But very soon it became apparent that a single number was not enough to describe the complex phenomenology of the atom; in particular, it could not explain the "fine structure" of the spectral lines. Indeed, each line, if analyzed with suitably sensitive instruments, appears to be formed by many, very close lines, corresponding to very small variations of the energy. It became necessary to improve the model, by adding new quantum numbers and suitable selection rules, able to discriminate the admissible transitions between atomic states among all those that could be a priori possible. A fundamental contribution to this program was given by Sommerfeld from 1914 onwards.[56]

The "Bohr-Sommerfeld model" (see Fig. 2.7) is a sophisticated evolution of Bohr's rudimentary atomic model. In accordance with classical mechanics, the orbits of the electrons are no longer circular but elliptic, so that the speed of the electrons is no more constant. This modification allowed Sommerfeld to take advantage of the results of theory of relativity: the variation of the speed induced, as a relativistic effect, a variation of the mass of the electron, and this fact justified the fine structure of the spectrum of the hydrogen atom. The quantum numbers were now three: n, l, and m. The first (principal) quantum number quantized the

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Radioelemente, das periodische System und die Konstitution der Atome, Physikalische Zeitschrift 14 (1913), p. 32; *Intra-atomic charge,* Nature 92 (1913), p. 372. For a more detailed study of van den Broek's contribution see G. Bruzzaniti, *op. cit.*

[55] H. G. J. Moseley, *The high-frequency spectra of the elements. Part I,* Philosophical Magazine 26 (1913), p. 1024; *The high-frequency spectra of the elements. Part II,* Philosophical Magazine 27 (1913), p. 403.

[56] A. Sommerfeld, *Die Feinstruktur der Wasserstoff und der Wasserstoff-ähnlichen Linien,* Sitzungsberichte der mathematisch-physikalischen Klasse der K. B. Akademie der Wissenschaften zu München (1915), p. 459; *Zur Quantentheorie der Spektrallinien,* Annalen der Physik 51 (1916), pp. 1–94, 125–67.

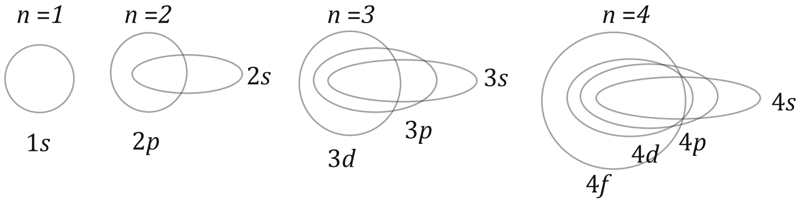

Fig. 2.7 Elliptical orbits in the hydrogen atom according to the Bohr-Sommerfeld model. They are labeled by a number and a letter; the number n is the main quantum number, and the letter l is the azimuthal quantum number, with the correspondence $s = 1, p = 2, d = 3, f = 4$, etc. For example, 4$d$ denotes the orbit with $n = 4$ and $l = 3$.

energy of the orbit, as we saw above, and was geometrically related to the major axis of the ellipses; the second number (azimuthal quantum number) quantized the angular momentum and fixed the ratio between the axes of the ellipses; and the third (magnetic quantum number) quantized the projection of the magnetic moment of the electron along the direction of the external magnetic field (if any). In the absence of an external magnetic field, the quantum number m has no meaning, but an external magnetic field singles out a direction in space. Thus, the Bohr-Sommerfeld model provided a complete explanation of the Zeeman effect; the different spatial orientations of the orbits, each characterized by a different value of m, correspond to different values of the magnetic energy, and this induces a separation of the spectral lines, in full agreement with the experimental results.

Around 1920 the *OQT* was a very successful theory.[57] Due to its strong connection with classical mechanics, it was called "semiclassical theory." The name given to the separation of the spectral lines in a magnetic field, the "normal Zeeman effect," is very suggestive in this connection; the effect was "normal" because classical physics was able to predict that in the presence of an external magnetic field every spectral line splits into three. However, very few lines split into just three parts; most of them split into many more parts. This is the "anomalous" Zeeman effect (see Fig. 2.8). This showed how the theory was based on unsound principles. In his autobiography Einstein writes about this period:

> It was as if the ground had been pulled out from under one's feet, with no firm foundation to be seen anywhere, upon which one could have built. That this insecure and contradictory foundation was sufficient to enable a man of Bohr's unique instinct and tact to discover the major laws of the spectral lines and of the electron-shells of the atoms together with their

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[57] In 1914 Franck and Hertz made an experiment which proved the existence of quantized atomic energy levels. Franck and Hertz bombarded the vapors of different elements with electrons having a known kinetic energy, and observed that the atoms in the vapor were excited only for specific values of the energy of the incident electrons. J. Franck and G. Hertz, *Über Zusammenstöße zwischen Elektronen und den Molekülen des Quecksilberdampfes und die Ionisierungsspannung desselben,* Verhandlungen der Deutsches Phykalische Gesellschaft 16 (1914), p. 457.

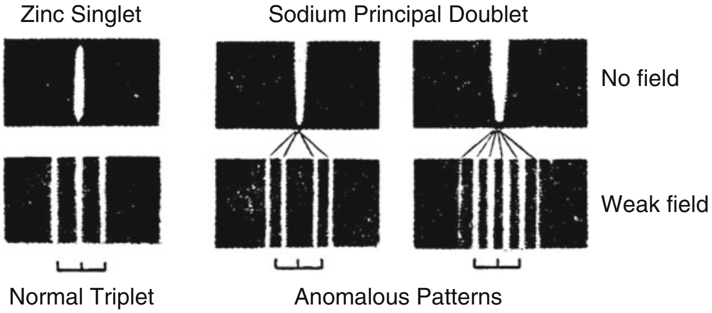

Fig. 2.8 On the left, the normal Zeeman effect in zinc; in the presence of a week magnetic field, the line splits into three. On the right, anomalous Zeeman effect in sodium; in the presence of the magnetic field, the two lines of the doublet split into more than three lines.

> significance for chemistry appeared to me like a miracle — and appears to me as a miracle even today. This is the highest form of musicality in the sphere of thought.[58]

In spite of the contradictions and the uncertainty, it seemed quite evident that the *OQT* hinted at the existence of an underground river, a deeper theory which, if discovered, would explain the microscopic properties of matter. The failure to explain the spectrum of the helium atom, the anomalous Zeeman effect, and the intensity of the spectral lines, the difficulty in relating the electronic structure with the position of the elements in the periodic table, the need to introduce new quantum numbers to cope with particular, problematic situations, all testified to a crisis of the *OQT* which was not the manifestation of a single problem, but was rather due to the accumulation of several different anomalies.

The formulation of Pauli's exclusion principle[59] and the discovery of spin by Uhlenbeck and Goudsmit in 1925[60] (see Appendix C.5) were the forerunners of an imminent, radical turn in the theory. Pauli, in particular, spoke very keenly about a quantum property of the electron that he called "a double valence that cannot be

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[58] P. A. Schilpp, *Albert Einstein, Philosopher-Scientist,* MJF Books, New York, 1949, pp. 46–47.

[59] W. Pauli, *Über den Einfluß der Geschwindigkeitsabhängigkeit der Elektronenmasse auf den Zeemaneffekt,* Zeitschrift für Physik 31 (1925), p. 373; *Über den Zusammenhang des Abschlusses der Elektronengruppen im Atom mit der Komplexstruktur der Spektren, ibid.,* p. 765. The first paper claims the existence of a fourth quantum number than cannot be described classically; the second contains the formulation of Pauli's principle: "There can never be two or more equivalent electrons in an atom for which in strong fields the values of all quantum numbers [...] are the same. If an electron is present in the atom for which these quantum numbers (in an external field) have definite values, this state is 'occupied'." (From the English translation *On the connexion between the completion of electron groups in an atom with the complex structure of spectra,* in D. ter Haar, *The Old Quantum Theory,* Pergamon Press, Oxford-London-Edinburgh 1924. pp184–203.)

[60] G. E. Uhlenbeck and S. Goudsmit, *Ersetzung der Hypothese vom unmechanischen Zwang durch eine Forderung bezüglich des inneren Verhaltens jedes einzelnen Elektrons,* Naturwissenschaften 13 (1925), p. 953.

described classically." In other words, physicists were more and more aware that adding *ad hoc* hypotheses to a theory whose structure was still basically classical was not sufficient to provide a viable description of nature at the atomic scale.

1924 and 1925 were decisive years for the *OQT*. In addition to the difficulties in the comparison with the empirical data, as in the above mentioned case of the helium atom, there was the audacious theoretical proposal by Bohr, Kramers, and Slater,[61] known as BKS theory, which basically represented a refusal of the corpuscular nature of the electromagnetic radiation. A few years later this would be remembered by Heisenberg as the apex of the crisis of the *OQT*.[62] To give a historical interpretation of the BKS theory, which was the most evident manifestation of the crisis, one should remark that the survival of a theory, in spite of its uncertain and shaky principles, is usually guaranteed by the continuous agreement with the experimental data. If the latter fails, there is no reason why the basic principles should not be critically rediscussed, to settle the contradictions of the theory.

The BKS proposal originated from an idea of young Slater, which interpreted the wave-particle duality for the electromagnetic radiation by assuming that waves and particles coexist:

> I have both the waves and the particles, and the particles are sort of carried along by the waves, so that the particles go where the waves take them, instead of just shooting in straight lines, as other people assume.[63]

This idea aimed to dispose of the autonomous existence of the light quantum, in an attempt to recover the classical, wave-theoretic foundations of the theory. This is quite evident from the abstract of the paper:

> In this paper we endeavor to provide a reasonable description of the optical phenomena, closely related to the meaning of spectra according to the quantum theory, without deviating from the classical law of propagation of radiation in empty space. The continuous phenomena that characterize radiation are related with the discrete atomic processes by means of probabilistic laws, according to Einstein's procedure. The introduction of virtual oscillators, which can be related to the continuous processes thanks to the correspondence principle, allows these laws to be interpreted in a rather different way than it is usually done.[64]

The price paid by the BKS proposal to save the preeminence of the undulatory concept was, however, very heavy; the principles of conservation of energy and of impulse were no longer valid in a universal sense, but only statistically. In a single process at the atomic level energy and impulse needed not be conserved. In 1925 Compton and Simon made an experiment to verify the principles of conservation

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[61] N. Bohr, H. A. Kramers and J. C. Slater, *Über die Quantentheorie der Strahlung,* Zeitschrift für Physik 24 (1924), p. 69.

[62] W. Heisenberg, *Die Entwicklung der Quantentheorie 1918–1928,* Naturwissenschaften 17 (1929), p. 490.

[63] From a letter by Slater to his family, reported by A. Pais, *Niels Bohr's Times, op. cit.*, p. 235.

[64] N. Bohr, H. A. Kramers and J. C. Slater, *op. cit.*

of energy and impulse at the level of a single atomic process.[65] The results left no room for doubt: : “They are, on the other hand, in direct support of the view that *energy and momentum are conserved during the interaction between radiation and individual electrons.*”[66] As a consequence, in a letter to Darwin, Bohr wrote “It seems . . . that there is nothing else to do than to give our revolutionary efforts as honourable a funeral as possible.”[67]

The *OQT* was declining, leaving behind many shadows and unresolved problems. As we already mentioned, the community of physicists started realizing the impossibility of building a theory describing the microscopic world that was founded on the principles of classical physics. Again in 1925, Bohr concludes a paper by writing:

> In this state of affairs one must be prepared to find that the generalization of the classical electrodynamic theory that we are striving for will require a fundamental revolution in the concepts upon which the description of nature has been based until now.[68]

This resounded the words of Pierre Duhem, about the perseverance in maintaining

> at all costs, at the price of continuous reparations and of a forest of tangled posts, the worm-eaten pillars of a shaking building, while by throwing these pillars away, one could build a simple, elegant and robust system based on new hypotheses.[69]

2.8 Nuclear protophysics and the proton-electron nuclear model

The introduction of the notion of nuclear atom marked the beginning of the *OQT,* but also set off, around 1920, a new area of research that we have called “nuclear protophysics.”

Figure 2.9 shows the two main areas of research which originated nuclear protophysics: isotopy and the scattering of α particles. These investigations allowed the collection of a huge amount of experimental data, and highlighted a complex phenomenology, whose interpretation led to the construction of many nuclear models, each suited to the particular set of phenomena considered. The theoretical environment in which the theory developed is the *OQT.* The problems that this project met reflect the general issues which still affected the semiclassical approaches which were at the basis of the physical explanation of the atomic and nuclear phenomena.

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[65] A. H. Compton and A. W. Simon, *Directed quanta of scattered X-rays,* Physical Review 26 (1925), p. 289.

[66] *Ibid.,* p. 299. Italics in original.

[67] Letter by N. Bohr to Ch. G. Darwin, cited in A. Pais, *Niels Bohr’s Times, op. cit.*, p. 238.

[68] *Ibid.,* p. 239.

[69] P. Duhem, *op. cit.,* p. 357.

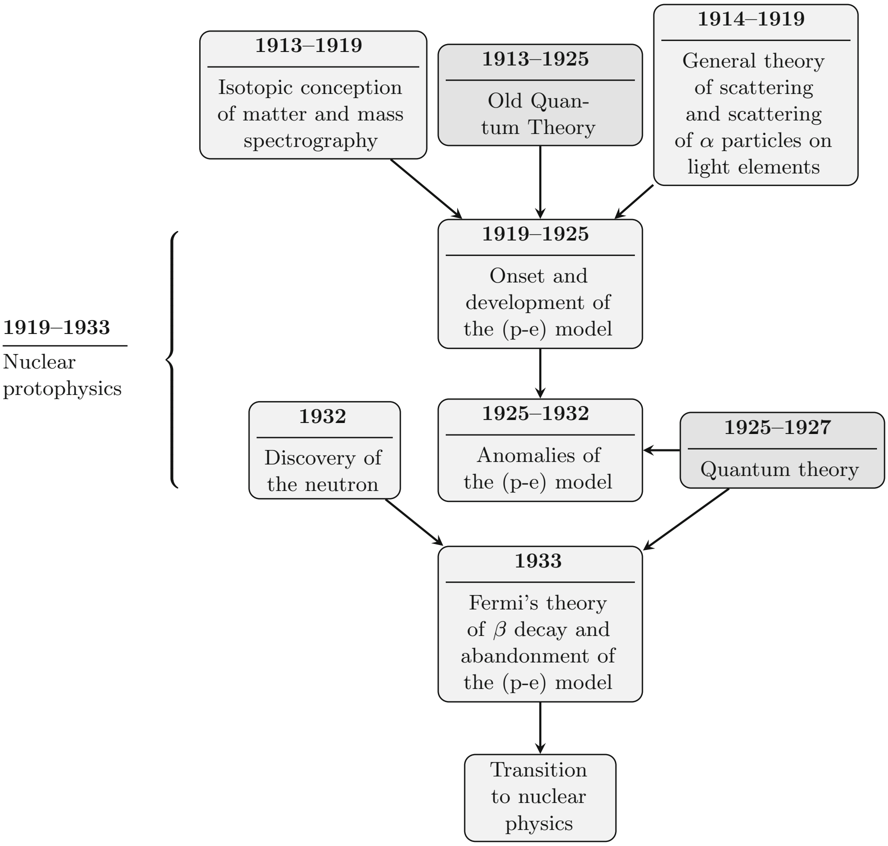

Fig. 2.9 A historical scheme of the "nuclear protophysics," characterized by the onset and decline of the proton-electron nuclear model.

2.8.1 The isotopic conception of matter

The term "isotope" was used for the first time by Frederick Soddy in a 1913 paper:

> The successive expulsion of one α and two β particles in three radio-active changes in any order brings the intra-atomic charge of the element back to its initial value, and the element back to its original place in the table, though its atomic mass is reduced by four units [...] The same algebraic sum of the positive and negative charges in the nucleus, when the arithmetical sum is different, gives what I call "isotopes" "isotopic elements," because they occupy the same place in the periodic table. [70]

The notion of isotopy was a synthesis between van den Broek's hypothesis (the order of the elements in the periodic table is not given by their weight but

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[70] F. Soddy, *Intra-atomic charge,* Nature 92 (913), p. 399.

rather by their nuclear charge) and the law of radioactive displacement. To illustrate the meaning of this law, let us recall that α particles have charge $+2$ (assuming the electron charge as unit) and mass 4 (with unit given by the hydrogen nucleus mass), while β particles have charge -1. If a nucleus with atomic number Z and atomic weight A emits an α particle, its charge decreases by 2 and its weight by 4, if it emits a β particle, its charge increases by 1 and its mass remains unchanged, as the electron mass is negligible. The law of radioactive displacement affirmed that if an element with atomic number Z emits an α particle, it transforms into the element with atomic weight $Z - 2$, that is, the element preceding the original one by two places in the period table; if it emits a β particle, it transforms into the element with atomic number $Z + 1$, i.e., the element which immediately follows the original one in the table.

The investigations made by Francis W. Aston in 1919[71] extended the notion of isotopy from the radioactive elements to all elements in the periodic table. To settle a long-standing controversy about the nature of the atmospheric neon, Aston set up an experimental apparatus, the mass spectrograph, which allowed him to obtain remarkable results. The data showed, with an accuracy of one part over a thousand, that atmospheric neon (whose atomic weight is 20,20) is actually a mixture of two isotopes, whose atomic weights are 20 and 22, in the proportion of 90 and 10 percent. In the same way Aston was able to show that many elements are a mixture of different isotopes. After Aston's work, the notion of isotopy was unanimously accepted by the scientific community, as shown, for instance, by the famous 1921 *Discussion on isotopes.*[72] A few years later Marie Curie published a treatise devoted to the isotopic elements. In the introduction one can read:

> [They] extended and completed the notion of isotopy in all its generality, and definitely deprived the atomic weight of the role it had been assigned in the periodic system. The place that atomic weight occupied in that system has been given to a physical quantity which has a more fundamental importance: the positive charge of the *nucleus,* the central part of the atom.
>
> The periodic classification, thus generalized with a new interpretation, takes us to the idea of unity of matter; a grandiose idea, as old as the atomic theory, which however was superficially refuted by the very precise determination of the atomic weights. These were assigned by chemistry a simple meaning which, however, they do not possess.[73]

Curie's reference to the unity of matter expressed a concept which was quite common within the scientific community of that time. It evoked Prout's old "law of the integers," according to which all atoms were made from a primordial entity, identified with the hydrogen atom. Ever more, the possibility appeared to formulate

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[71] F. W. Aston, *A positive ray spectrograph,* Philosophical Magazine 38 (1919), p. 707; *The constitution of atmospheric neon,* Philosophical Magazine 39 (1920), p. 449; *The mass-spectra of chemical elements,* Philosophical Magazine 40 (1920), p. 628. A more detailed historical reconstruction can be found in G. Bruzzaniti, *op. cit.*

[72] *Discussion on isotopes opened by Sir J. J. Thomson,* Proceedings of the Royal Society A 99 (1921), p. 87.

[73] M. Curie, *L'isotopie et les éléments isotopes,* Presses Universitaires de France, Paris 1924, p. 12.

hypotheses about the constituents of a structure that seemed to be more and more complex, and was most likely formed by hydrogen nuclei, electrons, and, perhaps, α particles. (For further information about the notion of isotope see Appendix C.6.)

2.8.2 The scattering of α particles

In 1911 Rutherford developed a theory for the scattering of α particles by atoms with a large atomic weight. In particular, in Rutherford's experiment, the target was formed by gold atoms. But what was one to expect in case of other atoms? In an important paper published in 1914,[74] Charles G. Darwin tried to answer this question, with a theoretical study of the scattering of α particles by light elements, including the cases when the target had the same mass as the α particles (scattering of α particles by helium) or even smaller (scattering by hydrogen). It was a theory based on the classical conservation principles, where nuclei were assumed to be point-like and to interact by means of the Coulomb force. The first experiments to verify Darwin's theory were made by Marsden in 1914,[75] who analyzed the scattering of α particles by hydrogen. The results were in good agreement with the predictions. In any case these first experiments were just the preliminary stages of a more comprehensive project, which aimed at a detailed investigation of all the consequences of the theory.

However, the onset of the First World War determined an almost complete stop of the scientific activity. Rutherford, at the end of the war, resumed Marsden's experiments, and in 1919 published a series of four very important papers. The most striking result, published in the fourth paper of the series, was the first evidence of the disintegration of the nitrogen atom:

> From the results so far obtained it is difficult to avoid the conclusion that the long-range atoms arising from collision of alpha particles with nitrogen are not nitrogen atoms but probably atoms of hydrogen, or atoms of mass 2. If this be the case, we must conclude that the nitrogen atom is disintegrated under the intense forces developed in a close collision with a swift alpha particle, and that the hydrogen atom which is liberated formed a constituent part of the nitrogen nucleus. [...] It is of interest to note, that while the majority of the light atoms, as is well known, have atomic weights represented by $4n$ or $4n+3$ where n is a whole number, nitrogen is the only atom which is expressed by $4n + 2$. We should anticipate from radioactive data that the nitrogen nucleus consists of three helium nuclei each of atomic mass 4 and either two hydrogen nuclei or one of mass 2.[76]

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[74] Ch. G. Darwin, *On collision of α particles with light atoms,* Philosophical Magazine 27 (1914), p. 499.

[75] E. Marsden, *The passage of α particles through hydrogen, ibid.,* p. 824.

[76] E. Rutherford, *Collision of α particles with light atoms,* Philosophical Magazine 37 (1919), pp. 537–61. The paper is made of four parts: *I. Hydrogen,* pp- 537–561; *II. Velocity of the hydrogen atom,* pp. 562–571; *III. Nitrogen and oxygen atoms,* pp. 571–580; *IV. An anomalous effect in nitrogen,* pp. 581–587.

The naturality with which Rutherford mentions the constituents of the nitrogen nucleus makes it clear that the idea of the atomic nucleus as a complex entity, formed by more elementary constituents, was by then well established. It was supported by several experimental evidences, such as isotopy, the radioactive phenomena, and the disintegration of nitrogen.

Around 1920 the investigation of the nuclear structure has become an autonomous field of study, characterized by a specialized language and specific programs. Rutherford's introduction in 1921 of the term "proton" is symptomatic; it attests not only the understanding of the role played by the hydrogen nucleus as a constitutive element of the atomic nuclei, but also the interest of the scientific community for the new research area.[77]

2.8.3 The (p-e) model

As it commonly happens, also nuclear protophysics showed uncertainties and interpretative problems, due to the lack of a theoretical language powerful enough to describe the huge amount of available experimental data. By inspecting the literature around the year 1920, these uncertainties are in particular shown by the great number of nuclear models, interpretative schemes, and bold conjectures.[78]

We use here the generic term "(p-e) model" to denote a number of different models, all built on the same assumption and a common methodological principle, i.e., that all atomic nuclei are made of elementary particles (protons and electrons);

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[77] It is interesting to recall what Rutherford wrote in 1921 as a remark about a paper of D. O. Masson, where the term *baron* was suggested to designate the hydrogen nucleus: 'At the time of writing this paper in Australia, Professor Orme Masson was not aware that the name "proton" had already been suggested as a suitable name for the unit of mass nearly 1, in terms of oxygen 16, that appears to enter into the nuclear structure of atoms. The question of a suitable name for this unit was discussed at an informal meeting of a number of members of Section A of the British Association at Cardiff this year. The name "baron" suggested by Professor Masson was mentioned, but was considered unsuitable on account of the existing variety of meanings. Finally the name "proton" met with general approval, particularly as it suggests the original term "protyle" given by Prout in his well-known hypothesis that all atoms are built up of hydrogen. The need of a special name for the nuclear unit of mass 1 was drawn attention to by Sir Oliver Lodge at the Sectional meeting, and the writer then suggested the name "proton." Professor Orme Masson sent the present paper for publication through the writer, and in order to avoid the long delay involved in correspondence, his paper is printed in its original form. If the name "proton" is generally approved, it is merely necessary to change the symbol "b" into "p" in the chemical equations given in the paper. It should be pointed out that a somewhat similar type of nomenclature for the constituents of atoms has been suggested in the interesting paper of Professor W. D. Harkins, entitled *The Nuclei of Atoms and the New Periodic System* (Physical Review, 15 (1920) p. 73), in D. O. Masson, *The constitution of atoms,* Philosophical Magazine 41 (1921), p. 281'.

[78] Almost 25 different nuclear models were proposed during those years. The situation has been analyzed in detail in G. Bruzzaniti, *op. cit.,* and R. H. Stuewer, *The nuclear electron hypothesis,* in W. R. Shea (ed.), *Otto Hahn and the Rise of Nuclear Physics,* Reidel, Dordrecht 1983.

the principle assumed that if a nucleus emits a particle, then the latter already existed inside the nucleus. The principle was so deeply rooted in the scientific community that it was never explicitly stated. However, Aston wrote in 1922:

> It has been stated that the presence of helium nuclei inside the nuclei of radioactive atoms is definitely proved by the ejection of α particles by the latter. In the writer's opinion this is much the same as saying that a pistol contains smoke, for it is quite possible that the α particle, like the smoke of the pistol, is only formed at the moment of its ejection.[79]

Nevertheless, Aston's voice remained unheard, and had no consequence on the development of nuclear protophysics. The principle was held true until 1933, when it was eventually demolished by Fermi's theory of the β decay.

2.8.4 *Quantum statistics*

Ludwig Boltzmann's profound intuition that "the problems of the mechanical theory of heat are [...] problems in the theory of probability" gave rise to statistical mechanics.[80] This merit is acknowledged by the inscription on the great Austrian physicist's grave in Vienna's central cemetery: $S = k \log W$. This equation relates the entropy S of the state of a thermodynamical system with a quantity W, which is proportional of the probability of that state. Actually the equation was not written by Boltzmann, but rather by Planck, who, in paper which followed Boltzmann's after a few weeks, wrote the equation if a more complete form, stressing that the entropy is defined up to an additive constant: $S = k \log W + \text{const}$. However, it remains true that the equation was the result of Boltzmann's research and of his understanding "the problems of the mechanical theory of heat as [...] problems of the theory of probability," of his perception — as written by Abraham Pais — that "the second law of thermodynamics can be understood only in terms of a connection between entropy and probability."[81] For a better understanding of the relations between thermodynamics and statistical mechanics let us once more resort to Enrico Fermi (he called π the quantity we denoted by W).

> The fact that the entropy of an isolated system can never decrease during any transformation has a very clear interpretation from the statistical point of view. Boltzmann has proved that the entropy of a given state of a thermodynamical system is connected by a simple relationship to the probability of the state. We have already emphasized the difference between the dynamical and thermodynamical concepts of the state of a system. To define the dynamical state, it is necessary to have the detailed knowledge of the position and motion of all the molecules that compose the system. The thermodynamical state, on the other hand, is defined by giving only a small number of parameters, such as the temperature, pressure, and so forth. It follows, therefore, that to the same thermodynamical state there corresponds a

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[79] F. W. Aston, *Isotopes,* Arnold, London 1922, p. 102.

[80] L. Boltzmann, *Weitere Studien über das Wärmegleichgewicht unter Gasmolekülen,* Wiener Berichte 66 (1872), pp. 275–370.

[81] A. Pais, *Subtle is the Lord, op. cit.,* p. 60.

> large number of dynamical states. In statistical mechanics, criteria are given for assigning to a given thermodynamical state the number π of corresponding dynamical states. [...] This number π is usually called the probability of the given thermodynamical state, although, strictly speaking, it is only proportional to the probability in the usual sense. The latter can be obtained by dividing π by the total number of possible dynamical states.
> We shall now assume, in accordance with statistical considerations, that in an isolated system only those spontaneous transformations occur which take the system to states of higher probability, so that the most stable state of such a system will be the state of highest probability consistent with the given total energy of the system.
> We see that this assumption establishes a parallelism between the properties of the probability π and the entropy S of our system, and thus suggests the existence of a functional relationship between them. Such a relationship was actually established by Boltzmann, who proved that $S = k \log \pi$.[82]

The main objective of statistical mechanics is to determine W, the quantity that expresses the number of microscopic ways in which the same macroscopic state can be realized. As Erwin Schrödinger wrote

> There is, essentially, only one problem in statistical thermodynamics: the distribution of a given amount E of energy over N identical systems. Or perhaps better: to determine the distribution of an assembly N of identical systems over the possible states in which the assembly can find itself, given that the energy of the assembly is a constant E.[83]

The merit for finding the general solution to this problem goes to Boltzmann, who introduced the so-called “Boltzmann statistics,” or “classical statistics,” to distinguish it from the “quantum statistics” of Bose-Einstein and Fermi-Dirac. We shall use a simple example to understand, at least in principle, what is going on.[84] Let us suppose we have two identical small balls, one white and one black, and two glasses, A and B. How can we place the balls under the glasses? Clearly, as in Figure 2.10. Would the situation be anyhow different if the balls were of the same color? According to classical physics, the answer is no. The balls are identical, but nevertheless they can always be distinguished, since after observing one of them at some instant of time, we can follow its trajectory, and after some time we know for sure that we are still watching the same ball. So we still have the four cases depicted in the figure: 1) two balls under A; 2) two balls under B; 3) one ball under A, and the other under B; and 4) the same as in 3), but with the two balls swapped. The probabilities to have one of the configurations are easily computed: the probability that the two balls are under A is 1/4; that they are under B is 1/4; and that they are one under A and one under B is 1/2.

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[82] E. Fermi, *Thermodynamics,* Dover, New York 1936, p. 56–57.

[83] E. Schrödinger, *Statistical Thermodynamics,* Cambridge University Press, Cambridge, U.K. 1946.

[84] This example is taken, with some small changes, from G. Parisi, *La statistica di Fermi,* in C. Bernardini and L. Bonolis (eds.), *Conoscere Fermi,* Compositori, Bologna 2001. That is in turn basically taken from an example by Fermi, published in the entry “Meccanica Statistica” of G. Treccani’s *Enciclopedia Italiana* (all in Italian).

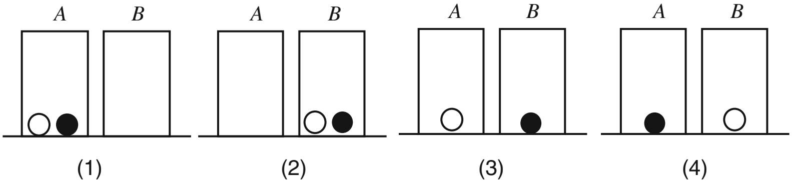


Fig. 2.10 There are four ways to place a white and a black ball under two glasses.

Fig. 2.11 There are three ways to place two indistinguishable balls under two glasses.

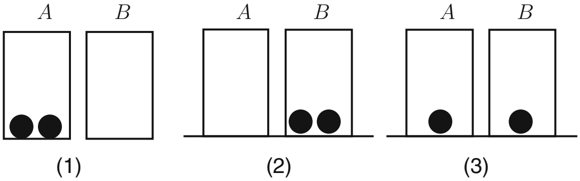
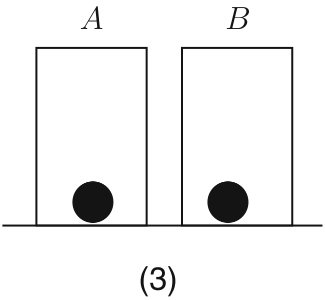


Fig. 2.12 The only possible configuration for two indistinguishable balls that obey Pauli's exclusion principle.



From the viewpoint of quantum mechanics the situation is radically different. Indeed, as we shall discuss in the next Section, we cannot follow the trajectory of a ball, at least not without strongly affecting the physical system. The two balls become indistinguishable, and this has deep effects on the probability pattern. Taking the indistinguishability into account, the cases 3) and 4) of Figure 2.10 become the same, as in Figure 2.11. Now the probabilities are 1/3 for all cases: two balls under *A*, two balls under *B*, and one ball under *A* and one under *B*. Let us eventually assume that the two indistinguishable balls, like electrons, satisfy Pauli's exclusion principle, so that they cannot be put under the same glass. The statistics changes again: the cases (1) and (2) in the figure no longer take place, and the only possibility is case (3), as in Figure 2.12. So, the probability to have one ball under *A*, and one ball under *B*, is 1.

The three cases (identical distinguishable particles, identical indistinguishable particles, identical indistinguishable particles following Pauli's exclusion principle) correspond, respectively, to the three different statistics we have previously mentioned: Boltzmann's, Bose-Einstein's, and Fermi-Dirac's. It seems that to formulate the two non-classical statistics one needs to know one of the most important features of quantum mechanics, namely, the relation between identity and distinguishability, which is a direct consequence of the loss of the notion of trajectory. However, the historical documents show that the two statistics were discovered somehow before the formulation of quantum mechanics. They were, so to say, *OQT*'s swan song.

Very briefly, the first documents attesting the birth of the non-classical statistics are two. The first is a letter the young and unknown Bengali student Satyendra Nath Bose wrote to Einstein in 1924. The letter included the text of a paper that had been rejected by the authoritative journal *Philosophical Magazine,* and contained a new deduction of the spectral distribution of Planck's black body radiation. The deduction used a very original method; the electromagnetic radiation was treated as a gas of particles (a photon gas we would say nowadays) and Planck's formula was deduced by means of statistical arguments, using however a counting of the distributions different from Boltzmann's. Einstein was impressed by the paper, translated it into German, and had it published in the journal *Zeitschrift für Physik,* adding the note

> In my opinion, Bose's derivation of the Planck formula constitutes an important advance. The method used here also yields the quantum theory of the ideal gas, as I shall discuss elsewhere in more detail.[85]

However, it was not clear why Bose was getting the correct result, and indeed Einstein wrote "[the] derivation is elegant but the essence remains obscure."[86] Not even Bose had it clear:

> I had no idea that what I had done was really novel. . . . I was not a statistician to the extent of really knowing that I was doing something which was really different from what Boltzmann would have done, from Boltzmann statistics.[87]

Most likely, Pais is right in affirming "I believe there had been no such successful shot in the dark since Planck introduced the quantum in 1900."[88] In any case, the Bose-Einstein statistics was born.

The second document is a 1926 paper by Fermi entitled *Sulla quantizzazione del gas perfetto monoatomico.*[89] The paper concluded a research project that we shall analyze more in detail in Chapter 3. As in Bose's paper, the topic is the statistical properties of a gas, but while Bose treated a photon gas, here Fermi considered a gas made of particles. By extending Pauli's exclusion principle, introduced since less than a year to interpret the behavior of the atomic electrons, Fermi got a new statistics, different from Boltzmann's classical statistics. Fermi's work was carried out in the *OQT* framework and offered a solution to some problems of the *OQT* treatment of the monoatomic ideal gas, such as the independence of the specific heat from temperature. With Fermi's statistics, the specific heat of the gas is no longer constant with respect to temperature, but goes to zero linearly with the temperature.

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[85] A. Pais, *op. cit.,* p. 423.

[86] *Ibid.,* p. 424.

[87] *Ibid.*

[88] *Ibid.,* p. 428.

[89] On the quantization of the ideal monoatomic gas. Fermi [30].

Since the relation between spin and statistics was not yet clear at the time, Fermi's proposal had some inaccuracies, and nobody in those years attempted to relate it to Bose's statistics. In any case, a new statistics was born, which would have been called "Fermi-Dirac statistics," since about six months later, Paul Adrien Maurice Dirac[90] obtained the same results, albeit in a completely different way and in the framework of the arising quantum mechanics.

2.8.5 Quantum mechanics

Between 1924 and 1925 the *OQT* lost most of its impetus, due to some incurable difficulties whose solution required the construction of a new theoretical perspective. This state of affairs was very well described by Enrico Fermi in 1930, in a popular talk in which he reviewed the development of the "new physics":

> In the first attempts, one tried to understand the atom by means of those physical laws we know to hold for macroscopic phenomena. However, it became very soon clear that those laws cannot be applied to bodies of such small dimensions. Then Bohr's theory came, which attempted to modify the usual laws to adapt them to the new problems, obtaining a great number of results, mostly of qualitative nature. Almost invariably, however, when one tried to get precise quantitative predictions, Bohr's theory was insufficient. Thus it became clear to the physicists that it was not enough to modify the old laws, but it was rather necessary to replace them with new ones.[91]

The "new laws" were formulated following two different paths. The first line of research, initiated by Louis de Broglie,[92] resulted in Schrödinger's "wave mechanics";[93] and the second, which started with Werner Heisenberg's "matrix mechanics,"[94] led, via researches done by Born, Jordan, and Heisenberg himself,[95] to what was initially called "quantum mechanics."

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[90] P. A. M. Dirac, *On the theory of quantum mechanics,* Proceedings of the Royal Society A 92 (1926), p. 661.

[91] Fermi [62], *CPF I*, p. 375.

[92] L. de Broglie, *Recherches sur la théorie des quanta,* Annales de Physique 3 (1925), p. 22.

[93] E. Schrödinger, *Quantisierung als Eigenwertproblem,* Annalen der Physik 49 (1926), pp. 361–76.

[94] W. Heisenberg, *Über die quantentheoretische Umdeutung kinematischer und mechanischer Beziehungen,* Zeitschrift für Physik 33 (1925), p. 879.

[95] M. Born and P. Jordan, *Zur Quantenmechanik,* Zeitschrift für Physik 34 (1925), p. 858 (English translation B. L. van der Waerden, *Sources of Quantum Mechanics, op. cit.*); M. Born, W. Heisenberg and P. Jordan, *Zur Quantenmechanik II, ibid.* (1926), p. 557 (English translation in B. L. van der Waerden, *op. cit.*).

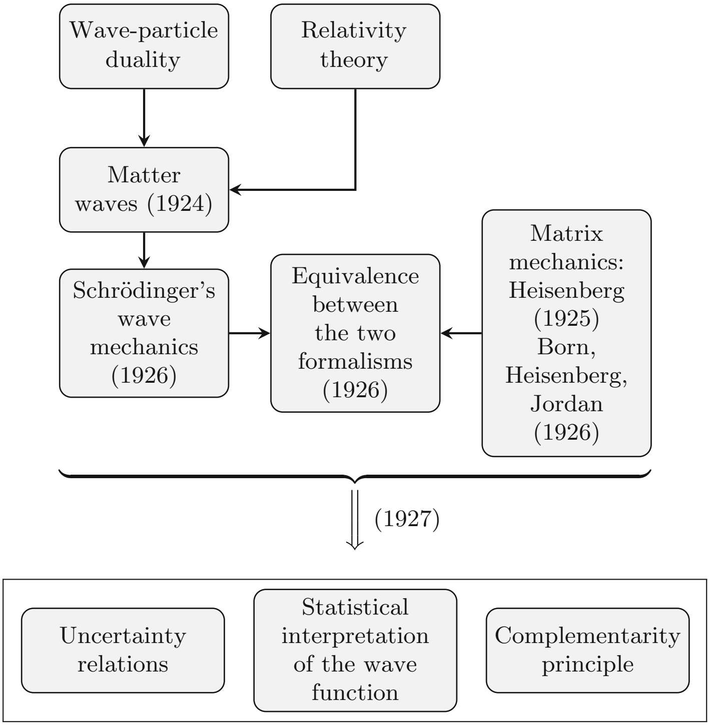

Fig. 2.13 Lines of research from the beginnings of quantum mechanics to the Copenhagen interpretation.

Schrödinger[96] and Carl Eckart[97] are to be credited for proving the equivalence between the two formalisms. Starting from the end of the 1920s, the term "quantum mechanics" referred both to wave and matrix mechanics, and became synonymous to "quantum theories," denoting the complex of theories and interpretative apparatuses that replaced the *OQT.*

Figure 2.13 sketches the most important moments of the formulation of quantum mechanics. In this theory, perhaps more than in any other, the interpretative and more essentially philosophical level played a fundamental role in its birth, and in its acceptance by the scientific community. The physical interpretation of the new theory turned out to be a much more challenging problem than just writing the equations.

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[96] E. Schrödinger, *Über das Verhältnis des Heisenberg-Born-Jordanschen Quantenmechanik zu der meinen,* Annalen der Physik 79 (1926), p. 734.

[97] C. Eckart, *Operator calculus and the solution of the equations of quantum dynamics,* Physical Review 27 (1926), p. 711.

De Broglie's proposal, as he told in his 1929 Nobel prize speech,[98] was rooted in a deep feeling of inadequacy of the wave-particle duality, together with the impossibility to understand why, among the infinite number of classical motions that are possible according to classical physics, only a few actually take place. On the other hand, De Broglie remarked again, the equation defining the energy of a light quantum ($E = h\nu$) is not, from the particle viewpoint, a satisfactory definition, due to the presence of the quantity ν that expresses a frequency. De Broglie stressed another aspect: the stationary motions of the electrons inside the atomic structure. These were characterized by integer numbers, and it was known that "the physical phenomena that involve only integer numbers are those related to interference." It was this consideration that suggested De Broglie to represent electrons not just as simple corpuscles, but rather to assign them a "certain periodicity":

> On the other hand the determination of the stable motions of the electrons in the atom involves whole numbers, and so far the only phenomena in which whole numbers were involved in physics were those of interference and of eigenvibrations. That suggested the idea to me that electrons themselves could not be represented as simple corpuscles either, but that a periodicity had also to be assigned to them too. I thus arrived at the following overall concept which guided my studies: for both matter and radiations, light in particular, it is necessary to introduce the corpuscle concept and the wave concept at the same time. In other words the existence of corpuscles accompanied by waves has to be assumed in all cases.[99]

Relativity theory was the formal instrument which allowed the French scientist to associate a wave with the motion of a particle. In relativity, energy and momentum are treated as quantities of the same kind.[100] If a particle of energy E, according to the relation $E = h\nu$, is associated with a wave of frequency ν, then a wave of wavelength λ should be associated with a particle with momentum $p = mv$, according to the relation $\lambda = h/p = h/mv$.

Schrödinger's "wave mechanics" was based indeed on this relation between momentum and the associated wavelength. By assigning a wave nature also to matter, one could establish a strict analogy between optical and mechanical phenomena; as in optics the behavior of a wave packet is determined by the refraction index of the medium in which it propagates, in the same way in mechanics the motion of a point particle is determined by the force field in which it moves. Moreover, as a ray of light traveling between two points in a certain medium chooses the path that is traversed in the least time (Fermat's principle), also in mechanics the trajectory of a point particle is determined by a minimum principle; it should indeed minimize the *action,* a quantity depending on the kinetic and potential energy. But there is more: this analogy allows one to understand why classical physics fails to describe

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[98] L. de Broglie, *La nature ondulatoire de l'électron,* Nobel Prize acceptance speech given in Stockholm on 12 December 1929. In *Nobel Lectures, Physics 1922–1941*, Elsevier Publishing Company, Amsterdam, 1965, p. 247.

[99] *Ibid.*

[100] In the theory of relativity, momentum is the spatial part of a four-vector, whose time component is energy.

the atomic structure. In optics indeed one can assume that light rays propagate along straight lines (the approximation on which geometric optics is based) until the dimensions of the physical system under study are big as compared with the wavelength of the light ray. In the same way, the laws of classical mechanics cease to be valid when the wavelength of the particle, as given by De Broglie's relation, is comparable with the dimensions of the system with which it interacts.

De Broglie's hypothesis was confirmed by an experiment performed by Davisson and Germer in 1927,[101] exactly as the photoelectric and Compton effects[102] had provided an empirical foundation to support the particle-like nature of the electromagnetic radiation. Davisson and Germer's experiment showed beyond any doubt that electrons are subject to diffraction phenomena, the trademark of a wave-like behavior.

This analogy between optics and mechanics is very important from the formal viewpoint. In the same way as electric and magnetic fields satisfy a wave equation, one can write an equation that the wave associated with the particles, the *wave function* $\Psi(x, y, z, t)$, a function of the coordinates and time, must obey. This is Schrödinger's equation. To study the dynamics of a system, thus, one needs to determine the time evolution of the wave function. Schrödinger's equation, moreover, allows one to compute the permitted values of the energy for a given atomic system; this is one of the most important and innovative features of wave mechanics. The discontinuity of the stationary states, which was one of the most unsatisfactory aspects of quantum theory, is a natural consequence of the properties of the solutions of Schrödinger's equation. The latter only admits regular solutions when the energy E takes some well-determined values (the *eigenvalues* of the system), which, according to the specific situation under study, can be distributed continuously in an interval, or form a discrete series.

Also for Heisenberg the starting point for the construction of the new mechanics was a matter of interpretation. In this case it was the belief that the theory had to be founded only on relations among observable quantities. In the new formalism there was to be no room for quantities such as the diameter or the eccentricity of the orbit of an electron, as opposed, for instance, to the frequency of the emitted radiation; this is an observable quantity, contrary to the diameter of the orbit. To understand the implications of Heisenberg's perspective for the formalism of the theory, let us consider the radiation emitted by an atom. Its frequency depends on the transition of an electron between two energy levels; we can therefore denote it by ν_{nm}, where the indexes n and m denote the two levels. If we want to represent all emitted frequencies, we need a matrix with an infinite number of rows and columns:

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[101] C. J. Davisson and L. H. Germer, *Diffraction of electrons by a crystal of nickel,* Physical Review 30 (1927), p. 705.

[102] The Compton effect is due to the particle-like interaction between electrons and electromagnetic radiation, and may be thought of as the scattering between an electron at rest and a photon of energy $h\nu$ and momentum $h\nu/c$. During the scattering the photon is deviated from its original direction, and its frequency changes, due to the loss of energy. As in all scattering processes, momentum is conserved, and the momentum of the incident photon is the same as the sum of the momenta of the electron and the photon after the collision.

$$\begin{vmatrix} \nu_{11} & \nu_{12} & \dots & \nu_{1m} & \dots \\ \nu_{21} & \nu_{22} & \dots & \nu_{1m} & \dots \\ \dots & \dots & \dots & \dots & \dots \\ \nu_{n1} & \nu_{n2} & \dots & \nu_{nm} & \dots \\ \dots & \dots & \dots & \dots & \dots \end{vmatrix}$$

Matrices are not just plain tables of numbers, but rather are mathematical objects, which can be summed and multiplied. The algebra of matrices is substantially different from that of the real numbers; for instance, it is not commutative, that is, the product AB, where A and B are matrices, is in general different from BA.

Heisenberg's 1925 proposal took place within this formalism. It was developed over the next few months by Born and Jordan, and completed, still in 1925, by Born, Heisenberg, and Jordan. The idea was to associate a matrix to any physical quantity, and transpose the equations of classical physics into the new mechanics, considering them as relations between matrices. In this perspective, also the position q and the momentum p of a particle are non-commuting matrices, i.e., $qp \neq pq$. This relation expresses the deviation between the new "matrix mechanics" and classical mechanics; while position and momentum commute in classical mechanics, they do not in quantum mechanics. The deviation between qp and pq is given by the Planck constant, according to the formula

$$pq - qp = \frac{h}{2\pi i} I,$$

where i is the imaginary unit and I is the identity matrix, with entries 1 on the main diagonal, and 0 elsewhere (it is the equivalent of the number 1 for the product of real numbers).

A few months after the publication of the long paper by Born, Heisenberg, and Jordan, whose final part was devoted to the physical application of the theory, Pauli published an important paper,[103] where matrix mechanics was applied, with brilliant results, to the computation of the spectrum of the hydrogen atom.

After 13 years, Bohr's queer quantization rules, which had raised so many perplexities, found a formal interpretation within two theories that led to the same results, but were otherwise very different in what concerns their starting point, methods, concepts, and formalism. The matrix mechanics of Born, Heisenberg, and Jordan, by replacing the continuous variables of classical physics with discrete sets of numerical quantities, signed, as it was indeed in the intentions of its authors, a radical break from classical physics; on the contrary, Schrödinger's wave mechanics moved in the opposite direction, toward a theory of the continuum.

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[103] W. Pauli, *Über das Wasserstoffspektrum vom Standpunkt der neuen Quantenmechanik,* Zeitschrift für Physik 36 (1926), p. 336 (English translation in L. van der Waerden, *Sources of Quantum Mechanics, op. cit.*).

Schrödinger's proof, and the independent one by Eckart, of the substantial equivalence between the two theories, was no little surprise for the scientific community. As George Gamow famously said,

> It was just as surprising as the statement that whales and dolphins are not fish like sharks or herring but animals like elephants or horses.[104]

But what is really a wave function? What really are the matter waves? To answer these questions is not easy now, as it was not at that time. Schrödinger, however, was firmly convinced that the true nature of particles was wave-like. Particles move like the impulsion given to a stretched string causes a perturbation that propagates along the string (a *wave packet*). Already in 1926, Max Born's statistical interpretation of the wave function[105] showed that the truth was different; the function $\Psi(x, y, z, t)$, or better said, the quantity $|\Psi(x, y, z, t)|^2\, dx\, dy\, dz$, represents the probability that the particle at the instant t is in the volume $dx\, dy\, dz$ located at the position (x, y, z).

Heisenberg in 1927 introduced the celebrated "uncertainty relations,"[106] and Bohr formulated the "complementarity principle."[107] In this way, the last conceptual cruxes of the so-called "Copenhagen interpretation" were fixed, and that extraordinary process of construction of the new physics, that had started only a few years before, came to an end (see Appendix C.7 for a more detailed discussion). As we have already mentioned several times, the cognitive importance of the scientific theory does not reduce to having solved the problems that originated it, but rather, is its capacity to be a starting point for the formulation of new problems and the creation of new interpretation paradigms. The creation of quantum mechanics was not only a goal, but also opened new and promising research paths. The most important among these was the construction of a *quantum electrodynamics* (see Appendix C.8), namely, a theory capable of quantizing the electromagnetic field, also when interacting with electrons, and compatible with relativity (Schrödinger's equation, indeed, is not relativistic). The first hints of this project can be found in the two already cited 1925 papers, one by Born and Jordan, the other by Born, Heisenberg, and Jordan, where the electromagnetic field *in vacuo* (in the absence of matter) was quantized. However, a viable and successful theory of quantum electrodynamics was eventually formulated by Dirac in his 1927 papers.[108] That theory would have been studied and improved over the following 20 years.

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[104] G. Gamow, *op. cit.,* p. 105.

[105] M. Born, *Zur Quantenmechanik der Stoßvorgange,* Zeitschrift für Physik 37 (1926), p. 863.

[106] W. Heisenberg, *Über den anschaulichen Inhalt der quantentheoretischen Kinematik und Mechanik,* Zeitschrift für Physik 43 (1927), p. 172. English translation *The actual content of quantum theoretical kinematics and mechanics,* in *NASA Technical Memorandum,* TM-77379, https://archive.org/details/nasa_techdoc_19840008978.

[107] N. Bohr, *The quantum postulate and the recent development of atomic theory,* Nature 121 (1928), p. 580.

[108] P. A. M. Dirac, *The quantum theory of the emission and absorption of radiation,* Proceedings of the Royal Society A 114 (1927), p. 243.

2.8.6 *Quantum mechanics and nuclear protophysics: the anomalies of the (p-e) model*

Quantum mechanics played a decisive role in the development of nuclear protophysics. The several variants of the (p-e) model, which were created to give a coherent organization to the rapidly increasing amount of experimental data, needed to be confronted with a theory which was becoming the official language of atomic physics. This confrontation had two effects. On the one hand, the success of the application of the theory to some nuclear phenomena was regarded as a token of the effectiveness of the theory, which therefore could claim to be the most natural framework to develop a theory of the atomic nucleus. On the other hand, some features of the (p-e) model, considered within the new theory, led to apparent anomalies.

The most striking result of quantum mechanics in the interpretation of nuclear phenomena is the explanation of the α emission given in 1928 by George Gamow, and, independently, by Roland W. Gurney and Edward Condon.[109] In the latter paper one can read:

> It seems, however, that the new quantum mechanics has had sufficient success to justify the hope that it is competent to carry out an effective attack on the problem. The quantum mechanics has in it just those statistical elements which would seem appropriate to an explanation of the phenomenon of radioactive decay.

Both papers aimed to explain the radioactive decay as a consequence of the quantum mechanical laws, in particular, of the wave-like properties of matter. Let us have a look at Figure 2.14. It represents a "potential well" containing a particle. We may think of a person in an enclosure surrounded by a fence; the gray areas of the graph represent the fence, and its height denotes the energy spent by the person to jump over the fence. From the classical viewpoint, if the energy of an α particle is smaller than the confining potential, the particle will never leave the nucleus that contains it. The viewpoint of quantum mechanics is completely different. The solution of the Schrödinger equation for a particle in that potential is an oscillating

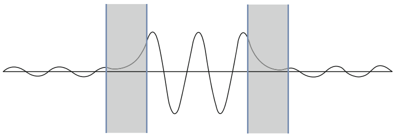

Fig. 2.14 Particle in a potential well. The curves in the three regions (inner, outer, and inside the walls) represent the wave function of the particle, computed by solving Schrödinger's equation.

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[109] G. Gamow, *Zur Quantentheorie des Atomkernes,* Zeitschrift für Physik 51 (1928), p. 204; R. W. Gurney and E. Condon, *Wave mechanics and radioactive disintegration,* Nature 122 (1928), p. 439; *Quantum mechanics and radioactive disintegration,* Physical Review 33 (1928), p. 127.

function inside and outside the well, and is exponentially decreasing (toward the exterior) inside the walls (the regions a, b, and c in the figure). According to the probabilistic interpretation of the wave function, these solutions show that there is a nonzero probability that an α particle can be found outside of the potential well (i.e., the nucleus). In other terms, there is no need to figure out cataclysmic scenarios to account for the expulsion of an α particle; it is enough to solve a differential equation to understand that a particle has a certain probability to escape from the nucleus, although the confining potential is larger than its energy. As Gurney and Condon observe,

> Much has been written of the explosive violence with which the α-particle is hurled from its place in the nucleus. But from the process pictured above, one would rather say that the α-particle slips away almost unnoticed.[110]

The anomalies of the (p-e) model, i.e., its contradictions with quantum mechanics, are essentially four, all about the presence of electrons inside the nucleus. Let us examine them one by one.

Confinement anomaly. The uncertainty principle tells us that the indeterminacies Δp and Δq in the measurements of the position and impulse of a particle are related by the inequality $\Delta p \cdot \Delta q \geq \hbar/2$. If one assumes that an electron is confined inside the nucleus of an atom, the uncertainty Δq is of the order of the linear dimensions of the nucleus. Plugging the data into the equation, one obtains a completely unreasonable value for the momentum of the electron. This problem was first mentioned in a 1929 paper by Rutherford,[111] and was afterwards examined in detail by Klein[112] and Gamow,[113] who stressed the importance of the anomaly.

Spin-statistics anomaly. From the quantum mechanical perspective, the notion of spin, and the relations between Pauli's principle and the statistical properties of a collection of identical particles, establish new features of the electron and of the photon. As a consequence, also the atomic nucleus, regarded as a system formed by electrons and protons, can be studied in more detail. This poses three problems.

a) Given a system formed by a collection of particles whose spin is known, what is the spin of the system?
b) Given a system formed by a collection of particles, and knowing what statistic every particle obeys, what is the statistics of the system?
c) What are the spin and statistics of the proton?

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[110] R. W. Gurney and E. Condon, *Wave mechanics and radioactive disintegration, op. cit.*

[111] E. Rutherford, *Discussion on the structure of atomic nuclei,* Proceedings of the Royal Society A 123 (1929), p. 373.

[112] O. Klein, *Die Reflexion von Elektronen an einem Potentialsprung nach der relativistischen Dynamik von Dirac,* Zeitschrift für Physik 53 (1929), p. 157.

[113] G. Gamow, *Constitution of Atomic Nuclei and Radioactivity,* Oxford University Press, Oxford 1931.

We shall consider the first two questions in the most significant case, when all particles have half-integer spin, or have Fermi statistics, respectively. Quantum mechanics gives an immediate answer to a):

a) A system formed by *N* particles, each having half-integer spin, has integer spin if *N* is even, half-integer spin if *N* is odd.

The answer to b) is

b) A system formed by *N* particles, each having the Fermi statistics, obeys the Bose-Einstein statistics if *N* is even, the Fermi-Dirac statistics if *N* is odd.

The story of this question is more complex.[114] Its first rigorous deduction was given in a 1929 paper by Wigner.[115]

Question c) was studied by David M. Dennison[116] on the basis of a preliminary study by Friedrich Hund.[117] Dennison analyzed the anomalous behavior of the hydrogen at low temperatures and deduced that the proton has spin 1/2 and obeys the Fermi-Dirac statistics.

The first signs of the anomalous behavior of the nuclear electrons with respect to spin appeared before the formulation of quantum mechanics.[118]

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[114] A detailed analysis was made in G. Bruzzaniti, *op. cit.*

[115] E. P. Wigner, *Ősszetett rendszerek statisztikája az új quantum-mechanika szerint,* Matematikai és Terméѕzettudománti Értesítő 46 (1929), pp. 576 ff. (German abstract at p. 584). Being written in Hungarian, the paper was unnoticed by most physicists, and indeed in 1931 P. Ehrenfest and J. R. Oppenheimer (*Note on the statistics of nuclei,* Physical Review 37 (1931) p. 333), starting from different considerations, gave a new and definitive proof.

[116] D. M. Dennison, *A note on the specific heat of the hydrogen molecule,* Proceedings of the Royal Society A 115 (1927), p. 483.

[117] F. Hund, *Zur Deutung der Molekelspektren. II,* Zeitschrift für Physik 42 (1927), p. 93.

[118] Kronig was the first to notice an immediate consequence of Uhlenbeck and Goudsmit's hypothesis about the electron spin (R. Kronig, *Spinning electrons and the structure of spectra,* Nature 117 (1926), p. 550). Kronig had already thought of relating the electron spin to Pauli's exclusion principle, but, after a suggestion of Pauli's, did not publish the idea. According to Kronig, due to its spin, the electron must have an intrinsic magnetic moment, of the order of magnitude of Bohr's magneton. One should expect the same behavior when an electron is part of the nuclear structure. Then the nucleus should have an intrinsic magnetic moment of the same order of magnitude, unless a very unlikely mechanism takes place, namely, that the magnetic moments of all the electrons cancel each other. If this were true, by the Zeeman effect there would be a splitting of the energy levels, which is not observed. Fermi and Rasetti also entered the discussion, remarking that Kronig's objection had another consequence; a nuclear magnetic moment should induce a paramagnetic behavior of the atom, which, however, is not observed. Fermi and Rasetti, on the other hand, thought that the magnetic moments of the nuclear electrons could well cancel each other. However, they put forward another important consideration. The electron magnetic moment corresponds to some energy, which, according to the electromagnetic theory of mass, yields an increase of the mass, and therefore of the electron radius. They wrote "This value is about 20 times larger of what the electron radius is usually supposed to be. Actually, there are no direct measures of the electron radius; however, this is a serious drawback, as we know that the nucleus contains a large number of electrons. On the other hand, the linear dimensions of the nuclear structure are known with a fairly good precision [...] and they are of about 10^{-12} cm. The two facts cannot be

However, the most striking evidence was provided by a 1928 paper by Kronig,[119] who, interpreting Ornstein and van Wijk's[120] experiments on the spectrum of the nitrogen molecule, concluded that the spin of that molecule is 1. This allowed Kronig to detect the following anomaly: the nitrogen nucleus, having charge 7 and mass 14, is formed by 14 protons and 7 electrons, i.e., by an odd number (21) of particles of spin 1/2, so one should expect a half-integer value of the total spin, contrary to the experimental data. As Kronig wrote,

> We know that both electrons and protons have a rotational impulse $s = 1/2$, and one expects, therefore, that, as for the electrons outside the nucleus, the rotational impulse of an odd number of particles can only take half-integer values. [...] One is therefore compelled to believe that inside the nucleus protons and electrons do not maintain their identity, as they do out of the nucleus.[121]

Kronig's conclusion was typical of the standing of the entire scientific community; the anomalies concerning the presence of electrons inside the nucleus did not affect the conviction of their existence, but rather incepted a critical process that will end in a redefinition of the properties of the electrons.

The anomaly of the statistics followed a similar pattern. In 1929 Heitler and Herzberg,[122] while analyzing the results of a series of experiments on the Raman effect for hydrogen and nitrogen made by Rasetti,[123] concluded that the nitrogen nucleus obeys the Bose-Einstein statistics. Since according to the (p-e) model, as we saw above, the nucleus was formed by an odd number of particles obeying the Fermi-Dirac statistics, it should have obeyed that statistics as well. From Heitler-Herzberg's paper:

> This is quite an unexpected fact. The nitrogen nucleus contains altogether 14 protons and 7 electrons, or, if we figure out the maximum possible number of α-particles, it contains three α particles, two protons, and one electron. From quantum mechanics one obtains that systems made up of an even/odd number of protons or electrons obey the Bose/Fermi statistics, since protons and electron themselves obey the Fermi statistics. This rule, if Rasetti's measurements are correct, does not apply anymore in the nucleus [...] It seems

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reconciled, unless one assumes that the nature of the electron changes substantially when it is part of the nuclear structure." (Fermi [35], p. 227.)

[119] R. Kronig, *Der Drehimpuls des Stickstoffkerns,* Naturwissenschaften 16(1928), p. 335.

[120] L. S. Ornstein and W. R. van Wijk, *Untersuchungen über das negative Stickstoffbandenspektrum,* Zeitschrift für Physik 49 (1928), p. 315.

[121] R. Kronig, *Der Drehimpuls des Stickstoffkerns, op. cit.*

[122] W. Heitler and G. Herzberg, *Gehorchen die Stickstoffkerne der Boseschen Statistik?*, Naturwissenschaften 17 (1929), p. 673.

[123] F. Rasetti, *Raman effect in gases,* Nature 123 (1929), p. 205; *On the Raman effect in diatomic gases,* Proceedings of the National Academy of Sciences 15 (1929), p. 234; *Selection rules in the Raman effect,* Nature 122 (1929), p. 757; *On the Raman effect in diatomic gases II,* Proceedings of the National Academy of Sciences 15 (1929), p. 515; *Incoherent scattered radiation in diatomic molecules,* Physical Review 34 (1929), p. 367; *Alternating intensities in the spectrum of nitrogen,* Nature 124 (1929), p. 792; *Sopra l'effetto Raman nelle molecole biatomiche,* Nuovo Cimento 6 (1929), p. 356.

> that the electron, inside the nucleus, loses, together with its spin, also the right to take part in the statistics of the nucleus (in the sense of the previously cited rule). [. . .] Quite evidently, the mechanical properties of nuclear electrons are modified much more deeply than those of protons and α particles. Only the conservation of charge is certain.[124]

As with spin, also in this case the scientific community, faced by the failure of all attempts to apply quantum mechanics to the (p-e) model, did not doubt of the existence of the electrons inside the nucleus, but rather hypothesized a possible dependence between the properties of the electron and its role inside the atom.

Anomaly of the continuous spectrum of the β decay. The history of the discovery of the β decay is very long. It started in the first years of the 20th century and ended in 1933 with Fermi.[125] The most important dates are 1914 and 1927; let us see what happened in those years.

In 1914 Chadwick discovered that the electrons emitted by the radioactive substances have a continuous spectrum. This gave rise to a serious problem. Indeed this result radically deviated from the general picture of radioactivity as provided by the *OQT,* which on the other hand was well confirmed by the α and γ emissions:[126]

$$\text{parent nucleus} \rightarrow \text{daughter nucleus} + \text{emitted particle.}$$

If one admitted the existence of stationary states for the nuclei, i.e., states with well-defined constant energy, so that the energies of the parent and of the daughter nucleus had some fixed value, then the β particles had to have a well-defined energy, while the experiments showed a range of values for the energy, continuously distributed between a minimum and a maximum (see Figure 2.15). The principle of conservation of energy was at stake, unless one speculated, like Lisa Meitner,[127] that the measured energy distribution originated from a secondary phenomenon. According to Meitner, the electrons were emitted by the nuclei with a fixed speed, corresponding to the upper limit of the energy spectrum, but then, due to collision processes with the atomic electronic structure, lost part of their energy, giving

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[124] W. Heitler and G. Herzberg, *Gehorchen die Stickstoffkerne der Boseschen Statistik?, op. cit.*

[125] There is an extensive literature about the history of the β decay. We may cite, among the works of general nature that contain further references: A. Pais, *Inward Bound,* Oxford University Press, New York 1986; *Niels Bohr's Times, op. cit.;* C. S. Wu and S. A. Moszkowski, *Beta Decay,* Academic Press, New York 1966; G. Bruzzaniti, *op. cit.*.

[126] Very accurate measurements of the γ-rays frequencies allowed Ellis (Ch. D. Ellis, *The magnetic spectrum of the β-rays excited by γ-rays,* Proceedings of the Royal Society 99 (1921), p. 261; *β-rays spectra and their meaning, ibid.* 101 (1922), p. 1) to introduce also for nuclei the notion of "stationary state." In the second paper, Ellis remarkably wrote, about the measurement of the frequency of the γ-rays emitted by radium B: "The information [. . .] about the energies of the stationary states of the radium B nucleus is extra-ordinarily detailed, but, on the other hand, this information is very limited. There is no evidence which indicates whether these levels are occupied by positively charged particles or by electrons."

[127] L. Meitner, *Über die Entstehung der β-Strahl-Spektren radioaktiver Substanzen,* Zeitschrift für Physik 9 (1922), p. 131; *Über den Zusammenhang zwischen β und γ Strahlen, ibid.*, p. 145; *Über die β-Strahl-Spektra und ihren Zusammenhang mit der γ-Strahlung, ibid.* 11 (1922), p. 35.

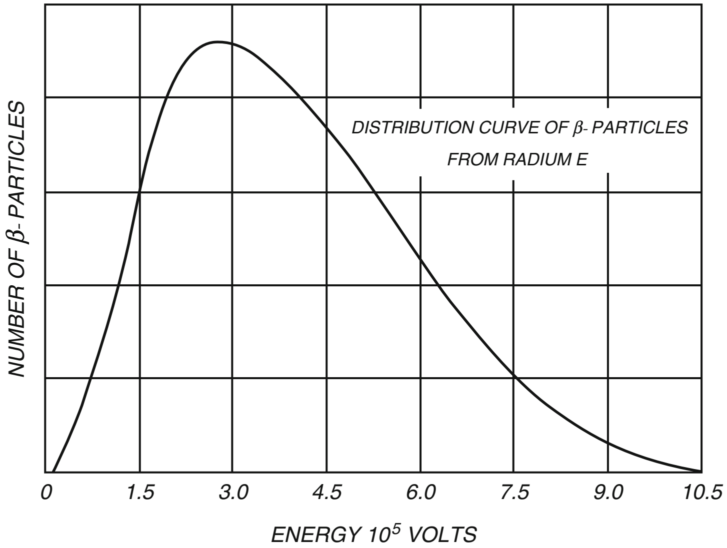

Fig. 2.15 The average energy of electrons emitted by radium E as measured by Ellis and Wooster.

rise to a continuous spectrum. This proposal immediately sparked an animated controversy; the continuous spectrum was the result of a primary phenomenon, the emission of electrons with the measured energy, or rather of a secondary phenomenon?

The answer was given in 1927 by a famous experiment made by Ellis and Wooster,[128] based on a simple procedure that made it "completely free from any hypothesis." They placed the substance emitting the electrons inside a calorimeter, whose walls could completely absorb the β radiation, together with any other radiation. Now, if quantum mechanics was correct, each emitted electron carried an energy e, and if N electrons were emitted, a total energy $E = e \times N$ was discharged during the process. Even if there was some secondary process (the emitted electrons interacted with the orbital electrons, for instance), and if the conservation of energy held, all the energy E would have been sooner or later revealed by the calorimeter. The average energy would have amounted to 1,05 MeV. However, the measurements gave the value of 350 keV, with an experimental uncertainty of 40 keV. This perfectly fitted the value provided by the curve in Figure 2.15 (390 keV), thus confirming that the energy spectrum was continuous.

Three hypotheses were put forward to solve this serious anomaly (the existence of a continuous spectrum): a) abandon the principles of quantum mechanics; b) abandon the principle of conservation of energy; and c) assume that during the β

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[128] C. D. Ellis and W. A. Wooster, *The average energy of disintegration of radium E,* Proceedings of the Royal Society A 117 (1927), p. 109.

emission, an unknown particle is emitted, so that the sum of its energy with that of the emitted electron equals the difference between the energies of the parent and the daughter nucleus. The proposal a) was mainly supported by Ellis,[129] b) by Bohr,[130] while c), which would turn out to be correct explanation, was backed by Wolfgang Pauli. Appendix B.2 includes the letter in which Pauli introduced, in a somehow unusual way, the hypothesis of the existence of this new particle, called "neutron" by Pauli, and then identified by Fermi with the neutrino. There Pauli used the term "exchange theorem" to refer to the spin-statistics connection.[131]

2.8.7 New discoveries and first nuclear theories

The years from 1928 to 1930 were decisive for the nuclear protophysics. The application of quantum mechanics to the theory of the nucleus sparked the crisis of the (p-e) model, and triggered a growing interest in nuclear physics, as witnessed by the first Conference of Nuclear Physics that took place in Rome in 1931, in the eve of a series of discoveries that laid the foundations for Fermi's theory of the β decay. This in turn induced the abandonment of the (p-e) model.

In 1932, the "annus mirabilis" according to Emilio Segrè's definition, three extraordinary discoveries were made.[132]

Deuterium. On 5 December 1931 Harold C. Urey, Ferdinand C. Brickwedde, and George M. Murphy announced the discovery of deuterium, an isotope of hydrogen of mass 2, whose relative abundance with respect to usual hydrogen is about 1/4,000.[133] The hydrogen atom, which is the simplest atomic structure, was the best candidate for investigations on atomic physics, with the hope of getting a better understanding of more complex atomic structures; analogously, the deuterium nucleus, the simplest among the complex nuclei, gave hopes to reach a better understanding of the laws that regulate, in general, the structure of all nuclei.

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[129] *Ibid.*

[130] N. Bohr, *Faraday Lectures: chemistry and the quantum theory of the atoms constitution,* Journal of the Chemical Society (1932), p. 349.

[131] For more details see Appendix C.5.

[132] Among the many historical reconstructions of these discoveries, we mention the following: E. Amaldi, *From the discovery of the neutron to the discovery of nuclear fission,* Physics Reports 11 (1984), p. 1; J. Six, *La découverte du neutron,* Editions du Centre National de la Recherche Scientifique, Paris 1987; M. De Maria and A. Russo, *The discovery of positron,* Rivista di Storia della Scienza 2 (1985), p. 237.

[133] H. C. Urey, F. C. Brickwedde and G. M. Murphy, *A hydrogen isotope of mass 2,* Physical Review 39 (1932), p. 164. The paper by R. Stuewer, *The naming of the deuteron,* American Journal of Physics 54 (1986), p. 206, is an interesting rendering of the debate that between 1993 and 1935 took place about the naming of the mass 2 isotope of hydrogen.

Positron.

> On August 2, 1932, during the course of photographing cosmic-ray tracks produced in a vertical Wilson chamber (magnetic field of 15,000 gauss) designed in the summer of 1930 by Professor R. A. Millikan and the writer, the tracks shown in Fig. 1 were obtained, which seemed to be interpretable only on the basis of the existence in this case of a particle carrying a positive charge but having a mass of the same order of magnitude as that normally possessed by a free negative electron.[134]

With these words, Carl D. Anderson communicated in 1933 the discovery of a new particle, that he called "positron." The previous year he had published a short paper in the journal *Science.* Anderson's discovery was confirmed by Blackett and Occhialini,[135] who related the existence of the positron with the hypotheses formulated by Dirac in 1930. Dirac had shown that the validity of a relativistic wave equation for the electron implied the existence of electron states of both positive and negative energy. To overcome this difficulty, Dirac made the hypothesis that all negative energy states, called "gaps," are fully occupied. Since electrons obey the Pauli exclusion principle, no transition to a negative energy state is possible. If, for some reason, a gap should not be occupied, it would behave as a positively charged electron.

Neutron. The neutron was discovered in 1932, but the story actually started in 1930, when Bothe and Becker,[136] bombarding light atoms with α particles emitted by polonium, obtained very penetrating secondary rays. The effect is very strong with beryllium, but is also present with lithium, boron, fluorine, aluminum, magnesium, and sodium. Bothe and Becker's interpretation was that the radiation is of an electromagnetic nature. By measuring the absorption by lead of the radiation emitted by beryllium and boron, they managed to estimate the energy of the radiation. The next step toward the discovery of the neutron was taken by Irène Curie and Frédéric Joliot, who repeated Bothe and Becker's experiment with the aim of testing the existence of other effects produced by the penetrating radiation while traveling in matter.[137] The most striking result was obtained when the secondary radiation produced by beryllium hit some hydrogenated substance; in this case the ionizing effect became almost twice as strong with respect to the Po + Be case (i.e., when radiation emitted by beryllium was absorbed by lead). Substances not containing hydrogen produced on the contrary no increase in the ionization. The Joliot-Curies, after several experimental checks, concluded that the hydrogenated

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[134] C. D. Anderson, *The positive electron,* Physical Review 43 (1933), p. 491.

[135] P. M. S. Blackett and G. P. S. Occhialini, *Some photographs of the tracks of penetrating radiation,* Proceedings of the Royal Society A 139 (1933), p. 699.

[136] W. Bothe and H. Becker, *Kunstliche Erregung von Kern γ–Strahlen,* Zeitschrift für Physik 64 (1930), p. 289.

[137] F. Joliot and I. Curie, *Emission de protons de grande vitesse par les substances hydrogénées sous l'influence de rayons-γ très pénétrants,* Comptes Rendus de l'Académie des Sciences 194 (1932), p. 273; *Effet d'absorption des rayons-γ de très haute frequence par projection de noyaux légers, ibid.,* p. 708.

substances hit by the radiation emitted protons, due to a Compton effect between the photons in the Po + Be radiation and the hydrogen nuclei contained, e.g., in paraffin. James Chadwick, after a careful analysis, stressed some serious problems in this interpretation of the emission of protons as due to the Compton effect.[138] His observations left no room to doubt: Joliot-Curies' interpretation necessarily implied the violation of the principles of conservation of energy and momentum. The difficulties disappeared if one interpreted the radiation exited by α particles in beryllium as formed by particles of zero charge and mass one, i.e., "neutrons." In Chadwick's words:

> It has been shown by Bothe and others that beryllium when bombarded by α-particles of polonium emits a radiation of great penetrating power, which has an absorption coefficient in lead of about 0.3 cm^{-1}. Recently Mme. Curie-Joliot and M. Joliot found, when measuring the ionisation produced by this beryllium radiation in a vessel with a thin window, that the ionisation increased when matter containing hydrogen was placed in front of the window. The effect appeared to be due to the ejection of protons [...] They suggested that the transference of energy to the proton was by a process similar to the Compton effect, and estimated that the beryllium radiation had a quantum energy of 50×10^6 electron volts. I have made some experiments [...] These results, and others I have obtained in the course of the work, are very difficult to explain on the assumption that the radiation from beryllium is a quantum radiation, if energy and momentum are to be conserved in the collisions. The difficulties disappear, however, if it be assumed that the radiation consists of particles of mass 1 and charge 0, or neutrons. [...] It is to be expected that many of the effects of a neutron in passing through matter should resemble those of a quantum of high energy, and it is not easy to reach the final decision between the two hypotheses. Up to the present, all the evidence is in favour of the neutron, while the quantum hypothesis can only be upheld if the conservation of energy and momentum be relinquished at some point.[139]

The discovery of the neutron had an enormous impact on nuclear protophysics, but not because it was an expected event, that the scientific community was awaiting to solve the problem of the nuclear structure. In a sense, the discovery of the neutron did not directly trigger the end of the (p-e) model; it rather made the problem more complicated, but in an intelligent way.

Dmitri Ivanenko was among the first who regarded the neutron as new constituent of the nucleus. The Russian physicist, in a short but important paper in *Nature,* wrote:

> Is it not possible to admit that neutrons play also an important rôle in the building of nuclei, the nuclei electrons being all packed in α-particles or neutrons?[140]

It is not hard to interpret Ivanenko's question as the germ of a strategy that would characterize the first "post-neutron" nuclear theories: try to solve the anomalies of the (p-e) model by means of suitable hypotheses on the structure of the neutron.

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[138] J. Chadwick, *Possible existence of a neutron,* Nature 129 (1932), p. 312.

[139] *Ibid.,* pp. 312 ff. A more extended and detailed discussion of the experiment was communicated by Chadwick to the Royal Society on 10 May 1932; cf. J. Chadwick, *The existence of a neutron,* Proceedings of the Royal Society A 136 (1932), p. 692.

[140] D. Ivanenko, *The neutron hypothesis,* Nature 129 (1932), p. 798.

Another sentence by Ivanenko shows how the underlying principles of the (p-e) model were still strongly upheld by the scientific community:

> The lack of a theory of nuclei makes, of course, this assumption rather uncertain, but perhaps it sounds not so improbable if we remember that the nuclei electrons profoundly change their properties when entering into the nuclei, and lose, so to say, their individuality, for example, their spin and magnetic moment.[141]

The proceedings of the 7th Solvay Conference (1933)[142] provide a detailed testimonial of the discussions about the structure of the neutron that were taking place at that time. The main question was, is the neutron an elementary particle, or it is formed by a proton and an electron? The second hypothesis was corroborated by the measurement of the neutron mass; if it was smaller than the mass of the hydrogen atom, made up by a proton and an electron, one could think that the mass difference was due to the energy necessary to bind the electron and the proton. However, Chadwick[143] made it clear that that was not a correct reasoning. According to quantum mechanics, the hydrogen atom is the only possible combination of a proton and an electron. Moreover, if the neutron had that structure, the hydrogen atom should transform into a neutron, releasing energy. There is also an argument about spin. Experimental data attribute the neutron spin 1/2 and Fermi statistics, and since also the proton has spin 1/2, if the neutron was an electron-proton combination, the electron should have spin zero and obey the Bose statistics, contrary to the experimental evidence.

So, the discovery of neutron in 1932 was not enough to dispose of the nuclear electrons. This clearly emerges from Gamow's report at the Solvay Conference, where he stressed the possibility of building up models where the nucleus was formed by electrons and protons, together their compounds, such as neutrons and α particles. And also accepting that the neutron was elementary, the nuclear electrons still existed. Dirac clearly stated this:

> I think there is no final evidence against the hypothesis that electrons, having a nonzero spin and obeying the Fermi statistics, can be inside the nucleus as elementary particles. If we consider protons and neutrons as elementary particles, we have three kinds of elementary particles forming the nuclei. This number may seem to be big, but from this viewpoint, two is already a big number.[144]

The neutron thus was regarded as a new nuclear constituent, which joined the previously known constituents, but did not replace them.

This incertitude about the role of the nuclear electrons can be seen in what was the first attempt to a general schematization of the atomic nucleus, the Heisenberg-

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[141] *Ibid.*

[142] *Structure et propriétés des noyaux atomiques. Rapports et discussions du Septième Conseil de Physique,* Gauthier-Villars, Paris 1934.

[143] J. Chadwick, *Diffusion anomale des particules α, ibid.,* p. 102.

[144] P. A. M. Dirac, *Théorie du positron, ibid.,* p. 203; W. Heisenberg, *Considérations théoriques générales sur la structure du noyau, ibid.,* p. 328.

Majorana theory. This term refers to a paper by Heisenberg, published in three parts in 1932–1933,[145] followed by a 1933 paper by Ettore Majorana, which included important changes. Heisenberg's proposal was based on five assumptions:

a) Nuclei are made of protons and neutrons. The neutron has spin 1/2 (in units $h/2\pi$) and obeys the Fermi-Dirac statistics.
b) The neutron may be formed by an electron and a proton. In this case the electron inside the neutron loses its spin and obeys the Bose-Einstein statistics. Moreover the disintegration of a neutron onto an electron plus a proton does not obey the conservation of energy.
c) The interaction between neutron and proton is not representable as an ordinary force, but rather as an "exchange force." The model which suggests this hypothesis to Heisenberg is the ionized hydrogen molecule H_2^+, whose chemical bond results from the two protons sharing one electron. In the same way, according to Heisenberg, a proton and a neutron interact inside a nucleus by exchanging a common electron. As D. G. Cassidy nicely put it,

> In a sense, the neutron and proton play a wild game of catch with the one available electron, the proton turning into a neutron when it catches the electron, the neutron turning into a proton when it releases the electron — then back again.[146]

d) Again by analogy with the hydrogen molecule, this time not ionized, the neutron-neutron interaction is attractive, and is negligible with respect to the neutron-proton interaction. Both must vanish at distances greater than approximately 10^{-12} cm.
e) The proton-proton interaction is only due to the Coulomb force.

These assumptions were part of a framework that was very innovative as regards the way the proton electric charge was treated, in analogy with the electron spin; for this reason, Heisenberg's hypothesis was called "isotopic spin formalism." Its basic feature was the attribution to the heavy particles of a new coordinate r', that could only take the values 1 and -1: when $r' = 1$ the particle was a neutron, when $r' = -1$ a proton. The theory had many consequences, some of which, as it has been noticed in several historical reconstructions,[147] were evidently contradictory. As written by Cassidy,

> In other words, Heisenberg's neutron was simultaneously indivisible (fundamental), compound, and a contradiction of both quantum mechanics and conservation laws![148]

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[145] W. Heisenberg, *Über den Bau der Atomkerne, I,* Zeitschrift für Physik 77 (1932), p. 1; II, *ibid.* 78 (1932), p. 156; III, *ibid.* 80 (1933), p. 587. On this aspect of Heisenberg's work, see the excellent biography by D. Cassidy, *Uncertainty. The Life and Science of Werner Heisenberg,* Freeman, New York 1992, and J. Bromberg, *The impact of the neutron: Bohr and Heisenberg,* Historical Studies in the Physical and Biological Sciences 3 (1971), p. 307; D. M. Brink, *Nuclear Forces,* Pergamon Press, London 1965. The latter also contains the English translation of some papers of Heisenberg and Majorana's.

[146] David G. Cassidy, *Beyond Uncertainty: Heisenberg, Quantum Physics, and the Bomb.* Bellevue Literary Press, New York 2009. p. 203.

[147] See references in note 145.

[148] David G. Cassidy, *op. cit.,* p. 202.

Indeed, on the one hand, the isotopic spin formalism introduced a perfect symmetry between proton and neutron, and therefore gave the two particles the same ontological status; but on the other hand, the theory hypothesized an attractive interaction between neutrons, and a Coulomb interaction between protons. This, at least implicitly, denoted that the proton had a truly elementary nature, while the neutron was still composite: the attractive force between two neutrons was a manifestation of the fact that actually they are two protons which exchange two electrons.

In any case, Heisenberg's theory was prone to a very serious criticism: the proton-neutron interaction depended on the directions of the spin of the two particles. This implied that the proton-neutron bond leads to saturation: it is not possible that a second neutron binds to the proton. The theory also implied that the deuteron (the deuterium nucleus) was the most stable nucleus, contrary to the evidence that, as an analysis of the binding energies shows, it is the α particle which is most stable. Majorana had the merit of disposing of this difficulty by modifying the exchange interaction mechanism.[149] In his report at the 7th Solvay Conference, Heisenberg abandoned his original model in favor of the interaction mechanism proposed by Majorana. While the Heisenberg-Majorana theory was immediately hailed as "a general scheme of the nuclear structure," it was precarious and incomplete, as it was unable to solve the problem of the neutron structure. Moreover, the existence of the positron greatly amplified the problem of what really were the elementary particles; as observed by Anderson, why it is not possible that the neutron is elementary, and the proton is formed by a neutron and a positron?

As we see, the nuclear physics scenario had become much more complicated. The new discoveries had added new difficulties to the already existing anomalies, adding new pieces to a huge and intricate jigsaw. But at least, all pieces were there. The problem was to find the key idea to decipher the mystery, which at the time was still the methodological principle of the (p-e) model; if a particle is emitted by a nucleus, then it already existed inside the nucleus. This was clearly expressed by Heisenberg's above mentioned report at the 7th Solvay Conference: "To believe that electrons are part of the nuclei has no precise meaning, up to the fact that some nuclei emit β particles."

Enrico Fermi had the merit to understand that the way to reconstruct the jigsaw was to get rid of that principle. The decisive paper appeared in 1933,[150] and contained the basic idea of his theory; the electrons emitted in the β decay do not pre-exist inside the nucleus, but are created at the moment of their emission. This is analogous to what happens with the electromagnetic field; a photon is created at moment of its emission, as a consequence of the transition of a charged particle between two different quantum states. Moreover, to preserve the conservation of

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[149] As already noted, according to Heisenberg, the proton-neutron interaction was due to the exchange of an electron, while spin was left unchanged. Majorana, on the contrary, postulated that the interaction involved the exchange of both charge and spin.

[150] Fermi [76, 80a].

energy, Fermi endorsed Heisenberg's proposal of a new particle, the neutrino, which is created together with the electron during a β decay process.

In addition to the genial assumption that nuclei are only formed by protons and neutrons, Fermi's theory was based on several ideas, that we give here in the author's words.

> a) The total number of electrons and neutrinos is not necessarily constant. Electrons (or neutrinos) can be created or annihilated. This possibility however has no analogy with the creation of an electron-positron pair; indeed, if one interprets a positron as a Dirac "hole," the latter process can be simply regarded as a quantum jump of an electron from a negative energy state to one with positive energy, with conservation of the total (infinite) number of electrons.
>
> b) According to Heisenberg, the heavy particles, neutron and proton, are two different inner quantum states of the same particle. This will be formalized by introducing an internal coordinate of the heavy particle, which takes only two values: $r = 1$ if the particle is a neutron, $r = -1$ if the particle is a proton.
>
> c) The Hamiltonian function[151] of a system formed by heavy and light particles must be chosen so that every transition from neutron to proton is accompanied by the creation of an electron and a neutrino. Note that in this way there is conservation of the total electric charge.[152]

Fermi's formalism allowed him to encompass inside a unified theoretical framework the non-contradictory aspects of the Heisenberg-Majorana theory (nuclei formed only by protons and neutrons), the method of second quantization, the quantization of the electromagnetic field, and the description of the 1/2 spin particles provided by the Dirac equation. By disposing of the nuclear electrons, the theory solved the spin, statistics, and confinement anomalies. Moreover, the reaction hypothesized by Fermi

$$\text{neutron} \rightarrow \text{proton} + \text{electron} + \text{neutrino}$$

explains the continuous spectrum of the β decay without giving up the principle of conservation of energy. The last section of Fermi's paper was indeed devoted to the "comparison with the experimental data." He showed that the speed distribution of the β rays as predicted by his theory fitted the experimental data, up to a small disagreement at low energies.

These were already remarkable achievements, but even more important was the conceptual structure of the theory, which decreed the end of the (p-e) model, setting the grounds for the construction of a theoretical language capable of treating not only problems in nuclear physics, but also in a new theory, that was indeed incepted by Fermi's work: the theory of elementary particles. But this is another story, and we shall deal with it in the 4th Chapter.

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[151] The Hamiltonian function is a function which is associated with a physical system and determines its evolution, both in classical and quantum mechanics.

[152] Fermi [80a], *CPF I,* p. 560.

2.9 A note of the dynamics of the global maps: the regulating principles and the birth of nuclear physics

Figure 2.16 describes the research lines that in the first 30 years of the 20th century led to the creation of a subject called "nuclear physics." The picture reproduces the plan of this second chapter: the reconstruction of a fragment of the global maps of our knowledge in which it is possible to identify Enrico Fermi's legacy. This reconstruction is possible only *a posteriori,* and does not include processes that can be attributed to the protagonist of the moment.[153] It is like describing Christopher Columbus's 1492 enterprise: from the viewpoint of global maps we are talking of the discovery of a new continent, that cannot be reduced to the project of the Genoese sailor of discovering a new way to the Indies; this is rather the local research itinerary.

We have given special attention to the development of nuclear protophysics, stressing how it can be identified with the birth, maturation, and final decline of the (p-e) model, that is, of the idea that there are electrons inside the nucleus. In 1931 Gamow published the first treatise on nuclear physics, entirely based on the (p-e) model. From the first page the author makes it clear that, in accordance with the tenets of modern physics, all nuclei are "built up elementary particles — protons and electrons."[154] The book pays much attention to the role of the α particles. Formed by four protons and two electrons, an α particle is so stable that it can be treated as a single entity. It is this remark that gives rise to the classification of the atomic nuclei into four classes: $4n$, $4n + 1$, $4n + 2$, and $4n + 3$, with n an integer number. Since protons aggregate in the most stable configurations, the nuclei of the first type must be formed only by α particles, while in the nuclei of the other types there are one, two, or three free protons, respectively. A similar argument is applied to the electrons; there exist nuclei where not all nuclear electrons are bound to α particles. The question here becomes very delicate, and indeed Gamow decided to mark with a special typographical character ($\sim$) the sections concerning the difficulties raised by the properties of the nuclear electrons.

Gamow's treatment essentially concerned the difficulties that here we have called "anomalies," whose structure is represented in Figure 2.17. As shown by the documents, these difficulties did not affect the plausibility of the underlying principles of the (p-e) model, but rather changed the meaning of the term "electron."

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[153] I owe this remark to G. Bachelard, who characterized the history of science in these terms: "[. . .] it is a history that starts from the certainties of the present time and discovers in the past the progressive forms of the truth. Thus the history of science appears as the most irreversible of all histories. By discovering the truth, the man of science cancels irrationality. Irrationalism may certainly appear elsewhere, but by now some routes are impossible. The history of science is the history of the defeats of irrationalism." (G. Bachelard, L'activité rationaliste de la physique contemporaine, PUF, Paris 1965.)

[154] G. Gamow, *Constitution of Atomic Nuclei and Radioactivity,* Clarendon Press, Oxford 1931. p. 1.

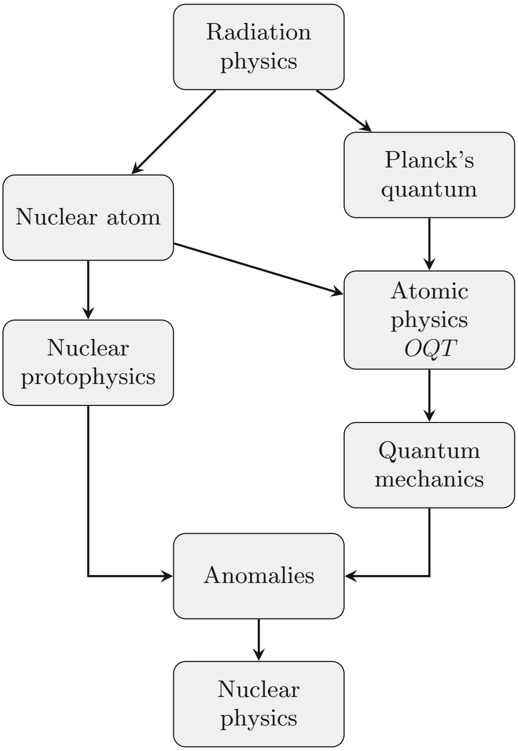

Fig. 2.16 Main research lines leading to the creation of nuclear physics.

In the early 30s, so, the term "electron" denoted a rather complex object. When it was not inside an atomic nucleus, it had mass, charge, statistical properties, and spin, and participated in processes where the conservation of energy held. On the contrary, when it was inside a nucleus, it lost all its properties, with the only exception of charge and mass. Moreover, it took part in processes where the energy was not conserved. Even the extraordinary discoveries made in 1932 did not affect the (p-e) model. Neutrons entered the atomic nuclei, but did not solve the anomalies of the nuclear electrons, that indeed were regarded as constituents of the neutrons.

It is not difficult to find the roots of such an obstinate defense of the (p-e) model in the methodological principle that we have often mentioned; if a particle is emitted by a nucleus, then it pre-existed in the nucleus. It was a deeply rooted conviction, that had controlled from the outset the evolution of the nuclear protophysics, jeopardizing any attempt to develop a quantum theory of the atomic nucleus. It is interesting to compare the starting sentence of Gamow's book we have cited above, or Heisenberg's statements at the 7th Solvay Conference, with what Bethe and Bacher wrote in a 1936 paper that would have become the Bible of nuclear physics:

> Therefore we are forced to assume that the *electrons observed in β-disintegration did not pre-exist in the emitting nucleus. We suppose that they are formed in the same moment when*

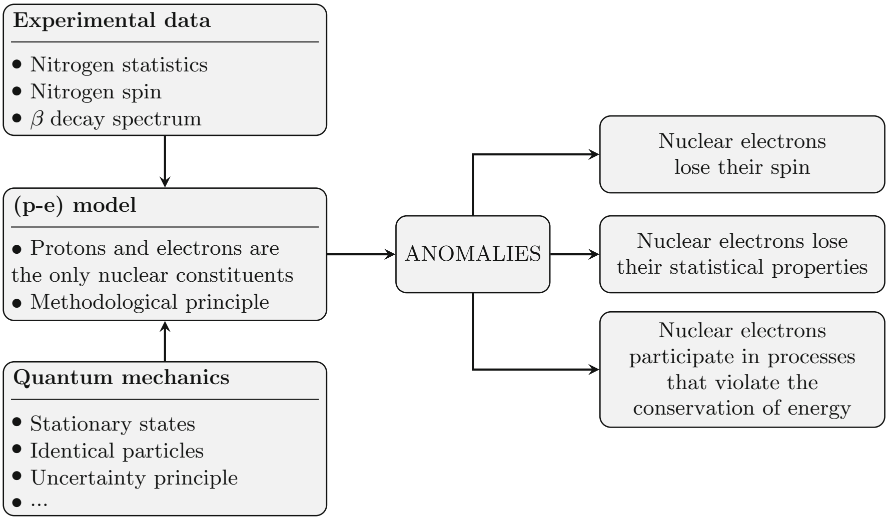

Fig. 2.17 Anomalies of the (p-e) model. These did not induce the abandonment of the notion of nuclear electrons, but changed the properties that were attributed to them.

> *they are actually emitted,* and that it is this process of formation which is so improbable that it accounts for the long lifetime of β-emitting nuclei.[155]

and again, with reference to the nature of the neutron:

> [...] the *neutron* should *not* be considered as *composed of a proton, an electron and a neutrino, but is only able of transforming into these three particles,* and similarly for the proton.[156]

It was the final demolition of the principle of pre-existence of the emitted electrons in the nuclei, and the birth certificate of the modern theories of the atomic nucleus. This certificate, as recorded in the global maps, bears Enrico Fermi's signature.

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[155] H. A. Bethe, R. F. Bacher, *Nuclear Physics,* Review of Modern Physics vol. 8 (1936), p. 184. Italics in original.

[156] *Ibid.,* p. 189. Italics in original.

Chapter 3
Enrico Fermi: research itineraries 1921–1933

What was Enrico Fermi's scientific itinerary, and how did it impact on the Italian and international scientific landscape? When and how his research paths intersected the global maps of knowledge? Here and in Chapter 5 we try to answer these questions. Before starting, let us spend a few words about the method we shall follow. In reconstructing the global maps, the "influences" and the "confluences" are evaluated *a posteriori*. For instance, we can say that the Raman effect measurements made by Rasetti on the nitrogen molecule had an influence on the development of the (p-e) model, regardless of the fact that Rasetti was not interested in the structure of nuclei, but rather in the Raman effect. One can say the same about the confluence of different lines of research, whose significance can only be assessed with the wisdom of the hindsight. As Fermi said in a 1927 speech,

> It has often happened in the history of the natural sciences that the most important advances resulted from the merging of research lines that up to that moment were considered to be completely different.[1]

The criteria that guide the local research itineraries are very different. In this case the historian tries to reconstruct the creative process of the scientist, with its interactions with the social and cultural environment. These processes usually ensure the local continuity of the scientific enterprise, and do not highlight those fractures that only appear in the global reconstructions.

3.1 Systematics of Fermi's research between 1921 and 1933

Fermi's scientific production started in 1921 with some theoretical papers. However, it was clear from the beginning that three characters coexisted in Fermi: the theoretical physicist, the experimenter, and the teacher and popularizer. In the first

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[1] Fermi [41], *CPF I,* p. 251.

G. Bruzzaniti, *Enrico Fermi*, Springer Biographies,
DOI 10.1007/978-1-4939-3533-8_3

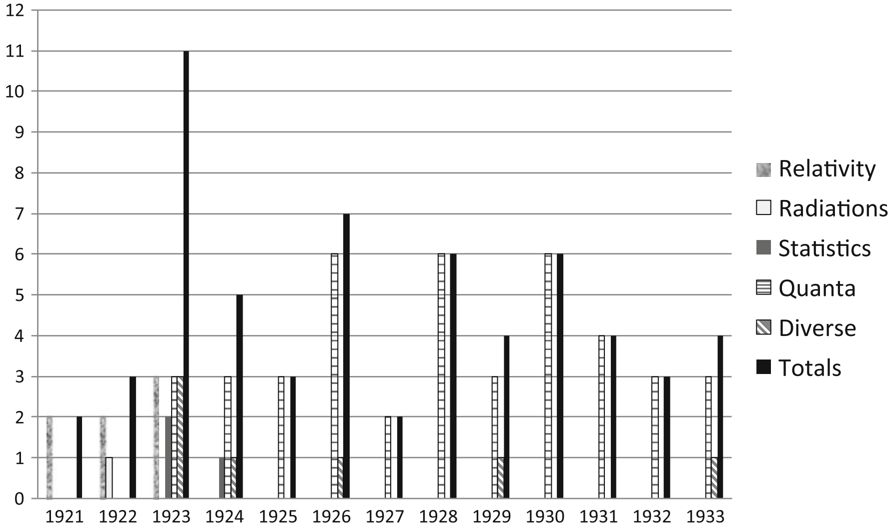

Fig. 3.1 Distribution of Enrico Fermi's theoretical papers between 1921 and 1933.

stage of his scientific career, between 1921 and 1933, Fermi published 77 papers, among which 60 were theoretical, 7 experimental, and 10 were popularizations. Papers that were sent to different journals have been counted only once, and the same for different editions of the same paper, although sometimes these are somewhat different from the first version. Figures 3.1 and 3.2 show the amount of Fermi's production. In particular, Figure 3.1 shows the distribution of his papers with respect to the area of research (some papers concern more than one area).

In addition to writing this imposing number of papers, all of them of very high quality, in the same period Fermi also wrote two textbooks: one on atomic physics (the first ever published in Italy) and one for the high school.[2]

Comparing Figure 3.1 with Figure 5.1, which shows Fermi's scientific production between 1934 and 1954, one clearly notes that the year 1933 neatly separates two stages in Fermi's scientific life. After that year, Fermi's theoretical production decreased markedly, while the number of experimental papers sharply increased. In 1934, for instance, Fermi published only three theoretical papers (one of them was the final version of the paper on β decay of the previous year), and 15 experimental ones, all devoted to artificial radioactivity.

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[2] E. Fermi, *Introduzione alla fisica atomica* [Introduction to atomic physics], Zanichelli, Bologna 1928; *Fisica ad uso dei Licei* [Physics for the high school], Zanichelli, Bologna 1929.

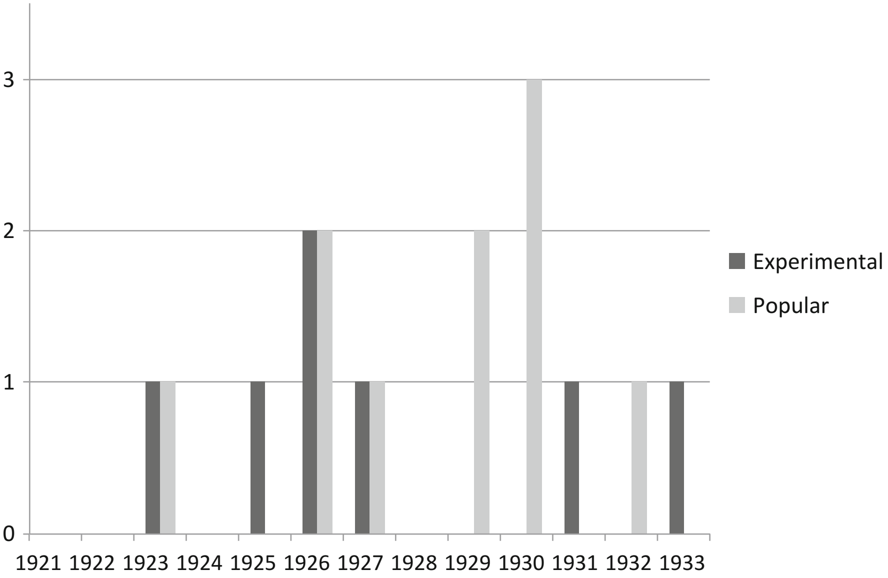

Fig. 3.2 Distribution of Enrico Fermi's experimental and popular papers between 1921 and 1933.

3.2 Italian physics in the 20s

In 1871 there were in the Italian universities 13 professors of physics, and 15 assistant professors. In 1926 the professors were 20, and the assistant professors 45. Also the number of universities did not increase much; they were 25 in 1870, and 32 in 1926.[3] The structure of the program leading to the physics degree did not change much either; a typical "plan of study," at the beginning of the 20th century, included courses in calculus, algebra, analytical geometry, and experimental physics in the first year; the second year included calculus again, descriptive geometry, and chemistry; experimental physics, classical mechanics, and problem solving in physics, in the third year; and eventually problem solving in chemistry and theoretical and experimental physics, and a course of free choice in the fourth year. A degree program with this structure shows very clearly that in the first 20 years of the 20th century, the study of physics in Italy was still deeply rooted in the tradition of the previous century. In particular, there was a strong emphasis on the experimental aspects, thus shielding the study of physics from the most innovative trends that were taking place at the international level. The marginal role of Italian physics in the international landscape is confirmed by the analysis

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[3] The data in this section are taken from two very detailed papers on this topic: P. Marazzini, *Nuove radiazioni, quanti e relatività in Italia: 1896–1925,* La Goliardica Pavese, Pavia 1996; G. Giuliani, *Il "Nuovo Cimento,"* La Goliardica Pavese, Pavia 1996.

of the papers published in the two most authoritative specialized Italian journals of the time, *Nuovo Cimento* and *Rendiconti dell'Accademia dei Lincei.* Between 1905 and 1924, when on the other side of the Alps special and general relativity were created and developed, and the Old Quantum Theory lived its short life, *Nuovo Cimento* published 18 papers devoted to relativity or quantum mechanics written by Italian authors, out of a total of about 900; and *Rendiconti* published only 17.

More than a simple lack of interest of the physics community for the new theories, there was an actual and widespread hostility for them. In the same years when important expeditions were organized to test the predictions of general relativity, one of the most authoritative Italian physicists of the time, Augusto Righi, was still skeptical about special relativity, and severely criticized the interpretation of Michelson and Morley's experiment, arguing that its outcome was wrong.[4] Righi's was not an isolated voice. Other illustrious Italian physicists were equally distrustful. Fermi's predecessor in the direction of the physics section of the *Enciclopedia Italiana,* Michele La Rosa, in 1912 disputed Einstein's postulate on the constance of the speed of light,[5] and did not change his mind until the 20s.[6] The first quantum theories were received in the same way. There was a hardly concealed distrust, most often expressed with silence, which produced an absurd delay in their dissemination and acceptance. The first paper written by an Italian physicist about the black body radiation was published in 1909,[7] however, very surprisingly, to describe the spectral distribution of the radiation, it did not use Planck's new formula, but Wien's old one. Still in 1917, in one of the most popular treatises on elementary physics,[8] the author described the atomic structure only citing Thomson's 1904 model as an "ingenious attempt."[9] Meanwhile, in the rest

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[4] It may be interesting to report some of his observations: "[...] One should think that, if the Michelson-Morley experiment had not been invented, or the misleading delusion of that interference pattern had not been formulated, plausibly nobody would have conceived the contraction hypothesis, and nobody would have thought of the relativity principle, thus shaking certain basic intuitive ideas, which perhaps, after all, as any intuition, are nothing but logical consequences unconsciously deduced from the intellectual knowledge accumulated by the race during centuries of observations and rational use of the human intelligence." (A. Righi, *Sulle basi sperimentali della teoria della relatività,* Nuovo Cimento 19 (1920), p. 142).

[5] M. La Rosa, *Fondamenti sperimentali del secondo principio della teoria della relatività,* Nuovo Cimento 3 (1912), p. 350.

[6] M. La Rosa, *La velocità della luce si compone con quella della sorgente? Prove in favore offerte dai fenomeni delle "stelle variabili" e delle "nuove,"* Atti della Reale Accademia dei Lincei. Rendiconti, 1 (1923), p. 590. In general, the space devoted to relativity theory in the Italian textbooks was very scant, if it was mentioned at all.

[7] V. Polara, *Sul potere emissivo dei corpi neri,* Atti della Reale Accademia dei Lincei. Rendiconti, 18 (1909), p. 513.

[8] O. Murani, *Trattato elementare di fisica,* Hoepli, Milano 1917.

[9] An ample documentation on the standing of the Italian physicist with respect to the emerging quantum theories can be found in P. Marazzini, *op. cit.* and G. Giuliani, *op. cit.*

of the world, the 1918 Nobel Prize for physics was attributed to Max Planck for his investigations on the energy quanta, and four years later it was bestowed on Niels Bohr for his research on atoms and radiation.

The situation was radically different for mathematics and mathematical physics. Already in 1870, the level of Italian research in mathematics was very high; Enrico Betti (1823–1892), Francesco Brioschi (1824–1877), Luigi Cremona (1830–1903), Eugenio Beltrami (1835–1900), and Felice Casorati (1835–1890) were the most important figures in a new season of the Italian mathematics. After a deep lethargy that started after Lagrange, important schools were founded, dealing with calculus and algebraic and differential geometry. Ulisse Dini (1845–1918), Giuseppe Veronese (1854–1917), Guido Castelnuovo (1865–1952), Federigo Enriques (1871–1946), Luigi Bianchi (1856–1928), Gregorio Ricci-Curbastro (1853–1925), Tullio Levi-Civita (1873–1941), Guido Fubini (1979–1943), Giuseppe Peano (1858–1932), Francesco Severi (1879–1943), and Vito Volterra (1860–1940) deeply influenced the history of mathematics, and their value and importance were adequately recognized. This is confirmed by the number of chairs in mathematics; they were 59 in 1871, and 64 in 1926, more than four and three times, respectively, than the chairs in physics in the same years.

The high level and international prestige attained by the Italian mathematics reflected positively on another important area of investigation, mathematical physics. From the methodological point of view, this discipline can be hardly distinguished from mathematics, since it is based on the rigorous proofs of theorems. The physical aspects, the comparison with the experiments, play a role in mathematical physics, but in a different way than in theoretical physics; the investigations are mostly led and catalyzed by the mathematical aspects. This discipline developed in the early 20th century especially in connection with classical mechanics (called "rational mechanics" in Italy, as in France), including analytical mechanics, with continuum mechanics (elasticity, hydrodynamics), potential theory (a branch of calculus), and the theory of relativity. The fact that mathematicians made researchers in relativity is particularly important. While relativity and quantum theories were basically ignored by the Italian physics community of those years, at least relativity entered the Italian scientific and cultural life via mathematical physics. For instance, Gregorio Ricci-Curbastro created the so-called *absolute differential calculus,* and his pupil Levi-Civita earned a place in the history of physics by using it in general relativity in a crucial way.[10]

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[10] One can see, for instance, S. Di Sieno, A. Guerraggio and P. Nastasi, *La matematica italiana dopo l'unità* [Italian Mathematics after unity], *op. cit.,* and G. Fichera, *L'analisi matematica in Italia fra le due guerre* [Mathematical analysis in Italy between the two wars], *op. cit.* In Fichera's paper one can find part of the report made by Einstein in 1929 to propose Levi-Civita's candidacy to the Berlin Academy of Sciences: "[. . .] with the notion of parallel transport, he has created a very useful tool to study the theory of the *n*-dimensional continuum. This notion not only has led to a wonderful simplification of the Riemannian theory of curvature, but, most of all, has given rise to some generalizations of geometry, whose theoretical importance has been amply recognized, and whose physical significance cannot yet be gauged."

The fact that the Italian mathematical physicists showed no interest for quantum mechanics is not accidental, but it is rather due to the different role played by the experiments in the two theories. Quantum mechanics, contrary to relativity, was developed by means of a steady comparison with experiments, which most often suggested new hypotheses, and determined the way the theory was constructed.

The hegemonic role of mathematical physics is most likely the reason why theoretical physics had no official recognition in Italy until 1926. Mathematical physics was in practice a branch of mathematics, and this had disastrous effects from the cultural viewpoint. Mathematicians had full control of the professor positions in mathematical physics, and this prevented new ideas and developments in physics from entering the Italian environment. The situation was getting worse and worse, since it was impossible to train new researchers to work on the new theories.

The situation changed with Orso Mario Corbino, who was responsible for the creation in 1926 of the first three chairs in theoretical physics, that were given to Fermi, Persico, and Pontremoli. As we saw in Chapter 1, Corbino's action started after the assignment of a chair in mathematical physics at the University of Cagliari, in 1925. The chair was given to Giovanni Giorgi, and Enrico Fermi ranked second. The assignment of the chair in Cagliari, and the almost immediate creation of the first chair in theoretical physics, reflected to some extent the contrast between different cultural approaches and academic powers. However, they also showed Corbino's deep conviction, and of other important figures in the Italian mathematical physics community, that it was necessary to open the Italian cultural and academic environments to the main lines of thought that were prevailing in Europe. Corbino's merits, however, have to be shared with Enrico Fermi, who, at the age of 25, and as a self-taught researcher (actually, the only option he had), opened a new page of the Italian physics.

3.3 Fermi's two itineraries

As we saw in Chapter 2 in our investigation of the global maps of the international physics research in the first quarter of the 20th century, the great themes under study were the theory of relativity and quantum mechanics. As we have noted, those theories were virtually ignored by the Italian physics community. However, already at the beginning of the 30s, Italian physics started playing an important role on the international scene. This fast improvement was basically due to Fermi, not only for his scientific merits, but also for his great skills in teaching and in the organization and coordination of research. That allowed him to gather a number of young brilliant researchers, who were going to deeply mark the history of the 20th century physics.

The papers published by Fermi during the first 12 years of his activity (1921–1933) can be divided into two groups according to the topic treated, i.e., relativity and quantum mechanics. Let us analyze them in some detail.

3.4 The beginnings

Fermi’s debut as a scientific researcher took place within the theory of relativity. That first stage lasted only two years, but was very important from the historic and biographic viewpoint, both for the importance of the results and for the precocious emergence of some aspects of Fermi’s complex scientific personality. Fermi’s first publication was a paper in theoretical physics, dated January 1921. The author was not yet 20, was in the third year of the physics program and studied a topic that in the Italian universities was not officially recognized. In the first two years of his research activity, Fermi published nine papers.[11] Two of them are the first and second part of a work of which Fermi was particularly proud, and were published in the *Rendiconti* of the “Accademia dei Lincei.”[12] The two parts were published again, with small changes and as a single paper, by *Nuovo Cimento,*[13] and, in German translation, by the prestigious journal *Physikalische Zeitschrift.*[14] For reasons that will be clarified later on, the 7th paper was particularly important. It was a popular paper published as an appendix to the Italian translation of a German treatise on relativity.[15] The last paper of this series devoted to the theory of relativity is the only one of this period written in collaboration (Figure 3.3).[16]

3.5 The “4/3 saga,” Fermi coordinates, and the atomic bomb

An important record of Fermi’s early activity is the booklet that was found in Chicago among his personal documents (cf. Section 1.2). It has 25 pages, dated “Rome, July 1919,” and contains a resume of the theories of special relativity, of the black body radiation, and of diamagnetism and paramagnetism. It cites the second edition of a famous book by Owen W. Richardson,[17] an advanced treatise based on a course given in Princeton, dealing with the cutting-edge topics of the physics of those years, from relativity to quanta. At the beginning of the treatise we can read that the electron is a particle consisting of “nothing more than a geometric configuration of electricity, whose mass, therefore, is entirely of electromagnetic origin.”[18] And later on, “The idea of electromagnetic inertia, due to J. J. Thompson,

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[11] Fermi [1–3, 4a, 4b, 4c, 5, 8, 10].

[12] Fermi [4b].

[13] Fermi [4c].

[14] Fermi [4a].

[15] Fermi [5].

[16] Fermi [10], written with A. Pontremoli.

[17] O. W. Richardson, *The Electron Theory of Matter,* Cambridge University Press, Cambridge, U.K. 1916 (first ed. 1914).

[18] *Ibid.,* p. 8.

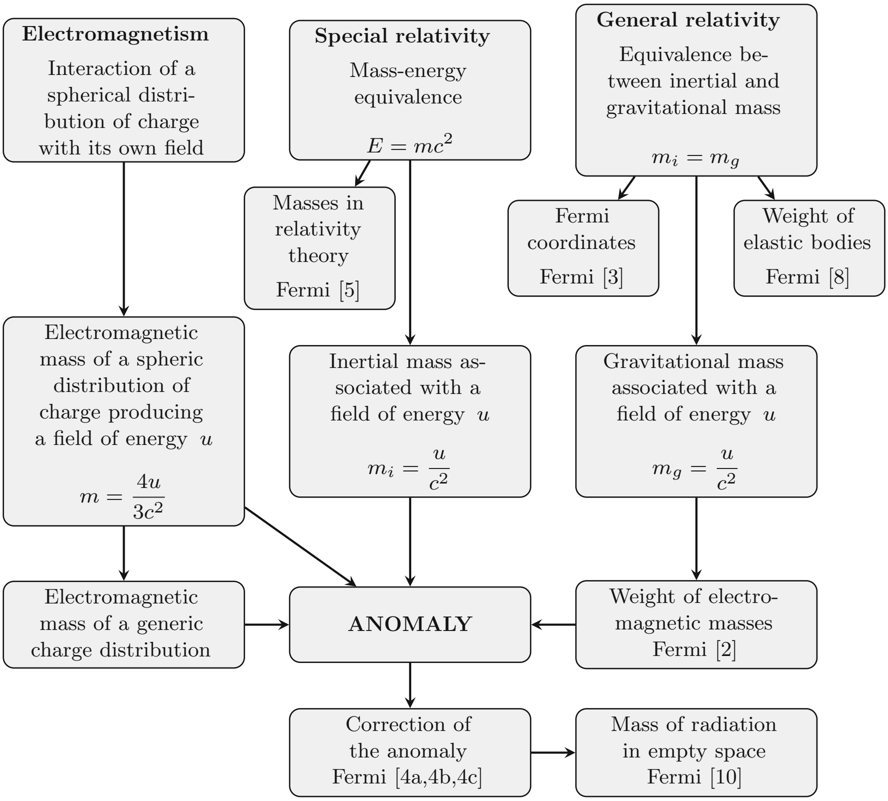

Fig. 3.3 Enrico Fermi's research in relativity. The numbers in brackets refer to the bibliography.

is a basic one in the electronic theory of matter, since it allows the possibility that the mass of all matter is nothing more than the electromagnetic mass of the electrons that are part of it — and perhaps are all of it."[19] Richardson's book most likely played an important role in Fermi's early research, and was indeed the only reference in his first paper.

Still as a student, Fermi chose the computation of the electromagnetic inertia of a system of electric charges in translational motion as his first research problem (see Appendix C.9). The logical path he followed was perfectly linear. First, he generalized the expression of the electromagnetic mass, already obtained by several authors (including Richardson) in the case of a spherical distribution of electric charge, to the case of any system of charges. This first result is quite remarkable; the electromagnetic mass of the system is no more a scalar, but is rather a tensor, which in the case of spherical symmetry reduces to the value $m = 4u/3c^2$ (u is the energy of the field). The next step was the removal of the assumption of small

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[19] *Ibid.*, p. 229.

speeds; this was accomplished using the principles of Einstein's relativity theory. As Fermi wrote, this first paper, where the electromagnetic mass was considered as an inertial mass, was to be followed by a second one, where the gravitational properties of the electromagnetic mass were considered.

The second paper is dated March 1921, and its scope is stated in the first few lines. "The purpose of this paper is to apply the general theory of relativity to investigate the alteration produced by a uniform gravitational field on the electrostatic phenomena that take place in it."[20] The idea made a lot of sense; if an arbitrary amount of energy is associated with an inertial mass given by the ratio between the energy and the square of the speed of light, and the inertial mass equals the gravitational mass, then also the energy of the electrostatic field of any system of charges is associated with a gravitational mass. The direct calculation of the electromagnetic mass, based on the general theory of relativity, required a series of deductions that show the great skill and high proficiency that the young scientist already possessed. After showing that a gravitational field applied to a conducting sphere produces a polarization, that is, a modification of the distribution of charge on the surface of the sphere, Fermi gave the most significant application, i.e., the computation of the gravitational mass of a distribution of electric charges. According to Fermi's words, "we reach in this way the conclusion that *the weight of an electromagnetic charge always has a vertical direction, and equals the weight of a material mass* u/c^2*,*"[21] where u is the electrostatic energy of the system.

Why was this result so important? The answer was given in the introduction to the paper:

> We find that the gravitational mass, that is, the ratio between weight and gravitational acceleration, does not coincide in general with the inertial mass, as, for instance for a spherically symmetric system, the latter is given by $4u/3c^2$.[22]

The disagreement highlighted by Fermi was another manifestation of the conflict between special relativity and the electromagnetic derivation of mass. To solve it, the young scientist wrote a paper that was published between 1922 and 1923 in three different journals.[23] Fermi was justly proud of this work. Enrico Persico reports a recollection by Polvani:

> The problem was discussed in Pisa on a winter evening in 1922, while Fermi, Puccianti, Polvani and other friends were walking in Via San Frediano, from the University to Scuola Normale Superiore, where the group split without reaching a satisfactory conclusion. During the next two days Fermi did not show up at the Institute of Physics. On the third day, he arrived with a paper ready for publication, entitled "Correction of a serious discrepancy..." Puccianti, who had stressed the need for a clarification, was exceedingly happy.[24]

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[20] Fermi [2], *CPF I,* p. 8.

[21] *Ibid.,* p. 16 (italics in the original).

[22] *Ibid.,* p. 8.

[23] Fermi [4a, 4b, 4c].

[24] E. Persico, *CPF I,* p. 24.

Fermi's approach was very clear:

> This discrepancy was particularly jarring in two recent papers. In one of them, on the basis of the standard theory of electrodynamics, I considered the electromagnetic masses of systems having any symmetry, finding that in general they are represented by tensors, and not scalars. These tensors reduce naturally to the quantity $4u/3c^2$ in the case of spherical symmetry. In the second paper, starting from the general theory of relativity, I considered the weight of those systems, which I found to be always given by uG/c^2, where G is the gravitational acceleration. In this paper we shall prove, in a precise way, that the difference between the two values of the mass originates in the notion of rigid body that is used in electrodynamics, which contradicts the principle of relativity, and leads to the value $4u/3c^2$ for the mass. However, another notion of rigid body, which is more justified and compatible with the theory of relativity, leads to the value u/c^2.[25]

The tool that allowed Fermi to clarify the deep roots of the disagreement between the two formulas, its "original flaw" in his words, was Hamilton's principle. The way it was applied was taken from one of the most advanced treatises available at the moment, that of Hermann Weyl.[26] It was a highly complex text, whose reading required very sophisticated notions. However, Fermi made his best to make his paper as self-sufficient as possible. Weyl's book provides a systematic treatment of special and general relativity, and devotes much space to tensor calculus, "by means of which, alone, — as Weyl wrote in the preface to the first edition — it is possible to express adequately the physical knowledge under discussion."[27]

Fermi used the fourth edition, where Weyl had included a chapter about a theory developed by Mie, which can be considered as the first attempt ever to explain the existence of charged elementary particles.[28] Weyl gave precise prescriptions, faithfully followed by Fermi in his paper, on the application of Hamilton's principle to the deduction of the equations of the electromagnetic field. In a nutshell, Hamilton's principle, or principle of least action, states that the dynamical evolution of any physical system must minimize a certain quantity, called *action.*[29] According to Fermi, to apply Hamilton's principle to the case of a charged sphere in a translational motion, one needs to consider infinitesimal variations of the coordinates of the charges in the system, respecting the constraint of rigidity of the motion.

What does it mean that a translational motion of a body is rigid? It means that there is a reference frame in which the body always has the same shape. This interpretation is coherent with the theory of relativity, as Max Born had already

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[25] Fermi [4c], *CPF I,* p. 26.

[26] H. Weyl, *Raum, Zeit Materie,* Springer, Berlin 1918; English translation *Space Time Matter,* Methuen, New York 1952.

[27] H. Weyl, *op. cit.* (English translation), pp. ix–x.

[28] G. Mie, *Grundlagen einer Theorie der Materie,* Annalen der Physik 37 (1912), p. 511; *ibid.,* 39 (1912), p. 1; *ibid.* 40 (1913), p. 1. A concise description of Mie's proposal is contained in W. Pauli, *Theory of Relativity,* Pergamon Press, Oxford 1958.

[29] For a conservative system, the action is the difference between the kinetic and potential energy, which is evaluated on an evolution of the given system, and integrated between two fixed instants of time. Hamilton's principles affirm that the system will evolve so as to minimize (or, to be more precise, extremize) this quantity.

recognized in 1909;[30] it is however Fermi’s merit to have understood that this interpretation solves the controversial question of the 4/3 factor. Fermi’s main result, thus, is the proof that the choice of a set of variations of the positions that satisfies the rigidity constraint expressed in this way yields the result u/c^2, in accordance with the relativistic principle of equivalence between mass and energy.

The factor 4/3 also appears in the computation of the mass associated with the electromagnetic radiation contained in a box with reflecting walls. The application of the method devised to solve the “serious discrepancy” that emerged in 1922 allowed Fermi to obtain, together with Pontremoli, the value that one expects according to the theory of relativity. The paper containing this result was published in 1929[31] and concluded Fermi’s work on relativity.

Fermi also wrote two papers about general relativity, published in 1922 and 1923.[32] The first paper left a mark on the history of the relativistic theory, tying the scientist’s name to the so-called “Fermi’s coordinates.” The choice of a coordinate system is vital in the study of the physical phenomena; laws that seem very complicated take a simpler form when expressed in a suitable reference. To give a trivial example, let us think of a point constrained to move on a spherical surface of radius R; in a cartesian coordinate system, the equation verified by the coordinates of the point is $x^2 + y^2 + z^2 = R^2$, while in polar coordinates (r, ϑ, φ) the same condition assumes the simpler form $r = R$. The 1922 paper starts from the possibility to define, within the general theory of relativity, a coordinate system which simplifies the study of the physical phenomena that take place in a small region of space. This result, that was defined by Segrè as “Fermi’s first accomplishment of permanent value,”[33] affirms that, given any world line (the path described in space-time by a particle), one can always find a “simple” local reference system; in the author’s words, “a suitable coordinate system, such that near the given world line, the element ds^2 takes a simple form.”[34]

The deep geometric notions needed to prove the existence of such coordinate systems were taken by Fermi from Weyl’s treatise, and from Levi-Civita (the only sources cited in the paper). In particular, Fermi drew from Levi-Civita the notion of “parallel transport,” which was essential to the definition of Fermi’s coordinate system.[35] And it was Levi-Civita who, in his *Lectures on absolute differential calculus,* stressed the importance of Fermi’s contribution:

> In general, a system of co-ordinates in which ds^2 is represented by a form with constant coefficients is called Cartesian. It is not always possible to choose co-ordinates of this kind in a given V_n; it is however always possible to find a system of co-ordinates which

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[30] M. Born, *Die Theorie des starren Elektrons in der Kinematik des Relativitätsprinzip,* Annalen der Physik 30 (1909), p. 840.

[31] Fermi [10].

[32] Fermi [3, 8].

[33] E. Segrè, *Enrico Fermi Physicist,* The University of Chicago Press, Chicago 1970, p. 22.

[34] Fermi [3], *CPF I,* p1̇7.

[35] T. Levi-Civita, *Lezioni di calcolo differenziale assoluto (Raccolte e compilate da Enrico Persico),* Stock, Roma 1925, p. 187.

> behave like Cartesian ones *in the immediate vicinity of a point P assigned beforehand,* or, more precisely, which are such that the derivatives of the coefficients of ds^2 (which would vanish identically if the co-ordinates were Cartesian) all vanish at the point P. Such co-ordinates are called *locally geodesic,* or *locally Cartesian,* co-ordinates. [...] Prof. Fermi has recently established an important extension of this result by showing that, given any curve whatsoever, it also possible to choose co-ordinates which are locally geodesic at every point of the curve.[36]

The second paper on general relativity was published in 1936.[37] It was another sophisticated piece of work in mathematical physics. The problem was the weight of the elastic bodies. From a relativistic viewpoint, the mass of an elastic body is not constant, but depends on how the body is stretched; the elastic potential energy, indeed, must correspond, according to the relativistic equivalence between mass and energy, to an increase of the inertial mass of the body. But there is more. What happens if an elastic body is placed in a space whose geometry is not Euclidean? This is the true, basic problem. Indeed in this situation the geometry of the space originates stretches, which imply an increase of the gravitational mass. Fermi's conclusion is that the weight of an elastic body is formed by

> ... two distinct parts. The first is nothing but the usual weight of the mass, and has the same direction of the gravitational acceleration G. The second, which is due to the deviation of space from the Euclidean geometry, has in general a different direction, determined by the curvature [....] Therefore it can exists also when the first does not exist; namely, an elastic body can weigh also in a field where there is no gravitational acceleration in the usual sense.[38]

Levi-Civita and Armellini, in the report accompanying the presentation of the paper to the Accademia dei Lincei, wrote:

> The way the work is done show the author's complete mastering of the analytical techniques. [...] the referees deem that Dr. Fermi's work gives a sound and substantial contribution to the problem it studies, and therefore propose its full inclusion in the proceedings of the Academy.[39]

To close our reconstruction of Fermi's work on the theory of relativity, we want now to analyze a paper that was included in the Italian translation of a German introductory treatise on relativity.[40] Fermi's contribution was entitled *Mass in the theory of relativity.* It was a popularizing paper, but not less important for this reason. In a moment when the relativistic theories were not yet justly recognized in the Italian universities, Fermi stressed the most important aspect of relativity from the experimental viewpoint: the convertibility between mass and energy,

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[36] *Ibid.,* p. 190.

[37] Fermi [8].

[38] *CPF I*, p. 66.

[39] *Ibid.,* p. 60.

[40] A. Kopff, *I fondamenti della relatività einsteiniana,* R. Contu ed., Hoepli, Milano 1923. Original ed. *Grundzuge der einsteinschen Relativitätstheorie,* Hirzel, Leipzig 1921. English translation *The Mathematical Theory of Relativity,* Methuen & Company Limited, London, 1923.

according to the celebrated equation $E = mc^2$. This paper needs to be analyzed with care, as it reveals interesting aspects of Fermi's complex scientific personality, namely, what in the first chapter we called the "Galilean dominant." The very strict relation between theory and experiment did not only emerge in the practice of the investigation, that is, in the choice of the problems to study; but was rather a deeply rooted intellectual standing, which allowed Fermi to glimpse theoretical questions in his experimental works, and *vice versa,* and detect experimental issues in these theoretical constructions. The case of general relativity is very representative of this attitude; when that theory was considered by the Italian academe as a purely formal exercise, the expression of an "amazing mathematical construction,"[41] Fermi stressed its empirical aspects, which were important also from the quantitative viewpoint:

> The mass of a body, the theory of relativity says, is equal to its total energy divided by the square of the speed of light. A cursory examination already shows us that, at least for physics as it is done in the laboratories, this relation between mass and energy easily overshadows the other consequences, very slight from a quantitative viewpoint, to which our mind gets accustomed with a bigger effort. Just an example: a body, three feet long, moving at the respectable speed of 20 miles per second (approximately, the Earth's speed in space), would appear to have the same length of three feet to an observer moving along with it. To a standing observer, its length would appear to be three feet, minus 20 billionths of a foot. This effect, however strange and paradoxical it may appear to be, is very small, and the two observers will definitely not quarrel over such a trifle. The relation between mass and energy, on the contrary, is expressed by huge numbers. For instance, by releasing the energy contained in an ounce of matter, one would obtain the energy produced in 84 years of continuous work of a 1000 HP engine (no comments needed!).[42]

So, for Fermi the most consequential fact about relativity is not the radical revision of the Kantian categories of space and time, as the philosophers often emphasize, but rather the equivalence between mass and energy; this is indeed what entails the most important consequences of the theory. Fermi went on with a sentence that looks like a premonition:

> One can rightly say that it does not seem possible, in the near future, to find the way to release these frightful amounts of energy. On the other hand, there is no reason to hope for this, as the first effect of the explosion of such a huge amount of energy would be to disintegrate the physicist who should have been so disgraced to find the way.[43]

The final part of the paper is also very interesting. Fermi showed how the relativistic relation between mass and energy is able to explain some important properties of matter.

> There are reasons to believe that the helium nucleus is formed by four nuclei of hydrogen. The atomic weight of helium is 4,002, and that of the hydrogen 1,0077. The difference between the quadruple of the mass of hydrogen and the mass of helium is due to the energy

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[41] P. Emanuelli, *Sulla documentazione astronomica della teoria della relatività* [On the astronomical evidence of the theory of relativity], contribution no. 5, *ibid.,* p. 341.

[42] Fermi [5], *CPF I,* p. 53.

[43] *Ibid.*

> of the bounds that keep together the four hydrogen nuclei to form the helium nucleus. This difference is 0,029, and, according to the relativistic relation between mass and energy, it corresponds to an energy of about 6 billion calories for a gram atom of helium. These figures show us that the energy of the nuclear bounds is some million times bigger than that of the strongest chemical bounds, and explain why the problem of the transformation of matter, the alchemists' dream, eluded the efforts of the best minds for so many centuries. Only now, using the most energetic means at our disposal, we have been able to obtain this transformation, and only in very tiny quantities that defy the most accurate analyses.
>
> These brief hints should be enough to prove how the theory of relativity, in addition to giving us a clear interpretation of the relations between space and time, will be, perhaps in some near future, the key for solving the problem of the structure of matter, the ultimate and hardest problem in physics.[44]

3.6 Why relativity?

Fermi's relativistic itinerary developed between 1921 and 1923. In 1922 he took his Physics degree. In the same period Fermi published three more papers, that cannot be included in this itinerary, nor in the quantum one, which was starting exactly at that period.[45] Two were about X-rays, and were basically taken from his thesis. The first was quite long, 30 pages, and dealt with the basic aspects of the problem; the second, much shorter, was devoted to the experimental work on the creation of images by means of X-rays.[46] The third paper was about the dragging of the polarization plane by a rotating medium.[47] It was a sophisticated exercise in electromagnetism, and most likely it was one of those side problems that he used to meet during his research, and was able to solve quickly, getting a result that was suitable for publication.[48]

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[44] *Ibid.*

[45] The moment when Fermi started researching on quantum theory can be determined with precision. In a letter to Persico dated 18 March 1922 he wrote: "In addition to this work I have devoted myself to *Quantentheoretische* [sic] *Betrachtungen,* [theoretical questions about quanta] which up to now have brought me to a justification of the blackbody formula from the point of view of Bohr's theory; they could perhaps carry me much further if it were not for the almost insurmountable difficulties caused by the extremely complicated calculations required. The fundamental idea of this investigation is to consider the atom and the electromagnetic field to which it is coupled as a single system and to calculate the *statischen Bahnen* [stationary states] of this system as a whole. The maximum program would be to remove all the incompleteness of Bohr's theory. I expect, however, that because of the difficulties I mentioned above I will not be able to conclude the investigations (E. Segrè, *op. cit.,* p. 199).

[46] Fermi [6, 7].

[47] Fermi [9].

[48] According to Segrè, Fermi around 1950 gave this problem as a PhD exam at the University of Chicago. A student intuited that it was a question that Fermi had studied during the first years of his activity, went to the library, and found the paper. It seems this was the only student who gave the correct solution.

So, during this period, Fermi's research was almost entirely focused on the theory of relativity. It is quite natural to wonder whether this choice was dictated by other reasons than his personal interest. After all, the available documentation (letters and recounts by friends and collaborators) tells us of a self-taught young researcher, who was educated on the most advanced available texts, ranging from relativity to the structure of matter and *OQT*. Moreover, as he wrote to his friend Persico,[49] in the Institute of Physics in Pisa he was considered as the most influential authority about quantum theory, of which he was a keen propagandist. Why then Fermi addressed his researches mainly to relativity? We can only make some conjectures. As we already saw, both quantum theory and relativity were at the time still received with hostility by the Italian physics community, but there was a marked difference between the two theories. Quantum theory, which was at the center of the interest of the international scientific community, was in Italy a "no man's land," deemed as uninteresting by the mathematical physicists, and considered as a necessary evil by those physicists who did not oppose it openly. In 1912 Corbino (who later changed his mind radically) wrote, referring to Planck:

> The hypothesis of sudden changes of the molecular energy, by exact multiples of the quantum $h\nu$, is deeply at variance with all received mechanical conceptions [...] since energy is a quantity for which we cannot conceive a discontinuity [...] While no one dares to defend the theory because of its intrinsic content, nobody is able to find a way out, or at least to provide some evidence that that will be possible in the future, and that we shall be able to disenthrall the admirable edifice of Theoretical Physics from such an obnoxious but necessary guest.[50]

After nine years the situation had not changed much. Rosa Brunetti, definitely one of the physicists who were most favorable to the quantum theory, referred to Bohr as "the Danish mathematician,"[51] thus confining the quantum theory to mathematical physics, while Enrica Tedeschi, referring to Millikan who had experimentally verified Einstein's equation for the photoelectric effect, wrote:

> However, he, as many other physicists, considers as untenable the semi-corpuscular theory that Einstein uses for its derivation. Untenable because on the one hand, it is at variance with our notion of energy, and on the other hand, it fails to explain the interference phenomena, which have on the contrary completely confirmed Maxwell's electrodynamics.[52]

The situation was different for relativity. While it had not yet gained a general consensus, it was supported by a number of mathematical physicists, and was the object of a cultural debate that included several philosophers. This was substantiated

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[49] See Chapter 1, page 6.

[50] O. M. Corbino, *La teoria dei quanti e le sue applicazioni all'ottica e alla termodinamica* [Quantum theory and its applications to optics and thermodynamics], Nuovo Cimento 3 (1912), p. 368.

[51] R. Brunetti, *Il nucleo atomico* [The atomic nucleus], Nuovo Cimento 22 (1922), p. 215.

[52] E. Tedeschi, *Il fenomeno fotoelettrico* [The photoelectric phenomenon], Nuovo Cimento 23 (1922), p. 133.

by the Italian translation of the book by Kopff,[53] a treatise of high quality, that had been published in Germany in 1920. The Italian edition was supplemented by two contributions by Guido Castelnuovo and Tullio Levi-Civita, and a long appendix entitled *Value and interpretation of the theory.*[54] The translation and publication of this book, due to Raffaele Contu and Tomaso Bembo, was a conscious cultural operation, as shown by Contu's preface to the appendix. "Relativity — he wrote — is triune. It is one because is it an organic system, but it has three faces: Mathematics, Physics, and Philosophy. But no face can be separated from the others." In the editors' intentions, the appendix was to contain 40 contributions, and it is reasonable to assume that they wanted to offer the widest possible panorama of interpretations, from the most favorable to the most hostile; this hypothesis is supported by two contributions by foreign authors. One, by Émile Borel, was quite cautious; the other, signed by Hermann Weyl (Fermi's main reference in his first works), was a beautiful essay, expressing an unconditional trust:

> [...] I believe that with the theory of relativity, the physical thought has gained wide freedom, and a sure depth; it represents one of the main steps of the reconstruction of our Universe by means of reason.[55]

By inspecting the different contributions one can have an idea of the standing of the Italian academe with respect to relativity. Most had a position of cautious prudence, as the astronomer and mathematical physicist Emilio Bianchi or the mathematical physicist Pietro Burgatti,[56] or an open hostility, as the physicists

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[53] A. Kopff, *op. cit.*

[54] The appendix was made of two parts. The first *(On the astronomical evidence of the theory of relativity)* included contributions by astronomers, physicists, and mathematical physicists (Emilio Bianchi, Giovanni Boccardi, Piero Burgatti, Vincenzo Cerulli, Pio Emanuelli, Enrico Fermi, Guido Fubini, Giuseppe Gianfranceschi, Michele La Rosa, Quirino Majorana, Eugenio Rignano, Émil Borel, and Hermann Weyl); the contributions in the second part were of a philosophical character: Antonio Liotta *(The philosophical value of Einstein's theory)*, Alessandro Bonucci *(Idealistic aspects of the relativistic theories)*, Federigo Enriques *(Relativity of motion in ancient Greece)*, Ugo Spirito *(Einstein's relativism and philosophy)*, Adriano Tilgher *(The philosophical meaning of Einstein's theory)*, and Erminio Troilo *(Around the philosophical meaning of the theory of relativity)*.

[55] H. Weyl, in A. Kopff, *op. cit.,* p. 372.

[56] The contribution of Emilio Bianchi, director of the Brera Observatory, ends in this way: "[...] in recalling with complete impartiality the present state of the astronomical evidence of Einstein's theory, it seems fair to say that this evidence is, at present, very far from reaching that degree of soundness which is necessary in science. To be more precise, we want to give a cautious, but not skeptical judgement; a judgement, therefore, very different from that which is insistently given, about relativity and its astronomical evidence, by its most strenuous opponents (A. Kopff, *I fondamenti della relatività einsteniana, op.cit.,* p. 335). Burgatti, a professor of Classical Mechanics at the University of Bologna, gave a similar assessment: "The theory of relativity deserves the greatest attention; it is the work of a man of genius, and as such, it will have a great impact. But at the moment it has not been proved. Any firm judgment about its validity would be at present premature."

Giuseppe Gianfranceschi, Michele La Rosa, and Quirino Majorana.[57] Only the mathematicians Guido Castelnuovo,Tullio Levi-Civita, and Guido Fubini,[58] and Enrico Fermi are decidedly in favor. So one is led to think that young Fermi was

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[57] Gianfranceschi, a Jesuit, professor of physics, and rector of the Pontificia Università Gregoriana in Rome, expressed a very sharp opinion: "However, Minkowski's space-time continuum and Einstein's equivalence principle are just ideal notions. One cannot understand why, in a time like ours when so much emphasis is put on the criticism of the principles and of the postulates, somebody accepts the conclusions of general relativity so lightheartedly, as they were describing the physical state of things" (*ibid.,* p. 350). The harsh words of La Rosa, professor of experimental physics in Palermo, give an idea of the atmosphere in Italy: "My voice will be out of tune in the general choir that in the whole world has risen to exalt the new Word. Who tries to divest the theory of its rich mathematical apparel, and translate its wonderful results into concrete language, that is, into ideas and concepts, cannot but feel dizzied; and not only for the frightful demolitions of the general concepts that were at the very basis of our knowledge, but especially for the terrifying and horrible vacuum that it leaves with us" (*ibid.,* p. 351). Quirino Majorana, engineer and physicist, professor at the University of Bologna, was more measured but not less skeptical: "An experimental physicist can hardly express an opinion on Einstein's work. Evidently, he is not at ease with that topic, since he usually does not possess enough mathematical culture to understand in detail the new theories. [. . .] Also the remarkable observations and experiments that are often mentioned in support of Einstein's theories [. . .] leave the experimentalist skeptical. He, even admitting the correctness of the experiments, thinks, often without expressing this, that simpler theories could explain them (*ibid.,* p. 355)".

[58] Castelnuovo was professor of geometry at the University of Rome, and was one of the main exponents of the Italian algebraic geometry school. His contribution to the volume was 23 pages long and extremely clear. Castelnuovo was totally appreciative: "The theory of relativity calls today the attention of everyone who is interested in science. Somebody receives it with unlimited admiration, other with cold skepticism. While the confirmations given by the observations are mentioned by the former as triumphs of the theory, the latter consider them as uncertain, or susceptible of a different explanation. Now, without diminishing the value of those confirmations, one should remember that any physical phenomenon can be interpreted in many different ways. Therefore, it is not advisable to find one's judgement only on the basis of very delicate observations. It is rather necessary to assess whether the new theory provides, better than the old mechanics, a synthetic and harmonic view of the geometric and physical properties of space. [. . .] A theory is to be judged by the capacity it has to deduce from a few basic principles, and without artificial procedures, an explanation of the known phenomena, or predict new ones. If one looks at the many, seemingly disparate facts that Einstein's theory has been able to coordinate and present as natural consequences of a few simple postulates, one is led to attribute to that theory the character of a vital and fecund hypothesis. In any case, even without making any prediction, it is fair to recognize that, for the grandiosity of the ideas that have inspired the new views, and the intellectual movement" they have elicited, the theory of relativity is one of the main steps of the history of human thought (*ibid.,* pp. 283–305). Fubini, professor of higher analysis at Politecnico di Torino, expressed a more prudent, but still positive assessment: "The huge popularity of the new theory is well deserved, in view of its ingenuity, and the great intellectual power of its creator" (*ibid.,* p. 346). However, there were also mathematical physicists who totally disagreed with the theory of relativity. We may cite Boccardi, a priest, professor of astronomy at the University of Turin, and director of the observatory in Pino Torinese. He wrote: "One should have better left Relativity among the innumerable other geometric figments, or at least, to make it clear to the general public that these are cosmic theories formulated *a priori,* which have no justification. [. . .] There is an old Italian saying, "if it is not true it is well found," but in this case I would say perhaps "it is well found, but it is not true." (*Ibid.,* p. 336–337).

asked to contribute to the volume not because of his authoritativeness, which at the time was not yet much affirmed, but rather because he was perhaps the only physicist in Italy who was openly in favor of the theory of relativity. Thus, in the years when Fermi took his degree and started his research activity, the Italian academe had different positions with respect to the quantum theory and relativity — two theories which Fermi knew very well, and for which he had autonomously acquired all the needed skills and techniques. Quantum theory had no room within mathematical physics, and when not opposed, was given a lukewarm reception by the experimental physicists; relativity was usually dismissed by the physicists, but was cultivated by the mathematical physicists. In this perspective, the essay written by Fermi for Kopff's treatise was quite important from the cultural viewpoint, in that, by stressing the role of relativity in the study of the structure of matter, it made an attempt to make the theory of relativity known among the experimental physicists.

We can now make a conjecture about the reasons of Fermi's choice. He was well aware of his value, and, as Segrè wrote,[59] he knew that sooner or later his merits would have been recognized. His strong aspiration to a position in the academe appears clearly in the letters which in that period he wrote to his friend Persico. On 8 August 1922, about one month before his graduation, he wrote:

> As to the competitions (God be praised), last Saturday I took to the ministry the entire, heavy file of my documents — consisting of neither one nor two, nor three, nor four, nor five, neither six, nor seven, nor eight, nor nine, nor ten, but eleven publications, among which is the one I concocted while I was at S. Donato. It deals with the behavior of elastic bodies according to general relativity. In it I obtained some curious results, which I want to talk to you about. [. . .] I do not think I will note the results of the competition for a couple of months, because the committee will not meet before the end of September, the gentlemen of the committee having no desire to enjoy the heat of Rome. In any case, I will keep in touch with some big shot or other so as to know something as soon as possible, since my next decisions depend on the result. [60]

Fermi was referring to his application for a grant for a period of study abroad. He got the grant and used it to spend 8 months in Göttingen. And from there he wrote to Persico:

> Carrara has just written me that the University of Pisa has opened a competition in higher mechanics.
> Although the probability of winning is rather small, because as usual there will be applicants with 20 or more years' experience, I want to try in case — the more so since I have three rather important publications ready which might be considered as belonging to higher mechanics. I hope they arrive in time.
> By the way, I would like you to ask if there would be space for two or three of my publications, about 30 pages altogether, at the Lincei.[61] I write you not only to tell you

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[59] "He also knew that inevitably his abilities had to be recognized. He was in a hurry, however, and in typical fashion was ready to work hard to obtain his goal. His letters to Persico show the unexpected: Fermi active in academic maneuvers" (E. Segrè, *op. cit.*, p. 39).

[60] *Ibid.,* p. 203.

[61] Fermi means the journal *Rendiconti dell'Accademia dei Lincei.*

> of my plan but also to ask you to spread about some rumor of it — so that you may hear whether people consider the idea of my taking part in the competition too crazy.[62]

Fermi eventually did not apply to the opening in Pisa, but in 1925 he applied for a chair in mathematical physics at the University of Cagliari, after being charged in 1924 to teach that discipline at the University of Florence. “As you probably know — he wrote to Persico on 13 September 1924 from Leiden — the job at the University of Florence can now be considered an accomplished fact, and I will go there in December.”[63] The failure to get the position in Cagliari was Fermi’s biggest (and perhaps only) academic disappointment. However this, as we know, opened the way to the creation of the first Italian chair in theoretical physics.

It is therefore clear that, at the beginning of the 20s, mathematical physics was the area where most likely Fermi could get a proper recognition of his talent. However this choice had a price for Fermi. As we know, his stay in Göttingen was not particularly rewarding; there he was somehow isolated, and was not able to have a profitable interaction with the researchers who were there at the moment, and would have soon become the main actors of the 20th century physics. As Segrè wrote:

> Unfortunately, it seems that Fermi did not become a member of that extraordinary group or interact with them. I do not know the reason for this. Fermi’s German was certainly good enough to allow easy communication. Born was cordial with Fermi, but may not have fully appreciated his ability. Fermi’s private papers contain several communications from Professor and Mrs. Born that are couched in very friendly terms, one of which is an invitation to Fermi’s sister, Maria, to stay in Göttingen with the Borns. The Borns, moreover, while visiting Rome, called on Fermi’s family. The fact is, however, that Fermi’s sojourn in Göttingen was not so profitable as might have been expected. Fermi wrote some papers that he could as well have written in Rome, and he does not seem to have responded to the exciting environment. It is possible that the physicists of his age group — Heisenberg, Pauli, and Jordan, all exceptional men who should have been Fermi’s companions — were so engrossed in the their own problems that they failed to recognize his ability? Fermi, moreover, was shy, proud, and accustomed to solitude. Perhaps this is why he remained aloof.[64]

But there could be another, simpler explanation; before his stay in Göttingen, Fermi had been mainly interested in relativity. His skills and notions about the quantum theory were certainly quite deep, but they were not enough to interact on an equal footing with Heisenberg, Jordan, and Pauli,[65] who in those years were laying the very foundations of the quantum theory. In any case, independently of the reasons of Fermi’s choice, his researches in relativity projected him onto the international research scene, and at the same time triggered a process of regeneration of the Italian physics, whose results were soon to be seen.

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[62] E. Segrè, *Enrico Fermi Physicist, op. cit.,* p. 204–205.

[63] *Ibid.,* p. 206.

[64] *Ibid.,* p. 32–33.

[65] Pauli was not in Göttingen during Fermi’s visit. The first meeting between the two scientists took place in Como in 1927, at the conference commemorating the 100th anniversary of A. Volta’s death.

3.7 The quantum itinerary

As we saw, Fermi's interest for quantum theory started during his student times. His education in this topic did not depend on the official courses at the University; as a specialist in quantum theory, Fermi was self-taught. This is the key for assessing his first paper about the quantum theory,[66] which seems to be the consequence of his reading the above cited book by Richardson.[67] The topic was Richardson's statistical theory of the photoelectric effect. Richardson had obtained Einstein's equation without resorting to the hypothesis of the light quanta, only on the basis of thermodynamical and statistical considerations, and using Wien's rather than Planck's expression for the spectral distribution of the black body radiation. He had, as Einstein, proved that the photoelectric emission of light only takes place when the frequency of the light is higher than ω_0/h, where ω_0 is the energy needed to extract an electron from the metal, and h is Planck's constant. The purpose of Fermi's work was to check what happened if Richardson's argument was followed, using however Planck's formula instead of Wien's. The result he obtained is very interesting: in addition to the threshold in the frequency of the incident light, he found another phenomenon. The function $\phi(\nu)$, expressing the number of electrons emitted as a function of the frequency of the incident radiation ν, jumps at every value ν such that $h\nu = n\omega_0$, where n is any positive natural number. Richardson, on the contrary, had obtained a function that, for $\nu \geq \omega_0/h$, was continuous, together with its first derivative.[68]

This paper cannot be considered as the starting point of Fermi's work in quantum theory. While it gives evidence of Fermi's interest in the topic, however, it does not belong to the area of research that during those years was defining the quantum theories, namely, atomic physics. The true start was during Fermi's stay in Göttingen, even though the papers he wrote there were about classical mechanics. Notwithstanding Segrè remarks about "…some papers that he could as well have written in Rome," those papers are paramount from the historiographic viewpoint, as they document the start of Fermi's transition from mathematical to theoretical physics, that is, from relativity to quantum theory.

Figure 3.4 shows the structure of Fermi's itinerary in quantum theory from 1923 to 1933.

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[66] Fermi [14].

[67] O. W. Richardson, *op. cit.*

[68] Giovanni Polvani, while commenting Fermi's paper *CPF I,* p. 102, wrote: "For some time Fermi considered the idea of making some experiments […] on the discontinuity suggested by his theory. However afterwards he abandoned the idea."

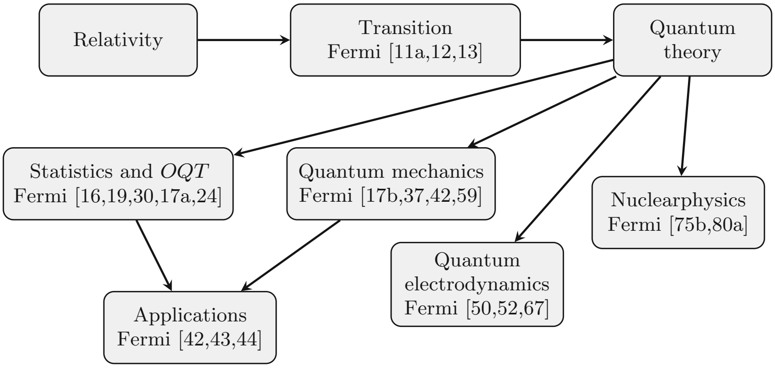

Fig. 3.4 Main areas of Fermi's research in quantum theory with references to some of the main contributions.

3.8 The transition period. From relativity to quanta

It is a widespread opinion among historians that his visit to Born's institute was not much influential on the way Fermi chose his research themes. However, while it is definitely true that his stay left no trace in Göttingen's academic environment, this does not entail that he received no stimulus. On the contrary, here we want to support the thesis that Fermi's Göttingen stay played a fundamental role in the choice of his subsequent researches, catalyzing already existing interests, and giving way to the passage from mathematical to theoretical physics. It was not an abrupt change, but rather a smooth transition. The themes of Fermi's research changed, but the rigorous approach typical of mathematical physics, based on the proof of theorems, was still there. There are no direct evidences or witnesses of the influence of Göttingen's professional environment on Fermi's work, but there are hints, clues that can be traced to some coincidences among Born's research topics, and Fermi's interests during that period.[69]

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[69] There were at least two important coincidences. The first, documented in the Born-Einstein correspondence, was about Poincaré's perturbation theory, a topic that Fermi somehow considered while in Göttingen. In a 7 April 1923 letter addressed to Einstein, Born wrote: "We have studied [Poincaré's] perturbation theory to understand if it is possible to obtain from Bohr's model, with exact calculations, the observed values of the energy levels [...]" (A. Einstein and M. Born, *Briefwechsel, 1916–1955,* Nymphenburger Verlag, München 1969). The second coincidence was about the problem of the entropy constant, which most likely Fermi started considering while he was in Göttingen, as his first paper on this topic (Fermi [19]) was presented to Accademia dei Lincei on 2 December 1923, a few months after he was back to Italy. During Fermi's visit, Born completed a monumental review paper (250 pages) on the "Atomic theory of the solid state" (M. Born, *Atomtheorie des festen Zustandes,* in *Encyclopädie der mathematischen Wissenschaften,* vol. 5, Teubner, Leipzig 1923). A section of that paper was devoted to Stern's contribution to the problem of the entropy constant. Born remarked in his autobiography and in the introduction to his review paper, that he found a paper by the Hungarian physicist E. Brody (*Zur theoretischen*

One should also reconsider Fermi's papers on analytical mechanics from the early 20s, as they somehow foreshadowed his transition toward the quantum theory. They do not prove Fermi's imperviousness to Göttingen's scientific environment; quite the contrary, they stress a typical aspect of his scientific personality, that had already emerged in connection with the "4/3 issue." In that case, Fermi went back to Hamilton's action principle to solve the issue; as we shall see, when he turned to quantum theory, Fermi concentrated his attention on its founding principles, in particular the "adiabatic principle," trying to appraise its domain of validity. It was an approach that revealed his initial training, his nature of a mathematical physicist, which stressed the rigorous search of the principles, the investigation of their generality and their limits. This view is supported by Tullio Levi-Civita's talk at the 1927 Como conference,[70] organized to celebrate the hundredth anniversary of Alessandro Volta's death. The talk was devoted to the adiabatic invariants, and made ample reference to the papers written by Fermi while in Göttingen.

Fermi wrote three papers in Göttingen; one in German,[71] which, split into two papers, was also published in Italian,[72] and two published directly in Italian.[73] The papers are dated February and April 1923. It difficult to ascertain when they were written, but one can conjecture that the paper in German was written between the two in Italian.[74] The first paper was dated February 1923, and was devoted to the study of the validity of the adiabatic principle (see Appendix C.10). Fermi's aim was to show by way of a simple example that

> . . . if a system adiabatically transforms into another, and both the initial and final state admit the separation of variables, but the intermediate states do not, [. . .] the foundations of the adiabatic principle cease to hold.[75]

This happens because the quantities that should be adiabatic invariant are no longer so. The example was very simple: a point-like particle moving in a plane, inside a rectangle whose border undergoes an adiabatic transformation. The analysis of this system allowed Fermi to prove his claim, showing that to have adiabatic invariants, the separation of variables must hold also in the intermediate states of the system. Or, more generally, the system should

> . . . always [admit] a system of angular coordinates, but this, at least in general, is true only if the system always has a multiply periodic motion.[76]

Bestimmung der chemischen Konstante einatomiger Gase, Zeitschrift für Physik 6 (1921), p. 79) very useful. Fermi cited Brody's work in his 1924 paper (*CPF I,* p. 19).

[70] *Congresso internazionale dei fisici, 11–20 settembre 1927,* Zanichelli, Bologna 1928.

[71] Fermi [11a].

[72] Fermi [15, 11b].

[73] Fermi [12, 13].

[74] In footnote 5 in Fermi [13], he makes reference to a result obtained in Fermi [11a], so that the sequence of the papers presumably is [12, 11a], and [13].

[75] Fermi [12], *CPF I,* p. 89.

[76] *Ibid.,* p. 90–91.

The final sentence is particularly important, as it shows that Fermi was well aware of the relevance of his research for the *OQT:*

> After all, also from the viewpoint of the quantum theory, this fact is quite easily explained. Indeed, according to Bohr, a well defined quantization is possible only when the motion of the system is multiply periodic. Thus, if in the intermediate states of the transformation our system cannot be consistently quantized, the same feature will propagate to the final state.[77]

The second paper was written in German, and this probably means that Fermi considered it to be more important, or at least thought it deserved to be more widely known. While it had important consequences for the adiabatic principle, this was never mentioned. Nor did Fermi hint that the results in that paper could be relevant to quantum theory, albeit, as we have just seen, Fermi was aware of the importance of his research for quantum theory. One could think, according to a suggestion by Giovanni Gallavotti,[78] that his omission was dictated by reasons of opportunity. Fermi's result can be indeed regarded as a criticism of one of the two principles that at the time were at the basis of quantum theory. Thus it is not surprising that, while visiting an institution where quantum theory was being created, a young scientist as Fermi would prefer to conceal this criticism "behind an 'innocent' but very important result."[79]

Fermi's work started with a generalization of an important theorem of Poincaré, and was present as an attempt to discuss a problem concerning the foundations of statistical mechanics. Let us start with Poincaré's theorem. We need to introduce the *phase space* (see Appendix C.10), a space whose points represent the states of a dynamical system. The dimension of this space depends on the number of *degrees of freedom* of the system, i.e., the number of coordinates q needed to specify the positions of all particles in the system. If this number is n, there are n additional variables, denoted p, which tell us how the positions q vary with time (that is, how they are related to the velocities). So the dimension of the phase space is $2n$. The evolution of the system is controlled by a set of equations, called *canonical equations.* One can define a function which expresses all the features of the system. This is called the *Hamiltonian function,* usually denoted by H; in general, it depends on the variables q and p and on time t. If the Hamiltonian does not depend on time, its value during the evolution of the system remains constant. Moreover, under some assumptions that here we assume to be satisfied, the Hamiltonian function coincides with the energy of the system. The quantities that are conserved in time are called *first integrals.*

Poincaré's theorem has a geometric interpretation as follows. If we consider in phase space the family of hypersurfaces E = constant, the point which in phase space describes the state of the system moves along a path which lies on one of

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[77] *Ibid.,* p. 91.

[78] G. Gallavotti, *La meccanica classica e la rivoluzione quantistica nei lavori giovanili di Fermi* [Classical mechanics and the quantum revolution in young Fermi's works], in C. Bernardini and L. Bonolis, *op. cit.,* p. 76.

[79] *Ibid.,* p. 78.

these hypersurfaces. There are no other families whose hypersurfaces contain the trajectories of the system. Fermi's generalization consists in showing that, if the number of degrees of freedom is greater than 2, not only there are no families of hypersurfaces, other than those of constant energy, which contain the trajectories of the system, but there is not even "a single hypersurface [. . .] which contains all trajectories of the representing point that originate from any of its points."[80] This result casts a shadow on Ehrenfest's adiabatic principle. Briefly, an adiabatic invariant is a quantity depending on p and q which, during a very slow perturbation of the system, remains unchanged. Therefore, an adiabatic invariant defines exactly one of those hypersurfaces whose existence is ruled out by Fermi's theorem.[81] However, Fermi did not draw this conclusion, but rather used his result to show that every mechanical system after a perturbation becomes *ergodic*.[82] However he was wrong.[83]

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[80] Fermi [15], *CPF I,* p. 109.

[81] For a more detailed discussion of this point, see G. Gallavotti, *op. cit.*

[82] To understand what the ergodic problem is, let us again consider a system described by a time-independent Hamiltonian H given by the energy. In these conditions, the energy is conserved, and the point representing the system in phase space moves along a hypersurface H = constant. As Fermi wrote, "The shape of these trajectories, which lie on a hypersurface H = const., is in general quite complicated; it is very important in the statistical problems. In this connection, let us consider the *ergodic* systems, namely, those for which the previous trajectories, wrapping around the hypersurfaces of constant energy, eventually go through all points in the surface. The system is said *quasi-ergodic* if there are no trajectories that go through all points of the hypersurface H = const., but there exist, however, trajectories that fill it densely; that is, they get arbitrarily close to every point. An essentially equivalent definition is the following: a system is quasi-ergodic if, given on any hypersurface H = const. two arbitrarily small surface elements, there is always a trajectory which crosses both. To deduce the statistical properties of a system it is essential to know if it is, or not, ergodic or quasi-ergodic. Indeed, to obtain a mechanical deduction of Boltzmann's law, that is, the fundamental law of statistical mechanics, it is necessary to assume that the system under study (a gas, a solid body, etc.) is at least quasi-ergodic. There have been long discussions to decide the validity of the hypothesis that the systems, to which statistical mechanics is usually applied, can be considered to be ergodic or quasi-ergodic. The conclusion was that, while there exist no ergodic systems, there can exist quasi-ergodic ones; indeed, it seems that very complicated mechanical systems with no special symmetry have to be necessarily quasi-ergodic. Now, the systems that one considers in the applications of statistical mechanics are always very complicated, both for the huge number of their constituents, and because, in principle, one must consider as parts of the system also all bodies that can, for instance, by means of heat transmission, influence the movement of the body under consideration. The hypothesis that all systems to which statistical mechanics is applied are quasi-ergodic is, therefore, quite plausible (quasi-ergodic hypothesis) (E. Fermi, *Meccanica statistica,* in *Enciclopedia Italiana*, mentioned in *Enrico Fermi e l'Enciclopedia Italiana,* Istituto dell'Enciclopedia Italiana, Rome 2001).

[83] In Chapter 5 we shall see an important contribution of Fermi, Pasta, and Ulam, which showed experimentally that the 1923 proof of the quasi-ergodicity of mechanical systems was wrong. Fermi's proof was commented by W. Urbanski (*Über die Existenz quasi-ergodischer Systeme,* Physikalische Zeitschrift 25 (1924), p. 47) with two criticisms. The first, acknowledged by Fermi, was that Fermi's surface elements had regularity properties that cannot be satisfied. The second, as clarified by Fermi, was just about a different definition of quasi-ergodic system.

Definitely, Fermi's paper was, albeit implicitly, critical with respect to the adiabatic principle (one should note that, as remarked by Gallavotti,[84] a criticism of the adiabatic principle was already contained in a little known paper by Einstein[85]). However, it is quite likely that Fermi's Göttingen papers did not imply that Fermi had a cautious and skeptical standing with respect to the emerging quantum theories. Rather, the Göttingen papers show Fermi's awareness that the theory needed a sounder foundation. This is confirmed by the third paper, written only in Italian,[86] where Fermi analyzed in detail the limits of Ehrenfest's adiabatic principle. In view of the failure of the attempts to apply the *OQT* to systems more complex than the hydrogen atom (e.g., the helium atom), the purpose was

> [. . .] to look if, and to what extent, one can try to extend the adiabatic principle to more general systems, in the hope that it may give some information useful for the search of the rules that determine the distinguished orbits of these more general systems.[87]

Fermi's result was completely negative: "in general [. . .] it is not possible to apply Ehrenfest's principle to systems with many characteristics [systems that admit further first integrals in addition to the energy]."[88]

It is important to note the way Fermi expressed his final considerations, which stressed a feature of his scientific personality that we have remarked already a few times, namely, the tight and inextricable synergy between the experimental and theoretical aspects of the scientific research. Indeed he observed that there exist "some important classes of exceptions" to the results he had obtained. These are provided by "systems [. . .] for which the adiabatic principle is *confirmed by the experience,*[89] as a logical consequence of Sommerfeld's conditions."[90] In any case, Fermi concluded, referring to the rules for determining the quantum orbits,

> Ehrenfest's principle alone, even if experience should confirm it in this more general application, is not enough to determine these rules, since it only permits one, if the distinguished orbit of a system are known, to find the distinguished orbits of all those system that can be obtained from the given system by an adiabatic transformation.[91]

It is not easy to make a scientific assessment of the Göttingen trilogy. The three papers, written in the first eight months of the year 1923, were a distillation of the knowledge accumulated by a century-old discipline such as analytical mechanics. Young Fermi was well aware of this. In a postcard to his friend Persico he wrote:

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[84] G. Gallavotti, *op. cit.,* p. 78.

[85] A. Einstein, *Zum Quantensatz von Sommerfeld und Epstein,* Verhandlungen Deutschen Physikalische Gesellschaft 29 (1917), pp. 82–92 (talk given on 11 May 1917).

[86] Fermi [13].

[87] *Ibid.,* pp. 92–93.

[88] *Ibid.,* p. 100.

[89] Italics in the original.

[90] *Ibid.*

[91] *Ibid.,* p. 101.

> I am working a lot on a paper on the border between celestial mechanics, statistical mechanics and quantum theory. But I cannot foresee where this will take me. I cannot even say if this will take me somewhere at all.[92]

We can recognize in the three areas of research cited by Fermi his generalization of Poincaré's theorem, the ergodicity problem, and the problem of the adiabatic invariants. We can therefore think, as it has been already conjectured,[93] that Fermi's initial project was to write a single paper, but then, in view of the complexity of the problem, he decided to split his results into three separated papers. In any case, beyond any possible conjecture, these documents depict a crucial juncture of Fermi's scientific life, which marked the beginning of a transition from mathematical to theoretical physics.

3.9 Contributions to the "Old Quantum Theory"

In 1925 Fermi, in a popular paper,[94] described three important problems in atomic physics. In the author's words:

> 1) What are the rules for selecting the quantum motions among the continuous spectrum of the mechanically possible motions [. . .] 2) What is the probability that, at a given temperature, an atom is moving according to one of the mechanically possible motions; 3) What is the probability that an atom in a given quantum state, in a time interval *dt* moves to another state.[95]

These were basic problems, deeply related to *OQT*'s founding principles: Ehrenfest's adiabatic principle and Bohr's correspondence principle. The solution to the first problem would have allowed for a theoretical determination of the spectral lines, since it would permit one to compute all the frequencies that an atom can emit. A solution to the second and third problem, on the other hand, would have allowed one to determine the strength of the spectral lines.

The three problems yield a rather precise idea of the evolution of Fermi's work on the *OQT*. The analysis of the first, whose solution is Ehrenfest's principle, characterized Fermi's transition period. The solution of the second was his first true contribution to the quantum theory. It was first published in Italian in 1923, and immediately afterwards in German, denoting the importance that Fermi was attributing to this work.[96] The aim of the paper was to circumvent an obstacle in the standard computation of the probability p_i of finding, inside a gas at temperature T, an atom in a given quantum state i. The computation relied on the assumption that

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[92] F. Cordella and F. Sebastiani, *Fermi a Gottinga e a Leida: gli anni che precedono la statistica quantica* [Fermi in Göttingen and Leiden: the years before quantum statistics], Quaderni di Storia della Fisica 6 (2000), p. 17.

[93] *Ibid.*

[94] Fermi [22]

[95] *Ibid., CPF I,* p. 119.

[96] Fermi [17a] (Italian), [17b] (German).

a priori all possible states have the same probability. Assigning the same statistical weight to all states, however, led to a serious difficulty; the normalization condition, namely, the obvious requirement that the sum of all probabilities is 1, resulted in a divergent series.[97] This problem was usually solved with the *ad hoc* hypothesis that in gas at a given pressure, the possible quantum states are those for which the atomic radius does not exceed the average distance between the atoms. This allowed one to limit the series to a finite number of terms, instead of summing on an infinite number of them. Fermi’s idea is

> [...] to get rid of this difficulty in a more precise way. We shall therefore try to make a thermodynamical computation of the equilibrium state among the various quantum states, taking the volume of the molecules into account. We shall see that this leads to a quantitative expression of the (qualitatively evident) fact that the atoms of bigger sizes are more difficult to form, as they are more disturbed by the collisions with the other atoms.[98]

This result solved the problem at the root, as in the expression obtained by Fermi there is no need to limit the sum to a finite number of terms, since each terms included an exponential factor which decreased when the size of atom increased, making the series convergent. In other terms, the statistical weight was not the same for all quantum states, but decreased with the increase of the size of the atom in a given quantum state.[99]

The results of this paper were almost immediately applied by Fermi to a question that had arisen in 1921 in astrophysics.[100] In that year, Meghnad Saha proposed a theory for the thermal ionization of gases “where the process when a neutral atom transforms into a positive ion and an electron is treated in all respects as a chemical reaction of dissociation.”[101] Saha’s theory was very interesting, as it produced a formula for the ratio between the concentration of the neutral atoms and that of the electrons as a function of the temperature and of the ionization energy of the atom. This had important applications; indeed, from the analysis of the stellar spectra it was possible, using Saha’s formula, to evaluate the temperature of the stars (albeit with little precision). Saha’s theory rested on the hypothesis that the neutral atoms could exist only in their fundamental state. For Fermi this was not correct, since

> This assumption is plausible only at low temperatures, when the higher quantum states are not excited in an appreciable way. For higher temperatures, when the thermal dissociation takes place, there will be ionized atoms, and a considerable number of atoms with a higher energy level than the minimum. One should of course take account of these energy levels in the computation of the ionization equilibrium.[102]

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[97] Denoting by A a suitable constant, k Boltzmann’s constant, and T the temperature, the series is $\sum_i \exp(A/kTi^2)$, which is divergent.

[98] Fermi [17a], *CPF I,* p. 119.

[99] The German version had some changes with respect to the Italian one. They were due to a suggestion by Born, as acknowledged by Fermi.

[100] Fermi [18].

[101] *Ibid., CPF I,* p. 130.

[102] *Ibid.,* p. 131.

To correct Saha's formula, in a way to take account of all energy levels, Fermi used his results about the statistical weights of the quantum states. However, he obtained only very small corrections, which could be appreciated only at very high temperatures.

Excluding, for the moment, the papers that Fermi wrote before the celebrated work on "Fermi's statistics," and opened the way to it, there are two more contributions to the *OQT* that we have still to examine. Let us start with the second, which is less important, but is nevertheless relevant for reconstructing Fermi's scientific personality. This paper was presented by Corbino to the *Accademia dei Lincei* during its 4 January 1925 meeting,[103] at the end of Fermi's second scientific visit abroad, in Ehrenfest's group in Leiden. It was, according to Rasetti, the revision of a communication presented by Fermi at the 1 November 1924 meeting of the *Nederlandsche Natuurkundige Vereeniging*.[104] It was about the relations among the strengths of some spectral lines. We do not want here to go into the details of the paper, which dealt with a very technical problem internal to *OQT*. Rather, we like to stress that Fermi's objective in this paper was, once more, to correct and improve something already existing. In this case it was Sommerfeld and Heisenberg's contributions to the problem. Fermi wrote:

> It is known that Sommerfeld and Heisenberg found, by way of the correspondence principle, a formula which provides the ratio among the strengths of the components of a multiple line. The formula[105] [...] only permits conclusions of a qualitative nature. In this paper we aim to show that a slight modification of Sommerfeld and Heisenberg's considerations leads to some formulas which, with only a few exceptions that can be explained, reproduce quantitatively the empirical results.[106]

It seems that Fermi, in this period of his scientific life, after leaving the theory of relativity and analytical mechanics in favor of the quantum theory, nevertheless, as a result of his initial training, still had a research style that stressed the clarification and rigorous systematization of the problems he considered.

Let us consider the most important contribution of Fermi during this period. He worked on this problem in the summer of 1924, just before leaving Leiden. The paper dealt with the collision between atoms and charged particles, and was published both in Italian and German.[107] As noted by Enrico Persico, it was the first paper showing "the features of Fermi's most mature style,"[108] i.e., the application of a simple but clever idea to a concrete problem, using sophisticated mathematical tools, but without indulging in the formalism, and always under the guidance of the physical principles underlying the phenomenon under study.

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[103] Fermi [21b].

[104] Fermi [21a]. Rasetti's information about the presentation at the Nederlandsche Natuurkundige Vereeniging is from *CPF I,* p. 134.

[105] It was Sommerfeld's *Intensitätregel*.

[106] Fermi [21b], *CPF I,* p. 134–135.

[107] Fermi [23a] (Italian), [23b] (German), *CPF I*, pp. 134–135.

[108] E. Persico, *CPF I,* p. 142.

Fermi's simple idea is the following:

> When an atom which is in its normal state is hit by light having a suitable frequency, it can be excited, i.e., it can move to a quantum state with greater energy, absorbing a quantum of light. If the exciting light quantum is bigger than the energy needed to ionize the atom, the atom can ionize by losing, according to the frequency of the light, an electron belonging either to a superficial stratum, or a to a stratum deep inside the atom. When the atom rearranges, it emits light, and according to the situation, the phenomena of optical resonance or fluorescence take place. Similar phenomena are observed also when the excitation is induced by a collision. Indeed, if the atoms of a gas are bombarded with fast enough electrons, they can be excited or ionized, and, if the speed of the exciting electrons is big enough, they can also lose electrons from the deep strata. The aim of this paper is to make the analogies between these two classes of phenomena more precise, and in particular, to quantitatively deduce the phenomena related to the excitation by collision from those related to the optical absorption.[109]

The problems where the idea could be applied were three. The first was the excitation of the line 2537 of mercury. The other two were about the motion of α particles coming from radium C in helium; the second studied the number of pairs of helium ions produced, and the third the average length of the path traveled by the α particles.[110] In all cases, Fermi's theory produced results in very good agreement with the experimental data.

In spite of some criticism advanced by Bohr,[111] Fermi's idea was, within a few years, fully acknowledged within the new quantum theory, and was analyzed and applied to various phenomena by Evan J. Williams and Carl F. von Weizsäcker.

3.10 Interlude: the second adventure in the experimental arena

As we have already repeatedly stressed, the harmonious synergy of his theoretical, experimental, and pedagogical work has been a constant feature of Fermi's work, and deeply characterized his complex scientific personality. It comes therefore to no surprise to learn that during his stay in Pisa he was simultaneously working on his experimental thesis on the X-rays, was giving talks on the theory of quanta and relativity, and was preparing the work on his important papers in relativity theory.

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[109] Fermi [23a], *CPF I,* p. 143.

[110] It may be interesting to briefly describe how Fermi's theory explains the deceleration of α particles in helium. Fermi wrote: "While moving, the α particle generates a varying electric field, whose energy is absorbed by the surrounding atoms as if it were the electric field of a light wave. The energy so absorbed must be subtracted from the kinetic energy of the α particle, which therefore is decelerated."

[111] N. Bohr, *Über die Wirkung von Atomen bei Stößen,* Zeitschrift für Physik 34 (1925), p. 142. The criticism was about the speed of the electrons emitted by the fast particles, which according to Bohr had no experimental evidence. Fermi's method is also known in the literature as the "method of virtual quanta."

After the X-rays, his “second adventure in the experimental arena,”[112] according to a definition by Rasetti, was the analysis of the atomic spectra by means of a particular technique, based on electromagnetic fields at radio frequency. The work was done with Rasetti in early 1925, during Fermi’s stay in Florence after his visit to Leiden. Fermi was charged to teach a mathematical physics course at the Institute in Arcetri directed by Antonio Garbasso. Rasetti had been there since the fall of 1922. This experimental work resulted in four papers. The first was a short communication to *Nature,* and the other three actually contained the main results of the investigation. Among these, two were published in Italian, and the third, an abridged version of the other two, was published in German in *Zeitschrift für Physik.*[113] The starting point were the investigations of R. W. Wood, A. Ellet, and W. Hanle on the effect of weak magnetic fields on resonance light.[114] In particular, Wood and Ellet discovered that a magnetic field destroyed the polarization of the resonance light. Hanle and Ellet, using very weak fields, observed that in the case of mercury and sodium, there was an incomplete polarization, together with a rotation of the polarization plane of the scattered light, i.e., a Larmor precession.[115]

Fermi had already been interested in the optical resonance.[116] The purpose of the new investigations was twofold: to analyze theoretically the effect of an alternating magnetic field on the resonance light and check the results obtained with an experiment. The theoretical analysis was contained in the first of the Italian papers, and the experimental analysis in the second. The model used by the two

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[112] F. Rasetti, *CPF I,* p. 159.

[113] Fermi [26, 27, 28(1), 28(2)].

[114] To understand the phenomenon of resonance light let us resort to Fermi’s very clear words: “The phenomenon of optical resonance, discovered by Wood, consists, as it is known, in the fact that, by illuminating a metallic vapor with light having the frequency of one of its absorption lines, the light is scattered by the vapor with a very high intensity. This phenomenon is easily explained by the classical theory of emission; according to the latter, the emitted light is due to the presence in the atoms of the emitting substance of oscillators having same frequency as the absorption lines. If we illuminate the atom with a light having the frequence of these oscillators, the latter will be excited by the vibration of the electric field of the light with a frequency equal to their proper frequency; due to the resonance phenomenon, they will start vibrating with a great amplitude; and these vibrations of the oscillators produce the emission of light in all directions. The light so emitted is indeed the resonance light” (E. Fermi, *Introduzione alla fisica atomica, op. cit.,* p. 108).

[115] In Fermi’s words: “Let us consider a system made by a certain number of point particles, all having the same electric charge e and mass m; and assume that all points move under the action of a central force pointing to a fixed point O, and of mutual attractions and repulsions. Let us put this system into a uniform magnetic field of strength H; [. . . One can] prove that, at a first approximation, the motion of the system is given by the superposition of the motion it would have in the absence of the field, and a uniform rotation with angular speed $\omega_L = eH/2mc$ (Larmor’s angular speed) around an axis parallel to direction of H and passing though O” (*Ibid.,* p. 160).

[116] Fermi [18]. The problem studied in this paper was the formulation of a mathematical theory of the optical resonance capable of interpreting some features of the phenomenon. In particular, Fermi was interested in the fact that for suitably small pressures of the gas, the resonance light is scattered in all directions, while for higher values of the pressure, “most of the light is reflected regularly, and only a small part is scattered in all directions” (*CPF I,* p. 121).

scientists was completely classical; the electrons were bound by elastic forces, and oscillated with the same frequency of the incident light. In the theoretical part, they showed that if the frequency of the alternating magnetic field was great in comparison with Larmor's frequency, the depolarizing effect of the field was very small. The frequencies needed for an experimental verification of the theoretical predictions were of the order of a few MHz (millions of Hertz, i.e., radio frequencies). Rasetti wrote that neither he nor Fermi had any experience with radio circuits, and that "Fermi computed the characteristics of a simple oscillating circuit which should have produced a field with the appropriate strength and frequency."[117] Moreover, "We discovered in a cabinet some triodes which Fermi deemed suitable for the circuit he had designed," and, in spite of the difficulties, "when the circuit was assembled, it immediately worked as Fermi had expected."[118] The theoretical predictions were thus confirmed.

3.11 A new statistics

The roots of Fermi's path toward the discovery of the statistics which bears his name may most likely be traced to his stay in Göttingen.[119] His investigations resulted in a celebrated paper, published in 1926,[120] about the quantization of a monoatomic ideal gas. It was preceded by two somehow preparatory papers, published in 1923 and 1924,[121] respectively. The main topic of the first paper was a formula, independently obtained by Sackur and Tetrode,[122] for the absolute entropy constant of a monoatomic ideal gas, and in particular, its deduction made by Stern.[123] The latter was alternative to Sackur and Tetrode's derivation, which was not considered to be fully satisfactory, in particular for what concerned the quantization of phase space. Stern's method was based, as Fermi wrote,

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[117] *Ibid.,* p. 159.

[118] *Ibid.*

[119] The genesis of Fermi's statistics was reconstructed in F. Cordella and F. Sebastiani, *Sul percorso di Fermi verso la statistica quantica* [On Fermi's path toward the quantum statistics], Il Nuovo Saggiatore 16 (2000), p. 11; *I due lavori di Fermi che preludono alla statistica quantica* [Fermi's two papers leading to quantum statistics], Giornale di Fisica 41 (2000), p. 83; *La statistica di Fermi* [Fermi's statistics], Giornale di Fisica 41 (2000), p. 131; F. Cordella, A. De Gregorio and F. Sebastiani, *Enrico Fermi: gli anni italiani* [Enrico Fermi's Italian years], Editori Riuniti, Rome 2001, pp. 145–69; L. Belloni, *Una nota su come Fermi giunse alla statistica di Fermi-Dirac* [A note on how Fermi obtained the Fermi-Dirac statistics], Scientia 113 (1978), p. 431.

[120] Fermi [30].

[121] E. Fermi [16, 19].

[122] H. M. Tetrode, *Die chemische Konstante der Gase und das elementare Wirkungsquantum,* Annalen der Physik 38 (1912), p. 434; O. Sackur, *Die universelle Bedeutung des sog. elementaren Wirkungsquantums,* Annalen der Physik 40 (1913), p. 67.

[123] O. Stern, *Zur kinetischen Theorie des Dampfdrucks einatomiger fester Stoffe und über die Entropiekonstante einatomiger Gase,* Physikalische Zeitschrift 14 (1913), p. 629.

> [...] on the following principle. If we consider our gas as the vapor of a solid body, we can compute its maximal tension in two ways. 1) Using the kinetic theory of gases, obtaining a completely determined result, with no arbitrary constant; 2) using thermodynamics. In this way, the resulting arbitrary constant is exactly the absolute entropy constant of the gas, since the constant relative to the solid body can be computer by means of Nernst's theorem. Comparing the two expressions, Stern is able to determine the absolute constant.[124]

However, also Stern's computation was based on an *ad hoc* hypothesis. One needed to assume that at the absolute zero, the mean energy of an oscillator was not zero, but the value of half a quantum, that is, $h\nu/2$. And here came Fermi's contribution; he showed that "this unnatural hypothesis is by no means necessary," but it was enough to slightly modify Stern's computation, "taking into account that the molecules of the solid body can only move according to quantum orbits."[125]

The 1924 paper was about the quantization of systems containing identical elements. The main idea was already presented at the beginning of the paper.

> Sommerfeld's rules for determining the quantum orbits of systems admitting the separation of variables, which, as we know, dictate that for such orbits the phase integrals $\oint p\,dq$ are all integer multiples of Planck's constant h, are in perfect agreement with the experiments when applied to the hydrogen atom, and explain, within the experimental errors, all known facts about the spectroscopy of that element; however, all attempts made so far to extend those rules to more complex systems, have only produced qualitative results. In spite of many efforts, a quantitative agreement between theory and experiment has not been obtained even for helium, the simplest atom after hydrogen.
>
> This failure is usually ascribed to the fact that more complex systems do not admit the separation of variables, and that the perturbation method, which has been devised to extend Sommerfeld's rules to those systems, is for some reason unsuitable to compute the orbits.
>
> My aim in this work is to show that there are reasons to believe that the failure is rather due to the fact that Sommerfeld's rules fall short to compute the static orbits of those systems that contain identical elements, whether they admit the separation of variables or not (in the helium atom, for instance, the two electrons are indistinguishable).[126]

The example made by Fermi to support the proposed modification of the quantization rules was, as usual for him, very easy and clear. Let us consider three electrons moving along a ring, positioned on the vertexes of an equilateral triangle. Since the three particles are indistinguishable, the period of their motion is not 2π, but $2\pi/3$. In Fermi's words:

> So we see that, if the electrons could be distinguished, their motion along the ring would be periodic with period 2π, but as they are indistinguishable, the period is $2\pi/3$. If p is the areal momentum of the ring, under the first hypothesis $2\pi p$ should be a multiple of h, under the second hypothesis $2\pi p/3$ should be an integer multiple of h, so that in the second case the minimum value of p is three times bigger than in the first case.[127]

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[124] Fermi [16], *CPF I,* p. 114–115.

[125] *Ibid.*

[126] Fermi [19], *CPF I,* p. 124–125.

[127] *Ibid.*

Fermi noticed that similar considerations had already been made by Gregory Breit and Arthur H. Compton in their work on the construction of a quantum theory of X-ray diffraction;[128] as all atoms in a crystal are identical, in quantizing the crystal lattices they considered a translation parallel to the edge of an elementary parallelepiped, and having the same length, as a period.

To show the inadequacy of Sommerfeld's rules for quantizing a system admitting the separation of variables, Fermi considered a gas formed by n point-like particles contained in a volume v. The aim is to compute the entropy of the gas using different quantization procedures, so that the correct one could be chosen by checking its agreement with the Sackur-Tetrode formula. A remark by Fermi is quite interesting from the historic viewpoint: "to get a finite value for the entropy of an ideal gas, it is necessary to quantize it in some way, as the classical treatment would always lead to an infinite value."[129] The result is obtained by subdividing the volume into elementary cells having the shape of a parallelepiped, whose number depends on how many molecules of the gas we allow to stay in each cell. Fermi showed that

> The value of the entropy constant which we know from the experiments can only be obtained by dividing the volume in parallelepipeds and placing only one molecule in each of them, while quantizing systems that contain even two identical molecules, one always finds results that do not agree with the experience.[130]

Giorgio Parisi has rightly noted that up to there, albeit the two papers were quite interesting and full of clever observations, there was "nothing extraordinarily new."[131] The truly important novelty appeared in the 1926 paper,[132] which was written after Pauli had formulated in 1925 his exclusion principle.[133] Fermi immediately realized the deep implications of the exclusion principle for the statistical physics problems he was studying, as the introduction to his paper clearly shows:

> In classical thermodynamics, the specific heat at constant volume of a monoatomic ideal gas is $c = 3k/2$ per molecule. It is clear that if we want Nernst's principle to hold also for an ideal gas, we must assume that this value of c is only an approximation at high temperature, and that for $T = 0$ the specific heat c approaches zero, so that the integral which expresses the entropy can be extended down to the absolute zero, without any undetermined constant. To understand how c can vary in this way, one must admit that also the motions of an ideal gas must be quantized. The quantization will not only change the energy content of the gas, but also its equation of state [. . .] The aim of this work is to describe a method for quantizing the ideal gas which we think is as independent as possible from unjustified hypotheses about the statistical behavior of the molecules of the gas.[134]

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[128] A. H. Compton, *The quantum integral and diffraction by a crystal,* Proceedings of the National Academy of Sciences 9 (1923), p. 359; G. Breit, *Note on the width of spectral lines due to collisions and quantum theory,* Proceedings of the National Academy of Sciences 9 (1923), p. 244.

[129] Fermi [19], *CPF I,* p. 125.

[130] *Ibid.,* p. 125.

[131] G. Parisi, in C. Bernardini and L. Bonolis, *op. cit.,* p. 73.

[132] Fermi [30] (Italian), [31] (German).

[133] See footnote 59, Chapter 2.

[134] Fermi [30], *CPF I*, p. 180.

Fermi's method, the true novelty in this paper, consisted in applying Pauli's principle, originally formulated for the atomic electrons, to the molecules in an ideal gas. Thus the exclusion principle became a universal rule for the quantum behavior of identical particles. In the same way as there cannot be more than one electron in a quantum state characterized by the same quantum numbers,

> We shall assume that in our gas there cannot be more than one molecule whose motion is characterized by certain quantum numbers, and we shall show that this hypothesis leads to a perfectly consistent theory for the quantization of the ideal gas, which in particular explains the predicted decrease of the specific heat at low temperatures, and yields the exact value of the entropy constant of the ideal gas.[135]

To implement this plan, Fermi chose a specific model of gas, which was very convenient for the computations. He assumed that the molecules were acted upon by an elastic force, that is, a force pointing to a fixed point O, with a strength proportional to the distance of the molecules from O.[136] After computing the distribution of the energy levels, Fermi checked the thermodynamical properties of the gas, obtaining the expected results at high temperatures, and marked deviations at low temperatures; in particular, the specific heat went linearly to zero for temperatures approaching the absolute zero. At high temperatures he found the Sackur-Tetrode expression of the entropy.

Fermi maintained that his method could be applied to any system of identical particles. So, in early 1926, two different statistics, the Bose-Einstein and Fermi's statistics, seemed to solve the same problem.

On 1st October 1926, Paul Dirac published a paper entitled "On the theory of Quantum Mechanics."[137] The third paragraph of this very important paper was devoted to the treatment of systems of identical particles.

> In § 3 the problem is considered of a system containing several similar particles, such as an atom with several electrons. If the position of two of the electrons are interchanged, the new state of the atom is physically indistinguishable from the original one. In such a case one would expect only symmetrical functions of the co-ordinates of all the electrons to be capable of being represented by matrices. It is found that this allows one to obtain two solutions of the problem satisfying all the necessary conditions, and the theory is incapable of deciding which is the correct one. One of the solutions leads to Pauli's principle that not more than one electron can be in any given orbit, and the other, when applied to the analogous problem of the ideal gas, leads to the Einstein-Bose statistical mechanics.[138]

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[135] *Ibid.,* p. 183.

[136] Actually the result is independent from the chosen model. As noted by Parisi, Fermi, instead of assuming that the molecules of the gas were acted on by an elastic force, could have considered a gas inside a box. However, "the use of a harmonic potential has a technical advantage, since in the zone where the potential is high, the density is low, so that the gas behaves almost classically. This subtlety allowed Fermi to reach his result in a very simple way" (G. Parisi, *La statistica di Fermi, op. cit.*, p. 73).

[137] P. A. M. Dirac, *On the theory of Quantum Mechanics,* Proceedings of the Royal Society A 112 (1926), p. 661.

[138] *Ibid.,* p. 662.

The paper was not written in the language of the *OQT,* but rather used the formalism of the emerging new quantum mechanics. Each particle was assigned a wave function $\Psi(r)$ depending on its position r, and a system of two identical particles was assigned a wave function $\Psi(r_1, r_2)$. Dirac showed that if the wave function of the two-particle system is symmetric in the positions, $\Psi(r_1, r_2) = \Psi(r_2, r_1)$, then the system obeys the Bose-Einstein statistics; if the function is skew-symmetric, $\Psi(r_1, r_2) = -\Psi(r_2, r_1)$, the system obeys Pauli's principle. The theory was not able to predict which particles obeyed the first or the second condition. Only later it would appear that particles with half-integer spin obey the exclusion principle, while particles with integer spin obey the Bose-Einstein statistics.

After the publication of this paper, Fermi's statistics became widely known in the scientific environment. This may seem quite surprising, as Dirac did not even cite Fermi's paper. Let us try to understand the reason.

3.12 Local itineraries and global maps: the path toward the Fermi-Dirac statistics

In a letter sent to Dirac on 25 October 1926, Fermi wrote:

> In your interesting paper "On the theory of quantum mechanics" (Proc. Roy. Soc. 112, 661, 1926) you have put forward a theory of the Ideal Gas based on Pauli's Exclusion Principle. Now a theory of the ideal gas that is practically identical to yours was published by me at the beginning of 1926 (*Zs. f. Phys.*, 36, p. 902; *Lincei Rend.* February 1926).
> Since I suppose that you have not seen my paper, I beg to attract your attention on it. [139]

Dirac's comment is enlightening:

> When I looked through Fermi's paper, I remembered that I had seen it previously, but I had completely forgotten it. I am afraid it is a failing of mine that my memory is not very good and something is likely to slip out of my mind completely, if at the time I do not see its importance. At the time that I read Fermi's paper, I did not see how it could be important for any of the basic problems of quantum theory; it was so much a detached piece of work. It had completely slipped out of my mind, and when I wrote up my work on the antisymmetric wave functions, I had no recollection of it at all.[140]

Dirac's memory lapse is perhaps surprising, but a careful reading of the documents shows that the lapse was not so unlikely. Let us go back to the global maps that we drew in the first chapter. We saw that quantum mechanics has its sources in two different areas of research, spectroscopy and atomic physics. It was within this framework, which we might call the *received view* of the *OQT,* that Pauli's principle was formulated, while Dirac's investigations took place during the transition of the *OQT* to the new quantum mechanics. Dirac's lapse therefore is not

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[139] In H. Kragh, *Dirac: a Scientific Biography,* Cambridge Univ. Press, Cambridge, UK 1990. p. 36.

[140] *Ibid.*

so implausible; he saw in Fermi's paper a contribution completely extraneous to the *received view.* What was, then, Fermi's role? There is no unique answer; from the viewpoint of the local itineraries, Fermi solved a well-defined problem in statistical mechanics; from the viewpoint of the global maps, he extended Pauli's principle beyond the framework in which it was originally formulated. So, Heisenberg's sentence when he introduced Fermi to Pauli during the 1927 Como conference, "Let me introduce to you the application of the exclusion principle," was more than just a witticism.

Fermi's particular standing with respect to quantum mechanics is by now clear. He was convinced that it was necessary, but he was very critical of the founding principles of the *OQT;* he was like a spectator, working at the margins of the theory and waiting that the deepest conceptual problems were solved. This appeared very clearly during his Göttingen stay, and from his standing with respect to the adiabatic principle, one of the pillars of the *OQT.* Altogether, we can trace a path which developed quite far from the global maps of the time, and which shows, in many respects, striking analogies with Einstein's path. This is not so surprising, if we think that the problems, the methods, and the conceptual structure of the theory of relativity were the field where Fermi made his first experiences. In a way, it was a "heretical" itinerary, which did not develop through spectroscopy and atomic physics, but rather within statistical mechanics.

It is interesting what Rasetti wrote about this issue:

> Fermi, after some time, told Segrè that the subdivision of phase space in finite cells took him much time, and if Pauli had not discovered the exclusion principle, he could have discovered it in an indirect way, via the entropy constant.[141]

This is supported by the fact that the paper on Fermi's statistics appeared almost one year after the publication of Pauli's paper on the exclusion principle. Segrè, one of the most reliable commentators of Fermi's work, confirmed that Fermi wrote his paper a few weeks after reading Pauli's work.[142] It is hard to think that Fermi's delay in reading Pauli's paper was only due to practical reasons; it is more realistic to interpret the delay as the evidence of a strategy that was not developing inside the *received view.* One could object that all this is not so important, and that the history of quantum mechanics might have proceeded in a different way; quantum mechanics could as well have developed through statistical mechanics. However, these are idle considerations; the documents show us that the global maps have developed along some paths, and not others.

What we can detect by following Fermi's work on the quantization of the monoatomic gas is a confluence between statistical mechanics and the *received view,* whose catalyzer is Pauli's exclusion principle. Indeed, Fermi implemented the exclusion principle in all its power and generality, as a principle which regulates

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[141] F. Rasetti, *CPF I,* p. 178.

[142] A few week after reading Pauli's paper, Fermi managed to have a communication on the new statistics presented to Accademia dei Lincei by Corbino (E. Segrè, *Enrico Fermi, fisico, op. cit.*, p. 41).

the quantum behavior of identical particles, independently of its origin in the realm of *OQT*'s atomic physics. With Fermi's work, the electrons in an atom, and the molecules in a gas, are regulated by the same principle. We can well say that this extension is the most important feature of the crossing between Fermi's local itinerary and *OQT*'s global maps.

Fermi was certainly aware of the fact that his work provided an important bridge between statistical mechanics and quantum theory. This is confirmed by what Rasetti wrote in a paper that was published in early 1926, and thus was written while Rasetti was collaborating with Fermi in Florence:[143]

> [...] we have studied the relations occurring between the quantum states and the entropy constant of gas. These considerations clearly show that a precise theoretical determination of the entropy constant for the polyatomic molecules will be not possible until we shall have more precise information about their quantum states, that is, their band spectra.
> All this is without doubt one of the most striking examples of the deep connections that the modern quantum theories have found between areas of physics that had been so far considered as completely independent, such as the thermodynamical properties of vapors and atomic spectroscopy.[144]

Going back to Fermi's discovery, its limit, as we have already noticed, was that the new statistics applied to all identical particles, without distinctions. So Fermi's theory somehow contrasted with the 1924 results of Bose and Einstein. Only Dirac's 1926 paper showed, working within the new quantum theory, that both statistics were equally valid. However, it was only with Heisenberg's contribution at the 1927 Solvay Conference that the first conjecture about the relation between spin and statistics was formulated.

It is not easy to track, within Fermi's complex personality, the reasons of his strategy in dealing with this problem. One hypothesis is that his approach was due to his scarce propensity to the extreme modelization of physical processes that was somehow typical of the *OQT.* This is clearly confirmed by comparing his paper on the new statistics with a subsequent one, written, again in 1926, with Rasetti.[145] As we already remarked, the exclusion principle was used by Fermi in its full generality, with no reference to its original context. There is no need, therefore, to adhere to overly precise models of the electron, such the "spinning electron." In his joint paper with Rasetti, entitled indeed "On the Spinning Electron," Fermi, after examining arguments pro and against the hypothesis by Uhlenbeck and Goudsmit, expressed his viewpoint in a very clear way:

> We think that this discussion allows us to conclude that, in spite of the serious energetic difficulties we have mentioned, there is no need to abandon the hypothesis of the spinning

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[143] F. Rasetti, *La costante assoluta dell'entropia e le sue applicazioni* [The absolute entropy constant and its applications], Nuovo Cimento 3 (1926), p. 67.

[144] *Ibid.,* p. 85.

[145] The paper was jointly written by Fermi and Rasetti, but in the introduction written for the *Collected papers,* Rasetti wrote "The ideas expressed in this paper are almost entirely due to Fermi" (*CPF I,* p. 212).

> electron. Obviously, we are not saying that it must be taken too literarily, i.e., by thinking the electron as a spinning charged macroscopic body; what is essential for the applications is that the electron is endowed with a mechanical and a magnetic moment, independently of the excessive modelizations about their origin.[146]

Also Fermi's attitude with respect to matrix mechanics and wave mechanics showed this kind of behavior. As we noted in the first chapter, he had no enthusiasm for matrix mechanics. "The zoology of the spectral data" (so he dubbed Heisenberg's quantum mechanics) appeared to him as "giving up to understand things."[147] His attitude about Schrödinger's wave mechanics was the opposite. Still in 1928, after the two formulations had been proved to be equivalent, he wrote in his treatise on atomic physics:

> Unfortunately the formalism of Heisenberg's mechanics is very complicated, mainly because it uses uncommon concepts and computational techniques. The second attempt to build an atomic mechanics is mainly due to Schrödinger [. . .] the basic ideas of the two approaches are very different, if not antithetical. It is therefore quite striking that the two new mechanics in all cases lead to the same results, so that they can be considered as two different aspects of the same thing. For this reason, in this treatment we shall only briefly consider the foundations of wave mechanics, which is much more intuitive, and formally simpler, albeit perhaps less organic than Heisenberg's theory.[148]

The "uncommon" concepts and computations definitely influenced Fermi's judgement, but also the origins of the two formulations are important in this regard. While Heisenberg's mechanics was created in Göttingen within the *received view,* wave mechanics, as showed by Pais,[149] had deep connections with statistical mechanics, exactly the field of interest of Fermi in the middle 20s.

The 1927 Como conference, and the Solvay Conference of the same year, confirmed Fermi's international prestige, and moreover highlighted the importance of his statistics. He was well aware of this, as corroborated by his contribution to the discussion after Bohr's talk in Como:

> I would like to make some considerations about the new statistical methods in quantum mechanics. It is known that quantum theory, together with the Boltzmann-Maxwell statistics, allows one to determine the volume of the cells in which phase space has to be subdivided. However, this prescription is not enough to get the statistics of the ideal gas, as when the dimensions of the vessel increase, the quantum states become more densely packed, and at the end, there are no more discontinuities. Two attempts have recently be made to overcome this difficulty, one by Einstein, and one by myself. Einstein assumes that there is a statistical dependence among the molecules of the gas, analogous to that proposed by Bose for the light quanta; in my case, I applied Pauli's exclusion principle to the whole gas, considered as a single system, formed by all molecules (which are indistinguishable).

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[146] *Ibid.,* p. 217.

[147] See footnote 26, Chapter 1.

[148] E. Fermi, *Introduzione alla fisica atomica, op. cit.,* pp. 300–301.

[149] A. Pais, *Subtle is the Lord, op. cit.,* p. 405.

> The relation between these two statistics have been clarified by means of the new mechanics of Heisenberg, Dirac and Winter; they showed that if one has a system containing identical particles, its terms divide in groups, and there is no way to pass between the terms that belong to different groups. One of these groups obey the Bose-Einstein statistics, the other the statistics proposed by the author. Experience has so far proved that the electrons in an atom, and also the positive corpuscles, always satisfy the exclusion principle.
> Applying this statistics to the electron gas inside a metal, Pauli was able to explain why the paramagnetism of the solid alkaline metals is considerably smaller than what should correspond to the value of the electron magnetic moment, and Prof. Sommerfeld showed how by using it one can explain many more properties of metallic conduction.
> One can also try to use the same hypotheses to build up a theory of metals capable of giving account of the *forces* that hold the metal together. It is enough to consider the positive ions of the metal as arranged at the vertexes of the crystal lattice, and compute the distribution of the valence electrons under the action of the electrostatic forces, with a method similar to that used by Debye and Hückel in their theory of strong electrolytes; obviously, one should apply the new statistics instead of the classical one. The computations involved in this theory however are very long, and have not been completed to date.[150]

It is easy to recognize in the last sentences a program which would soon give rise to a new officially recognized research area, which is nowadays known as solid state physics.

3.13 The acceptance of the quantum paradigm: solid state physics and the path toward nuclear physics

The years between 1926 and 1929 were dense with events. The move to Rome after Fermi was given the first Italian chair in theoretical physics and his well-deserved international recognition during the Como and Solvay conferences in 1927 were very important events in his life. From a more strictly scientific viewpoint, the discovery of the new statistics, the formulation of Schrödinger's wave mechanics, the proof of the equivalence of the latter with Heisenberg's matrix mechanics, the final acceptance of quantum mechanics by the physics community at the Solvay conference, Dirac's first papers on quantum electrodynamics were the milestones of Fermi's acceptance of the new quantum mechanics. The comparison of what he wrote in some popular papers, written before and after the formulation of wave mechanics, is quite instructive. In a paper written in early 1926 for *Nuovo Cimento,* one can read:

> The border between the quantum and the wave behavior is given by the visible region, where both interference and photoelectric phenomena take place. We understand that this dualism between wave and quantum theory is only a temporary arrangement, until we shall be able to accommodate the entire field of optics, from the radio-telegraphic waves to the Röntgen rays, in a unique, harmonious edifice.[151]

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[150] *CPF I,* pp. 180–181.

[151] Fermi [33], *CPF I,* p. 206.

And again, in a paper written in 1926, which was the text of a talk given in Milan on 30 October 1925, at a meeting of the Mathesis society:

> Bohr was compelled to admit that in atomic physics the laws of classical electrodynamics had to be modified. He tried to resume in some laws the effect of these modifications, whose exact form is still unknown, and whose determination is at present the most important problem in atomic physics.[152]

Fermi's contribution to a conference, whose text was published in 1929,[153] shows a completely different attitude. After tracing the main lines of development of the *OQT,* stressing its weak points and the little hope that they could be corrected, in view of its "logical contradictions," he expressed the following opinion about the new quantum mechanics:

> [. . .] it has confirmed all the results of the old theory that agreed with the experience, explaining them with a theory without internal contradictions. For the phenomena, for which the old theory only could give qualitative results, the new theory achieves a quantitative precision.[154]

And again, after discussing the most brilliant results of the new theory:

> These results, as I like to stress again, are obtained by means of a rigorous logical process, without the compromises and the many supplementary hypotheses that characterized the old quantum theory. Today, much more than in the past, we have a feeling of having a stable theoretical construction.[155]

If we want to fix a date for Fermi's transition from spectator to protagonist of the development of the quantum theory, our choice should probably fall between 1926 and 1927, in occasion of his definitive move to Rome. There are several hints of his complete acceptance of the new quantum paradigm, and the first paper he wrote in Rome, in 1926, was devoted indeed to an application of wave mechanics.[156] There Fermi showed that the value Schrödinger assumed for the electric current density leads to the correct value for the magnetic moment of an electron in a central field, for instance, the electron of the hydrogen atom. An even clearer hint was the project to create in Rome a group working in modern physics at the international level, having not only a theoretical character, but including also an experimental activity, directed by Rasetti, who had moved from Florence to Rome.

Also Fermi took part occasionally in these experimental researches, albeit this was not his main activity in that period.

Fermi's main lines of researches at the time were two, both related to the problem we have just discussed; one was the formulation of the new statistics and the other was related to his acceptance of Schrödinger wave mechanics.

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[152] Fermi [34], *CPF I,* p. 208.

[153] Fermi [56].

[154] *CPF I,* p. 330.

[155] *Ibid.,* p. 335.

[156] Fermi [39].

The application of the new statistics resulted in a number of papers which marked the birth of solid state physics. However, Fermi did not further develop the ideas contained in them.[157] The first of these paper was presented by Orso Mario Corbino at the 4 December 1927 meeting of *Accademia dei Lincei.* Fermi's project was clear from the first page:

> The purpose of this work is to show some results about the distribution of the electrons in a heavy atom, which can be obtained, in view of their great number, only treating them with a statistical method; or, in other terms, by considering them as an electron gas surrounding the nucleus. [. . .] In this paper we shall first of all show how one can compute by statistical methods the distribution of electrons around the nucleus; and based on that, we shall compute the energy necessary to completely ionize the atom, i.e., to strip it of all the electrons. Moreover, the computation of the distribution of the atoms around the nucleus allows one to determine the dependence of the potential on the distance from the nucleus, and thus, to know the electric field acting on the electrons in the atom. I hope to show in a future paper how to apply all this to the approximate computation of the binding energies of the single electrons, and to some questions related to the structure of the periodic system of elements.[158]

Fermi's method for determining these atomic properties is nowadays called the "Thomas-Fermi model," as almost one year earlier Llewellyn Thomas had published basically the same results in a little known journal (for this reason Fermi most likely was not aware of it).[159] As stressed by Fermi in the introduction to the paper, the core of the method is the determination of the potential inside the atom as a function of the distance from the nucleus. This leads to a certain integro-differential equation; in Fermi's words, "Since I was unable to find the general integral [. . .] I solved it numerically."[160]

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[157] Fermi [43–48]. Only the first three papers contained original results. The fourth is essentially a resume of the results in the second, and the fifth and sixth are German translations of the first three.

[158] Fermi [43], *CPF I*, p. 278.

[159] L. H. Thomas, *The calculation of atomic fields,* Proceedings of the Cambridge Philosophical Society 33 (1927), p. 542.

[160] Fermi [43], *CPF I,* p. 281. Rasetti wrote that Fermi solved the equation using a small Brunsviga table calculator, taking about a week (*CPF I,* p. 277). It is interesting to report an anecdote told by Segrè: "When Fermi found that to proceed he needed the solution to a nonlinear differential equation with unusual boundary conditions, he, with his usual energy, with a week of strenuous work computed the solution using a small manual computing machine. Majorana, who had just joined the group and was always very skeptical, decided that Fermi's solution was wrong, and one should have better checked it. He went home, transformed Fermi's equation into a differential equation of the Riccati type, and solved it without using any computing machine, simply relying on his extraordinary attitude to numerical calculus (with his astounding ability of mentally performing the most complicated arithmetical operations, he could easily make a living as a variety star). When he went back to the Institute and compared his result with Fermi's, he was marveled by their perfect coincidence" (E. Segrè, *Enrico Fermi, fisico, op. cit.*, pp. 52–53). Translator's note: we have preferred to translate these sentences from the Italian version of Segrè's book because they are more complete. For the original English version see E. Segrè, *Enrico Fermi Physicist, op. cit.*, pp. 53–54).

Fermi's idea, that should be considered, as stressed by Gianfranco Chiarotti, his work "most connected with the modern theory of electrons in solids"[161] has had important consequences on solid state physics. This is not his only contribution to that area of research.[162] In 1934 Amaldi and Segrè, in a series of experiments about the higher order absorption lines in alkali metals, observed a remarkable displacement when the metals were immersed in a gas. The effect was quite surprising, and the two young researchers discussed it with Fermi:

> We talked with Fermi about this unexpected phenomenon; he thought for a while and then said that most likely it was due to the dielectric constant of the gas. The thing seemed obvious, and we computed the formula describing the phenomenon by ourselves. However, for some gases the displacement was in the wrong direction! This was very surprising, and we had to resort to Fermi again. This time the explanation was not immediate. Only after a few days Fermi found it, and wrote an important paper which for the first time contained the idea of the pseudopotential.[163]

Fermi's explanations consisted in considering two causes for the displacement of the lines. The first, producing a displacement toward the red, is due to the polarization of the atoms of the perturbing gas. This takes place because

> when we excite an atom, moving an electron to a very far orbit, all the atoms of the perturbing gas positioned at a distance from the nucleus smaller than the radius of the electron orbit, feel an electric field, due to the fact that the screening of the nuclear charge by the electrons is diminished by the absence of the electron that has been displaced.[164]

The second, more important cause, is due to the fact that the atoms of the gas act on the optical electron as "potential holes distributed on its path." The effect of those holes, as shown by Fermi, is to cause a displacement toward the red or the violet, "so that, in accordance with the experiment, the total displacement of the lines takes place in some cases toward the red, in other cases toward the violet."[165] To assess the consequences of this second cause, Fermi treats the effect of the gas perturbatively, adding to the potential of the atom an averaged potential, proportional to the atom density and depending on a characteristic length a (positive or negative, depending on the type of atom), which took account of the experimental results of Amaldi and Segrè.

Let us now consider Fermi's second main research itinerary, namely, nuclear physics. It is a path that led to what he considered "his masterpiece, that will be

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[161] G. Chiarotti, *The debt of solid state physics to Enrico Fermi,* in *Symposium dedicated to Enrico Fermi on the occasion of the 50th anniversary of the first reaction,* Accademia Nazionale dei Lincei, Rome 1993, p. 113.

[162] Fermi's contributions to solid state physics are analyzed in the above cited paper by Chiarotti, and in F. Bassani, *Enrico Fermi e la fisica dello stato solido* [Enrico Fermi and solid state physics], in C. Bernardini and L. Bonolis, *op. cit,* p. 57.

[163] E. Segrè, *Autobiografia di un fisico* [Autobiography of a physicist], Il Mulino, Bologna 1955, p. 111.

[164] Fermi [95], *CPF I,* p. 707.

[165] *Ibid.*

remembered by posterity."[166] The underlying reasons of Fermi's success were many. The most important was his profound knowledge of quantum electrodynamics, that he acquired from Jordan's first formulations, and then mostly from Dirac. As reported by Amaldi,[167] he started working intensely on quantum electrodynamics in the winter of 1928. And again Amaldi informs us that Fermi was not entirely satisfied by Dirac's approach, and preferred to reformulate the theory in a mathematical language that was more familiar to him. The result of these researches is the long paper on quantum electrodynamics published in *Review of Modern Physics* in 1932. It contained the text of a series of lectures given by Fermi at the University of Michigan in Ann Arbor. As Marcello Cini recalled, "generations of young researchers have learned quantum electrodynamics from that text in the years immediately after the war";[168] it was an "unsurpassed example of simplicity in a difficult topic."[169] This paper contained the so-called "Fermi's golden rule," a formula for computing the transition probability for unit time between a state of a physical system, and another state which belongs to a continuum of states.[170] Fermi's works on quantum electrodynamics[171] are remarkably lucid and clearly written, and contain considerations that in some cases (for instance, in the paper on the electromagnetic masses in quantum electrodynamics) anticipated much ampler

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[166] See footnote 3, Chapter 1.

[167] E. Amaldi, *CPF I,* p. 305.

[168] M. Cini, *Fermi e l'elettrodinamica quantistica,* in C. Bernardini and L. Bonolis, *op. cit.*, p. 132.

[169] H. A. Bethe, *Memorial symposium . . . , op. cit.,* p. 253.

[170] This formula appeared as "golden rule No. 2" in a book edited by J. Orear, A. H. Rosenfeld, and R. A. Schluter, who took the notes of the lectures given by Fermi at University of Chicago from January to June 1949 (E. Fermi, *Nuclear Physics,* The University of Chicago Press, Chicago 1949, pp. 75 and 142). At page 14 it was used to compute the cross-section of a nuclear reaction where a nucleus *A*, hit by a particle *a*, transforms into a nucleus *B*, which emits a particle *b*. The "golden rule No. 1" is another formula to compute a transition probability for unit time, but it refers to another reaction, for instance, when a nucleus *A*, hit by a particle *b*, transforms into a nucleus *C*, which in turn transforms into a nucleus *B*, which emits a particle *b*. It was actually the computation of a cross-section made by means of the "compound model" of the atomic nucleus (see Chapter 4).

[171] Fermi [50, 52, 64–67, 70]. Fermi's formulation of quantum electrodynamics is essentially contained in the first and third of these papers. Particularly interesting is a remark in the third paper: "It is know that recently also W. Heisenberg and W. Pauli [. . .] have treated the problem of the quantum electrodynamics. However, as the methods followed by these authors are essentially different from mine, I think that publishing also my results should not be completely useless" (*CPF I,* p. 386). The "non-uselessness" of Fermi's result, as remarked by M. Cini (*Fermi e l'elettrodinamica quantistica, op. cit.*), appears just from the comparison of the number of pages in the papers: eight for Fermi's paper, and eighty for Heisenberg and Pauli. The second paper was devoted to the application of Dirac's theory to a typical interference phenomenon, i.e., the interference between the light waves hitting a plane mirror and those reflected by the mirror (Lippmann interference pattern). The fifth and sixth paper were about the lectures given by Fermi in Paris and Ann Arbor, respectively. The seventh paper was written with Hans Bethe, who at the time was visiting Rome, and was about the application of quantum electrodynamics to the scattering between two electrons.

and more general issues, such as the renormalization problem, a question that has been thoroughly studied in the ensuing fifty years.[172]

An important event that in some say directed Fermi in his itinerary toward nuclear physics was the change of the research lines followed by the Via Panisperna group. The change took place between 1930 and 1931, and was foreshadowed by the celebrated 1929 talk by Orso Mario Corbino,[173] which we already mentioned. Corbino stressed that the study of the atomic nucleus was the most promising field of physical investigation. There were two signals of the transition of the Roman group from atomic to nuclear physics: a research on the hyperfine structure (see Appendix C.11), which in some sense represents a bridge between the study of the atom and of the nucleus, and the first conference on nuclear physics, held in Rome and organized by Fermi.

The first paper on the hyperfine structure of the atomic spectra, due to the interaction between the motion of the atomic electron and the intrinsic nuclear magnetic moment, was presented on 30 January 1930.[174] The purpose was to deduce the values of the nuclear magnetic moments from the experimental data. In this first paper, the computation was made for the elements: sodium, rubidium, and cesium. The second paper was a natural continuation of the first. It was written with Segrè and was presented on 10 March 1933. It included the systematic study of the hyperfine structure of fourteen chemical elements (seventeen including some isotopes), and the determination of the corresponding nuclear magnetic moments. It appeared after the discovery of the neutron, and after Heisenberg's first ideas on the nuclear structure had been published. It is interesting to note that Fermi's work took place within the new nuclear theories. The two authors in the final conclusions of their paper, after resuming in a table the data obtained for a number of nuclei, wrote:

> One might be tempted to draw from this table some conclusions about the structure of the nuclei. However, we think that the modest precision of the experimental data, and the presence of many gaps in them, do not allow us to reach any conclusive result. An obvious remark is that in the table all nuclei having even atomic number and odd atomic weight (which, according to Heisenberg's theory, contain an even number of protons, and an odd number of neutrons), have a nuclear magnetic moment much smaller of those with an odd number of protons and an even number of neutron. But even this rule does not seem to be absolute, as it is most likely violated by K^{39}, which is not shown in this table.[175]

The first international conference on nuclear physics, organized by Corbino and Fermi, took place in Rome from 11 to 18 October 1931. While this was a clear sign of the transition of the Roman group to nuclear physics, one should also stress that this initiative was strongly supported by the fascist regime, eager to affirm Italy's international prestige also in the sciences. This was confirmed by Benito Mussolini's presence at the opening of the conference.

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[172] Fermi [65].

[173] See footnote 41, chapter 1.

[174] Fermi [57a].

[175] Fermi [75b], *CPF I,* p. 530.

The conference took place just before the discovery of the neutron, and can be regarded as the swan song of nuclear protophysics. The crisis was due to the seemingly insurmountable difficulty to reconcile a quantum description of the nucleus with the existence of nuclear electrons. Almost every talk heavily stressed this problem. "In conclusion" wrote Neville F. Mott, "we may say therefore that the spin of the electron is still not properly understood, and that it is not possible to use the Dirac equations to describe the behavior of the electrons in the nucleus."[176] Similarly, Samuel A. Goudsmit wrote:

> [...] the electrons in the nucleus seem to lose their spin and magnetic properties and that only the protons determine the spin moment and the magnetic moment of the nucleus.
> [...] It is my belief that the mechanics applicable to the nucleus must differ considerably from the quantum mechanics now used for the atom, in the same way as the latter differs from the classical mechanics for large masses. Classical mechanics has been partially successful in explaining atomic properties and similarly it is possible to describe at present some properties of the nucleus with the language of atomic mechanics, but one should not be surprised at finding great difficulties.[177]

And from George Gamow's talk:

> We know that there are two rather differing kinds of constituent parts of atomic nuclei, we may call them *heavy* and *light* constituent parts. To the first class belong the protons and also complex constituent parts such as α-particles. For these particles we can estimate that, due to the relatively great masses, their motion may be described according to unrelativistic mechanics and the nuclear processes involving these particles only can be treated in detail by means of the present quantum theory.
> On the other hand the light constituent parts, the nuclear electrons, move in the nucleus with velocities very near to that of the light and relativistical treatment is necessary. That is just the point where the present means of theoretical physics fail to help us. [178]

The most remarkable among all was Bohr's authoritative contribution. From it one can clearly perceive the importance of the "principle of pre-existence of the emitted particles," "the fetish of nuclear protophysics": if a particle is emitted by a nucleus, then it already existed there. The title of Bohr's contribution was "Atomic stability and conservation laws." The third section starts with this claim:

> The experimental evidence regarding the charges and the masses of atomic nuclei and their disintegrations finds, as is well known, an immediate explanation on the view that all nuclei are built up of protons and electrons.[179]

This conjecture was perhaps necessary at the time, but led to a number of anomalies that made quantum mechanics inapplicable to the nuclear structure. These anomalies were interpreted by Bohr as a failure of the conservation principles to hold at the nuclear level. Bohr justified this failure as follows:

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[176] N. F. Mott, *On the present status of the theory of the electron,* in *Convegno di Fisica Nucleare,* Accadемia d'Italia, Rome 1932, p. 32.

[177] S. Goudsmit, *Present difficulties in the theory of hyperfine structure, ibid.,* pp. 40 and 49.

[178] G. Gamow, *Quantum theory of nuclear structure, ibid.,* p. 65.

[179] N. Bohr, *Atomic stability and conservation laws, ibid.,* p. 127.

> In this situation, we are led to consider the capture or the expulsion of an electron of a nucleus plainly as an extinction or a creation, respectively, of the electron as mechanical entity.[180]

In other words, it was postulated that the electron, entering or getting off the nucleus, changed, at least partially, its nature. Bohr was fully aware of this, and indeed, a few lines later he observed "the properties of nuclear electrons are radically different from those of the electrons belonging to extra-nuclear configurations."

As we already stressed in Chapter 2, the nuclear electrons were the basic problem of nuclear protophysics. What was Fermi's opinion on the matter? One can find a first remark about this problem in a 1926 paper written with Rasetti, dealing with the Uhlenbeck-Goudsmit theory of the spinning electron. Fermi and Rasetti wrote: "It is well known that the nuclei contain electrons," and after computing the size of the "spinning electron" and getting the value $3{,}3 \times 10^{-12}$ cm, he added:

> This is about 20 times bigger than the accepted measure of the radius of the electron. It is true that there are not direct measurements of the latter, however, this is serious drawback, as we know that the nucleus contains a considerable number of electrons, and the linear dimensions of the nucleus are known with good precision from the measurements of the scattering of alpha particles through matter, and they are, as it is known, of the order of 10^{-12} cm. One sees that the two facts appear to be quite at a variance, unless we admit that the electron, entering the nuclear structure, changes its nature considerably.[181]

We can easily detect an analogy with the viewpoint expressed by Bohr in 1931. A review paper written by Fermi in 1932, entitled *Lo stato attuale della fisica del nucleo atomico* [The present state of nuclear physics],[182] opens with some similarly remarkable considerations. This paper was written just before the discovery of the neutron, or more precisely, of Chadwick's interpretation of the experiments made by the Joliot-Curies about the penetrating radiation, as the paper includes a final section, written after the main body of the paper had been redacted, with a suggestion about the correct interpretation of the experiment made by the Joliot-Curies. Fermi's paper contained a lucid exposition of the nuclear phenomenology and of the interpretations that had been proposed up to that moment. One finds here, for instance, Bohr's proposal of the non-validity of the conservation laws inside the nucleus, and many other topics and problems in nuclear protophysics. The introductory paragraph is very important for understanding Fermi's position in that crucial juncture:

> The present state of nuclear physics can be compared, under some aspects, with that of atomic physics thirty years ago. At that time, indeed, the study of the atomic properties was confronted by a number of phenomena that could not be explained with the theories of the time, basically founded on classical dynamics and electrodynamics. Since then, those theories have been framed in a natural way into quantum theory, first in a mostly qualitative way, and nowadays also in a quantitative form, at least in most cases. Obviously,

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[180] *Ibid.,* p. 128.

[181] Fermi [35], *CPF I,* p. 217.

[182] Fermi [72b].

> the quantum laws apply not only to phenomena at atomic scale, but also at the macroscopic level; however, for the latter their importance is much reduced, and the classical laws yield a perfectly satisfactory approximation. The reason why the classical laws should be replaced by the quantum ones is the different scale of the phenomena under study. In the same way, passing from atomic to nuclear physics, there is another change of scale; from the atomic dimensions, of the order of 10^{-8} cm, one goes down to the nuclear dimensions, of the order of 10^{-13} cm; that is, one studies objects that are about 100,000 times smaller than the atoms. The nature of the atomic phenomena makes us suppose that the laws that regulate the behavior of the corpuscles inside the atom are not applicable, without deep modifications, to the study of the corpuscles that make up the atomic nucleus. This hypothesis, especially for what concerns the electrons inside the atomic nuclei, seems to be confirmed by what we know at the moment about the atomic nuclei.[183]

It is easy to recognize here the ideas already expressed by Goudsmit in 1931 at the Rome conference. Fermi's position in 1932 for what concerned nuclear protophysics can be summarized as follows:

a) acceptance of the (p-e) model and of the principle of pre-existence of the emitted particles;
b) incompatibility of a quantum description of the atomic nucleus with the presence of nuclear electrons;
c) need of a new theory, adapted to the scale of nuclear phenomena.

The years 1932 and 1933 were crucial for the transition from nuclear protophysics to nuclear physics. We saw in Chapter 2 the different chips of a complicated jigsaw that Fermi would eventually fit together, with his "masterpiece which will remembered by posterity." These included the discovery of the neutron, and the first theories of the nuclear structure by Heisenberg and Majorana. It is interesting to compare Fermi's idea in 1932 with those expressed by him in a talk given at the 22nd meeting of the Italian Society for the Progress of Science (SIPS), held in Bari from 12 to 18 October 1933.[184] The talk was entitled *The last particles that make up matter,* and was both a popularizing talk and a review. After mentioning the main anomalies of the (p-e) model, in particular the one about the statistics, and remarking that they were so serious that "the model of the nucleus made of protons and electrons lost any credibility," Fermi advocated the Heisenberg-Majorana theory, according to which the nucleus was made of protons and neutrons. However, there was the problem of the structure of the neutron. The difficulty in accepting the elementary nature of the neutron stemmed from the need, according to the principle of pre-existence of the emitted particles, of a suitable explanation of the β emission. One can indeed read:

> According to the Heisenberg-Majorana theory, on the other hand, one should consider protons and neutrons as fundamental constituents of the nuclear structure. Perhaps one

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[183] *Ibid., CPF I,* p. 489.

[184] Fermi [79].

> could regard the neutron as a tight combination of a proton with an electron. This seems to be necessary to explain the occurrence of disintegrations with the emission of β-rays.[185]

This "tight combination" would be regulated by "different laws than those of ordinary quantum mechanics." As already suggested by Goudsmit in 1931 and again by Fermi in his 1932 review paper, this meant that a new theory was needed, which in view of the different scales of the nuclear and atomic dimensions, would by as different from quantum mechanics, as the latter is from classical mechanics. And Fermi added,

> if [...] we shall really be able to prove that the nuclear electrons, whose existence we have to admit to explain the emission of β particles, do not exist in a free state, but they are, for instance, tightly bound to protons to form neutrons, then there will be a reasonable possibility to construct, to some degree of advancement, a theory of the nucleus. Indeed it will be possible, unless one wants to study phenomena where the neutron structure is changed, to use the procedures and the general interpretative scheme of quantum mechanics.[186]

So, just a few months before his celebrated paper on the β decay, Fermi still believed in the principle of pre-existence of the emitted particles. The problem of the presence of the nuclear electrons was transferred from the nucleus to the neutron. But the essence of the problem was still there. In this connection Fermi wrote:

> We should subsequently clarify the structure of the neutron, to which, as we have discussed, the usual quantum mechanics should not be applicable. There are indeed in the continuous spectrum of the β-rays some indications that, according to Bohr, could suggest that in these new, unknown laws, the principle of conservation of energy is no longer valid, unless we accept Pauli's proposal of the existence of the "neutrino," a hypothetical particle with no electric charge, and a mass of the order of magnitude of the electron mass. This particle, in view of its enormous penetrating power, would elude any attempt to observe it, and its kinetic energy would re-establish the conservation of energy in the β disintegration. Certainly, the increase of our knowledge about the possible disintegrations of the nuclei will clarify these questions. The incredible speed of the experimental progress in this area over the last two years makes us hope for the best.[187]

In this way, the incurable difficulties of the (p-e) model are transferred to the structure of the neutron. It is on this problem that two hypotheses compete, Bohr's extreme proposal to abandon the conservation principles, and Pauli's bold conjecture about the existence of a new particle, the neutrino. It may be useful to recap Fermi's ideas about the structure of the nucleus in October 1933:

a) refusal of the (p-e) in its most primitive version;
b) acceptance of the Heisenberg-Majorana theory, and belief in the possibility to give a quantum description of the atomic nucleus as formed by neutrons and protons;

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[185] *Ibid.,* p. 556.

[186] *Ibid.,* p. 557.

[187] *Ibid.*

c) existence of problems about the structure of the neutron, in view of the principle of pre-existence of the emitted particles, and need for a new theory;
d) Bohr's and Pauli's hypotheses as possible alternative to explain the β decay.

The 7th Solvay Conference took place from 22 to 29 October 1933. Its topic was "Structure and properties of the atomic nuclei." Fermi was present, but we cannot know what he was thinking at the time. We can certainly say that a few weeks before, as confirmed by his talk at the Bari conference, his ideas about the structure of the nucleus were as uncertain as were those of the participants in the Solvay conference, and were driven by the pre-existence principle. We can with the same certainty say that during the conference he did not mention his ideas on the β decay; but a few weeks after his return from Brussels, he was ready to publish a revolutionary paper: a nucleus without electrons, neither free, nor hidden inside the neutrons. The paper, entitled *An attempt of a theory of β-rays emission,* was published in December 1933 by the journal *La Ricerca Scientifica,* after, as we have already mentioned, was rejected by *Nature* (see Figure 3.5). An enlarged version appeared in 1934 in *Il Nuovo Cimento* and *Zeitschrift für Physik.*[188] In Chapter 2 we analyzed the main features of the theory. Here we want to stress that this paper marked the definitive refusal of the (p-e) model by denying the pre-existence principle, and established a new theoretical framework able to accommodate the rich and complex nuclear phenomenology available at the time. The formalism of the theory allowed Fermi to distinguish between two kinds of decay, the allowed and the forbidden ones. The first take place even when the heavy particles, protons and neutrons, are at rest in the nucleus; the second only take place when the heavy particles are in motion. The computation of the mean lifetime of the two decays shows that the allowed decays have a mean lifetime which is in average a hundred times shorter than the forbidden decays. This agreed with the experimental data. Moreover, by measuring the mean lifetime of an allowed decay, one could estimate the only free parameter of the theory, which is nowadays known as the subject is the "Fermi constant," and measures the strength of the interaction introduced by Fermi.

Fermi's theory also provided important information about the neutrino mass. He claimed indeed that "the neutrino mass is zero, or, in any case, small in comparison to the electron mass." The comparison with the experimental data about the energy of the emitted electrons was made using the data provided by an experiment of B. W. Sargent.[189] As we already discussed in Chapter 2, there was a good agreement, with some discrepancies only at low energies, where Fermi obtained slightly larger values. But this did not worry him: "If further comparison with the experimental data were to show some contradiction, we could modify the theory without changing its conceptual foundations."[190] So, Fermi was well aware that in his theory, the

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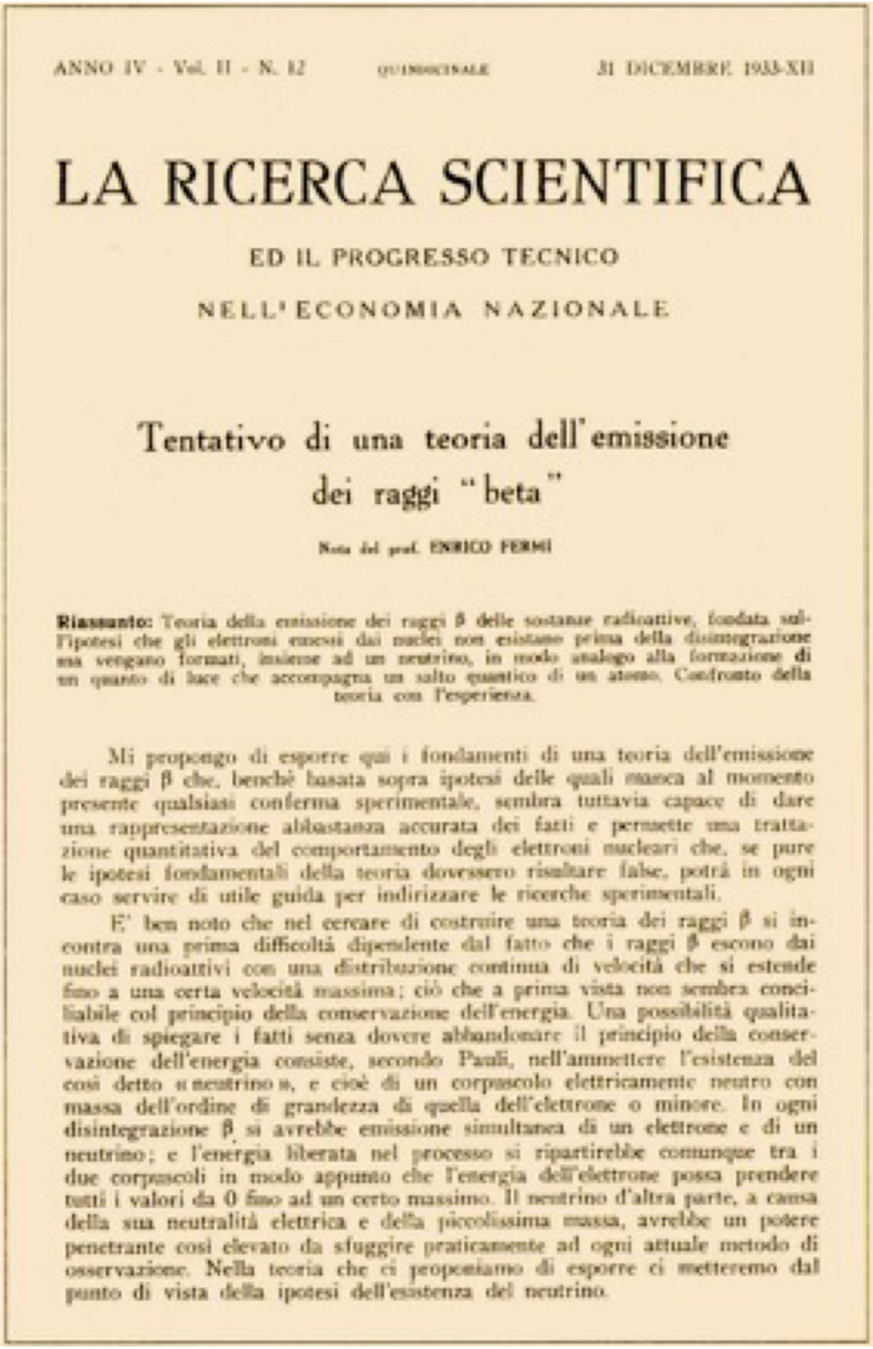
[188] Fermi [76, 80a, 80b].

[189] B. W. Sargent, *The maximum energy of the β-rays from Uranium X and other bodies,* Proceedings of the Royal Society 139 (1933), p. 659.

[190] Fermi [80a], *CPF I*, p. 574.



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LA RICERCA SCIENTIFICA

ED IL PROGRESSO TECNICO

NELL'ECONOMIA NAZIONALE

Tentativo di una teoria dell'emissione dei raggi "beta"

Nota del prof. ENRICO FERMI

Riassunto: Teoria della emissione dei raggi β delle sostanze radioattive, fondata sull'ipotesi che gli elettroni emessi dai nuclei non esistano prima della disintegrazione ma vengano formati, insieme ad un neutrino, in modo analogo alla formazione di un quanto di luce che accompagna un salto quantico di un atomo. Confronto della teoria con l'esperienza.

Mi propongo di esporre qui i fondamenti di una teoria dell'emissione dei raggi β che, benchè basata sopra ipotesi delle quali manca al momento presente qualsiasi conferma sperimentale, sembra tuttavia capace di dare una rappresentazione abbastanza accurata dei fatti e permette una trattazione quantitativa del comportamento degli elettroni nucleari che, se pure le ipotesi fondamentali della teoria dovessero risultare false, potrà in ogni caso servire di utile guida per indirizzare le ricerche sperimentali.

E' ben noto che nel cercare di costruire una teoria dei raggi β si incontra una prima difficoltà dipendente dal fatto che i raggi β escono dai nuclei radioattivi con una distribuzione continua di velocità che si estende fino a una certa velocità massima; ciò che a prima vista non sembra conciliabile col principio della conservazione dell'energia. Una possibilità qualitativa di spiegare i fatti senza dovere abbandonare il principio della conservazione dell'energia consiste, secondo Pauli, nell'ammettere l'esistenza del così detto «neutrino», e cioè di un corpuscolo elettricamente neutro con massa dell'ordine di grandezza di quella dell'elettrone o minore. In ogni disintegrazione β si avrebbe emissione simultanea di un elettrone e di un neutrino; e l'energia liberata nel processo si ripartirebbe comunque tra i due corpuscoli in modo appunto che l'energia dell'elettrone possa prendere tutti i valori da 0 fino ad un certo massimo. Il neutrino d'altra parte, a causa della sua neutralità elettrica e della piccolissima massa, avrebbe un potere penetrante così elevato da sfuggire praticamente ad ogni attuale metodo di osservazione. Nella teoria che ci proponiamo di esporre ci metteremo dal punto di vista della ipotesi dell'esistenza del neutrino.

Fig. 3.5 Title page of Fermi's paper on the theory of β decay.

"masterpiece" was its conceptual structure, capable of modeling a nucleus without electrons, which can disintegrate producing electrons and neutrinos.

The parabola of nuclear protophysics thus reached its end, giving rise to a new line of research; soon this would split into two well-defined research areas, nuclear physics and elementary particle physics. From the viewpoint of the global maps, Fermi's theory of β decays marks a sharp discontinuity between nuclear protophysics and nuclear physics. The meaning of the word "nucleus" in 1934 was very different from its meaning in 1932. It was not a nuance, but a radical semantic change, which, for instance, depleted the term "nuclear electron" by any meaning. The same process, if regarded from the viewpoint of local itineraries, appears to be much more continuous, as one can easily see by comparing Fermi's ideas in 1932, before the Rome conference and just before the discovery of the neutron, in 1933, before the Solvay conference, and eventually with the contents of his theory, published in 1933. The dynamics of a scientific theory cannot be analyzed only by contrasting continuity and discontinuity; these two categories belong to two different research areas — the first concerns the dynamics of the local itineraries, the second that of the global maps.

Chapter 4
20th century physics: 1934–1954

While scientific investigations proceed and human knowledge increases, the global maps become more complex. Then history can be told in different ways, as one can choose different paths to connect two nodes of a network.

This chapter is devoted to the reconstruction of the main lines of evolution that during the 30s and 40s led to creation of elementary particle physics. As we shall see, this new discipline was born, on the one hand, as a consequence of the tremendous development of the accelerating machines, and on the other hand, as a result of the confluence of two different research areas, nuclear physics and cosmic ray physics. The latter process found in the emerging quantum field theory a theoretical language suitable to describe the new discoveries, making them independent from their original context.

But, before starting this journey, a warning is in order. One might be surprised not to find in this chapter some important processes of contemporary physics, like those leading to the atomic pile, or the Manhattan Project. But these and other local itineraries of Enrico Fermi, who at the time was already a leader in the international scientific research, basically coincide with the global maps, and will be analyzed in the next chapter.

4.1 Nuclei and particle accelerators

We have seen how radioactivity, since its discovery, was a powerful tool of research, as it produces many types of accelerated particles which could be used to investigate the structure of matter, and in particular, in its early stages, of the atom. The study of the scattering of charged particles has been, since the early years of the 20th century, the main tool for the experimental research in atomic and nuclear physics. However, the energy of the particles emitted in natural processes is quite small,

G. Bruzzaniti, *Enrico Fermi*, Springer Biographies,
DOI 10.1007/978-1-4939-3533-8_4

and therefore it became necessary to design the *particle accelerators,* machines that dramatically changed high energy physics. One could say, establishing an analogy with an older and more familiar area of investigation, that the particle accelerators have allowed the researchers to explore the structure of matter at smaller and smaller dimensions, as the telescope enlarged our knowledge of the universe at much larger scales.

We can reconstruct global lines of evolution also for the particle accelerators. Figure 4.1 clearly shows the exponential increase of the energies provided by the

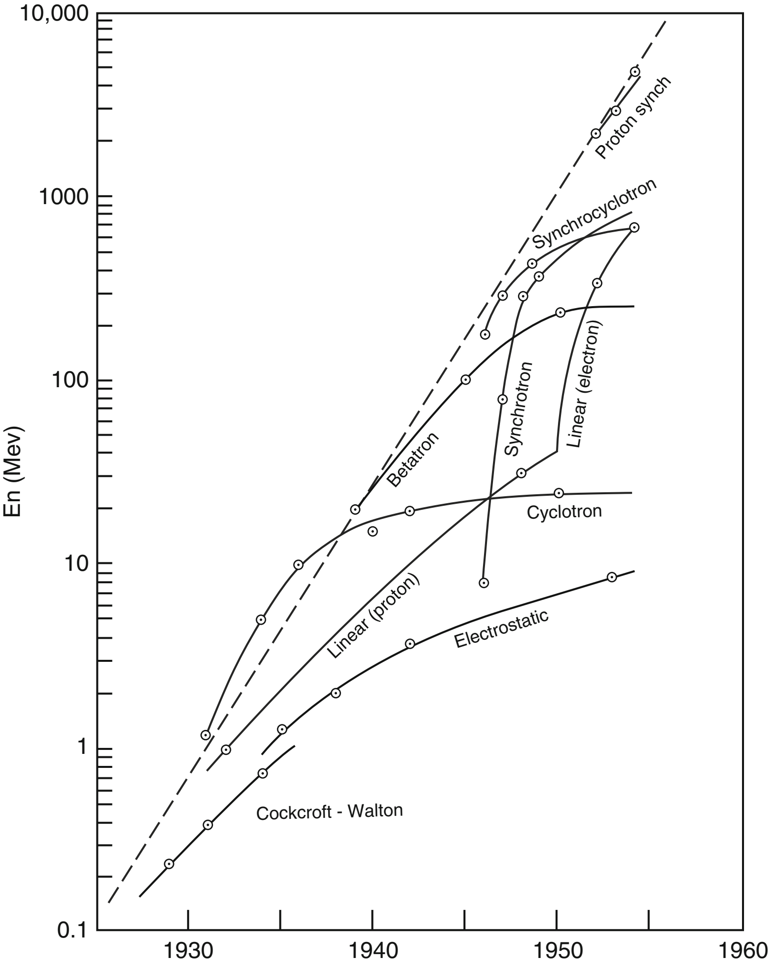

Fig. 4.1 The so-called *Livingston's Chart,* a diagram which represents the energy reached by the particle accelerators from 1930 to 1954. Its most evident feature is its basically linear behavior; as the scale is logarithmic, this means that the energy increased in an exponential manner.

accelerators during the period 1930–1954. The starting point is located within nuclear protophysics, in a 1930 paper by Cockroft and Walton:

> It would appear to be very important to develop an additional line of attack on problems of the atomic nucleus. The greater part of our information on the structure of the nucleus has come from experiments with α-particles and if we can supplement those with sources of positive ions accelerated by high potentials we should have an experimental weapon which would have many advantages over the α-particle. It would, in the first place, be much greater in intensity than α-particle sources, since one microampere of positive ions is equivalent, so far as numbers of particles is concerned, to 180 grams of radium equivalent. It would in addition have the advantage of being free from penetrating β and γ rays which are a complication in many experiments.[1]

Cockroft and Walton's project was motivated by some theoretical questions, whose clarification was of paramount importance. According to Gamow's theory, the wave-like properties of matter allow an α particle to enter a nucleus by crossing a potential well whose energy is greater than the energy with which the particle is emitted. But then it should be possible for a proton to enter a nucleus even if its energy is smaller than the energy barrier provided by the nucleus. Cockcroft and Walton's computations showed that a proton accelerated to an energy of only 300 keV (kilo-electronvolt) has a substantial probability to enter the nucleus of a light element, for instance, boron, or, even more easily, lithium. These computations convinced Rutherford, their director at the Cavendish Laboratory in Cambridge, to give his approval to the experiment. The announcement of the first disintegration triggered by artificially accelerated particles was given by Cockroft and Walton in June 1932; in their experiment, the emission of α particles by a nucleus of lithium was obtained by bombarding the latter with accelerated protons.

The importance of Cockroft and Walton's work is enormous, not only because it gave an experimental proof that quantum mechanics was adequate to treat the nuclear processes, but also because, at the global level, it foreshadowed what was going to be the future of nuclear physics, and more generally, of the physics of elementary particles: no longer an isolated work, which could be done in any laboratory, but an organized enterprise, that was possible only in well equipped centers, where the great accelerating machines could be built and operated. A research more and more demanding from the viewpoint of the financial resources, and therefore constrained by the availability of conspicuous funds. "Without the cooperation of many people this project could not have been carried through to its present state,"[2] wrote indeed Van de Graaff in a 1933 paper where he described his project of an electrostatic generator (see Figure 4.2), invented just a few years earlier, capable of reaching much higher voltages than Cockroft and Walton's machine.

In the same years when at the Cavendish Laboratory in Cambridge Cockroft and Walton were building their machine, in Berkeley Lawrence was working on his

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[1] J. D. Cockcroft and E. T. S. Walton, *Experiments with high velocity positive ions,* Proceedings of the Royal Society 129 (1930), p. 477.

[2] R. J. Van De Graaff, *A 1,500,000 Volt electrostatic generator,* Physical Review 38 (1931), p. 1919.

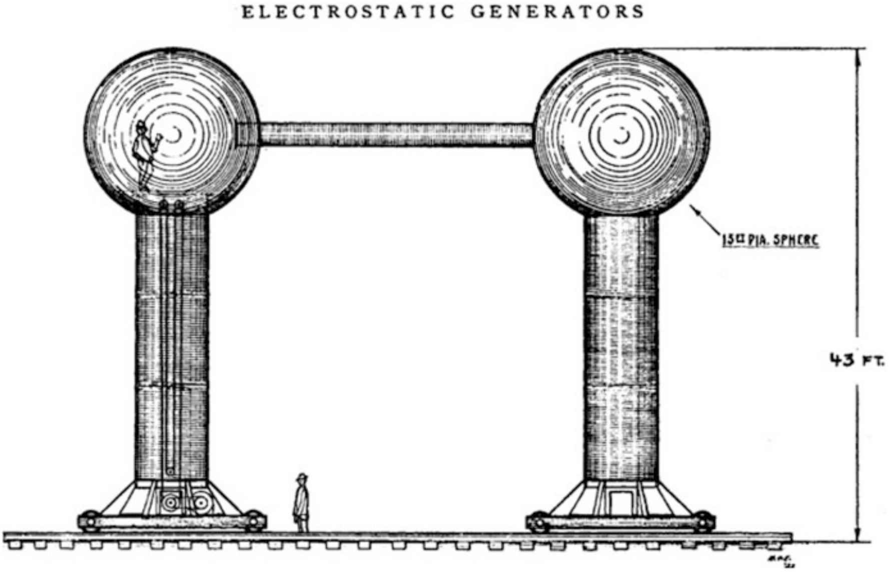

Fig. 4.2 A drawing of the electrostatic generator designed in 1933 by Van de Graaff to reach the voltage of 10 million volt.

cyclotron, which has been defined as "…the single most important invention in the history of accelerators."[3] The great limitations that one finds in accelerating particles, that is, the need of great voltages, the great sizes of the accelerating machines, and the difficulty in getting the necessary vacuum, were circumvented thanks to the combined action of a magnetic field, which makes the particles move along spiral instead of straight lines, and an oscillating electric field, which periodically accelerates them. Lawrence's project was based on the pioneering work of Rolf Wideröe, who in turn reconsidered a 1924 proposal by Gustaf Ising. The idea was to accelerate the particles not by means of a unique extremely high voltage, but rather with a sequence of smaller electrical pulses.[4] As Lawrence and Edlefsen wrote in 1930:

> Very little is known about nuclear properties of atoms because of the difficulties inherent in excitation of nuclear transitions in the laboratory. The study of the nucleus would be greatly facilitated by the development of a source of high speed protons having kinetic energies of about one million volt-electrons. The straightforward method of accelerating protons

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[3] E. M. McMillan, *Early history of particle accelerators,* in *Nuclear Physics in Retrospect,* R. H. Stuewer ed., University of Minnesota Press, Minneapolis 1977, p. 126.

[4] G. Ising, *Prinzip einer Methode zur Herstellung von Kanalstrahlen hoher Voltzahl,* Arkiv för Mathematik, Astronomi och Fysik 18 (1924), p. 1; R. Wideröe, *Über ein neues Prinzip zur Herstellung hoher Spannungen,* Archiv für Elektrotechnik 21 (1928), p. 387. For more details, see E. M. McMillan, *op. cit.;* P. Waloschek, *Life and Work of Rolf Wideröe,* Vieweg, Braunschweig-Wiesbaden 1994.

> through the requisite difference of potential presents great difficulties associated with the high electric fields necessarily involved. Apart from obvious difficulties in obtaining such high potentials with proper insulation, there is the problem of development of a vacuum tube suitable for such voltages.[5]

Lawrence's project was realized in October 1932. The first cyclotron he built, after a small prototype with a diameter of 4 inches, had a diameter of 11 inches and could accelerate protons up to the energy of 1.2 MeV, which was enough to trigger an artificial disintegration.[6] That was the first of a series of more and more powerful cyclotrons. The upper bound on these machines is set by the polar expansion of the magnet which creates the magnetic field (about 60 inches) and the energy of the particles (25 MeV for protons); in this range, the relativistic effects on the mass of the accelerated particles, which make a further acceleration almost impossible, can no longer be neglected. These limits were amply surpassed when cyclotrons were replaced by synchrocyclotrons and synchrotrons.

In 1954 magnets were larger than 14 feet, and protons were accelerated over 400 MeV.[7] Also the techniques of revealing particles underwent significant improvements (see Appendix C.12).

In the 50s the United States were leading the experimental research in elementary particle physics. As we shall see, that was due to World War II.

4.2 Cosmic ray physics

Most of us have seen an electroscope in our high school physics laboratory. It is a simple but very sensitive instrument, made by a glass bottle containing two thin gold strips, suspended from a conducting rod. The rod goes through an insulating cap and allows electric charge to be transmitted to the gold strips. Since the strips are charged with electricity of the same sign, they repel each other, and move apart. If the air around the electroscope is ionized, the electric charges in the air with opposite sign with respect to the charge in the electroscope will make the latter discharge. The time that the electroscope takes to discharge can be regarded as a measure of the ionization of the air. Actually, the electroscope always discharges spontaneously, which indicates that air always contains charged particles. It is know from the early 20th century that the discharge cannot be avoided even by insulating the electroscope, or by taking the instrument on top of the Eiffel tower (as the Jesuit Theodor Wulf did in 1910), to make sure that the production of charged particles is not due to the vicinity of the ground, for instance, for the presence of

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[5] E. O. Lawrence and N. E. Edlefsen, *On the production of high speed protons,* Science 52 (1930), p. 376.

[6] E. O. Lawrence and M. S. Livingston, *The production of high speed light ions without the use of high voltages,* Physical Review 40 (1932), p. 19.

[7] M. S. Livingston, *High-Energy Accelerators,* Interscience Publishers, New York 1954, p. 151.

radioactive substances. The mystery was solved in 1912, when Victor F. Hess, with an experiment with an aerostatic balloon, discovered that above the altitude of 3000 feet electroscopes discharge more rapidly. The scientist's conclusions left no room to doubt:

> The discoveries revealed by the observations here given are best explained by assuming that radiation of great penetrating power enters our atmosphere from the outside and engenders ionization even in [a] counter lying deep in the atmosphere. The intensity of this radiation appears to vary hourly. Since I found no diminution of this radiation for balloon flights during an eclipse or at night time we can hardly consider the sun as its source.[8]

In 1913 Hess found further experimental evidence confirming his hypothesis.[9] Later it was discovered that the ionizing power of the cosmic radiation is practically uniform at the sea level, and its value is 23 pairs of ions per cubic inch per second, at standard pressure. With the outbreak of the First World War the interest for this topic faded out, to revive in the 20s. *Cosmic ray physics,* as christened by Millikan in 1925, was born.[10]

4.3 The "birth song"

Nowadays we know that cosmic rays are mainly protons (about 85%), α particles (about 12%), and, for a very small fraction, nuclei of heavier elements, electrons and photons. In the 20s the most accredited hypothesis was that they were only radiation, that is, very energetic γ rays (also called, especially in the German literature, "ultragamma radiation"). The hypothesis was due to the fact that the γ rays were the most penetrating form of radiation known at the time (more penetrating than α and β rays), and since cosmic rays were able to propagate for hundreds of meters in the air before being absorbed, it was very natural to assume that they were γ rays. According to this idea, their energy loss was due to the Compton effect, i.e., their interaction with the electrons of the atoms in the atmosphere. This hypothesis was further corroborated by theoretical computations that predicted an increase of the mean free path of the γ rays with their energy. The latter was estimated to vary from 20 MeV to several hundreds MeV.

These estimates were at the basis of Millikan's captivating hypothesis on the origin of the cosmic rays, which he formulated after computing their absorption

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[8] V. F. Hess, *Penetrating radiation in seven free ballon flights,* Physikalische Zeitschrift 13 (1912), p. 1084.

[9] V. F. Hess, *Über den Ursprung der durchdringenden Strahlung,* Physikalische Zeitschrift 14 (1913), p. 610.

[10] For a reconstruction of the history of cosmic ray physics see R. Millikan, *Cosmic rays,* Cambridge Univ. Press, Cambridge, UK 1939.

curves.[11] In two papers written with Cameron in 1920,[12] Millikan observed that, although the experimental curves do not follow the theoretical predictions, it is possible to split them into the sum of four curves, each one corresponding to photons with a well-defined energy. The surprising fact is that the energy released in the creation of an atom of helium from four atoms of hydrogen (27 MeV) coincides, up to the experimental error, with that of the first group of photons forming the cosmic rays. Even more surprising is the fact that the energy released by the formation of a nitrogen or oxygen nuclei from hydrogen nuclei (about 100 and 120 MeV, respectively) is to a good approximation the energy of the second group of photons; the energy released by the fusion of 28 hydrogen nuclei into a silicon nucleus is that of the third group of photons; and the fourth group of photons has the energy corresponding to the formation of a nucleus of iron (although in this case the error is bigger).[13]

Nowadays we know that these are mere coincidences, but for Millikan they were the sign of a continuous production of the elements that are, by chance, the most common in the universe. A kind of "birth song" of matter:

> This whole work constitutes, then, very powerful evidence that the sort of creative, or atom-building processes discussed above, are continually going on all about us, possibly also even on the earth, and that each such event is broadcast through the heavens in the form of the appropriate cosmic ray.[14]

The first doubts about the wave nature of the cosmic rays arose little afterwards, with a famous experiment made in 1929 by Walther Bothe and Werner Kolhörster.[15] That was made possible by the invention, again in 1929, of the Geiger-Müller counter.[16] It was a very sensitive instrument, capable of detecting the passage of a single particle. The Bothe-Kolhörster experiment consisted in placing two Geiger-Müller counters one on top of the other, so that they could detect almost simultaneous events. The existence of such coincidences is difficult to explain

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[11] The radiation absorption curves express the strength of the radiation as a function of the thickness of the crossed absorbing material.

[12] R. A. Millikan and G. H. Cameron, *High altitude tests on the geographical, directional, and spectral distribution of cosmic rays,* Physical Review 33 (1928), p. 163; *New precision in cosmic ray measurements; yielding extension of spectrum and indications of bands, ibid.* p. 921.

[13] R. A. Millikan and G. H. Cameron, *Evidence for the continuous creation of the common elements out of positive and negative electrons,* Proceedings of the National Academy of Sciences 14 (1928), p. 445.

[14] *Ibid.,* p. 449. An ample discussion of these hypotheses was published by R. A. Millikan, *Sur les rayons cosmiques,* Annales de l'Institut Henri Poincaré 3 (1933), p. 447.

[15] W. Bothe and W. Kolhörster, *Das Weßen der Höhenstrahlung,* Zeitschrift für Physik 61 (1929), p. 751.

[16] The Geiger-Müller counter is basically a gas discharge tube. It is a metal tube, about one foot long and with a diameter of one or two inches (but the dimensions can vary), containing a conducting wire, disposed longitudinally. Tube and wire are kept at a high potential difference. If a charged particle crosses the tube, due to an ionization process, an electric discharge takes place, which signals the passage of the particle.

within the wave-like model of the cosmic rays, as it would correspond to a double Compton effect, which has very low probability. However, this was not a conclusive proof. It is indeed possible in principle that a photon in the cosmic radiation produces an electron in the atmosphere, and this secondary electron could be responsible of the coincidence in the counters. This possibility however is tested without difficulty. This secondary electron would easily go through the zinc tube of the Geiger counter, but would be absorbed by a thicker metal slab. Indeed Bothe and Kolhörster, to avoid the possibility that the coincidences were produced by secondary Compton electrons, inserted a gold slab, about 1.6 inches thick, between the two counters. The result was surprising; the number of coincidences was only a 20% less. So, the effect was not due to secondary reactions, and there was a strong evidence that cosmic rays were formed by charged particles with a very high energy (Figure 4.3).

It was not a definitive evidence, but it was enough to trigger a lively debate, which attracted interest and attention on the cosmic rays, and stimulated new investigations that would have soon produced unexpected results. This new interest is proved by the "Discussion on ultra-penetrating rays" that took place on 14 May 1931 at

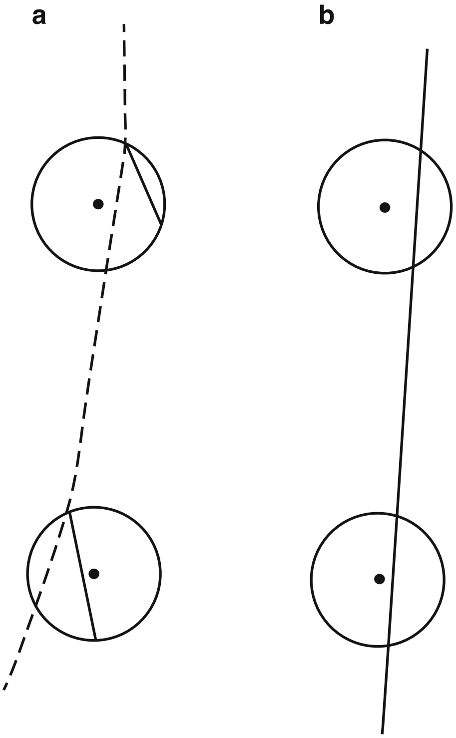

Fig. 4.3 Interpretation of a coincidence according to the wave *(a)* or particle *(b)* hypothesis. In case *(a)* the photon (dashed line), to give rise to a coincidence, must produce two Compton collisions. Since the probability of a single Compton collision is 5%, the probability of a coincidence is very low, about one in every 400 photons that go through both counters. In case *(b)* there is a coincidence for every particle that goes through both counters.

the Royal Society of London and was published in the Society's Proceedings.[17] The most eminent scientists of the time participated in the discussion: Rutherford, Geiger, Wilson, Eddington, and many more. They tried to take stock of the situation about the cosmic rays, but the difficulty in finding a coherent theoretical framework to interpret such enigmatic experimental results allowed for the boldest speculations, as Regener's suggestion that general relativity had to be used to explain the facts. Rutherford reply was quite trenchant:

> We have seen one of the examples of conjecture from Professor E. Regener. The last time I spoke on this subject was at the Volta Congress at Como, where Professor Millikan and others gave an account of experiments on cosmical radiation; and I expressed then, somewhat brusquely, the opinion that what we wanted in reference with cosmic radiation was more work and less talk.[18]

4.4 Cosmic rays from object of investigation to tools for research: toward elementary particle physics

One of the undisputed protagonists of cosmic ray physics was Bruno Rossi, a young Italian scientist who in the late 20s was an assistant professor at the Physics Institute of the University of Florence. His first contribution was the design of an electronic circuit to record the coincidences in the Bothe-Kolhörster experiment.[19] It was a great improvement of the experimental technique and had great resonance. However, Rossi's best results were those on the interaction of cosmic rays with matter. His research started from some results obtained by Dimitri Skobeltzyn, who in 1929 published an ample report on some very energetic negatively charged particles, which he regarded as electrons accelerated by collisions with the cosmic rays, assumed to be photons (Compton effect).[20] Skobeltzyn noted that some of recorded events corresponded to the production of multiple secondary particles. The experimental setup used by Rossi to investigate the interaction of the penetrating radiation with matter is sketched in Figure 4.4. Three Geiger-Müller counters were set at the vertexes of a triangle, and the whole apparatus was shielded by lead slabs. With this disposition, it was not possible that the three counters were excited by a single particle traveling along a straight line; a simultaneous excitation of the three counters could take place only if a photon gave rise to three particle productions.

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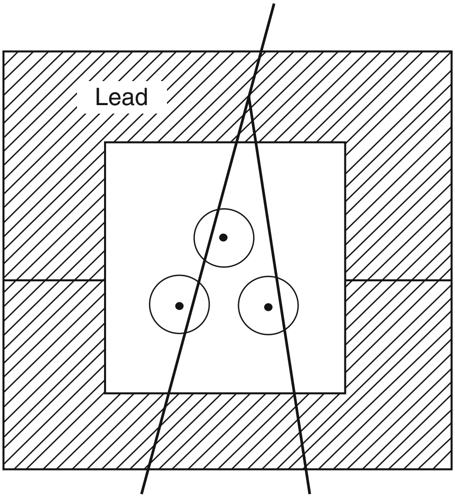
[17] *Discussion on ultra-penetrating rays,* Proceedings of the Royal Society 132 (1931), p. 331.

[18] *Ibid.,* p. 337.

[19] B. Rossi, *Method of registering multiple simultaneous impulses of several Geiger's counters,* Nature 135 (1930), p. 636.

[20] D. Skobeltzyn, *Über eine neue Art sehr schneller β-Strahlen,* Zeitschrift für Physik 54 (1929), p. 686. For a perspective on Skobeltzyn's contribution to the problem of cosmic rays see D. Skobeltzyn, *Early cosmic-ray particle research,* in *The Birth of Particle Physics,* L. M. Brown and L. Hoddeson eds., Cambridge Univ. Press, Cambridge 1983.

Fig. 4.4 Experimental setup used by Rossi to study the interaction of the cosmic rays with matter.



The experiment consisted in measuring the coincidences recorded by the counters with or without the shields. The results were unequivocal; with the shields on the coincidences were abundant, about 35 each hour, without the shields, there were virtually no coincidences.

Such an abundance of secondary rays generated by the cosmic rays during their interaction with the lead slabs was very surprising. As Rossi himself noticed, the result was hard to explain both with the wave or the particle hypothesis. If cosmic rays were photons, the only possible interaction was by Compton effect, and one expected the detection of just one electron; the probability of a multiple Compton effect is negligible. But also if the cosmic rays were particles, the effect was hard to explain; the electrons expelled from the lead atoms should have small energies, not enough to let them escape from the absorbing shield. Bruno Rossi recalled that juncture of his scientific life as follows:

> Nothing of what was known at the time could explain the abundant production of secondary particles revealed by the experiment. Actually, the results of this experiment appeared so incredible to the editors scientific journal to which I had first submitted my paper that they refused to publish it. The paper was later accepted by another journal.[21]

Rossi's results foreshadowed the birth of a new perspective in cosmic ray physics; thus, the penetrating radiation was not only the object of investigation, but became also, and mostly, a tool for research, as a source of new particles whose study allowed for a deeper knowledge of the subnuclear world. The Italian scientist was aware of this fact already in 1931, when, at the first conference on nuclear

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[21] B. Rossi, *Cosmic rays,* McGraw-Hill Book Company, New York 1964, pp. 48–49. The paper was rejected by *Naturwissenschaften* and was eventually published by *Zeitschrift für Physik.*

physics, he hinted that any meaningful research about the cosmic rays should proceed by means of experimental studies of their physical properties, and not with bold speculations about their origin.

> The fundamental problem on the nature and origin of the penetrating radiation, however, is still unsolved. Actually the most recent experiences have highlighted such strange facts that we are led to ask if the penetrating radiation is not by chance something different from the other known radiations; or, perhaps, if passing from the energies of the usual radioactive processes to those of the penetrating radiation, the behavior of photons and corpuscles changes much more radically than we have deemed so far. [. . .] Whatever is the form in which the penetrating radiation reaches the boundary of the atmosphere [. . .] we must take for granted that at the see level the penetrating radiation basically consists of particle radiation. [. . .] A physical study of the penetrating radiation should necessarily start with an investigation of the nature and properties of this particle radiation.[22]

The main events of this new stage of the research about cosmic rays were two. The first was the discovery of the positron, which we discussed in the second chapter; the second, which is a development of Rossi's work, is the discovery of the "swarms" by Blackett and Occhialini. The idea at the basis of their experiment was to couple two Geiger counters, which recorded the coincidences, with a cloud chamber, so that the expansion mechanism of the chamber was triggered by the coincidences recorded in the counters. In that way one could check what really happened when the cosmic ray interacted with matter. The results were stunning. Many photographs revealed a huge number of tracks; a single cosmic ray was capable of producing a swarm of particles.

> A most striking result of the present work has been to reveal the astonishing variety and complexity of these multiple tracks. Already 18 photographs have been obtained on which are the tracks of more than 8 particles of high energy and four photographs show more than 20 tracks. [. . .] A very lengthy investigation will certainly be required before it will be possible to give a complete interpretation of the extraordinarily complex atomic phenomena which are responsible for these groups of tracks.[23]

The long paper by Blackett and Occhialini contains no reference to the nature and origin of the cosmic rays. Their research was centered on the nature and properties of the particles in the swarms. Since that moment, cosmic rays became a fundamental tool for the emerging physics of the elementary particles.

4.5 Themes and problems of nuclear physics

If by "scientific revolution" we mean a short period of time in which a certain research perspective deeply changes, then we can well say that the transition from nuclear protophysics to nuclear physics marked indeed a revolution. In the little time

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[22] B. Rossi, *Il problema della radiazione penetrante,* in *Convegno di Fisica Nucleare,* Accademia d'Italia, Roma 1932, p. 51.

[23] P. M. S. Blackett and G. P. S. Occhialini, *Some photographs of the tracks of penetrating radiation,* Proceedings of the Royal Society 139 (1933), p. 699.

from 1932 to the end of 1933 the meaning of the term "nucleus" changed radically. The discovery of a new particle, the neutron, and the demolition of the principle of pre-existence triggered a series of theoretical considerations, and the same time gave a strong stimulus to the experimental research. In particular, the neutron itself became a powerful tool for the analysis of the nuclear structure.

Within a few years, nuclear physics was enriched by new and important discoveries, and while maintaining its specificity, it gave rise to new research paths that led to the inception of novel research areas.

4.6 Artificial radioactivity and neutron physics

The history of artificial radioactivity started on 19 June 1933 when Irène Curie and Frédéric Joliot published a paper in an important French scientific journal.[24] The paper described a simple experiment where the emission of positrons was obtained by bombarding light nuclei, such as aluminum, with α particles. The effect was very weak; for every two million incident α particles, the cloud chamber revealed the emission of one positron. The most surprising effect, however, was communicated on 15 January 1934 in another paper:

> The emission of positive electrons by certain light elements irradiated with α particles emitted by polonium continues for some time — more than half an hour for boron — after the α ray source has been removed.[25]

Curie and Joliot obtained similar results also with aluminum instead of boron.

It was a very important discovery, not only because of the artificial creation of radioactivity, a phenomenon still unknown at the time, but also because of the information about the nuclear structure it carried. According to the French scientists, what happened was the result of two successive nuclear reactions. In the first, the action of the α particles on aluminum produced an unstable isotope of phosphor, with the emission of a neutron:

$$^{27}_{13}\mathrm{Al} + ^{4}_{2}\mathrm{He} = ^{30}_{15}\mathrm{P} + ^{1}_{0}\mathrm{n};$$

in the second reaction the unstable phosphor isotope decayed, with a mean lifetime of 3 minutes and 15 seconds, emitting a positron:

$$^{30}_{15}\mathrm{P} = ^{30}_{14}\mathrm{Si} + e^{+}.$$

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[24] I. Curie and F. Joliot, *Électrons positifs de transmutations,* Comptes Rendus de l'Académie des Sciences 196 (1933), p. 1885.

[25] I. Curie and F. Joliot, *Un nouveau type de radioactivité,* Comptes Rendus de l'Académie des Sciences 198 (1934), p. 254

Authors	**Place**	**Date**	**Particles used**
Cockcroft, Gilbert and Wallace	Cambridge	24 February	protons
Crane, Lauritsen and Harper	Caltech	24 February	deuterons
Henderson, Livingston and Lawrence	Berkeley	24 February	deuterons
Crane and Lauritsen	Caltech	1 March	deuterons
Crane and Lauritsen	Caltech	14 March	protons
Neddermeyer and Anderson	Caltech	15 March	protons and deuterons

Fig. 4.5 Main results about radioactive processes obtained in 1934.

In their final remarks the authors advanced the hypothesis that the same effects could be obtained using other particles than the α particles as projectiles. The scientific community took that suggestion very seriously, and in the same year 1934 several researches were published, reporting on experiments where the first accelerating machines were used to obtain beams of protons and deuterons with sufficient energy to trigger radioactive processes in different materials. The table in Figure 4.5 summarizes the results of that year.[26]

Curie and Joliot's discovery was also at the root of the research program started off by Fermi and his group. The aim was to check if neutrons, which are neutral particles and therefore, differently from the α particles, are not subject to the repulsive force of the nucleus, could more easily trigger artificial radioactivity phenomena. The first positive result was published by Fermi in a paper dated 25 February 1934.[27] It was about the effects of neutron bombing on two elements, fluorine and aluminum. It was the starting point of a program whose aim was the identification of all elements that could be activated by neutron bombing. The purpose was not to classify the elements, but rather, to study the nucleus when

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[26] The papers reporting on the experiments summarized in the table are: J. D. Cockcroft, C. W. Gilbert and E. T. S. Walton, *Production of induced radioactivity by protons,* Nature 133 (1934), p. 328; H. R. Crane, Ch. C. Lauritsen and W. W. Harper, *Artificial production of radioactive substances,* Science 79 (1934), p. 234; M. C. Henderson, M. S. Livingston and E. O. Lawrence, *Artificial radioactivity produced by deuton bombardment,* Physical Review 45 (1934), p. 428; H. R. Crane and C. C. Lauritsen, *Radioactivity from carbon and boron oxide bombarded with deuterons and the conversion of positrons into radiation,* Physical Review 45 (1934), p. 430; *Further experiments with artificially produced radioactive substances, ibid.* p. 497; S. H. Neddermeyer and C. D. Anderson, *Energy spectra of positrons ejected by artificially stimulated radioactive substances, ibid.* p. 498.

[27] Fermi [84a].

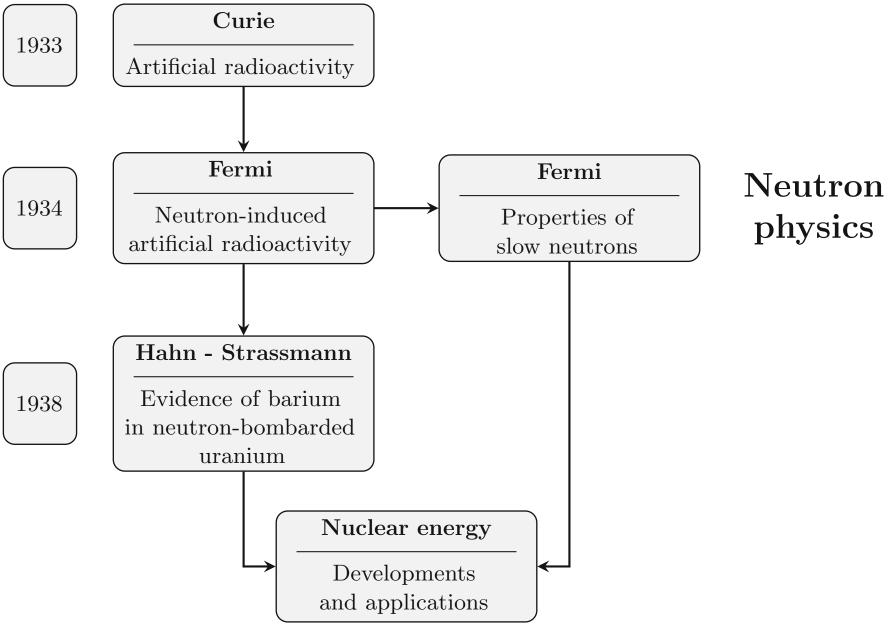

Fig. 4.6 The global maps between 1933 and 1938, after the discovery of artificial radioactivity. "Neutron physics" was a new research area, incepted by Fermi's work on neutron-induced artificial radioactivity, especially that on slow neutrons.

it undergoes a modification. In the next chapter we shall go into the details of these processes and will discuss issues such as the controversy about the supposed discovery of transuranic elements.

The most important experimental result was obtained on 22 October 1934. It was an unexpected increase of the activity of silver under the action of neutrons, when a paraffin slab was inserted between the source and the target. This was a fundamental event in the dynamics of the global maps, which was instrumental in defining a new research perspective in neutron physics. Figure 4.6 represents this itinerary in a very schematic way. A detailed analysis will be given in the next chapter, since in this case the development of the global maps coincided with Fermi's research itinerary.

4.7 Nuclear fission

> In the past one has found that transmutations of nuclei only take place with the emission of electrons, protons, or helium nuclei, so that the heavy elements change their mass only a small amount to produce near neighboring elements. When heavy nuclei are bombarded by neutrons, it is conceivable that the nucleus breaks up into several large fragments, which

> would of course be isotopes of known elements but would not be neighbors of the irradiated element.[28]

The first hint at nuclear fission which was founded on some experimental evidence was this conjecture of Ida Noddack, which remained unheard. It was advanced as a reply to a bold but reasonable hypothesis by Fermi; bombarding the last known element, uranium, which has atomic number 92, with neutrons, at least one new element was produced, having an atomic number of 93, and for this reason called *transuranic*. The hypothesis followed an experiment[29] that was part of the above mentioned program started by the Roman group, aiming at systematic investigation of the behavior of all elements in the periodic table under neutron bombing. In the case of uranium, the experiment showed two different kinds of β decay:

> [...] the decay curves of the β activity obtained from uranium [...] bombarded with neutrons for a time variable from a few seconds to a few hours can be analyzed in exponentials with the following periods: 10 seconds, 40 seconds, 13 minutes, and at least two longer ones.[30]

The existence of a new decay type, with its particular halving time, is in radiochemistry the signal of the existence of a new element. The next step then was a chemical analysis. “For practical reasons,” the Roman group decided to concentrate on the study of the elements whose activity had a halving time of 13 minutes. The observed chemical processes led to the exclusion of isotopes of uranium, protactinium, thorium, actinium, radon, bismuth, and lead. Together with the observation that, till then, the product of a neutron bombing was an isotope of the original element, or a nucleus which differed from the original one by one or two units of charge, this led the Roman group to believe they had discovered a transuranic element, with an atomic number greater than 92. That hypothesis, with the only exceptions of Aristid von Grosse (who believed that in Rome an isotope of actinium, whose atomic number is 91, had been discovered)[31] and Ida Noddack, as mentioned above, was shared by the most eminent nuclear chemists of the time.

This episode highlights the basic principle of this global map. The scientists of the time were prone to apply also to artificial radioactivity the “law of radioactive displacement”; the element produced by a radioactive transformation was situated in the periodic table at one place to the right, or two places to the left from the original

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[28] I. Noddack, *Über das Element 93,* Zeitschrift für Angewandte Chemie 47 (1934), p. 653. English translation in H. G. Graetzer and D. L. Anderson, *The Discovery of Nuclear Fission,* Van Nostrand-Reinhold, New York 1971. This book contains a detailed analysis of the discovery of nuclear fission, with original papers translated into English.

[29] Fermi [86a].

[30] Fermi [94], *CPF I,* p. 704.

[31] A. von Grosse and M. Agruss, *The chemistry of element 93 and Fermi’s discovery,* Physical Review 46 (1934), p. 241.

element. In the first case the transformation was a β decay, which increased the nuclear charge by one unit; in the second case, it was an α decay, which decreased the nuclear charge by two units. Amaldi's report of activities of the Roman group was very clear:

> We had learned that: (1) the neutrons of our sources produced (n,α), (n,p) and (n,β) reactions; (2) processes of the latter types had been observed for any value of Z; (3) the atomic number of the product of radiative capture by an isotope of an element of atomic number Z, has atomic number $Z + 1$ [...] Therefore the simplest interpretation of the radioactive body of 13 minute half-life was to attribute it to a nuclide of atomic number $Z = 93$, generated in the beta decay of a radioactive nuclide produced by radiative capture in uranium.[32]

On the one hand, the lack of suitable theoretical frameworks, capable of describing more and more complex and abundant experimental facts, led to hypotheses that were not always fortunate; on the other hand, the importance of the questions increased the strain in the laboratories working on these problems. As aptly stressed by Esther B. Sparberg, the discovery of nuclear fission was not the outcome of a lucky sequence of random events, but rather "[...] represented the climax of a crescendo of activity in nuclear science during the thirties."[33] Indeed, the discovery of the Roman group was for the most prestigious laboratories in Europe a stimulus for further investigation. Lisa Meitner, one of the most authoritative protagonists of those events, thus remembered her reaction to the announcement of Fermi's results:

> I found those experiments so fascinating that, as soon as *Nuovo Cimento* and *Nature* published their reports on them, I convinced O. Hahn to resume our direct collaboration, which we had interrupted a few years earlier, to study those problems. That is why in 1934, after an interval of 12 years, we started working together again, after a while also with Fritz Strassmann's precious collaboration.[34]

As reported by Otto Hahn,[35] the investigations took about 4 years, and gave rise to around 20 scientific papers. The first result was the solution of the Grosse-Fermi controversy; Meitner and Hahn, who discovered protactinium, had a better knowledge of its chemical properties, and their laboratory was better suited to check if the element with a radioactive halving time of 13 minutes discovered by Fermi was indeed an isotope of protactinium. The result was negative "beyond any doubt," and Fermi's hypothesis on the transuranic nature of the element, in their opinion, was not only confirmed, but was also the only possible explanation of the results of the experiment.

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[32] E. Amaldi, *From the discovery of the neutron to the discovery of nuclear fission,* Physics Reports 11 (1984), p. 274.

[33] E. B. Sparberg, *A study of the discovery of fission,* American Journal of Physics 32 (1964), p. 2.

[34] L. Meitner, *Vie giuste e sbagliate nel cammino verso la scoperta dell'energia nucleare* [Right and wrong paths in the discovery of nuclear energy], in *Enrico Fermi, significato di una scoperta* [Enrico Fermi, the meaning of a discovery], AIN-ENEA, Roma 2001, p. 43.

[35] O. Hahn, *Vom Radiothor zur Uranspaltung,* Vieweg, Braunschweig 1962.

After three years of work the two German scientists, analyzing the products of the bombing of uranium by neutrons, determined three new series of radioactive transformations, including new hypothetical transuranic elements:

$$\mathrm{U} + n \rightarrow {}^{10\ sec}_{\ \ \ 92}\mathrm{U} \xrightarrow{\beta} {}^{22\ min}_{\ \ \ 93}\mathrm{EkaRe} \xrightarrow{\beta} {}^{59\ min}_{\ \ \ 94}\mathrm{EkaOs}$$

$$\xrightarrow{\beta} {}^{66\ hrs}_{\ \ \ 95}\mathrm{EkaIr} \xrightarrow{\beta} {}^{22.5\ min}_{\ \ \ \ 96}\mathrm{EkaPt} \xrightarrow{\beta} {}_{92}\mathrm{EkaAu?}$$

$$\mathrm{U} + n \rightarrow {}^{40\ sec}_{\ \ \ 92}\mathrm{U} \xrightarrow{\beta} {}^{16\ min}_{\ \ \ 92}\mathrm{EkaRe} \xrightarrow{\beta} {}^{57\ hrs}_{\ \ \ 94}\mathrm{EkaOs} \xrightarrow{\beta} {}_{95}\mathrm{EkaIr?}$$

$$\mathrm{U} + n \rightarrow {}^{23\ min}_{\ \ \ 92}\mathrm{U} \xrightarrow{\beta} {}_{93}\mathrm{EkaRe?}$$

Nowadays, we know that transuranic elements such as plutonium or americium do exist, but we also know that they have nothing to do with the elements involved in these reactions. What prevented the scientists from giving those processes a correct interpretation? Otto Hahn's words are very interesting in this connection.

> How could we be wrong? Fermi's studies on the elements in the periodic system had shown the scheme of the transformations triggered by the capture of a neutron. The isotope of the irradiated elements, augmented by a neutron, emits β rays, and the positive charge of the new element increases by one unit; this reaction, as Fermi had so beautifully shown, took place especially with slow neutrons, namely, neutrons whose initial energy decreased by the interaction with hydrogen, in substances such as water, paraffin, etc. We too made experiments with uranium, at first with slow neutrons, and the phenomenon was clear; the β decay gave rise to the next element in the periodic system. After the discovery of artificial radioactivity, a number of other nuclear processes was discovered, also using fast neutrons or α particles (helium nuclei). However, the reaction always produced elements that were isotopes of the initial element, or had a nuclear charge differing by one or two units from the original one. For the chemical identification of the products one had to look only in the immediate proximity of the irradiated element. The production of fragments whose mass and nuclear charge were bigger than those of the incident particle or helium nucleus seemed to be "out of the question." This was the situation of nuclear physics till the end of 1938.[36]

The second fundamental contribution to the discovery of nuclear fission came from another great European laboratory. In 1937 Irène Curie and Paul Savitch in Paris announced they had determined in the products of uranium irradiated with neutrons a new element, with a β activity of 3 hours and a half, with chemical properties similar to lanthanum (the first element in the rare earth series).[37] The element was denoted by the two scientists with the symbol $\mathrm{R}_{3,5h}$. The fact that this element had chemical properties analogous to those of lanthanum induced the two scientists to believe that the new element could be an isotope of actinium

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[36] *Ibid.,* p. 117.

[37] I. Curie and P. Savitch, *Sur le radioéléments formé dans l'uranium irradié par les neutrons,* Journal de la Physique et le Radium 8 (1937), p. 385.

(atomic number 89). This indeed is the only element near uranium which belongs to the same group as lanthanum (compare with the period table of elements in Figure 4.7). As we already mentioned, the regulating paradigm was that by bombarding uranium one could only produce elements with an atomic number close to that of uranium. One, therefore, was expecting that after preparing a solution made by lanthanum, actinium, and $R_{3,5h}$, and after the subsequent separation of lanthanum from actinium (made with a fractional crystallization method devised by Marie Curie a few years before), one should find the element $R_{3,5h}$ with actinium. But the result of the separation process was surprising. The element $R_{3,5h}$ behaved exactly in the opposite way; it separated with lanthanum rather than with actinium, that is, it behaved as an isotope of lanthanum.

Curie and Savitch, and the researchers in many important laboratories throughout Europe, were astonished. However, the theoretical tools at their disposal did not yet allow them to read in this result a clear indication of the fission of uranium. An important paper written in 1938 ended with the words

> Therefore it seems that this body cannot but be a transuranic element with properties very different from those of the other known transuranic elements. However, this hypothesis raises interpretation problems.[38]

The discovery of the element $R_{3,5h}$ and the related problems of interpretation were the object of a 1938 paper by Hahn and Strassmann.[39] The results obtained by the two German scientists not only confirmed the existence of the element $R_{3,5h}$, but also determined the existence of new radioactive isotopes that had the chemical properties of radium, and that by β decay gave origin first to actinium, and then thorium:

$$_{88}\mathrm{Ra} \overset{\beta}{\rightarrow} {_{89}\mathrm{Ac}} \overset{\beta}{\rightarrow} {_{90}\mathrm{Th}}.$$

If we look at Figure 4.7, we see that to get radium (atomic number 88) from uranium (atomic number 92) two α particles must be emitted, as the emission of an α particle decreases the atomic number by two units. Then, how can radium be obtained by irradiating uranium, if the only observed radiations are β particles, and not α? But this was not the only problem Hahn and Strassmann had to cope with. To separate radium one used barium, as the two elements have similar chemical

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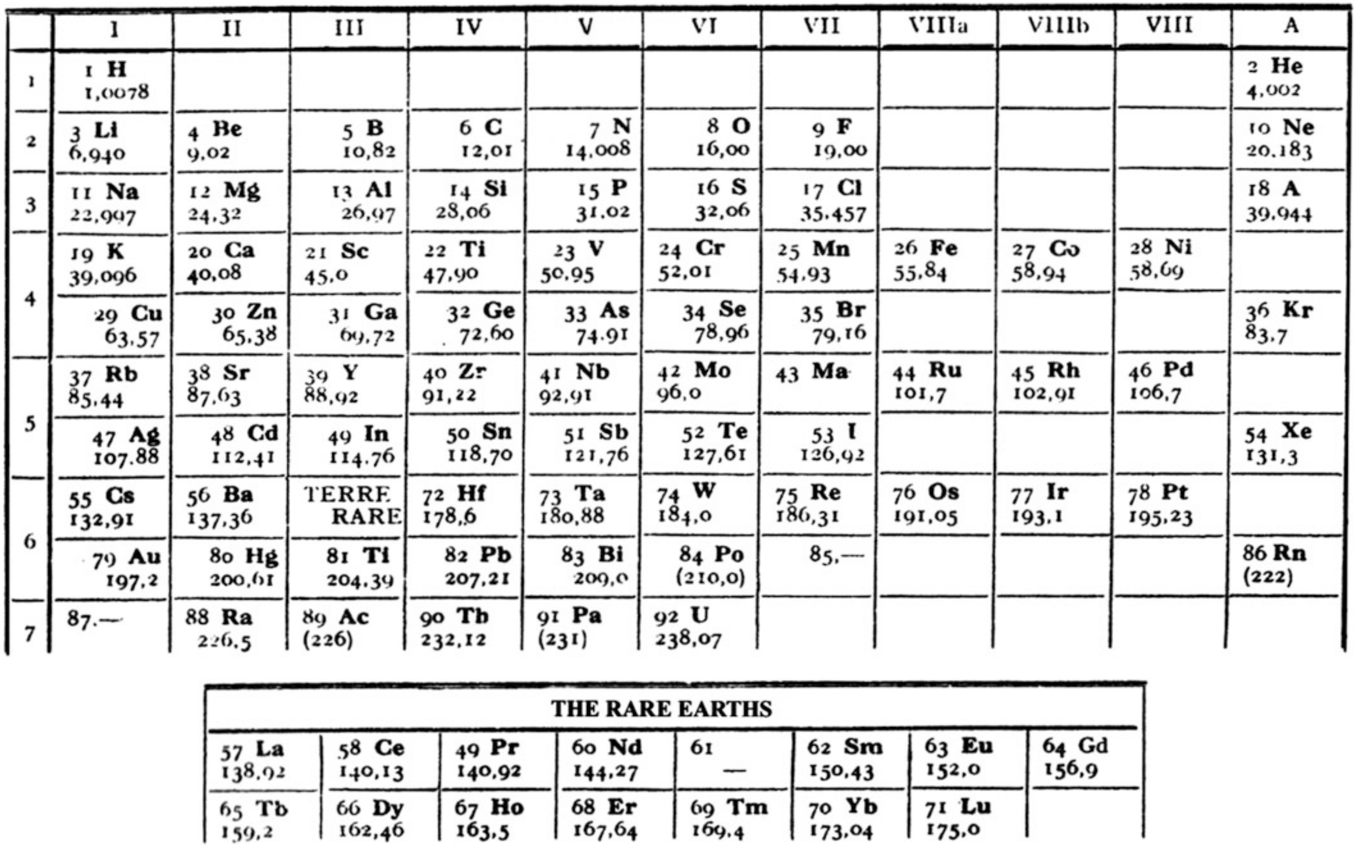
[38] I. Curie and P. Savitch, *Sur la nature du radioélément de période 3,5 heures formé dans l'uranium irradié par les neutrons,* Comptes Rendus de l'Académie des Sciences 206 (1938), p. 1645 . English translation *Concerning the nature of the radioactive element with 3.5 hour half-life, formed from uranium irradiated by neutrons,* in H. G. Graetzer and D. L. Anderson, *The Discovery of Nuclear Fission, op. cit.* p. 37.

[39] O. Hahn and F. Strassmann, *Über die Entstehung von Radiumisotopen aus Uran durch Bestrahlen mit schnellen und verlangsamten Neutronen,* Naturwissenschaften 26 (1938), p. 755. English translation *Concerning the creation in radium isotopes from uranium by irradiation with fast neutrons,* in H. G. Graetzer and D. L. Anderson, *The Discovery of Nuclear Fission, op. cit.* p. 41.



	I	II	III	IV	V	VI	VII	VIIIa	VIIIb	VIII	A
1	1 **H** 1,0078										2 **He** 4,002
2	3 **Li** 6,940	4 **Be** 9,02	5 **B** 10,82	6 **C** 12,01	7 **N** 14,008	8 **O** 16,00	9 **F** 19,00				10 **Ne** 20,183
3	11 **Na** 22,997	12 **Mg** 24,32	13 **Al** 26,97	14 **Si** 28,06	15 **P** 31,02	16 **S** 32,06	17 **Cl** 35,457				18 **A** 39,944
4	19 **K** 39,096	20 **Ca** 40,08	21 **Sc** 45,0	22 **Ti** 47,90	23 **V** 50,95	24 **Cr** 52,01	25 **Mn** 54,93	26 **Fe** 55,84	27 **Co** 58,94	28 **Ni** 58,69	
	29 **Cu** 63,57	30 **Zn** 65,38	31 **Ga** 69,72	32 **Ge** 72,60	33 **As** 74,91	34 **Se** 78,96	35 **Br** 79,16				36 **Kr** 83,7
5	37 **Rb** 85,44	38 **Sr** 87,63	39 **Y** 88,92	40 **Zr** 91,22	41 **Nb** 92,91	42 **Mo** 96,0	43 **Ma**	44 **Ru** 101,7	45 **Rh** 102,91	46 **Pd** 106,7	
	47 **Ag** 107,88	48 **Cd** 112,41	49 **In** 114,76	50 **Sn** 118,70	51 **Sb** 121,76	52 **Te** 127,61	53 **I** 126,92				54 **Xe** 131,3
6	55 **Cs** 132,91	56 **Ba** 137,36	TERRE RARE	72 **Hf** 178,6	73 **Ta** 180,88	74 **W** 184,0	75 **Re** 186,31	76 **Os** 191,05	77 **Ir** 193,1	78 **Pt** 195,23	
	79 **Au** 197,2	80 **Hg** 200,61	81 **Tl** 204,39	82 **Pb** 207,21	83 **Bi** 209,0	84 **Po** (210,0)	85,—				86 **Rn** (222)
7	87,—	88 **Ra** 226,5	89 **Ac** (226)	90 **Th** 232,12	91 **Pa** (231)	92 **U** 238,07					

THE RARE EARTHS

57 **La** 138,92	58 **Ce** 140,13	49 **Pr** 140,92	60 **Nd** 144,27	61 —	62 **Sm** 150,43	63 **Eu** 152,0	64 Gd 156,9
65 **Tb** 159,2	66 **Dy** 162,46	67 **Ho** 163,5	68 **Er** 167,64	69 **Tm** 169,4	70 **Yb** 173,04	71 **Lu** 175,0	

Fig. 4.7 The periodic table of elements as it appeared in the treatises in the late 30s.

properties. But all attempts to make a fractional crystallization failed. At this point, the two scientists decided to perform an experiment which turned out to be crucial.

The report on the experiment was published in January 1939. It contained an emblematic sentence: "Now we still have to discuss some newer experiments which we publish rather hesitantly due to their peculiar results."[40] It was a fully justified hesitation; their results showed indeed that the radioactive elements they discovered were not isotopes of radium, but rather of barium (atomic number 56). Therefore, uranium irradiated by neutrons gave rise to a new kind of nuclear reaction: not the usual emission of α or β particles, but the fission of the nucleus into two lighter isotopes, having roughly the same mass. Hahn and Strassmann's experiment was an *experimentum crucis;* if the isotope produced by the irradiation of uranium with neutrons was not radium but barium, then the subsequent product of the β decay was not actinium but lanthanum (cf. Figure 4.7). The experiment was concluded on 22 December 1938, and its result left no doubt about the presence of lanthanum; thus also Curie and Savitch's experimental results, so difficult to interpret, found an explanation. The last sentences in Hahn and Strassmann's paper have remained famous in the history of physics:

> As chemists we really ought to revise the decay scheme given above and insert the symbols Ba, La, Ce, in place of Ra, Ac, Th. However, as "nuclear chemists," working very close to the field of physics, we cannot bring ourselves yet to take such a drastic step which goes against all previous experience in nuclear physics. There could perhaps be a series of unusual coincidences that has given us false indications.[41]

These words were almost a surrender in front of the overwhelming experimental evidence of the fission of the atomic nucleus. But to fully accept such an event one needed a theoretical framework able to explain why, for a nucleus of a heavy element such as uranium, the capture of a neutron is capable to determine not the expulsion of some particles, but rather an extraordinary event such as the fission of the nucleus in two big fragments. It is as if by adding a drop of water to a full glass, one observed not some liquid spilling from the brim, but the violent exit of half the content. As we shall shortly see, the theoretical framework was to be provided by Bohr, and the correct explanation of the fission by Frisch and Meitner.

4.8 Guidelines: models and nuclear forces

Ugo Amaldi rightly remarked:

> Since the 30s, the experimental and theoretical study of the nuclei has followed two complementary and parallel approaches: the construction of models (which provide explanations of the energy levels, the probabilities of transitions between different levels, and the nuclear reactions), and the justifications of these models in terms of the forces between nucleons.

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[40] *Ibid.,* English translation p. 46.

[41] *Ibid.,* English translation p. 47.

> From the beginning there has been hope to be able to obtain the properties of the nuclei from the knowledge of the interactions among their components, but to date this goal has never been reached (with the exclusion of some light nuclei), and somebody thinks it will be never reached.[42]

In the development of the nuclear models in the 30s and 40s, a situation prevailed that, to some extent, repeated what we saw in chapter 2 about nuclear protophysics: a proliferation of models, each able to account for a specific phenomena. As Robley D. Evans wrote in a celebrated treatise written in the 50s:

> The complex interrelationships between nucleons when they aggregate to form medium and heavy nuclei will continue to defy precise analysis for a long time to come. In the absence of an exact theory, a number of nuclear models have been developed. These utilize different sets of simplifying assumptions. Each model is capable of explaining only a portion of our experimental knowledge about nuclei.[43]

The two models that were mainly used during those 20 years were the *compound* or *drop model* and the *shell model.* They are antithetical; in the drop model the nucleons cannot be regarded as separate entities, and are subject to strong forces which determine collective motions, exactly as in a liquid drop; in the shell model the viewpoint is opposite, and the nucleons are considered as moving independently. Let us review the main steps of the historical process that led to that situation.

Between 1936 and 1937 Hans Bethe, Robert F. Bacher, and M. Stanley Livingston published three long papers, for a total of 450 pages, which were the first treatise on nuclear physics.[44] The leading force in the group was Bethe; the trilogy, which resumed, in a detailed and complete way, the problems and the acquired knowledge in the topic, became known in the scientific community as "Bethe's bible." Section 51, the first of the 41 in the second paper, was devoted to Bohr's theory of the atomic nucleus. The author stressed how the Danish scientist was the first to think that the nuclear processes should be regarded as many-body problems. Bohr, indeed, had just published a paper, which did not contain a single equation, which marked a fundamental step in the history of nuclear physics. The paper was devoted to the formulation of a nuclear model known as "compound nucleus." The reasons which compelled Bohr to that hypothesis included the observation of the width of the spectral lines related to the emission of γ radiation by the radioactive nuclei after capturing a neutron. Analyzing the width of those lines, it was indeed possible to estimate the duration of the interaction between the neutron and the

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[42] U. Amaldi, *La fisica dei nuclei dagli anni trenta ai giorni nostri,* in *Conoscere Fermi,* C. Bernardini and L. Bonolis eds., Compositori, Bologna 2001, p. 152.

[43] R. D. Evans, *The Atomic Nucleus,* McGraw-Hill, New York 1955, p. 357.

[44] H. A. Bethe and R. F. Bacher, *Nuclear physics, A. Stationary states of nuclei,* Review of Modern Physics 8 (1936), p. 83; H. Bethe, *Nuclear physics, B. Nuclear dynamics, theoretical, ibid.,* p. 71; M. Stanley Livingston and H. Bethe, *Nuclear physics, C. Nuclear dynamics, experimental, ibid.* p. 246.

nucleus. The result was much bigger than the time taken by the neutron to cross a space of the dimensions of the nucleus. As Bohr wrote,

> The phenomena of neutron capture thus force us to assume that a collision between a high-speed neutron and a heavy nucleus will in the first place result in the formation of a compound system of remarkable stability. The possible later breaking up of this intermediate system by the ejection of a material particle, or its passing with emission of radiation to a final stable state, must in fact be considered as separate competing processes which have no immediate connexion with the first stage of the encounter.[45]

Differently to what happens in an atomic collision, where the scattering of the incoming particle by the atom is almost always elastic, in a nuclear collision the situation is the opposite; the particle hitting the nucleus cannot cross it without interacting with the nuclear constituents, exactly because the average distance between the latter and the particle is of the same order of magnitude of the radius of action of the nuclear forces. Thus the energy of the incident particle is distributed among all constituents of the nucleus. The energy of the latter increases, but not in an amount sufficient to expel them from the nucleus. The emission takes place randomly; after a relatively long time, the energy of the various nuclear particles may casually concentrate on one of them, determining its expulsion. Due to this redistribution, the expelled particle is not, in general, the incident one; and the energy is distributed between the expelled particle and the nucleus, which may remain in an excited state, and subsequently give rise to a γ decay. As stressed by Bethe, only if the incident and the expelled particles are of the same type, and the nucleus remains exactly in the same conditions, one can talk of an elastic collision. And Bethe concludes

> It is obvious that an elastic collision is only a very special case and must therefore be quite a rare event compared to the many kinds of possible inelastic collisions.[46]

Thus the model describes a *compound* nucleus, which, before disintegrating, survives for a relatively long time in comparison with the time the incident particle would take to cross it. One could say that that time is long enough to allow the energy redistribution to erase the “memory” of its initial state, and to give rise to a process which is completely uncorrelated from the initial collision. According to the scheme proposed by Bethe, the succession of events is as follows:

$$\begin{aligned} \text{initial nucleus} + \text{incident particle} &\rightarrow \text{compound nucleus} \\ &\rightarrow \text{final nucleus} + \text{emitted particle}. \end{aligned}$$

It is interesting, to understand how far the researchers in 1936 were from the idea of fission, in spite of Ida Noddack’s suggestion and Curie and Savitch’s controversial measurement, to read the final remarks in Bohr’s paper:

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[45] N. Bohr, *Neutron capture and nuclear constitution,* Nature 137 (1935), p. 344.

[46] H. Bethe, *Nuclear physics, B. op. cit.*, p. 72.

> [. . .] with neutrons or protons of energies of more than a hundred million volts, we should still expect that the excess energy of such particles, when they penetrate into a nucleus of not too small mass, would in the first place be divided among the nuclear particles with the result that a liberation of any of these would necessitate a subsequent energy concentration. Instead of the ordinary course of nuclear reactions we may, however, in such cases expect that in general not one but several charged or uncharged particles will eventually leave the nucleus as a result of the encounter. For still more violent impacts, with particles of energies of about a thousand million volts, we must even be prepared for the collision to lead to an explosion of the whole nucleus. Not only are such energies, of course, at present far beyond the reach of experiments, but it does not need to be stressed that such effects would scarcely bring us any nearer to the solution of the much discussed problem of releasing the nuclear energy for practical purposes. Indeed, the more our knowledge of nuclear reactions advances the remoter this goal seems to become. [47]

Bohr's model was the theoretical tool which allowed Lisa Meitner and his nephew Otto Frisch to correctly interpret Hahn and Strassmann's stunning results on the presence of barium among the products of the nuclear reactions undergone by uranium. The explanation was contained in two letters sent to the journal *Nature* on 16 January 1939.[48] The main point was, according to Bohr, to consider the uranium nucleus as a system which, due to its tight packing, has a collective motion, like that of a liquid drop. It is a system governed by two contrasting forces: an attractive one, like the superficial tension in a liquid drop, and a repulsive one, due to the electrostatic interaction among the protons. Both forces increase with the nuclear dimensions, but the repulsive Coulomb force increases more rapidly than the attractive nuclear force, and a computation shows that for a value of the nuclear charge around 100 the system becomes unstable. Thus, as a small perturbation is enough to split an unstable drop into two smaller droplets, in the same way in a nucleus the redistribution of the energy of the incident neutron can trigger deformations that lead to the formation of two smaller nuclei.

An estimate of the energy released during the fission of the uranium nucleus, made by Meitner and Frisch, gave a value of about 200 MeV. It was not a complicated computation. The binding energy per nucleon of the uranium nucleus is smaller than that of the two nuclei in which it splits; this means that the total mass of the two fragments is smaller than that of the original nucleus, and it is this difference that, converted into energy via the equation $E = mc^2$, yields the value 200 MeV. This does not seem to be a big energy, but one should think that it is released by a single nucleus, while the energy released by an atom in a chemical reaction does not even reach the value of 10 MeV.

On 13 January 1939 Frisch, in his laboratory in Copenhagen, obtained a confirmation of his hypothesis; the kinetic energy of the fragments in which the uranium nucleus splits was about 200 MeV. There was a biologist named William A. Arnold working in the same institute, and Frisch asked him the name of the

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[47] N. Bohr, *Neutron capture, op. cit.*, p. 348.

[48] L. Meitner and O. R. Frisch, *Disintegration of Uranium by neutrons: a new type of nuclear reaction,* Nature 143 (1939), p. 239; O. R. Frisch, *Physical evidence for the division of heavy nuclei under neutron bombardment, ibid.,* p. 276.

process in which bacteria split into two. “Binary fission,” answered Arnold. In this way, the term “nuclear fission” entered the history of physics. The explanation of the nuclear fission was definitely the biggest success of the compound nuclear model. Bethe reported that all computations in Los Alamos were done using that model.[49] But some features of the nucleus still resisted the explanation. For instance, the compound model could not explain the stability of some nuclei, for which the binding energy per nucleon is bigger. The nuclei having this property are those with a number of protons of 2, 8, 20, 28, 50, and 82, or those with a number of neutrons of 2, 8, 20, 28, 50, 82, and 126. The existence of *magic numbers,* according to Wigner’s terminology, leading to configurations of enhanced stability, recalls the case of the atom, for which there exist electronic configurations, corresponding to the noble gases, where the strata making up the electronic structure of the atom are completely filled up by a number of electrons given by 2, 10, 18, 36, 54, and 86. In the case of the atom the mechanism that oversees that filling of the energy levels is Pauli’s principle. The analogy gave the hope that the existence of the magic number could lead to the discovery of some regulating principle.

There is, however, a fundamental difference between the atomic and the nuclear cases; in the atom the electrons move in the field generated by the nucleus, while protons and neutrons do not move in an external field. The problem was systematically tackled by Maria Goeppert-Mayer, who in 1947 proposed the shell model of the atomic nucleus.[50] The basic idea is that the nucleons generate a mean field, a kind of potential well, in which every nucleon moves as an independent particle. However, the model did not work too well, in the sense that it did not reproduce the magic number. As Goeppert-Mayer remembered,

> It was kind of a jigsaw puzzle. One had many of the pieces (not only the magic number), so that one saw a picture emerging. One felt that if one had just one more piece everything would fit. The piece was found, and everything cleared up.[51]

The discovery of the missing piece is told by Goeppert-Mayer herself:

> At that time Enrico Fermi had become interested in the magic numbers. I had the great privilege of working with him, not only at the beginning, but also later. One day as Fermi was leaving my office he asked: “Is there any indication of spin-orbit coupling?” Only if one had lived with the data as long as I could one immediately answer: “Yes, of course and that will explain everything.”[52]

In other words, Fermi told Goeppert-Mayer that the energy of a single nucleon depends also on the orientation of its spin with respect to the total angular moment. This interaction is present also in the atom, but it is very weak, while in the atomic

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[49] H. A. Bethe, *The happy Thirties,* in *Nuclear Physics in Retrospect,* R. H. Stuewer ed., *op. cit.*

[50] M. Goeppert-Mayer, *On closed shells in nuclei,* Physical Review 54 (1948), p. 235; *On closed shells in nuclei II,* Physical Review 55 (1949), p. 1969.

[51] M. Goeppert-Mayer, *Nobel Lecture,* 12 December 1963, http://nobelprize.org/nobel_prizes/physics/laureates/1963/mayer-lecture.pdf.

[52] *Ibid.*

nucleus it is very strong. The process of “demagification” of the magic numbers, as was called by Hans D. Jensen, who got the Nobel prize with Goeppert-Mayer, had been terminated.[53] Concerning the interaction among nucleons, the most important result in the period of time we are considering was definitely the discovery of the *independence of the nuclear forces from charge,* that is, the fact that the nuclear interaction between two protons, or two neutrons, or a proton and a neutron are identical. The independence of nuclear forces from charge is a more restrictive condition than the *charge symmetry,* namely, the fact, already known in the middle 30s, that the force between two protons is the same as the force between two neutrons. To understand this it is enough to consider some empirical facts. For instance, the nucleus of many of the light stable elements of the periodic table is formed by the same number of protons and neutrons. Oxygen has 8 protons and 8 neutrons, nitrogen 7 protons and 7 neutrons; in no known light element there is a big difference between the two nuclear constituents. For most light stable nuclei, the difference $N - Z$ between the number of neutrons and protons does not exceed 10% of the atomic number Z. As Bethe noticed in 1937,[54] this leads to the conclusion that the main forces determining the nuclear dynamics are the proton-neutron interaction, while the proton-proton and neutron-neutron interactions, if they exist at all, are of the same magnitude, and in both cases weaker than the proton-neutron interaction. Otherwise, Bethe remarked, if two neutrons attracted each other with more strength than a proton and a neutron, most stable nuclei would be made only by neutrons. The first information about the proton-proton interaction came from the experimental work of Tuve, Heydenburg, and Hafstad[55] on the scattering between protons. The most striking fact was the presence of anomalies with respect to the predictions of the theory based only on the Coulomb interaction between the particles. The anomalies were quite marked, and increased with the scattering angle. Additional tests performed by the three scientists to rule out possible contaminations left no doubt, and led to the conclusion that the observed anomalies were due to the short-range proton-proton interaction (at less than 5×10^{-13} cm), which showed a marked deviation from the usual Coulomb interaction. As the authors wrote:

> [. . .] these proton-scattering experiments demonstrate the existence of a proton-proton interaction which is violently different from the Coulomb repulsion for distances of separation of the order of 10^{-13} cm. The measurements are quantitatively in agreement [. . .] [with] a new attractive force overpowering the Coulomb repulsion, and give a rather accurate measure of the “potential well” which is therefore permissible as representing the interaction. Interestingly enough, this potential well appears to be identical, within the limits of error of both determinations, with the potential well which represents the proton-neutron

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[53] H. D. Jensen, *Nobel Lecture,* 12 December 1963, http://nobelprize.org/nobel_prizes/physics/laureates/1963/jensen-lecture.pdf.

[54] H. A. Bethe and R. F. Bacher, *Nuclear physics,* Review of Modern Physics 8 (1936), p. 82.

[55] M. A. Tuve, N. P. Heydenburg and L. R. Hafstad, *The scattering of protons by protons,* Physical Review 50 (1936), p. 806.

> interaction as real beginning has been made toward an accurate and intimate knowledge of the forces which bind together the "primary particles" into the heavier nuclei so important in the structure and energetics of the material universe.[56]

The interpretation of the experimental results was done by Breit, Condon, and Present, who compared their deductions on the proton-proton interaction with data about the neutron-proton interaction, obtained both from the study of the binding energy of deuteron and Fermi and Amaldi's data on the scattering and absorption of slow neutrons. The comparison left no doubts about the independence of the interaction from charge, and obliged the physicists to reconsider Bethe's explanation of the substantial equality of the number of protons and neutrons in stable light nuclei:

> The close agreement between the empirical values of the proton-proton and proton-neutron interactions in ^1S states suggests that aside from Coulombian and spin effects the interactions between heavy particles are independent of their charge and that the apparent preference for equal numbers of protons and neutrons in the building up of nuclei is conditioned more by the operation of the exclusion principle than by the greater values of proton-neutron forces.[57]

The discovery of the charge independence of the nuclear forces played an important role in the dynamics of the global maps. It was indeed at the root of an important concept in the emerging particle physics, that of isotopic spin conservation.[58] To better understand this point, let us go back to classical mechanics, where the relation between the invariance of the equation of motions and the conservation laws is more evident (albeit it is a universal principle). For instance, we know that the translational invariance results in the conservation of momentum, while the invariance under rotations implies the conservation of angular momentum, and the invariance under time translation leads to the conservation of energy. Now, isotopic spin was supposed to be a quantum-mechanical observable attached to nucleons, with two eigenstates corresponding to neutron and proton. The invariance of the observed dynamics of nucleons from isotopic spin would explain the independence of the nuclear forces from charge. One is therefore led to assume invariance under isotopic spin transformations, and this yields a new conservation law, the conservation of isotopic spin.

The development of the concept of nuclear force in the 30th was marked by another very important contribution: the meson theory of nuclear forces. This plays a central role in our reconstruction of the global maps, because it established a strong link between nuclear and elementary particle physics. This is one more example of a research with originates within a certain discipline (nuclear physics), but

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[56] *Ibid.,* pp. 824–825.

[57] G. Breit, E. U. Condon and R. D. Present, *Theory of scattering of protons by protons,* Physical Review 50 (1936), p. 825.

[58] Isotopic spin had been introduced in 1932 by W. Heisenberg, as we have discussed in section 2.8.7.

produces results that are basic for the construction of another discipline (elementary particles). Let us analyze in detail this process.

4.9 At the origins of elementary particle physics

Quantum field theory (QFT) originated with the 1927 paper by Dirac we discussed in chapter 2, and has steadily developed till today. Nowadays it provides the theoretical framework for elementary particle physics. In the period of time we are now considering there were no radical breaks with respect to the past, as it happened with quantum mechanics and relativity theory, but rather there was a conceptual reorganization, pointing to an increasing complexity and wider generalizations. As Weinberg compellingly remarked in 1977,

> If quantum mechanics and relativity were revolutions in the sense of the French Revolution of 1789 or the Russian Revolution of 1917, then quantum field theory is more of the order of the Glorious Revolution of 1688: things changed only just enough so that they could stay the same.[59]

QFT is a theoretic framework where processes involving creation and annihilation of particles can be described. It is a set of techniques which allow one to compute the probabilities of such events, for instance, the cross-sections in the scattering between particles. It is the language of elementary particle physics, and is one of its main features.

In the next sections we shall try to detect and discuss the main guidelines that, within the global maps, governed the creation and first development of this new discipline.

4.10 Confluence processes: nuclei, cosmic rays, and quantum field theory

The 1968 Nobel prize in physics was awarded to Luis Walter Alvarez

> for his decisive contributions to elementary particle physics, in particular the discovery of a large number of resonance states, made possible through his development of the technique of using hydrogen bubble chamber and data analysis."[60]

In his Nobel prize acceptance speech, he had the following words:

> As a personal opinion, I would suggest that modern particle physics started in the last days of World War II, when a group of young Italians, Conversi, Pancini, and Piccioni, who

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[59] S. Weinberg, *The search for unity: notes for a history of quantum field theory,* Dedalus 2 (1977), p. 17.

[60] http://www.nobelprize.org/nobel_prizes/physics/laureates/1968/

> were hiding from the German occupying forces, initiated a remarkable experiment. In 1946, they showed that the "mesotron" which had been discovered in 1937 by Neddermeyer[61] and Anderson and by Street and Stevenson, was not the particle predicted by Yukawa as the mediator of nuclear forces, but was instead almost completely unreactive in a nuclear sense.[62]

Therefore, to track the most important research line that guided theoretical nuclear physics in the second half of the 30s, leading to the birth of particle physics, we need to go back to Hideki Yukawa, a Japanese physicist, 1949 Nobel laureate. Yukawa's proposal was a direct filiation of Fermi's theory of β decay. As he wrote in his first paper on the meson[63] theory of nuclear forces:

> At the present stage of the quantum theory little is known about the nature of interaction of elementary particles, Heisenberg considered the interaction of "Platzwechsel" between the neutron and the proton to be of importance to the nuclear structure. Recently Fermi treated the problem of disintegration on the hypothesis of "neutrino." According to this theory, the neutron and the proton can interact by emitting and absorbing a pair of neutrino and electron. Unfortunately the interaction energy calculated on such assumption is much too small to account for the binding energies of neutrons and protons in the nucleus.[64]

Yukawa's project was to describe the strong interaction between neutron and proton by analogy with the theory of the electromagnetic field. As it is the case with the Coulomb interaction, where every electric charge creates a field which acts on the other charges, one can introduce a nuclear field (denoted by U) that occupies the space around the nucleon, so that one can consider nuclear interaction as the action of this field on the other nucleus. And again, in the same way as the photon is the quantum of the electromagnetic field, the nuclear field must be quantized, and there must exist a particle, much heavier than the electron, which is the quantum of the nuclear forces.[65] Yukawa remarked that the nuclear field should be quantized according to the general rules of quantum mechanics; thus, as neutrons and protons obey the Fermi statistics, this new field should obey Bose's statistics, otherwise the conservation of the angular momentum would be violated. Moreover, to comply with charge conservation, it must be a charged particle, with charge $\pm e$. The most

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[61] Seth Neddermeyer was an experimental physicist, a former student of Oppenheimer's at the California Institute of Technology, who worked at the National Bureau of Standard.

[62] http://www.nobelprize.org/physics/laureates/1968/alvarez-lecture.pdf.

[63] While Yukawa at first used the term "mesotron," after Heisenberg's suggestion that was changed to "meson." The latter is the term that remained in use.

[64] H. Yukawa, *On the interaction of elementary particles,* Proceedings of the Physical-Mathematical Society of Japan 17(1935), p. 48. A detailed analysis of the meson theory of nuclear forces can be found in V. Mukherji, *History of meson theory of nuclear forces from 1935 to 1952,* Archive for the History of Exact Sciences 13 (1974), p. 27.

[65] The first step in Yukawa's reasoning was the introduction, in analogy with the electromagnetic field, of an interaction field, which can be expressed in terms of a potential U. This decays with distance much faster than the electromagnetic potential; the latter goes like $1/r$, while the potential U has the form $g^2 e^{-\eta r}/r$, where g is a constant having the dimension of an electric charge, while η is the inverse of the action radius of the nuclear force. The values of g and η were chosen by Yukawa according to the experimental data.

striking prediction, however, was that about the mass of this mysterious mediator of the nuclear force; Yukawa obtained a value of about 200 times the electron mass,[66] and this led him to a rather odd remark:

> [...] as such a quantum with large mass and positive or negative charge has never been found by the experiment, the above theory seems to be on a wrong line.

Yukawa's hypothesis was only about the neutron-proton interaction, while for the proton-proton interaction he only refereed to the Coulomb force. Only the fundamental work of Tuve, Heydenburg, and Hafstad,[67] that we have already cited, proved, by studying the proton-proton scattering, the existence of a short-range attractive force between these particles. In the same issue of the journal the experimental results of the three physicists were published, a few pages later, an important work of Cassen and Condon appeared, where they advanced the hypothesis of the independence of the nuclear forces from charge.

In 1937 this feature of nuclear forces was considered as an acquired fact, and this allowed for a further development of Yukawa's theory, namely, that the meson should exist also as a neutral particle, not only as a negative or positive particle. This is necessary if the same mechanism used to describe the interaction between different particles (proton-neutron) should also describe the interaction between particles of the same kind (neutron-neutron and proton-proton) without violating the conservation of charge.

At their appearance Yukawa's ideas did not attract much interest from the international scientific community. In a paper written to celebrate the 20th anniversary of the meson, Nicholas Kemmer wrote:

> Though Yukawa's idea was basically so simple and also so clearly stated, it attracted no attention. Perhaps part of the explanation was that the journal in which it was published was not widely read. But this cannot be the whole story for in those days the volume of work published in this field was so small compared with today that any serious minded student would find no difficulty whatever in taking note of the contents of all relevant papers in whatever journal they were published. More important perhaps was the fact that among the leaders of theoretical physics in Western Europe all important results were instantly

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[66] The reasoning behind this estimate of the mass was briefly the following. For the electromagnetic field there is a relation among speed, frequency, and wavelength of the electromagnetic waves, namely, $c = \nu\lambda$. One can therefore wonder what is its analogue for the Yukawa field. The wave equation for the potential U leads to the relation

$$\left(\frac{\nu}{c}\right)^2 = \left(\frac{1}{\lambda}\right)^2 + \left(\frac{h\eta}{2\pi}\right)^2 \qquad (*)$$

(here η has the same meaning as in note 65). This is true at the classical level; in the quantum setting, one should take into account the relations $E = h\nu$ and $p = h/\lambda$, which yield the energy and the momentum of the quanta. Substituting these into the equation (*) one obtains $(E/c)^2 = p^2 + (h\eta/2\pi)^2$. This equation, by comparison with the relativistic relation $(E/c)^2 = p^2 + m_0c^2$, yields a relation between the mass of the quantum and the radius of action of the nuclear force, in the form $m_0 = h\eta/2\pi c$.

[67] M. A. Tuve, N. P. Heydenburg and L. R. Hafstad, *The scattering of protons by protons, op. cit.*

> communicated at private meetings or by correspondence, while Yukawa's ideas never even started being spread by this "grapevine" method. But having said all this, it is quite clear that Hideki Yukawa in 1935 was ahead of his time and found the key to the problem of nuclear forces when no other theoretical physicist in the world was ready to accept it. All this was changed in 1937 after Anderson announced his discovery in cosmic radiation of a particle of approximately the mass required by Yukawa's theory. Within weeks we were studying and attempting to extend Yukawa's ideas. And within a few months, if not weeks, workers in Japan and in Europe discovered that they were thinking on practically identical lines and Yukawa's ideas had been completely assimilated.[68]

The discovery mentioned by Kemmer was communicated by Anderson and Neddermeyer on 30 March 1937[69] and was further confirmed by Street and Steven in October 1937.[70] The new particle discovered in the cosmic rays, with an intermediate mass between the electron and the proton, was called *mesotron,* and was identified with the quantum of the nuclear field conjectured by Yukawa. The researches on the properties of the new particle, such as its mean lifetime and its decay modes, kept many physicists busy in the late 30s and early 40s. While it was soon clear that the meson was an unstable particle, there was no precise measurement of its mean lifetime. The first direct measurements were made by Franco Rasetti, at the time working in Canada, and they gave a value of $(1{,}5 \pm 0{,}3) \times 10^{-6}$ seconds.[71]

The reconstruction of the global maps leads us, after the discovery of the meson and the study of its properties, to the work done by Conversi, Pancini, and Piccioni in 1947.[72] It is a long lapse of time, but one should consider that in the meantime World War II took place, and the Manhattan Project completely conditioned the research in nuclear physics. The work of the three scientists was at a turning point. Their experiment showed that the particle found by Anderson and Neddermeyer in the cosmic rays, the meson, could not be the quantum of nuclear force. Conversi, Pancini, and Piccioni tested a consequence of Yukawa's hypothesis that was spelled

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[68] N. Kemmer, The impact of Yukawa's meson theory on workers in Europe — A reminiscence, Progress of Theoretical Physics. Supplement, Commemoration issue for the 30th anniversary of the meson theory by Dr. H. Yukawa, 1965, p. 602.

[69] S. H. Neddermeyer and C. D. Anderson, *Note on the nature of cosmic-ray particles,* Physical Review 51 (1937), p. 884.

[70] J. C. Street and E. C. Stevenson, *New evidence for the existence of a particle of mass intermediate between the proton and electron,* Physical Review 52 (1937), p. 1003.

[71] F. Rasetti, *Mean life of slow mesotrons,* Physical Review 59 (1941), p. 613; *Evidence for the radioactivity of slow mesotrons, ibid.* p. 706; *Disintegration of slow mesotrons,* Physical Review 60 (1941), p. 198. An ample and well documented review of meson physics during those years can be found in some articles included in *The Birth of Particle Physics,* L. M. Brown and L. Hoddeson eds., *op. cit.,* namely: B. Rossi, *The decay of "mesotrons" (1939–1943): experimental particle physics in the age of innocence;* G. Bernardini, *The intriguing history of the μ meson;* C. D. Anderson and H. L. Anderson, *Unraveling the particle content of cosmic rays;* O. Piccioni, *The observation of the leptonic nature of the "mesotron" by Conversi, Pancini, and Piccioni.*

[72] M. Conversi, E. Pancini and O. Piccioni, *On the disintegration of negative mesons,* Physical Review 72 (1947), p. 209.

out by Shin Ichiro Tomonaga and Toshima Araki.[73] According to the two Japanese scientists, the behavior of the mesons when they move through matter must heavily depend on their charge; the electrostatic repulsion from the protons should decrease the probability of the interaction of a positive meson with the nucleus, while this effect is absent in the case of negative mesotrons. So, the positive mesons do not live long enough to be captured by the nucleus, and decay, while the negative mesons are captured by the nucleus before they can decay. If the mesons in the cosmic rays were the mediators of Yukawa's nuclear forces, "then practically all the decay processes which one observes should be owing to positive mesons."[74]

The experiment made by the Italian scientists, however, had a very definite result; also the negative mesons decay, that is, they do not interact with nuclei and therefore cannot be identified with Yukawa's particles. "Wonder and bewilderment" were the terms used by Giorgio Salvini to describe the impact of the Conversi-Pancini-Piccioni experiment on the scientific community. As Salvini wrote, "The result was published on the most rapid and prestigious journal of the time, the *Physical Review Letters.*"[75] Piccioni, when, much time later, remembered the events that led to that fundamental discovery, wrote:

> Our result caused great consternation among theorists. Bohr advanced the bold hypothesis that contrary to the computation of Tomonaga and Araki the mesotrons spent a long time in atomic orbits of large angular momenta, thus with minimal density inside the nucleus of carbon. I never heard any detail of such suggestion which seems to postulate a sort of Pauli principle among particles with masses different by two orders of magnitude. Years later Prof. Bohr told me of having called our experiment "the Pinocchio effect" which name I thought derived from the first two letters of my name and from his displeasure with our result.[76]

In the history of physics all important events had their liturgical moment. So it happened for the *OQT*, the Old Quantum Theory, with the First Solvay Conference on 1911, for quantum mechanics with the 5th Solvay Conference in 1927, and for nuclear physics with the Rome Conference in 1931 and the 7th Solvay Conference in 1933. Also elementary particle physics had a solemn celebration of its birth: the Shelter Island Conference. Between 1947 and 1949 the National Academy of Sciences promoted three theoretical physics conferences. The first took place on Shelter Island, at the eastern end of Long Island, from 2 to 4 June 1948; its theme was the foundations of quantum mechanics. The second took place in Pocono Manor, Pennsylvania, from 30 March to 2 April 1948, and was about Julian

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[73] S. Tomonaga and G. Araki, *Effect of the nuclear Coulomb field on the capture of slow mesons,* Physical Review 58 (1940), p. 90.

[74] M. Conversi, E. Pancini and O. Piccioni, *op. cit.*, p. 209.

[75] G. Salvini, *La vita di Oreste Piccioni e la sua attività scientifica in Italia,* Giornata Lincea in ricordo di Oreste Piccioni (Rome, 12 November 2003), Accademia Nazionale dei Lincei. Classe di scienze matematiche, fisiche e naturali 15 (2004), serie 9, p. 289.

[76] O. Piccioni, *The discovery of the leptonic property,* in *Present Trends, Concepts and Instruments of Particle Physics. Symposium in honour of Marcello Conversi's 70th birthday,* Rome, 3–4 November 1987, G. Baroni, L. Maiani and G. Salvini eds., Compositori, Bologna 1988, p. 171.

Schwinger's approach to quantum electrodynamics; the third took place at Oldstone, on the Hudson River near New York City, and was again about Richard Feynman's approach to quantum electrodynamics. Referring to the Shelter Island Conference, Feynman in 1966 remarked "there have been many conferences in the world since, but I've never felt any to be as important as this."[77]

The participants in the Shelter Island Conference were 24, the Gotha of the international research in physics.[78] The arguments treated were two: the Conversi-Pancini-Piccioni experiment and the experimental results of Willis E. Lamb on what would be later called the *Lamb shift*. [79] The latter is a high precision measurement on the energy levels of the hydrogen atom; two levels, that according to the theory should be identical, show a small difference.[80] As Fermi wrote two years later,

> The attempts that were made and the papers that were written to explain this discrepancy between theory and experience led to the remarkable advancements that took place in quantum electrodynamics over the last two or three years. It is difficult to attribute this progress to specific names, as the work originated at a conference attended by about 30 theoretical physicists, where the problem was discussed; and many of the general ideas that were developed afterwards were, at least qualitatively, advanced there, without developing the computations. In any case, the first published paper, where 99% of this situation was explained, was due to Bethe.[81]

The story says that Bethe, traveling by train from Shelter Island to the General Electric Laboratories in Schenectady, New York after the conference, was able to translate into a computation the discussions and the hypotheses made during the conference.[82] In particular, the discussions among Kramers, Oppenheimer, Schwinger, and Weisskopf showed that the failure to predict the Lamb shift was not

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[77] Interview with Dr. Richard Feynman by Charles Weiner, at Altadena, California, on 27 June 1966. Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA, http://www.aip.org/history/ohilist/5020_3.html.

[78] Perspective participants included Hans Bethe, Felix Bloch, David Bohm, Gregory Breit, K. K. Darrow, Albert Einstein, Enrico Fermi, Herman Feshbach, Richard Feynman, H. A. Kramers, Willis Lamb, Duncan MacInnes, Robert Marshak, C. Møller, Arnold Nordsieck, John R. Oppenheimer, Abraham Pais, Linus Pauling, Bruno Rossi, Isidor Isaac Rabi, Julian Schwinger, Robert Serber, Edward Teller, George Uhlenbeck, H. Van Vleck, Victor Weisskopf, and John Wheeler. Three scientists could not attend, among them Fermi, who was prevented by an eye problem.

[79] W. E. Lamb and R. C. Rutherford, *Fine structure of the hydrogen atom by a microwave method,* Physical Review 71 (1947), p. 241.

[80] For the expert reader, the problem is about the $n = 2$ term of the hydrogen spectrum, which according to the theory, contains the terms $2S_{1/2}$, $2P_{1/2}$, and $2P_{3/2}$. A theoretical analysis predicts that the lines $2S_{1/2}$ and $2P_{1/2}$ coincide, while $2P_{1/2}$ and $2P_{3/2}$ have a separation of 0,365 cm^{-1}. Lamb and Rutherford's measurements find the expected value for the separation between $2P_{1/2}$ and $2P_{3/2}$, but show a 0,033 cm^{-1} displacement of $2S_{1/2}$ with respect to $2P_{1/2}$, almost 10% of the displacement between $2P_{1/2}$ and $2P_{3/2}$.

[81] Fermi talked about the Lamb shift in the sixth of the nine lectures he gave in Italy in 1949 (Donegani Lectures). The lecture was devoted to the new developments of quantum electrodynamics (*CPF II,* p. 749).

[82] H. A. Bethe, *The electromagnetic shift of energy levels,* Physical Review 72 (1947), p. 339.

due to an inadequacy of the theoretical model, but to the fact that the computation was stopped at the first order of approximation to avoid divergences. The explanation of the Lamb shift originated by the Shelter Island Conference was a kind of trustful promise of a "renormalization program" (see Appendix C.13), capable of effectively treating the fundamental problems of the elementary particle physics. Till then, as remarked by Weinberg, "The one missing element was confidence in renormalization as a means of dealing with infinities." Indeed, Weinberg again recalled,

> [...] renormalization was widely discussed in the late 1930s. But it had become accepted wisdom in the 1930s [...] that quantum electrodynamics could not be taken seriously at energies of more than about 100 MeV, and that the solution to its problems could be found only in really adventurous new ideas.[83]

The second topic that was discussed at Shelter Island was the Conversi-Pancini-Piccioni experiment. The failure of the conjecture that the meson was to be identified with Yukawa's particle motivated a proposal by Marshak, namely, that there existed two different particles, of different mass; a heavier one, coinciding with Yukawa's particle, which was later called π meson or *pion* and a lighter one, the one discovered by Anderson and Neddermeyer, which was called μ meson or *muon.* The pion was supposed to be the mediator of the nuclear force. It is mostly produced in the higher strata of the atmosphere, while the muon, which is observed at see level and interacts only weakly with matter, is a decay product of the pion. Marshak's proposal, published in paper signed also by Bethe,[84] was immediately confirmed by an experiment made by a group in Bristol, who detected the decay predicted by Marshak in the films exposed to the cosmic rays.[85] The group was formed by César M. G. Lattes, Hugh Muirhead, Giuseppe B. Occhialini, and Cecil F. Powell.

Let us summarize this fragment of the global maps. Yukawa predicted the existence of a particle, with an intermediate mass between the electron and the proton; Anderson and Neddermeyer believed to have found it in the cosmic rays; Conversi, Pancini, and Piccioni proved that the particle was not the one predicted by Yukawa; Marshak conjectured the existence of two mesons, which was confirmed by the experiment of the Bristol group. This is the sequence of events, which opened

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[83] S. Weinberg, *The Quantum Theory of Fields,* Cambridge University Press, Cambridge 1998, p. 38.

[84] R. E. Marshak and H. A. Bethe, *On the two-meson hypothesis,* Physical Review 72 (1947), p. 506.

[85] C. M. G. Lattes, H. Muirhead, G. B. S. Occhialini and C. F. Powell, *Processes involving charged mesons,* Nature 159 (1947), p. 694; C. M. G. Lattes, G. B. S. Occhialini and C. F. Powell, *Observation on the tracks of slow mesons in photographic emulsions,* Nature 160 (1947), p. 453. The first paper of the Bristol group was dated 24 May 1947, and therefore preceded Marshak's hypothesis. However, Marshak was informed of the discovery only after the Shelter Island conference. As he wrote, referring to the journal issue with the announcement of the discovery in Bristol, "but it did not reach the United States until several weeks later, after the Shelter Island conference, because journals were not sent airmail." (R. E. Marshak, *Particle physics in rapid transition: 1947–1952,* in *The Birth of Particle Physics,* L. M. Brown and L. Hoddeson eds., *op. cit.* p. 382.

a new chapter of physics. But was this just a series of predictions, discoveries, and confutations, or there was something more? We leave the answer to this question to Giorgio Salvini:

> During those years that closed the first half of the last century, the discoveries took place in an incessant, impressive, wonderful way. We had the experimental evidence of the existence of the pion, the pion disintegration was verified by the magnificent experiment of Powell and Occhialini and his group; and an esthetic and philosophical problem arose, namely, why the muon is there? Who invented it? Why the pion does not decay directly into an electron?[86]

This was the main merit of the work of Conversi, Pancini, and Piccioni. They opened the door to the discovery of a new family of elementary particles, the *leptons,* which play an essential role in the structure of the universe. The three scientists were well aware of this, as stressed by what Piccioni said at the conference held in honor of Marcello Conversi's 70th birthday:

> The first time I had the opportunity to describe our work in Chicago, in 1980, I insisted that the result of the experiment in Rome was interesting on its own, also after the wonderful work of the Bristol group. Indeed the experiment in Rome had discovered new features of the interaction among elementary particles, namely, leptonic properties. Obviously, the electron is a lepton, but this property is not an attribute it has by itself, as with its minuscule mass it could be regarded as an object which is "made of photons" and therefore does not share the properties of the strong interaction. To speak of a family one needed another member. As far as I know, physicist started to use the word "lepton" only after Ettore, Marcello and myself were so frustrated by our result that our noses started becoming longer, as Collodi said happened to Pinocchio.[87]

4.11 A wonderful mess

In 1947 there were not many known massive particles: neutron, proton, electron, the two muons, the two charged pions, and the positron, the only anti-particle known at the time. The anti-proton, anti-neutron, and the neutral pion had been theoretically predicted but had not been found experimentally yet. Figure 4.8 summarizes the situation at that date. From 1947 events happened with a bewildering speed, giving rise to what Abraham Pais, many years later, would have called a "wonderful mess."

> It was a wonderful mess at that time. Wonderful! Just great! It was so confusing — physics at its best, when everything is confused and you know something important lies around the corner.[88]

The confusion was increased by the discovery of more and more new particles, and new generalizations and new conservation laws were proposed, giving hopes

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[86] G. Salvini, *op. cit.* p. 305.

[87] O. Piccioni, *op. cit.*

[88] A. Pais, *Inward Bound, op. cit.* p. 117.

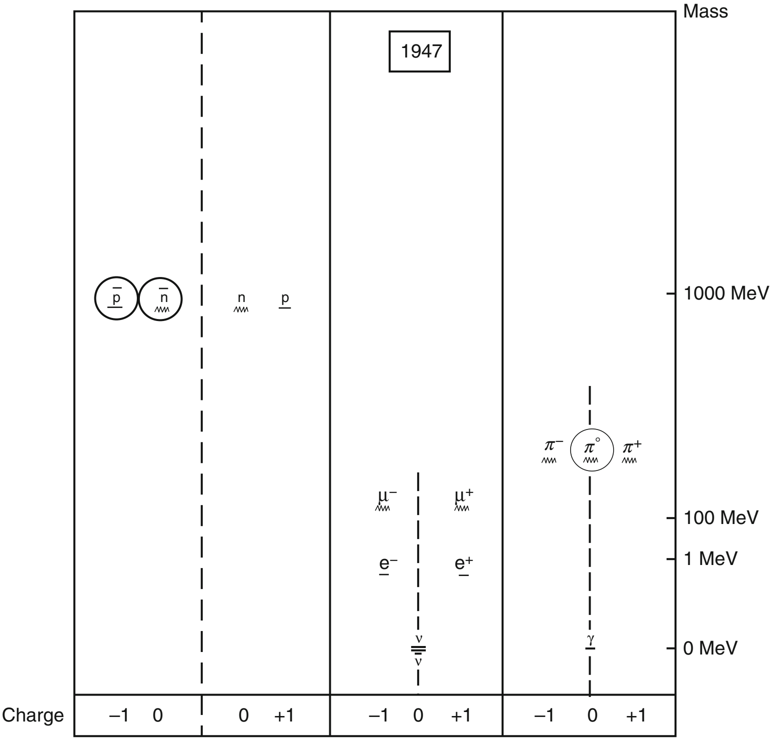

Fig. 4.8 Representation of the particles that were known or predicted in 1947. Masses are not represented in scale. The particles with a wavy underline are unstable. The circled ones (anti-proton and anti-neutron, neutral pion) were theoretically predicted but not yet experimentally revealed.

for the possibility of finding a systematization of what was becoming a "zoology" of the particles.

The first generalization directly stemmed from the Conversi-Pancini-Piccioni experiment, and can be traced to an idea that Bruno Pontecorvo had in 1947. It was based on the observation that the probability that a nucleus captures a μ meson is of the same order of magnitude of the probability of "k-capture," that is, a process where an electron is captured by a nucleus, which passes from atomic number Z to $Z-1$, and a neutrino is emitted. This process should be governed by the same interaction which produces the β decay. This coincidence is for Pontecorvo the hint of a general property:

> We assume that this is significant and wish to discuss the possibility of a fundamental analogy between β-processes and processes of emission or absorption of charged mesons.[89]

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[89] B. Pontecorvo, *Nuclear capture of mesons and the meson decay,* Physical Review 72 (1947), p. 246.

This was the starting point of a theoretical proposal that will be known as "Fermi universal interaction." Pontecorvo in 1985 summarized that idea as follows:

> Thus, in 1947, I started to think in terms of weak-interaction processes and understood first that both the muon capture by nuclei and the β decay are processes due to a definite weak interaction existing in nature. It was clear to me that the muon is a sort of heavy electron and that the muon-electron symmetry is taking place under a type of interaction that is properly called weak, thanks to the smallness of the corresponding constant G — the Fermi β decay constant. A similar point of view — namely, to include the muon decay among weak processes — was adopted later by others: Oskar Klein; Giovanni Puppi; T. D. Lee, M. Rosenbluth, and C. N. Yang; Jayme Tiomno and John A. Wheeler. The original 1947 idea that there exists a muon-electron symmetry in nature was the first hint of a universal weak interaction.[90]

Pontecorvo's idea was just one of the many results that made 1947 a year of great importance for elementary particle physics. Clifford C. Butler and George D. Rochester, in their researches on the nature of cosmic rays, analyzed about 5000 plates of cloud chamber processes and found two unusual phenomena, namely, tracks of a decay which bifurcated.[91] It was the first evidence of the existence of a new particle, with a mass between that of the muon and of the proton, to which the name "V particle" was given. It was the first of a long series of particles discovered in the late 40s and early 50s. For instance, the τ meson, whose discovery was announced by the French Leprince-Ringuet at a conference in honor of Millikan's 80th birthday, was a particle with a mass of about 700 m_e (where m_e is the electron mass).[92] Till that time the cosmic rays were the only sources of particles, but with the new large accelerators, that the main American universities were starting to build, the experimental research acquired a tool which in a few years would produce an extraordinary increase in the number of known particles.

Measurements were not so reliable to avoid misunderstanding. Sometimes the experimenters believed to have found new particles, while the events they discovered could be attributed to already known particles. Moreover, the terminology was not agreed by everybody. The first attempt to find a common terminology was made in July 1953 at a conference in Bagnères-de-Bigorre. It was an important meeting which stressed the fast spreading of experimental researches in particle physics. 20 research groups were represented; just to cite a few, Bombay,

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[90] B. Pontecorvo, *Recollections on the establishment of the weak-interaction notion,* in *Pions to Quarks,* L. M. Brown, M. Dresden and L. Hoddeson eds., Cambridge University Press, New York 1989, p. 369.

[91] C. C. Butler and G. D. Rochester, *Evidence for the existence of new unstable elementary particles,* Nature 160(1947), p. 855; reprinted in *The Experimental Foundations of Particle Physics,* R. N. Cahn and G. Goldhaber eds., Cambridge University Press, Cambridge 1991, p. 69.

[92] L. Leprince-Ringuet, *Photographic evidence for the existence of a very heavy meson,* Review of Modern Physics 21 (1949), p. 42.

Bristol, Genoa-Milan, Chicago, Manchester, Padua, Pasadena, Princeton, Rochester, and Rome. As reported by Pais[93] (there are no proceedings of the conference) it was decided to call:

- *L mesons* the π and μ mesons;
- *K mesons* the particles with mass between the pion and the proton; and
- *hyperons* the particles with mass between the neutron and the deuteron.

Next year Pais proposed to use the term *baryon* to designate both nucleons and hyperons. Comparing Figures 4.8 and 4.9 one can see what great proliferation of particles took place in 10 years. With the new discoveries, the meaning of the term "elementary particle" becomes problematic. The compound model of the nucleus, as we shall see in chapter 5, was the answer given by Yang and Fermi to this issue.

In those years researchers were busy with the general questions concerning the nature of elementary particles and their systematization into a coherent and comprehensive scheme. There were also serious conflicts with the existing theoretical paradigm, due to the multitude of particles and of their decay modes. The solution of these conflicts led to the discovery of new conservation laws; these are the most important nodes of the evolution of the global maps during those years.

In 1950 Yang and Tiomno, studying the Fermi interaction, remarked that if that interaction was universal, it would make possible also processes that however are "inconsistent with experience, such as $N + P \to e + \nu$ or $N \to e^+ + \mu^- + \nu$."[94] The answer to this question led, thanks to a suggestion by Fermi, to the formulation of the law of conservation of the baryon number.[95]

The new particles (k mesons and hyperions), however, displayed very peculiar features, and were indeed called "strange particles." Their peculiarity was the apparent impossibility of reconciling their abundance with their longevity. In general, indeed, it is quite reasonable to suppose that the production of a particle during a scattering process and its decay are governed by the same dynamical mechanics. One thus expects that if during a process involving two nucleons, or a nucleon and a meson, a certain particle is produced in abundance, that particle decays. If its production is regulated by the strong interaction, one would expect to find particles that decay after a relatively short mean lifetime. The empirical data, however, showed mean lifetimes that were 10 times bigger than what would be expected in consideration of the abundance of the production of the particles; in other terms, the decay processes seemed to be regulated by the weak interaction.

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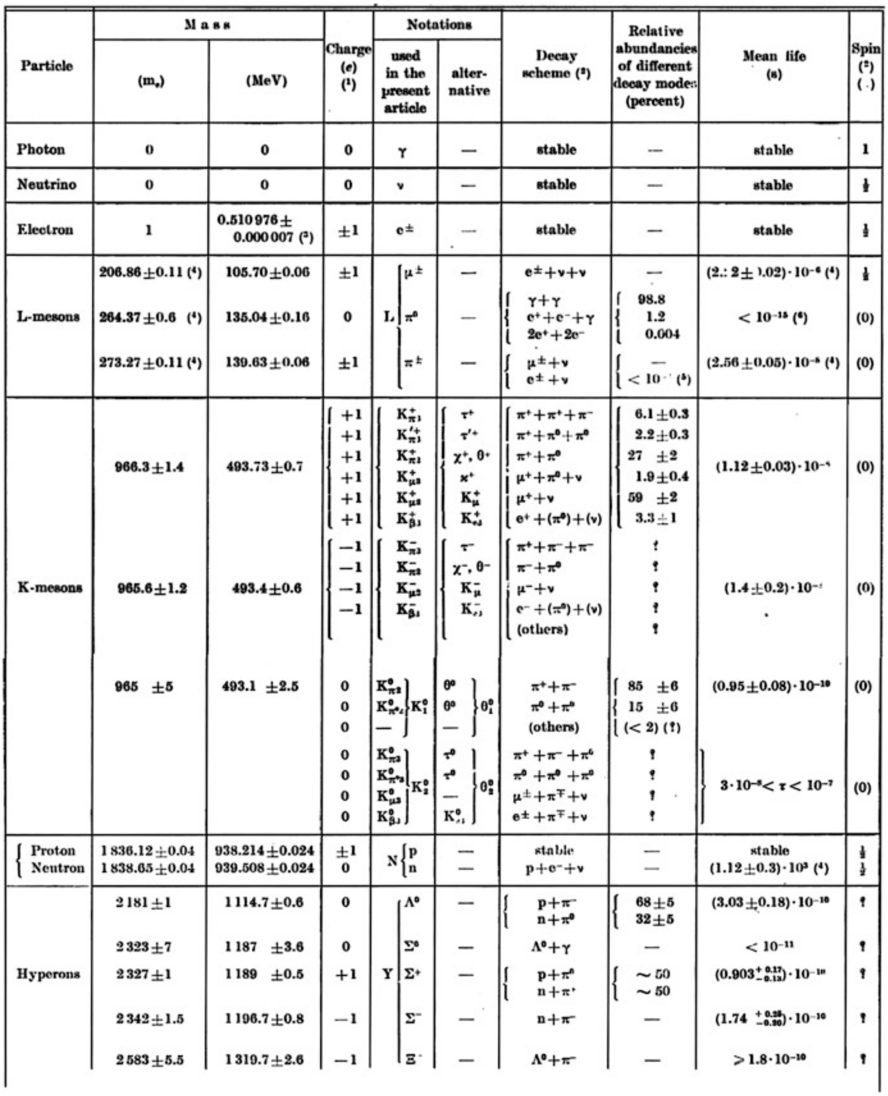
[93] A. Pais, *Inward Bound,* Oxford University Press, New York 1986, p. 514.

[94] C. N. Yang and J. Tiomno, *Reflection properties of spin 1/2 fields and a universal Fermi-type interaction,* Physical Review 79 (1950), p. 497.

[95] In the footnote 12 in that paper the authors wrote "This was pointed out by Professor E. Fermi in a seminar of about a year ago." The baryon number is defined as $B = 1$ for baryons, $B = -1$ for anti-baryons, and 0 for the other particles. The conservation of baryon number dictates that during any process the baryon number does not vary; actually, this simply means that a baryon cannot transform into a meson.



Particle	Mass		Charge (e) (1)	Notations		Decay scheme (2)	Relative abundancies of different decay modes (percent)	Mean life (s)	Spin (2) ( )
	(m_e)	(MeV)		used in the present article	alternative				
Photon	0	0	0	γ	—	stable	—	stable	1
Neutrino	0	0	0	ν	—	stable	—	stable	$\frac{1}{2}$
Electron	1	0.510976 ± 0.000007 (3)	± 1	$e^\pm$	—	stable	—	stable	$\frac{1}{2}$
L-mesons	206.86 ± 0.11 (4)	105.70 ± 0.06	± 1	L $\mu^\pm$	—	$e^\pm + \nu + \nu$	—	$(2.22 \pm 0.02) \cdot 10^{-6}$ (4)	$\frac{1}{2}$
	264.37 ± 0.6 (4)	135.04 ± 0.16	0	L π^0	—	$\gamma + \gamma$ $e^+ + e^- + \gamma$ $2e^+ + 2e^-$	98.8 1.2 0.004	$< 10^{-15}$ (6)	(0)
	273.27 ± 0.11 (4)	139.63 ± 0.06	± 1	L $\pi^\pm$	—	$\mu^\pm + \nu$ $e^\pm + \nu$	— $< 10^{-}$[illegible] (5)	$(2.56 \pm 0.05) \cdot 10^{-8}$ (4)	(0)
K-mesons	966.3 ± 1.4	493.73 ± 0.7	+1 +1 +1 +1 +1 +1	$K^+_{\pi 3}$ $K'^+_{\pi 3}$ $K^+_{\pi 2}$ $K^+_{\mu 3}$ $K^+_{\mu 2}$ $K^+_{\beta 3}$	τ^+ τ'^+ χ^+, θ^+ $\varkappa^+$ K^+_μ K^+_{e3}	$\pi^+ + \pi^+ + \pi^-$ $\pi^+ + \pi^0 + \pi^0$ $\pi^+ + \pi^0$ $\mu^+ + \pi^0 + \nu$ $\mu^+ + \nu$ $e^+ + (\pi^0) + (\nu)$	6.1 ± 0.3 2.2 ± 0.3 27 ± 2 1.9 ± 0.4 59 ± 2 3.3 ± 1	$(1.12 \pm 0.03) \cdot 10^{-8}$	(0)
	965.6 ± 1.2	493.4 ± 0.6	−1 −1 −1 −1	$K^-_{\pi 3}$ $K^-_{\pi 2}$ $K^-_{\mu 2}$ $K^-_{\beta 3}$	τ^- χ^-, θ^- K^-_μ K^-_{e3}	$\pi^+ + \pi^- + \pi^-$ $\pi^- + \pi^0$ $\mu^- + \nu$ $e^- + (\pi^0) + (\nu)$ (others)	? ? ? ? ?	$(1.4 \pm 0.2) \cdot 10^{-8}$	(0)
	965 ± 5	493.1 ± 2.5	0 0 0	$K^0_{\pi 2}$ $K^0_{\pi^0 2}$ — $\Big\}K^0_1$	θ^0 θ^0 — $\Big\}\theta^0_1$	$\pi^+ + \pi^-$ $\pi^0 + \pi^0$ (others)	85 ± 6 15 ± 6 (< 2) (?)	$(0.95 \pm 0.08) \cdot 10^{-10}$	(0)
			0 0 0 0	$K^0_{\pi 3}$ $K^0_{\pi^0 3}$ $K^0_{\mu 3}$ $K^0_{\beta 3}$ $\Big\}K^0_2$	τ^0 τ^0 — K^0_{e3} $\Big\}\theta^0_2$	$\pi^+ + \pi^- + \pi^0$ $\pi^0 + \pi^0 + \pi^0$ $\mu^\pm + \pi^\mp + \nu$ $e^\pm + \pi^\mp + \nu$	? ? ? ?	$3 \cdot 10^{-8} < \tau < 10^{-7}$	(0)
Proton Neutron	1836.12 ± 0.04 1838.65 ± 0.04	938.214 ± 0.024 939.508 ± 0.024	± 1 0	N p N n	— —	stable $p + e^- + \nu$	— —	stable $(1.12 \pm 0.3) \cdot 10^3$ (4)	$\frac{1}{2}$ $\frac{1}{2}$
Hyperons	2181 ± 1	1114.7 ± 0.6	0	Y Λ^0	—	$p + \pi^-$ $n + \pi^0$	68 ± 5 32 ± 5	$(3.03 \pm 0.18) \cdot 10^{-10}$	?
	2323 ± 7	1187 ± 3.6	0	Y Σ^0	—	$\Lambda^0 + \gamma$	—	$< 10^{-11}$	?
	2327 ± 1	1189 ± 0.5	+1	Y Σ^+	—	$p + \pi^0$ $n + \pi^+$	~ 50 ~ 50	$(0.903^{+0.17}_{-0.13}) \cdot 10^{-10}$	?
	2342 ± 1.5	1196.7 ± 0.8	−1	Y Σ^-	—	$n + \pi^-$	—	$(1.74^{+0.28}_{-0.20}) \cdot 10^{-10}$	?
	2583 ± 5.5	1319.7 ± 2.6	−1	Y Ξ^-	—	$\Lambda^0 + \pi^-$	—	$> 1.8 \cdot 10^{-10}$	?

Fig. 4.9 Properties and decay modes of particles as they were known in 1957. Data in brackets were not certain.

The solution of this enigma was the starting point of the work that led to the first classification schemes. The original idea appeared in a 1952 paper by Pais.[96] With his hypothesis, the production and decay processes were decoupled. According to Pais's idea, called "associated production," the strange particles were produced by the strong interaction only in pairs, but once the particles separated, they decayed in non-strange particles according to a reaction dominated by the weak interaction, as the relatively long decay times indicated.

If, on the one hand, the hypothesis of the associated production solved the paradox of the production-decay of the strange particles, on the other hand it raised an even deeper problem: what is the origin of this peculiar property of the strong interaction? A first answer to this question is contained in a paper by Murray Gell-Mann,[97] published one year after of Pais's paper, and consisted in attaching a quantum number, and therefore a conservation law, to the phenomena of associated production. The new quantum number was an extension to the new particles of the isotopic spin. So during a strong interaction the conservation of the isotopic spin takes into account not only protons, neutrons, or pions, but also the new particles.

The problem at the point was the assignment of the isotopic spin to the new particles. In this connection, Giacomo Morpurgo wrote:

> When this story started [. . .] we did not know exactly the multiplicities of the various multiplets, and therefore we could not immediately determine the isotopic spins with precision. We managed to compute them with a lot of work, using the conservation of isotopic spin in the strong interactions, and examining in detail many reactions where strange particles were produced.[98]

A new quantum number, that was to be conserved during the strong interaction, was proposed: *strangeness*, denoted S. This number is zero for nucleons and pions, so that in a collision between these particles, it will remain zero. If the reaction produces a strange particle such as Λ^0, which has $S = -1$, the reaction must also produce another particle with $S = 1$, so that the conservation of this new quantum number is satisfied. The idea of strangeness provided a safe guide for particle physics during the 50s, but it was only the first step of an itinerary which would have revealed theoretical structures capable not only of ordering the known elementary particles, but also of predicting the existence of new ones. Starting with the middle 50s, this program was dominated by the notion of *symmetry.* In 1961 Gell-Mann and Yuval Ne'eman, a colonel of the Israeli army, independently proposed a unified system of symmetries, which predicted eight quantum numbers, that is, eight conserved quantities. The known particles could be classified according to these numbers, and new particles were predicted, and experimentally found. The name chosen for this system was "The eightfold way," not only, as Gell-Mann

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[96] A. Pais, *Some remarks on the V-particles,* Physical Review 86 (1952), p. 663.

[97] M. Gell-Mann, *Isotopic spin and new unstable particles,* Physical Review 92 (1953), p. 833.

[98] G. Morpurgo, *Introduzione alla fisica delle particelle,* Zanichelli, Bologna 1987, p. 359.

wrote, because it works with eight quantum numbers, but also because it recalls a celebrated Buddhist maxim:

> Now this, o monks, is noble truth that leads to the cessation of pain: this is the noble *Eightfold Way:* namely, right views, right intention, right speech, right action, right living, right effort, right mindfulness, right concentration.[99]

The reconstruction of the global maps leading to the Eightfold Way, and from it to the quark model, however, is not in our plans; it was a path on which, unfortunately, Fermi could not walk anymore.

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[99] G. F. Chew, M. Gell-Mann and A. H. Rosenfeld, *Strongly interacting particles,* Scientific American 210 No. 4, February 1964.

Chapter 5
Enrico Fermi: research itineraries 1934–1954

Starting with the middle 30s, Enrico Fermi became one of the main personalities in nuclear physics. The 1938 Nobel Prize confirmed that role, and put the Italian scientist at the top of the international scientific research. The reconstruction of his research itineraries allows us to understand the dynamics of the global maps; they were fundamental to increase the understanding of the nuclear and subnuclear world, but also opened new research areas, such as the dynamics of nonlinear system. The breadth of Fermi's works, and especially his swiftness and easiness in switching from experimental to theoretical investigations, testify that "Galilean dominant" we have mentioned already many times, and which was one of the most typical aspects of his approach to research.

5.1 Fermi at work: 1934–1954

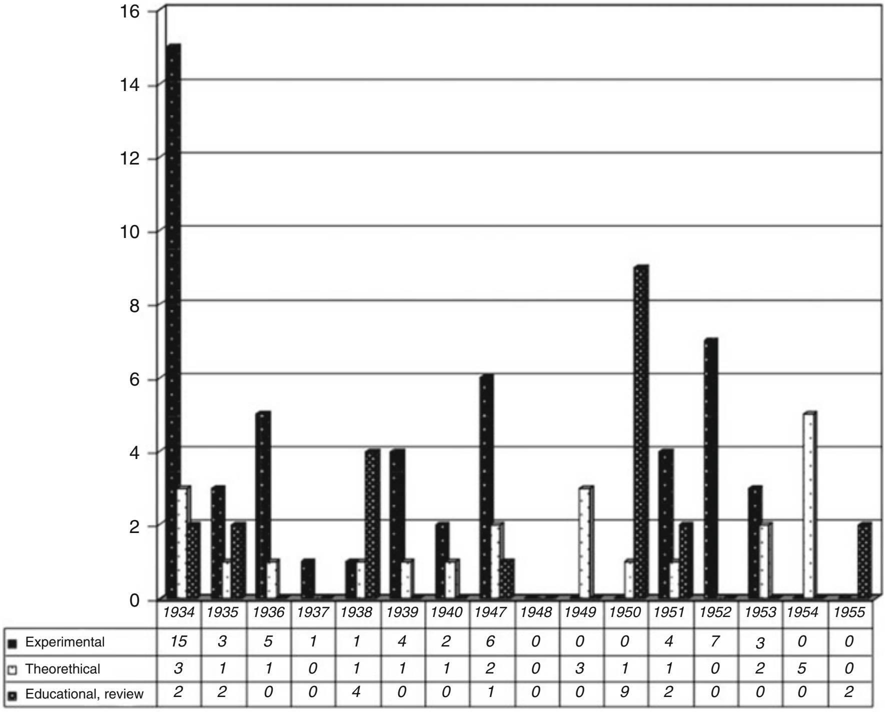
Any estimate of Enrico Fermi's scientific production in this period can only be indicative, as in wartime most of his work in nuclear physics was classified, and had a different nature with respect to the work destined to a scientific journal. As it was written by the editors of Fermi's collected papers,

> Volume II contains a large number of papers which were not destined for publication, such as the reports of work connected with the pile. These papers have been issued as classified reports by different agencies of the U.S. Government concerned with nuclear energy during the Second World War. Some are only records of work performed during a certain period of time, some contain only the result of one important measurement, some are more elaborate presentations of theory or of a series of experiments. Although we do not publish them all, the most important are here. A few are still classified.
> Some papers are reproductions of courses of lectures given in special circumstances during the war. They were written by members of the audience and were not revised by Fermi thus, they contain many imperfections which he would have removed if he had prepared them for publication. However, the Editorial Committee could not replace the Author in

G. Bruzzaniti, *Enrico Fermi*, Springer Biographies,
DOI 10.1007/978-1-4939-3533-8_5

> this work and the lectures are presented as written down, they are interesting samples of Fermi's didactic style in his later years.
> After the war, all these papers remained classified for some time and became available for publication at different dates. Fermi himself decided, for various reasons, not to publish them after declassification. The Editorial Committee, however, has included them in this volume because they are historically interesting. In particular, they are indispensable source material for any future history of nuclear technology.
> Because all these papers have not been published in standard journals, we have been faced with the problem of a suitable system of references. The papers were issued under code names such as CP-4I3, and often the same paper was reissued under different names: e.g., CP-4I3 is the same as AECD-3269.[1]

The number of declassified papers appearing in the unusual form mentioned by the editors is very high; about 90 papers over six years. The "usual" papers, published in scientific journals, were about 95. Figure 5.1 shows the distribution



	1934	1935	1936	1937	1938	1939	1940	1947	1948	1949	1950	1951	1952	1953	1954	1955
■ Experimental	15	3	5	1	1	4	2	6	0	0	0	4	7	3	0	0
□ Theorethical	3	1	1	0	1	1	1	2	0	3	1	1	0	2	5	0
▨ Educational, review	2	2	0	0	4	0	0	1	0	0	9	2	0	0	0	2

Fig. 5.1 The distribution of Enrico Fermi's work between 1935 and 1955. Papers have been recorded according to their publication date. We did not take into account many papers that were translated and published in English, although sometimes there are differences from the Italian original versions.

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[1] E. Amaldi, H. L. Anderson, E. Persico, F. Rasetti, C. S. Smith, A. Wattenberg and E. Segrè, *Preface to CPF II,* p. xv.

of these latter papers according to their experimental or theoretical character, and whether they contained original results, were reviews or had pedagogical nature. One should however note that in this case, differently from the analogous diagram in Chapter 3, this subdivision is more articulated; in some cases, the computations and the theoretical arguments are so closely connected to the acquisition and analysis of the experimental data that a precise classification of the paper is often difficult and sometimes ambiguous.

In addition to these papers, Fermi published several books: *Molecole e cristalli*, published by Zanichelli in 1934; *Nuclear Physics,* published by the University of Chicago press in 1949;[2] *Elementary Particles,* first published by the Yale University Press in 1951[3] and then translated into Italian, with some additions by Piero Caldirola, published by Einaudi in 1952; and *Notes on Quantum Mechanics,* posthumously published by the University of Chicago Press in 1961.[4]

The most evident fact we can draw from Figure 5.1 is Fermi's sudden change from theoretical to experimental research in 1934. Only after the War Fermi was again interested in theoretical problems, especially in the physics of elementary particles, an area which was becoming autonomous and was earning a status of independent and officially recognized discipline.

5.2 Neutron physics

In the first chapter we reviewed the birth of the Roman research group, the history of their research, with the transition, in the early 30s, from atomic to nuclear physics, and its fast dissolution, which started in 1935 and ended in 1938, with Fermi's relocation to America. Between 1934 and 1938 however the research of Fermi's group, or of what remained of it in view of the departure of many of its members, put the Italian research on neutron physics in a position of high prestige and authoritativeness at the international level.

The paper that marked the end of the group's research in atomic physics was published by Fermi and Amaldi in 1934.[5] It was a paper based on the Thomas-Fermi model, which in the original intentions of the authors was to be the starting kernel of a handbook, the so-called *Thesaurus* Ψ*-arum*. The idea was to list the values of the eigenfunctions, that is, the solutions of Schrödinger equations, for a large number of atomic states of many elements. It was a very complex work which required a huge amount of numerical computations, which were made by young students.[6]

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[2] The book contains the edited notes of a course given by Fermi in January-June 1949 at the University of Chicago, taken by J. Orear, A. H. Rosenfeld, and R. A. Schluster.

[3] This book is an elaboration of the material used by Fermi for the 1950 Silliman lectures at Yale.

[4] This book contains the notes Fermi prepared for the students of his course at University of Chicago in 1954.

[5] Fermi [82].

[6] Amaldi and Fermi in their paper acknowledged the help of the students A. Biava, F. Coljori, V. Crocco, G. Giovene, E. Medi, and R. Nuzzo.

From 1934, Fermi's group's research was addressed to artificial radioactivity. The idea of using neutrons to activate the radioactivity of the usual elements turned out to be a success. Let us analyze in detail what happened.

5.3 Artificial radioactivity: the transuranic elements

The group's publication on artificial radioactivity started with a letter to *La Ricerca Scientifica,* signed by Fermi, and dated 25 March 1934.

> In this letter I want to report on some experiments aimed to detect if a bombardment with neutrons can determine radioactive phenomena such as those observed by the Curie using α particles.[7]

The experimental setup was simple. The neutron source was a small glass tube containing beryllium dust and radon (Figure 5.2). The neutron flux was about 100,000 per second. The elements to be irradiated were contained in small cylinders and were exposed to the neutron flux for a time variable from some minutes to a few hours. After the irradiation, the samples were positioned near a Geiger-Müller counter, built by Fermi himself (Figure 5.3). The results obtained, initially only by Fermi, later by the whole group or some of its members, were published in 48 papers. Of these, 20 were in English, mostly translations of Italian originals.

The first activated elements were aluminum and fluorine. Also the halving time was measured. Fermi gave the following interpretation of the phenomenon:

> Fluorine bombarded by neutrons disintegrates emitting α particles. The most likely nuclear reaction is
>
> $$F^{19} + n^1 \rightarrow N^{16} + He^4.$$
>
> The reaction therefore would produce an atom of nitrogen of weight 16, which emitting a β particle would then transform into O^{16}. A similar interpretation might hold for aluminum, according to the nuclear reaction
>
> $$Al^{27} + n^1 \rightarrow Na^{24} + He^4.$$
>
> The sodium isotope Na^{24} would be a new radioactive element, and would transform into Ca^{24} by emitting a β particle.[8]

Fermi's second letter confirmed this hypothesis with a photograph of the produced electrons taken in the Wilson chamber, and extended to more elements (iron, silicon, phosphor, chlorine, vanadium, copper, arsenic, silver, tellurium, iodine,

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[7] Fermi [84a], *CPF I,* p. 645.

[8] *Ibidem.* Fermi used a different notion with respect to the Joliot; in Fermi the nucleus was labeled by the mass (upper right index), in Joliot both mass (upper right) and charge (lower right) appeared.

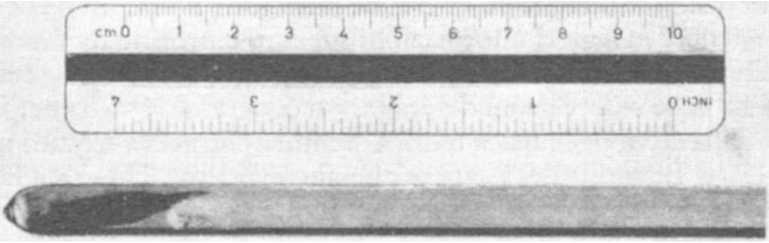

Fig. 5.2 Picture of one of the neutron sources used by Fermi.

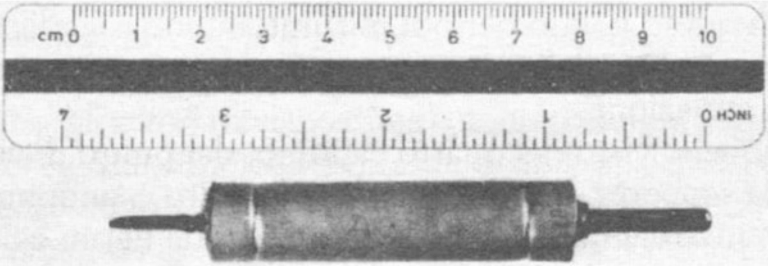

Fig. 5.3 Picture of a Geiger-Müller counter built by Fermi and used in the first experiments on the neutron-induced radioactivity.

chrome, and barium) this analysis of the effect of the bombardment by neutrons. For nine more elements there are only some incomplete records.[9]

The choice of writing short letters, and the choice of the journal "La Ricerca Scientifica," which had very short publication times, were due to the evident attempt to be first in publishing these results; it was clear that other laboratories were working on the same researches (cf. the table in Figure 4.5). The next letters were signed by Amaldi, D'Agostino, Fermi, Rasetti, and Segrè. The participation of Oscar D'Agostino, a young chemist we have already mentioned in the first chapter, was indispensable. Indeed one of the aims of the investigation was to determine with the maximum possible precision the chemical nature of the elements produced in the nuclear reactions.

The work was very intense and was rigidly organized; Fermi was making most measurements and computations. Amaldi was in charge of the instrumentation and the electronics, while Segrè dealt with the substances to irradiate and the neutron sources. The importance of what they were doing was very clear to the Via Panisperna boys. As Segrè reported,

> To communicate it rapidly to our colleagues, we wrote almost weekly short letters to *Ricerca Scientifica,* the journal of the National Research Council, and obtained what we would now call preprints of these letters. The preprints were then mailed to a list of about

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[9] Fermi [85a].

> forty of the most prominent and active nuclear physicists all over the world, and the letters appeared a couple of weeks later in the journal.[10]

Those first papers were important also from the sociological viewpoint; they made Fermi's scientific personality more widely known to the international community. Fermi's figure was no longer constrained within theoretical physics. Fermi's works were not sporadic incursions into the realm of experimental physics, as it had been the case in the past, but were part of a comprehensive, cutting-edge project. This is confirmed by the letter that Ernest Rutherford wrote to Fermi in 1934, after reading Fermi's papers.

> Dear Fermi,
> I have to thank you for your kindness in sending me an account of your recent experiments in causing temporary radioactivity in a number of elements by means of neutrons. Your results are of great interest, and no doubt later we shall be able to obtain more information as to the actual mechanism of such transformations. It is by no means clear that in all cases the process is as simple as appears to be the case in the observations of the Joliot. I congratulate you on your successful escape from the sphere of theoretical physics! You seem to have struck a good line to start with. You may be interested to hear that Professor Dirac also is doing some experiments. This seems to be a good augury for the future of theoretical physics!
> Congratulations and best wishes.
> Yours sincerely, Rutherford[11]

The first of the three letters having four authors is particularly important. In their systematic analysis of all elements in the periodic table, the group also considered uranium. Here the issue of the transuranic elements started. Under neutron bombardment, uranium reacts in different ways; in two cases, the decay time was 1 and 13 minutes, respectively; in the other cases, the group was unable to determine the decay times with sufficient precision. As we saw in Chapter 4, in radiochemistry the discovery of a new decay mode is tantamount to the discovery of a new element. The methods that Fermi and his group used to characterize the elements responsible for the new were those typical of radiochemistry, separating the active principle using chemical reactions, so to establish an analogy between the active principle and the elements with which it separates. The manipulations made by the Roman group led to exclude that the new element was an isotope of uranium or thorium, or of other elements with an atomic number close to that of uranium (86, 87, 88, and 89).

It is not difficult to understand the reason why the new elements, whose existence was known only for their radioactive properties, were supposed to have an atomic number in that range. For the general reasons we have discussed in chapter 4 (see section 4.7), Fermi and his group conjectured that the element with a 13 minute half-life was transuranic:

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[10] E. Segrè, *op. cit.,* p. 74.

[11] *Ibidem.*

> These conclusions, that we are trying to support with further experiments, naturally suggest the hypothesis that the active principle in uranium may have an atomic number of 93 (homologous to rhenium); under this hypothesis, the process could consist in the capture of a neutron by uranium with the formation of a nucleus of U^{239}, which would subsequently undergo β disintegrations.[12]

This was well explained in the first of two review papers published in the journal *Il Nuovo Cimento,*[13] which can be considered as the end of that first stage of the investigation, and discussed in detail the research techniques used in radiochemistry.

> To understand the nuclear reaction which gives rise to the active elements it is indispensable to determine the chemical nature of the latter. This is accomplished using the following technique. It is quite natural to assume that, when a chemical element is bombarded, it transforms into an element with a close atomic number. The number of active atoms that are created is very small (most likely in our experiments this number was never bigger than a billion), and therefore it is impossible to isolate them with the usual techniques of analytical chemistry. Therefore, the irradiated substance is first dissolved. Then small amounts of the elements that are suspected to be isotopes of the active elements are added to the solution. With the techniques of analytical chemistry these elements are separated from the original elements. The various fractions are separately tested with the [radiation] counters to detect in which fraction the active element has separated.[14]

The manipulations made by the Roman group excluded that the new element was an isotope of uranium or thorium, or of other elements with an atomic number close to uranium (i.e., 86, 87, 88, and 89). It is easy to understand the reasons why the new elements, whose existence is only known because of their radioactive properties, are looked for among the elements that in the periodic table are near to uranium. For the general reasons we discussed in Section 4.7, Fermi and his group conjectured that the element with a halving time of 13 minutes was transuranic:

> These conclusions, which we are trying to support with further experimental evidence, lead to the conjecture that the active principle of the element U may have atomic number 93, i.e., that it is a homologue of renium; under this hypothesis, the process might consist in the capture of an electron by the element U, with the formation of a U^{239} which would successively disintegrate by β processes.[15]

The Roman group resumed in a large table the results of three months of experimental work.[16] For each irradiated element, the main results and the techniques used were reported. In the case of uranium, "which gives rise to more complex phenomena,"[17] the authors refer to a separate publication, where Fermi, Rasetti, and D'Agostino, unable to associate the element with a 13-minute halving time with

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[12] Fermi [86a], *CPF I*, p. 650.

[13] Fermi [96, 97].

[14] Fermi [96], *CPF I*, p. 718.

[15] Fermi [86a], *CPF I,* p. 650.

[16] Fermi [97], *CPF I,* p. 730.

[17] Fermi [94], *CPF I*, p. 704.

a known element with an atomic number close to that of uranium, conjectured the existence of a new, transuranic element. The authors, however, proceeded with much caution.

> In view of all these negative results, it seems plausible to consider the already mentioned possibility that the atomic number of this element is greater than 92. If it were the element 93, it would be a homologue of manganese and rhenium. This hypothesis is somehow confirmed by the observed fact that the 13 minute activity is dragged to a precipitate of rhenium sulphide insoluble in HCl. However many heavy elements would precipitate in this way, so this evidence is not so conclusive.
> It would not be easy to distinguish this from the possibility of an atomic number of 94 or 95, as the chemical properties of these elements would quite plausibly be very similar. Most likely, one could obtain useful information about the processes that take place by looking for the emission of heavy particles. However, it would not be possible to observe disintegrations with very long half life, nor very quick disintegrations, as the observation of heavy particles requires to perform chemical manipulations to reduce the substance in a thin film. Under these conditions, therefore, it appears to be quite premature to make too precise hypotheses about the disintegrations that take place. More experiments are needed to clarify the phenomena.[18]

The official announcements did not show a similar caution. Although Fermi was quite annoyed by Corbino's announcement of two new elements, ausenium and hesperium, four years later, in his Nobel Prize speech, he explicitly mentioned the discovery of transuranic elements:

> We attempted, since the spring of 1934, to isolate chemically the carriers of these activities, with the result that the carriers of some of the activities of uranium are neither isotopes of uranium itself, nor of the elements lighter than uranium down to the atomic number 86. We concluded that the carriers were one or more elements of atomic number larger than 92; we, in Rome, use to call the elements 93 and 94 Ausenium and Hesperium respectively.[19]

While Fermi was in Stockholm to receive the prize (December 1938), the same laboratories that until a few years before had supported the hypothesis of the transuranic elements were going to announce the discovery of the nuclear fission. Fermi could not do more than adding a remark to the text of his speech:

> The discovery by Hahn and Strassmann of barium among the disintegration products of bombarded uranium, as a consequence of a process in which uranium splits into two approximately equal parts, makes it necessary to reexamine all the problems of the transuranic elements, as many of them might be found to be products of a splitting of uranium.[20]

It is difficult to understand the reasons why a great scientist does not make a discovery, while having enough data for it. The history of science abounds with such episodes; let us just think of the failed discovery of the neutron by the Curie. In the case of Fermi and the nuclear fission, the Roman group not only did not understand the phenomenon they were observing, but, as we saw in the previous chapter, they

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[18] *Ibid.,* p. 705.

[19] Fermi [128], *CPF I,* p. 1039–1040.

[20] *Ibid.*

also ignored Ida Noddack's suggestion. We do not want to adventure into bold psychological considerations, and will only reproduce some of the recollections of the protagonists of those events.

> The reason for our blindness is not clear. Fermi said, many years later, that the available data on mass defect at that time were misleading and seemed to preclude the possibility of fission. At any rate we believed we had produced transuranic elements when the intense work of 1934 came to a temporary halt with the summer vacation.[21]

And Amaldi wrote:

> It seems to me to remember some discussions among the members of our group, including Fermi, in which the ideas of I. Noddack were hastily set aside because they involved a completely new type of reaction: fission. Enrico Fermi, and all of us grown at his school followed him, were always very reluctant to invoke new phenomena as soon as something new was observed: new phenomena have to be proved! As later developments showed, a much more fruitful attitude would have been to try to test Noddack's suggestion and eventually disproving it. But Fermi and all of us were, in this occasion, too conservative: an explanation of the "uranium case" in terms of what we had found for all lower values of Z was much simpler and therefore preferable. Two reasons or, maybe, two late excuses, why I. Noddack's suggestion was not taken more seriously neither in Rome nor in Berlin or Paris, are the following. Her suggestion of what has turned out to be the correct explanation, appeared as a speculation aiming more to point out a lack of rigor in the argument for the formation of element 93, than as a serious explanation of the observations. This remark seems to be supported by the fact that she never tried, alone or with her husband, to do experiments on irradiated uranium as certainly they could have done. Furthermore in those years the Noddacks had failed in some discredit because of their claim to have discovered element $Z= 43$ that they had called "masurium."[22]

And finally, Teller's viewpoint, as reported by Richard Rhodes:

> Fermi refused to believe [Noddack] ... He knew how to calculate whether or not uranium could break in two ... He performed the calculation Mrs. Noddack suggested, and found that the probability was extraordinarily low. He concluded that Mrs. Noddack's suggestion could not possibly be correct. So he forgot about it. His theory was right [...] but [...] it was based on the [...] wrong experimental information.[23]

Segrè's comment to this interpretation, as reported by Rhodes, was "[it is] possible but not persuasive."[24]

One thing that can be safely said is that Fermi and his group were unable to get rid of the basic principle that at the time regulated the investigations in radiochemistry, namely, the axiom that all radioactive transformations, including the artificial ones, created elements that in the periodic table were close to the original element.

We do not know how much this failure affected Fermi. But there is an anecdote reported by Samuel Allison which is worth recalling:

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[21] E. Segrè, *op. cit.,* p. 76.

[22] E. Amaldi, *From the discovery of the neutron to the discovery of nuclear fission,* Physics Reports 111 (1984), p. 277.

[23] R. Rhodes, *The Making of the Atomic Bomb,* Penguin, London 1986, p. 231.

[24] *Ibid.*

> The first architect's sketches of the laboratory to be built for the Institute for Nuclear Studies at the University of Chicago showed a vaguely outlined human figure in bas-relief over the entrance door. When a group was speculating as to what the figure might represent, Fermi wryly guessed that it was probably a scientist not discovering fission.[25]

5.4 Slow neutrons

"What a stupid thing to have discovered this phenomenon without being able to predict it."[26] This was Enrico Fermi's comment after observing the surprising effects of paraffin on neutrons, and after supplying, just a few hours later, a theoretical justification. In chapters 1 and 3 we described, from different viewpoints, the sequence of events that from 22 October 1934 led Fermi to the discovery for which, four years later, he was awarded the Nobel Prize. Let us analyze in more detail the dynamics of those investigations.

As we already remarked, the surprising fact in the experiments made on 22 October was their counterintuitivity. One would expect that, to enhance the effect of the bombardment, neutrons should be accelerated, and not dampened by letting them go through matter. The explanation found by Fermi on that same afternoon is basically the following. Paraffin is able to slow down neutrons having energies of the order of 1 MeV, that is, very fast. As one knows from the elementary laws regulating the collision of massive bodies, a sizable transfer of energy is only possible in the collision of bodies having roughly the same mass. Paraffin contains hydrogen nuclei, that is, particles having almost the same mass of the neutron. So the neutrons, after many collisions with the hydrogen nuclei in paraffin, lose most of their initial kinetic energy; one says that they *thermalize,* that is, their kinetic energy reaches the value due to the thermal agitation of the particles making up the matter through which the neutrons move.

Why slow neutrons are more effective in inducing radioactivity in silver? At first sight, this fact appears to be paradoxical, as if it were easier to break a door open by gently approaching it rather than pushing it violently. But the paradox disappears if we think that the effect is due to the capture of the neutrons by the nucleus; and it is quite clear that the slower the neutrons are, the higher is the probability that they are captured. If we must swim across a river infested by crocodiles, the probability to be eaten by them decreases if we swim faster.

From 28 October 1934 to 14 June 1935 Fermi and the Panisperna boys published six papers, where they studied the properties of *slow neutrons;* this was the term

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[25] Samuel K. Allison, *Enrico Fermi 1901–1954,* Biographical Memoir, National Academy of Sciences, Washington 1957, p. 129.

[26] B. Pontecorvo, *Enrico Fermi,* Studio Tesi, Pordenone 1993, p. 83.

introduced by the group for the neutrons thermalized by paraffin.[27] The papers described a series of experiments aiming at both checking the relation between the thermalization of the neutrons and their absorption, and describing quantitatively the effect of hydrogenated substances on the activation of the neutrons.[28] Fermi and collaborators introduced a "sensitivity coefficient" α, expressed as the ratio between the activity measured by irradiating the body first inside a big paraffin slab, and then in air.[29] Other experiments aimed at assessing the speed of the neutrons in paraffin, the effect of temperature on the activation, the absorption of slow neutrons as a function of the thickness of the irradiated substance, the relation between the absorption of neutrons and the emission of secondary radiations, and more.

The final goal of the experiments of the Roman group was to understand the structure of the nucleus and the forces inside it. A letter to *La Ricerca Scientifica* dated 14 June 1934[30] (see Figure 5.4), following a suggestion by J. R. Tillman and Philip B. Moon,[31] includes a remark concerning some results about the dependence of the activity of the irradiated substances on temperature. The induced radioactivity increased when paraffin was kept at a lower temperature. As Fermi and his group wrote, "if these differences in the behavior of the various elements are confirmed, one will have to think that the capture of slow neutrons is a more complicated process than what we have so fare believed, based on the usual assumptions about the force among neutrons and nuclei (a potential well of the size of the nucleus). The differences could perhaps be interpreted by assuming that neutron and nucleus also interact with a very weak force with a relatively long radius of action."[32]

The hypothesis that the neutron-nucleus interaction could be described by a potential well, representing the mean field generated by the particles in the irradiated nucleus, was at that time the basic tenet of the theoretical investigations. This was an *ad hoc* hypothesis, evidently borrowed from atomic physics, which provided a first approximation to treat the problems involving the knowledge of the nuclear potential. It expressed a sort of physical pragmatism, which however provided a model of the nucleus that was certainly unsuitable for recognizing the possibility of nuclear fission in the enigmatic behavior of irradiated uranium. The treatment of nuclear fission requires indeed, as we saw in Chapter 4, a more detailed model of the atomic nucleus: Bohr's 1936 compound model.

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[27] Fermi [89a, 90a, 91a, 92a, 105a, 106a]. The authors of those papers are not always the same. Moreover the papers [89a, 90a, 91a, 92a] deal with the properties of hydrogenated substances within a more extended study of the "radioactivity induced by neutron bombardment."

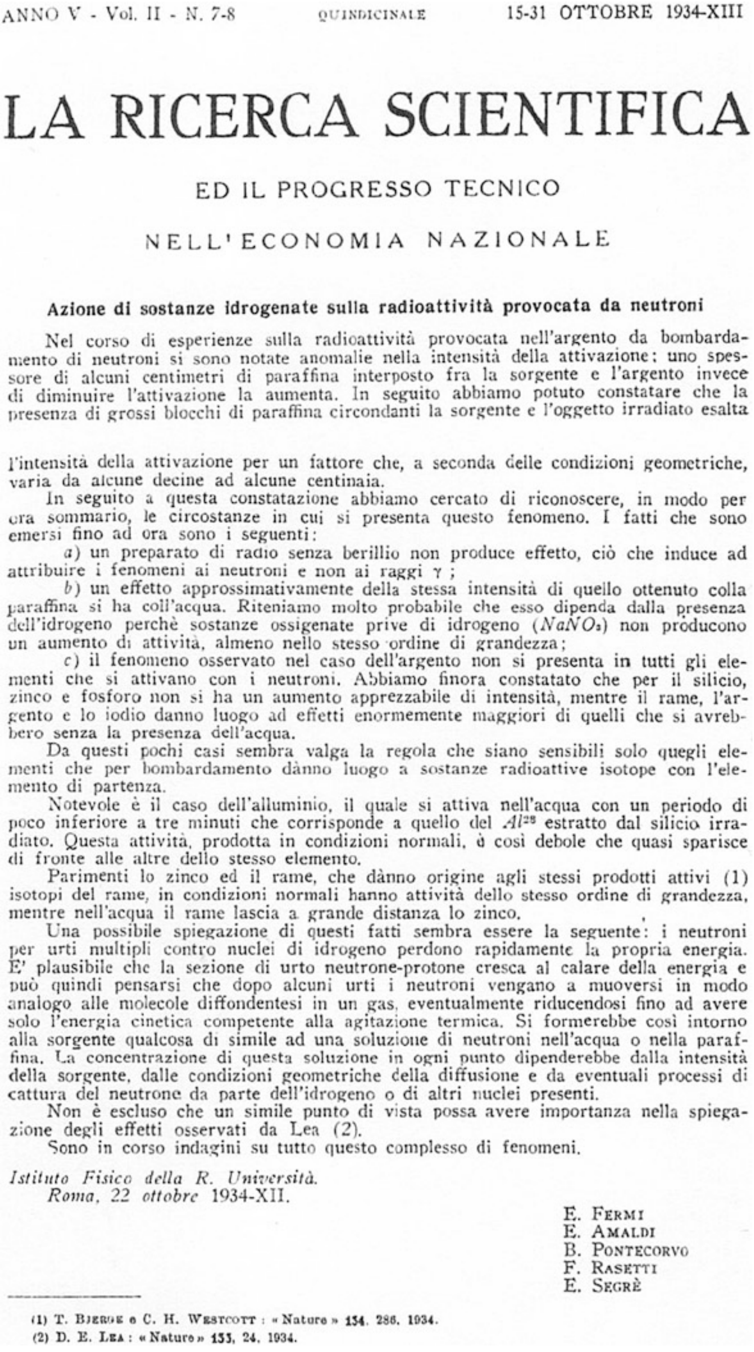
[28] Fermi [89a].

[29] $\alpha = 1$ means that the hydrogenated substance has no effect. The experiments showed a great variability of α according to the examined substance.

[30] Fermi [92a].

[31] J. R. Tillman and P. B. Moon, *Evidence on the velocity of slow neutrons,* Nature 135 (1934), p. 904.

[32] Fermi, *CPF I*, p. 670.



ANNO V - Vol. II - N. 7-8 QUINDICINALE 15-31 OTTOBRE 1934-XIII

LA RICERCA SCIENTIFICA

ED IL PROGRESSO TECNICO

NELL'ECONOMIA NAZIONALE

Azione di sostanze idrogenate sulla radioattività provocata da neutroni

Nel corso di esperienze sulla radioattività provocata nell'argento da bombardamento di neutroni si sono notate anomalie nella intensità della attivazione: uno spessore di alcuni centimetri di paraffina interposto fra la sorgente e l'argento invece di diminuire l'attivazione la aumenta. In seguito abbiamo potuto constatare che la presenza di grossi blocchi di paraffina circondanti la sorgente e l'oggetto irradiato esalta l'intensità della attivazione per un fattore che, a seconda delle condizioni geometriche, varia da alcune decine ad alcune centinaia.

In seguito a questa constatazione abbiamo cercato di riconoscere, in modo per ora sommario, le circostanze in cui si presenta questo fenomeno. I fatti che sono emersi fino ad ora sono i seguenti:

a) un preparato di radio senza berillio non produce effetto, ciò che induce ad attribuire i fenomeni ai neutroni e non ai raggi γ;

b) un effetto approssimativamente della stessa intensità di quello ottenuto colla paraffina si ha coll'acqua. Riteniamo molto probabile che esso dipenda dalla presenza dell'idrogeno perchè sostanze ossigenate prive di idrogeno ($NaNO_3$) non producono un aumento di attività, almeno nello stesso ordine di grandezza;

c) il fenomeno osservato nel caso dell'argento non si presenta in tutti gli elementi che si attivano con i neutroni. Abbiamo finora constatato che per il silicio, zinco e fosforo non si ha un aumento apprezzabile di intensità, mentre il rame, l'argento e lo iodio danno luogo ad effetti enormemente maggiori di quelli che si avrebbero senza la presenza dell'acqua.

Da questi pochi casi sembra valga la regola che siano sensibili solo quegli elementi che per bombardamento dànno luogo a sostanze radioattive isotope con l'elemento di partenza.

Notevole è il caso dell'alluminio, il quale si attiva nell'acqua con un periodo di poco inferiore a tre minuti che corrisponde a quello del Al^{28} estratto dal silicio irradiato. Questa attività, prodotta in condizioni normali, è così debole che quasi sparisce di fronte alle altre dello stesso elemento.

Parimenti lo zinco ed il rame, che dànno origine agli stessi prodotti attivi (1) isotopi del rame, in condizioni normali hanno attività dello stesso ordine di grandezza, mentre nell'acqua il rame lascia a grande distanza lo zinco.

Una possibile spiegazione di questi fatti sembra essere la seguente: i neutroni per urti multipli contro nuclei di idrogeno perdono rapidamente la propria energia. E' plausibile che la sezione di urto neutrone-protone cresca al calare della energia e può quindi pensarsi che dopo alcuni urti i neutroni vengano a muoversi in modo analogo alle molecole diffondentesi in un gas, eventualmente riducendosi fino ad avere solo l'energia cinetica competente alla agitazione termica. Si formerebbe così intorno alla sorgente qualcosa di simile ad una soluzione di neutroni nell'acqua o nella paraffina. La concentrazione di questa soluzione in ogni punto dipenderebbe dalla intensità della sorgente, dalle condizioni geometriche della diffusione e da eventuali processi di cattura del neutrone da parte dell'idrogeno o di altri nuclei presenti.

Non è escluso che un simile punto di vista possa avere importanza nella spiegazione degli effetti osservati da Lea (2).

Sono in corso indagini su tutto questo complesso di fenomeni.

Istituto Fisico della R. Università.
Roma, 22 ottobre 1934-XII.

E. Fermi
E. Amaldi
B. Pontecorvo
F. Rasetti
E. Segrè

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(1) T. Bjerge e C. H. Westcott: «Nature» **134**, 286, 1934.

(2) D. E. Lea: «Nature» **133**, 24, 1934.

Fig. 5.4 The paper by Fermi and his group about the discovery of slow neutrons.

The theoretical consideration leading to an estimate of the cross-section (basically, the probability) of the capture of slow neutrons by the nuclei were published in a lengthy paper signed by Amaldi, D'Agostino, Fermi, Pontecorvo, and Rasetti, which was received by the Proceedings of the Royal Society on 15 February 1935.[33] In addition to a law stating that the neutron capture cross-section is inversely proportional to the speed of the neutrons, the paper included a summary of the results obtained by systematically analyzing the behavior of all elements in the periodic system under neutron bombardment. The paper was not entirely devoted to slow neutrons (its title was indeed *Artificial Radioactivity Produced by Neutron Bombardment*), which were indeed treated as one aspect of a wider project. On the whole, it was a very important paper, which projected the Roman group to the top of the international experimental research. Moreover it marked the beginning of a new chapter of nuclear physics, for which Fermi was an undiscussed reference point: neutron physics.

1935 was an important year; the "small world" that had slowly taken shape in the Roman institute started to fall apart. But Fermi and Amaldi, the survivors of the group, certainly did not stop producing very good science. The starting point of the researches in the fall of 1935 were some papers of Bjerge and Westcott, and Moon and Tillman,[34] which reported about the different absorptions of slow neutrons by different substances. The experiments made by Amaldi and Fermi led to the introduction of the important notion of "selective absorption." Indeed they discovered that many elements display a very high absorption of neutrons having a kinetic energy in some well-defined, characteristic ranges; the neutron cross-section does not simply vary with the inverse of their speed, but has maxima and minima that are characteristic of the irradiated element. These results were certainly not unperceived by the international research community, and were indeed at the basis of Bohr's compound nucleus model. As Bohr wrote,

> Most interesting support for these considerations is afforded by the remarkable phenomena of selective capture of neutrons of very small velocities.[35]

For Bohr the selective absorption was an evidence that the nucleus has quantized energy levels, whose separation is bigger when the energy is lower. For this reason, the selective absorption is particularly evident when the energy of the incident neutrons is low. With increasing energy, the density of the energy levels of the compound nucleus increases as well, till it becomes a practically continuous

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[33] Fermi [107].

[34] T. Bjerge and C. H. Westcott, *On the slowing down of neutrons in various substances containing hydrogen,* Proceedings of the Royal Society 150 (1935), p. 709; J. R Tillman and P. B. Moon, *Evidence on the velocity of slow neutrons, op. cit.* These papers show that if a neutron beam which is not monochromatic (i.e., the neutrons have a continuous energy spectrum) hits a slab of some element, and the fraction of the neutrons that are not absorbed hits a second slab, the absorption by the second slab is very small if the two elements are identical.

[35] N. Bohr, *Neutron capture and nuclear constitution,* Nature 137 (1935), p. 346.

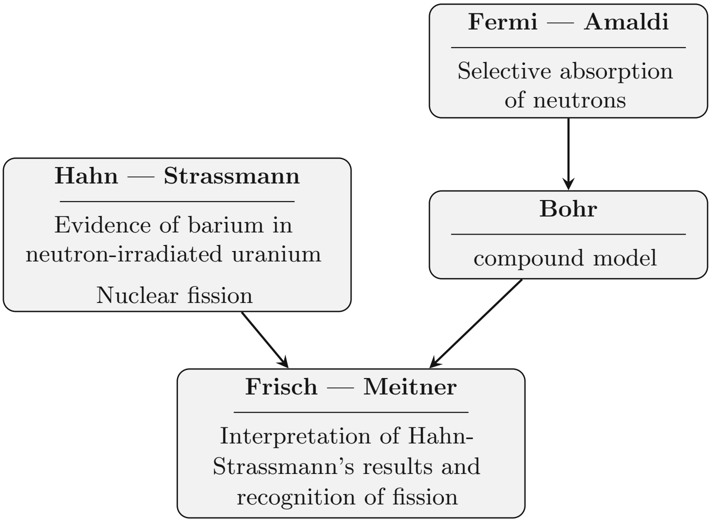

Fig. 5.5 Schematic itinerary of the discovery of fission.

spectrum; when this regime is reached, the selective absorption disappears, and the cross-section of the neutron capture just depends on the inverse of the speed of the incident neutrons.[36]

The diagram in Figure 5.5 shows how the discovery of the selective absorption played a very important role in the global maps. It is indeed reasonable to say that Fermi's contribution to the discovery of fission was twofold, albeit it was not explicitly acknowledged at the time of the issue of the transuranic elements; on the one hand, his papers on neutron-induced artificial radioactivity triggered the researches that lead to Hahn and Strassmann's result; and on the other hand, the discovery of selective absorption provided the experimental motivation for the introduction of the compound model, which eventually allowed for an understanding of nuclear fission.

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[36] Fermi's comment on this subject is quite interesting. "The analysis of the slow neutron absorption curves [...] has allowed us to realize that the elements that are capable of absorbing the slow neutrons usually have one or more characteristic absorption band, corresponding to very narrow intervals for the energy of the neutrons. [...] Bohr, Breit and Wigner interpreted these characteristic bands as due to a resonance between the energy of the slow neutron and a virtual energy level of the nucleus. Bohr has additionally given a suggestive interpretation of the high probability with which the bands appear for the different elements; he suggested that the nucleus has a very high number of degrees of freedom, so that the energy level density increases very quickly with the excitation energy of the nucleus. According to this proposal, in a nucleus of medium weight the low excitation energy levels would be separated by some tens of hundreds volt. For energies of 8 or 10 million volt the separation between consecutive energy levels would be enormously smaller, of some tens of volts or even less. For even greater excitation energies, the energy levels would become less and less separated, till their width would become smaller than the average distance between two consecutive levels" (Fermi [112], *CPF II,* p. 1025–1026).

The discovery of selective absorption was definitely the most important result of the collaboration between Fermi and Amaldi in the years just before the Nobel Prize. During the same period, however, the collaboration between the two scientists yielded other results, that were to be of great importance during the World War II. They introduce the notion of *neutron age,*[37] a term that was then used by Fermi to designate the dependence of the energy of the neutrons on the distance they cover in the medium they travel. In particular they made reference to the quantity $\bar{r}^2/6$, where $\bar{r}^2$ is the mean value of the square of the distances of the neutrons from the source. Also the computation of *albedo* was important, i.e., the reflection coefficients of the neutrons that hit a slab of paraffin.[38] The analysis of the motion of neutrons as a function of their energy (diffusion equation) was also very important, as well as the analysis of their angular distribution after crossing the substance that slows them down.

All these problems, as observed by Amaldi, were analyzed by using

> ...a one dimensional medium model, [...], which was enough, according to Fermi, to treat most diffusion problems. According to Fermi's opinion, [more] refined mathematical method would have been disappointing, as the uncertainty about the physical hypotheses that underlay the computation were far greater than the mathematical difficulties presented by the model.[39]

The second part of the theoretical paper where the diffusion equation was obtained was devoted to the "quantum-mechanical study of the collision between slow neutrons and the protons in paraffin."[40] According to Fermi, the aim was

> ...to analyze the collision mechanism, taking account of the fact that the hydrogen atoms are not free, but rather can be considered as elastically bound to an equilibrium position; we shall discuss the processes leading to the capture of the neutron by a proton, with the formation of a deuterium atom.[41]

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[37] This is was Amaldi referred about the use of the term "age" in connection with neutrons: "Rasetti wrote us telling what was happening at Columbia; Halban and Preiswerk's reprints kept us abreast of the work done in Paris; and the correspondence with Placzek kept us in touch with Copenhagen. We learnt about Bohr's work thanks to this correspondence [...] In Rome the joke circulated that, as the age of a captain can be estimated by the height of the masts of his ship, in the same way the energy of a bunch of neutrons could be estimated by measuring the distance at which it diffuses. The expression "age" [...] goes back to that period. At the beginning we said "the captain's age" to refer to experiments about the transformation of a bunch of neutrons to another bunch of smaller energy." (E. Amaldi, *CPF I,* p. 810).

[38] "We shall call albedo $\beta(a)$ of a layer of thickness a the probability that a neutron, after one or two collisions, exits from the same side it entered." (Fermi [119], *CPF I* p. 954). A small controversy arose (see a letter to *Physical Review,* Fermi [125]) about a paper by O. Halpern, R. Lueneburg, and O. Clark (*On multiple scattering of neutrons I. The theory of albedo of a plane boundary,* Physical Review 53 (1938), p. 173) where seemingly different results were obtained. However the discrepancy was due to different definitions.

[39] E. Amaldi, *CPF I,* p. 810.

[40] Fermi [119a], *CPF I,* p. 965.

[41] *Ibid.*

This ended Fermi's collaboration with Amaldi in this kind of investigations. It would be wrong, however, to see this as the last act of a research itinerary. As we shall soon see, the activity of these years laid the bases and gave Fermi the essential tools for his work at Columbia.

5.5 The end of the Italian years

Usually Fermi's work is divided into two periods, the "Italian" and the "American" years. This has of course a precised biographical meaning, but we support the idea that Fermi's emigration to the United States did not mark a radical change in his research, which actually took place a few years before, in the years 1933–1934; Fermi's masterpiece on the theory of the β decay ended a substantially theoretical period of his scientific life, giving place to his experimental work, which started in Italy and went on in the United States.

Let us examine the last scientific contributions and remarkable events that took place in Fermi's life in 1937–1938. In that period he published four papers; only two of them however referred to experiments he had made, and between those two, only one was the base of a future research project.[42] The project stemmed from

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[42] Fermi [121–125]. Paper [121] laid the bases of the project we are describing here. [125] was about the verification and explanation of some results by J. L. Michiels (*Anomaly in the apparent absorption of slow neutrons by iodine and boron,* Nature 142 (1938) p. 431), who, in some experiments about the activation of iodine by means of slow neutrons, observed a singular phenomenon: if iodine and boron screens were put on the trajectories of the neutrons, the absorption was different according to the order of the screens. If boron came before iodine, the absorption was about 75% with respect to the other case. Fermi and Rasetti's explanation, which was also tested experimentally, made use of the idea of "selective absorption." We report here the original explanation, which, according to Fermi's usual style, is very clear: "[. . .] we thought that this might be due to a variation of the energy of the neutrons when they cross the boron screen, due to elastic collisions. Usually one observes no scattering of slow neutrons by boron as this is masked by the very strong absorption, which, for thermal neutrons, is about a hundred times larger. For neutrons with larger energy, scattering becomes more important, since the scattering coefficient is roughly constant, while the absorption coefficient is inversely proportional to the neutron speed. Among the elements which display a selective absorption, iodine has definitely one of the largest resonance energies (of the order of 100 volt), and it is therefore plausible that in this energy range the cross-sections for elastic collision and capture are comparable. This is even more plausible for boric oxide, where the scattering effects of oxygen and boron add up. One can therefore explain the observed facts as follows. The first screen (iodine) subtracts from the continuous spectrum of the neutrons one or more narrow bands, corresponding to the characteristic spectrum of this element. In the next filter (boron) an actual absorption of the neutrons takes place, thus decreasing the intensity. But there are also elastic collision of neutrons with boron atoms. If these had a practically infinite mass, the energy of the neutrons would not be changed by these collisions, and the spectral distribution would be unchanged. But the mass of a boron atom is only ten times bigger than that of a neutron, so that also in an elastic collision the energy of the neutron decreases sensibly. Due to this effect, some neutrons whose energy is just a little larger that the iodine band, and had therefore crossed the first filter without being absorbed, are taken into that band by elastic collisions, and

the remark that the natural neutron sources no longer allowed for a cutting-edge research, which could permit the Roman group to maintain the international primacy it had reached. Edoardo Amaldi expressed the situation very well when he reported about an international conference, for which no proceedings were published, which took place in Copenhagen on 14 to 20 June 1936, on the topic "Problems in the physics of atomic nucleus." All active research groups were represented; Bohr reported on a compound model of the atomic nucleus and its application to nuclear reactions, Heisenberg talked about the proton-neutron model and the nuclear forces, Meitner about some issues related to uranium, Amaldi reported about the work with neutrons in Rome, etc. For the Roman physicist that conference marked the end of the experimental techniques that had been used so far in Rome.

> These methods had allowed a clarification of the main features of the interaction of slow neutrons with nuclei, but by the time of the conference all of them had become inadequate. There was a need of more intensive neutron sources and of new tools, in particular of neutron spectrometers. Cyclotrons and other accelerators already existed in a few laboratories, mainly in U.S.A., but many more were needed in other places.[43]

We have indeed already reported in chapter 1 about Fermi's attempts to create a National Institute for Radioactivity, and to build for it a large accelerating machine.

The paper published in 1937 with Amaldi and Rasetti[44] was a preliminary attempt to deal with the new requirements of the experimental research, waiting for the possibility to undertake more momentous projects. They reported on the construction of a small electrostatic accelerator, capable of accelerating deuterons with a voltage of 200,000 volt. It was not a large voltage, but using a reaction discovered by Oliphant and Harteck in 1934[45]

$$ {}^{2}_{1}\mathrm{D} + {}^{2}_{1}\mathrm{D} \rightarrow {}^{3}_{2}\mathrm{He} + {}^{1}_{0}\mathrm{n}, $$

the three scientists were able to obtain some very encouraging results; the neutron source so obtained was about three times stronger than the sources used up to that moment. However, in presenting these results the Roman group was aware that they were working on a provisional project, as they made clear in the conclusions of their paper: "Obviously, one could get much bigger activities if voltages of the order of 1000 kV were available."[46] The 1000 kV accelerator was then built, not in the new Institute of Physics, but in the National Health Institute. Fermi was already in America when it started working.

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therefore contribute to the activation of the detector. This effect evidently does not take place when the boron and iodine filters are set in the inverse order. This gives a qualitative justification of the different observed intensities in the two cases." (Fermi [125], *CPF I,* p. 1029).

[43] E. Amaldi, *From the discovery of the neutron to the discovery of nuclear fission, op. cit.*, p. 211.

[44] Fermi [121].

[45] M. L. E. Oliphant and P. Harteck, *Transmutation effects observed with heavy hydrogen,* Proceedings of the Royal Society 144 (1934), p. 692.

[46] Fermi [121], p. 1024.

The Italian years ended with the award of the Nobel Prize. Fermi's acceptance speech resumed the main results of the Roman group over the last five years,[47] recalling the experiments on neutron-induced artificial radioactivity, and claiming the discovery of two transuranic elements, ausenium and hesperium. He gave ample space to slow neutrons, stressing in particular the selective absorption and how it found a suitable explanation in terms of Bohr's compound nucleus model.

His last words were devoted to his collaborators and the various institutions that had allowed him, in different ways, to obtain those great results. In Stockholm, as the last act before sailing for America, he wrote a thank you letter to Adolfo Amidei, an engineer, his father's colleague and friend who had encouraged him to pursue the scientific studies, almost to symbolize the end of a lucky period of his life.

5.6 Nuclear energy and wartime physics. The atomic pile

The news about nuclear fission was not only spread in the international scientific community by the journal where Frisch, Hahn, Meitner, and Strassmann published their results. A very important role was also played by the 5th "Washington Conference" in theoretical physics, which took place from 26 to 28 January 1939. It was part of a monographic series of conferences founded by George Gamow. The topic for 1939 was low temperature physics. There were 51 participants; in addition to Stern, Uhlenbeck, Gamow, Teller, and Bethe, also Bohr and Fermi attended. However the physics of low temperatures was left aside, after the meeting was opened by the discussion of the stupefying results obtained by Hahn and Strassmann, and of the interpretation provided by Meitner and Frisch.

> The Fifth Washington Conference on Theoretical Physics, sponsored jointly by George Washington University and the Carnegie Institution of Washington, began January 26, 1939, with a discussion by Professor Bohr and Professor Fermi of the remarkable chemical identification by Hahn and Strassmann in Berlin of radioactive barium in uranium which had been bombarded by neutrons. Professors Bohr and Rosenfeld had brought from Copenhagen the interpretation by Frisch and Meitner that the nuclear "surface-tension" fails to hold together the "droplet" of mass 239, with a resulting division of the nucleus into two roughly equal parts. Frisch and Meitner had also suggested the experimental test of this hypothesis by a search for the expected recoil-particles of energies well above 100,000,000 electron-volts which should result from such a process. The whole matter was quite unexpected news to all present.[48]

This communication had such an impact on the participants that two of them, Richard B. Roberts and Lawrence R. Hafstad, immediately organized an experiment to check the phenomenon announced by Bohr. The experiment took place at the

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[47] Fermi [128].

[48] R. B. Roberts, R. C. Meyer and L. R. Hafstad, *Droplet fission of uranium and thorium nuclei,* Physical Review 55 (1939), p. 416.

Atomic Physics Observatory, and the results were shown to Bohr and Fermi in the evening of January 28th:

> We immediately undertook to look for these extremely energetic particles, and at the conclusion of the Conference on January 28 were privileged to demonstrate them to Professors Bohr and Fermi.[49]

The news of the nuclear fission spread very quickly not only thanks to the scientists, but also because the press became very interested. The discovery was announced in the *Washington Evening Star,* the *New York Times,* and the *San Francisco Chronicle* with rather sensationalist tones. Why all that fuss? It was just the great production of energy that called for such attention? Perhaps nuclear fission was not just a laboratory phenomenon, but could provide an energy source than could be used by man? An answer was provided by Fermi, who so remembered those days:

> ... there was a meeting in Washington organized by the Carnegie Institution in conjunction with George Washington University where I took part with a number of people from Columbia University and where the possible importance of the new discovered phenomenon of fission was first discussed in semi-jocular earnest as a possible source of nuclear power. Because it was conjectured, if there is fission with a very serious upset of the nuclear structure, it is not improbable that some neutrons will be evaporated. And if some neutrons are evaporated, then they might be more than one; let's say, for the sake of argument, two. And if they are more than one, it may be that the two of them, for example, may each one cause a fission and from that one sees of course a beginning of the chain reaction machinery.[50]

A chain reaction was immediately regarded as a possibility to realize a new weapon of enormous power. There are several testimonials of this. Charles Weiner reported that Philip Morrison remembered to have seen the drawing of a bomb on a blackboard in Oppenheimer's office the week after.[51] Fermi, as reported by Richard Rhodes on one of those days, while in front of a window in his office at Columbia which afforded a view of the entire Manhattan island, "cupped his hands as if he were holding a ball. 'A little bomb like that,' he said simply, for once not lightly mocking, 'and it would all disappear.'"[52]

What was the actual possibility to start a chain reaction? During the first days in February 1939 Fermi thought they were rather scant, not more than 10%. Leo Szilard reported a conversation with Rabi.

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[49] *Ibid.*

[50] J. W. Cronin, *Fermi Remembered,* The University of Chicago Press, Chicago 2004 p. 91.

[51] Ch. Weiner, *Exploring the history of nuclear physics,* Proceedings of the American Institute of Physics 7 (1972), p. 90.

[52] R. Rhodes, *The Making of the Atomic Bomb, op. cit.*, p. 275. This anecdote is also reported by Pais: "I vividly recall Uhlenbeck telling me how one day Fermi had stood at the window of their office in Pupin Laboratory, looking out over the city, then had turned around and said: 'You realize, George, that one small fission bomb could destroy most of what we see."' (A. Pais, *Niels Bohr's times, op. cit.*, p. 461).

> I said to him: “Did you talk to Fermi?” Rabi said, “Yes, I did.” I said, “What did Fermi say?” Rabi said, “Fermi said ‘Nuts!’” So I said, “Why did he say ‘Nuts!’?” and Rabi said, “Well, I don’t know, but he is in and we can ask him.” So we went over to Fermi’s office, and Rabi said to Fermi, “Look, Fermi, I told you what Szilard thought and you said ‘Nuts!’ and Szilard wants to know why you said ‘Nuts’!” So Fermi said, “Well … there is the remote possibility that neutrons may be emitted in the fission of uranium and then of course perhaps a chain reaction can be made.” Rabi said, “What do you mean by ‘remote possibility’?” and Fermi said, “Well, ten per cent.” Rabi said, “Ten per cent is not a remote possibility if it means that we may die of it. If I have pneumonia and the doctor tells me that there is a remote possibility that I might die, and it’s ten percent, I get excited about it.”[53]

The scientific community was well aware of the potential of nuclear fission for warfare, as confirmed by Szilard’s initiative to keep the secret about the results obtained in that area of research. Fermi’s attitude was completely different, as appears from his convictions about the “10% remote possibility.” Appendix B.4 includes a reconstruction of the discussions about the secrecy problem during those years, made by Fermi at a meeting at Columbia on 30 January 1954. The issue became very important in 1940, when the feasibility studies about the chain reaction started showing promising results. Indeed, in June 1940 a decision was taken to create a committee for the control of the scientific publications (Reference Committee). The committee was chaired by Luther P. Eisenhart, and his collaborators were Gregory Breit, W. M. Clark, Harvey Fletcher, E. B. Fred, George B. Pegram, Harold C. Urey, L. H. Weed, and E. G. Wever. The committee moreover was divided into subcommittees; one of them, devoted to the control of the work on the uranium fission, was chaired by Breit, and its members were Jesse W. Beams, Lyman J. Briggs, Pegram, Urey, and Eugene Wigner. The initiative was endorsed by 237 scientific journals.[54] The procedure for checking the information was easy; the managing editors of the journals sent the papers to Breit, directly or via Eisenhart. Breit in turn sent them to all members of the committee, gathered the opinions about the appropriateness of the publication of the papers, and informed the editors. It is interesting to report Smyth’s comment.

> This arrangement was very successful in preventing publication and was still nominally in effect, in modified form, in June 1945. Actually the absorption of most physicists in this country in war work of one sort of another soon reduced the number of papers referred to the committee practically to the vanishing point. It is of interest to note that this whole arrangement was a purely voluntary one; the scientists of the country are to be congratulated on their complete cooperation. It is to be hoped that it will be possible after the war to publish these papers at least in part so that their authors may receive proper professional credit for their contributions.[55]

Bohr in early 1939 formulated a conjecture which turned out to be a fundamental importance for the development of the researches on nuclear energy. As reported by

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[53] *Ibid.,* p. 280.

[54] This datum is taken from *The National Academy of Sciences: The First Hundred Years, 1863–1963,* National Academies Press, Washington DC 1978, p. 387.

[55] H. DeWolf Smyth, *Atomic Energy for Military Purposes,* Princeton University Press, 1945, p. 46.

George Placzek, the two uranium isotopes ^{235}U and ^{238}U behave in a very different way with respect to fission. To understand the issue, let us consider the mechanism leading to the fission of a nucleus of uranium. A neutron hits a uranium nucleus A and it is absorbed, giving rise to a new nucleus B. The nucleus B starts oscillating, and if the energy of the incident neutron was big enough, it splits into two fragments having approximately the same mass. It is quite natural to think that the higher the capture probability is, the higher is also the probability that the nucleus splits. By analyzing the experimental data, Placzek noticed a resonance peak for the neutron absorption at 25 eV, while for fission there is no such peak. In other terms, the 25 eV neutrons, which are quite easily absorbed by ^{238}U, instead of producing a marked increase of the fission, give rise to another isotope, ^{239}U, which is not fissile. Thorium 232 shows the same peculiarity; it has an absorption resonance at 25 eV, but, differently for uranium, it does not undergo fission when irradiated with slow neutrons.

A possible solution to the problem was published by Bohr in a short paper which, very much in his style, contained no equations.[56] Bohr's suggestion was that fission does not take place for ^{238}U, but only for ^{235}U, a much rarer isotope, is fissile. It is this isotope, which has no resonance at 25 eV, which splits under slow neutron bombardment. Bohr's idea had no experimental support; it was just a conjecture, and as such, not all physicists accepted it at the beginning. Fermi, in particular, as reported by Pais,[57] was very skeptical.

This idea was developed by Bohr and Wheeler in a paper published in September 1939.[58] Their paper contains an ample discussion of the drop model, and includes a computation of the critical fission energy, namely, the minimal vibrational energy of the nucleus capable of producing fission. One should remember that the nuclei of the isotopes ^{238}U and ^{235}U, after capturing a neutron, transform into ^{239}U and ^{236}U, respectively. The computation was made for the isotopes ^{239}U and ^{236}U, yielding 5.9 MeV for the former and 5.3 MeV for the latter. These values were then compared with the binding energy of the captured neutron, obtaining 5.2 MeV and 6.4 MeV, respectively. It was therefore evident that for ^{239}U the captured neutron must have an energy of at least 0.7 MeV to produce fission, while for ^{236}U no kinetic energy is necessary to activate fission. This is why, according to Bohr, only ^{235}U is fissile by slow neutrons. One had however to wait until 1940, when the first uranium samples were enriched (i.e., their content in ^{235}U was increased), to have an experimental confirmation of Bohr's hypothesis.

As reported by Fermi, at Columbia two different approaches to the concrete feasibility of a chain reaction were attempted. The first was followed by Dunning and Booth, and consisted in separating the ^{235}U isotope from ^{238}U; the other was

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[56] N. Bohr, *Resonance in uranium and thorium disintegrations and the phenomenon of nuclear fission,* Physical Review 55 (1939), p. 418.

[57] A. Pais, *Niels Bohr's times, op. cit.*, p. 457.

[58] N. Bohr and J. A. Wheeler, *The mechanism of nuclear fission,* Physical Review 56 (1939), p. 426.

followed by Fermi and Anderson, and aimed at realizing the chain reaction using natural uranium:

> Well, therefore, in those early years near the end of 1939 two lines of attack to the problem of atomic energy started to emerge. One was as follows. The first step should be to separate in large amounts, amounts of kilograms or maybe amounts of tens of kilograms or maybe of hundreds of kilograms, nobody really knew how much would be needed, but something perhaps in that order of magnitude, separate such at that time fantastically large-looking amounts of uranium 235 and then operate with them without the ballast of the associated much larger amounts of uranium 238. The other school of thought was predicated on the hope that perhaps the neutrons would be a little bit more and that perhaps using some little amount of ingenuity one might use them efficiently and one might perhaps be able to achieve a chain reaction without having to separate the isotopes, a task as I say that at that time looked almost beyond human possibilities. Now I personally had worked many years with neutrons, and especially slow neutrons, so I associated myself with the second team that wanted to use nonseparated uranium and try to do the best with it. Early attempts and studies, discussions, on how to separate the isotopes of uranium were started by Dunning and Booth in close consultation with Professor Urey. On the other hand, Szilard, Zinn, Anderson, and myself started experimentation on the other line whose first step involved lots of measurements.[59]

The "lots of measurements" mentioned by Fermi were reported in four papers published in 1939,[60] which aimed at providing quantitative answers to some fundamental questions. As Anderson wrote,

> Fermi knew what questions he wanted to answer. Were neutrons emitted in the fission of uranium? If so, in what numbers? How could these neutrons be brought to produce further fissions? What competitive processes were there? Could a chain reaction be developed?[61]

The first paper was a letter to *Physical Review* signed by Anderson, Booth, Dunning, Fermi, Glasoe, and Slack. It was a preliminary work aiming at quantitatively determining the most important parameter, the cross-section. The measurements made by Fermi and his group, using a source of neutrons made of radon and beryllium, provided the values for the cross-section for both slow and fast neutrons.

After that first work, Fermi's interest shifted to the main problem of chain reaction, namely, the production of neutrons during the fission. The second letter to *Physical Review,* entitled "Production of Neutrons in Uranium Bombarded by Neutrons," was signed by Anderson, Fermi, and Hanstein. They wrote:

> It is conceivable that the splitting of the uranium nucleus may have associated with it the emission of neutrons. These could either evaporate from highly excited fragments (this process is made more probable by the large neutron excess of the fragments, which lowers the binding energy of the neutrons) or be emitted at the instant of fission. This letter is a preliminary report on experiments undertaken to ascertain whether, and in what number,

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[59] Fermi [269], *CPF II,* p. 999.

[60] Fermi [129, 130, 132].

[61] H. L. Anderson, *CPF II,* p. 1.

> neutrons are emitted by uranium subject to neutron bombardment, and also whether the number produced exceeds the total number absorbed by all processes whatever.[62]

It is an explicit declaration that the investigations were addressed to a practical realization of a chain reaction, which is possible only if during the fission there is an emission of neutrons, and the neutrons emitted are more than the absorbed one. The results showed that there was an emission of neutrons, in accordance with other experiments simultaneously made by Szilard and Zinn,[63] which provided a first estimate of the number of neutrons (about two) produced during the fission of a uranium nucleus, and by Halban, Joliot, and Kowarski.

The basic principle of the experiment was easy. The source was put in the center of tank full of water (which slows down the neutrons) and the amount of neutrons was measured at various distances from the source, in the presence or absence of uranium. This procedure allowed the experimenters to estimate the order of magnitude of the absorbed and emitted neutrons.[64] Fermi's result was that

> These two contributions are of the same order of magnitude and the present accuracy is inadequate to decide which one is larger.[65]

This information was vital for the realization of a self-sustained chain reaction, as one could understand how many of the neutrons absorbed by the nucleus could produce a fission. The absorption process was the one already here described, which had a resonance at 25 eV: the isotope ^{238}U captures a neutron, transforming into ^{239}U, which decays with a half time of 23 minutes emitting β rays. One question was if this is the only process which gives rise to fission, or there are others. This problem was addressed by Fermi and Anderson in the third paper. Their results suggested that the process above described was the only one taking place, but their measurements were too imprecise to be definitive.[66] After many years Fermi, recalling those days,

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[62] Fermi [130], *CPF II,* p. 6. To better understand the position of the three scientists, let us consider the case of ^{238}U. Its nucleus has 92 protons and 146 neutrons. The great number of neutrons is explained by the necessity to balance, with their attractive force, the Coulombian repulsion among the protons; they act as a sort of nuclear glue. When ^{238}U fissions, let us say in two equal fragments, each fragment has 46 protons and 73 neutrons. The number of protons corresponds to palladium, whose heavier isotope, $^{110}_{46}$Pd, only has 64 protons; indeed, in this case, with fewer protons, there is less need of "nuclear glue." As the fission fragment has 9 extra neutrons, it is quite likely that they "evaporate."

[63] L. Szilard and W. H. Zinn, *Instantaneous emission of fast neutrons in the interaction of slow neutrons with uranium,* Physical Review 55 (1939), p. 799.

[64] The number of neutrons could be computed, for instance, by measuring the radioactivity that they induced on a small slab of rhodium. This element, when hit by neutrons, produces a radioactive isotope with a half-life of about three minutes.

[65] Fermi [130], *CPF II,* p. 6.

[66] Summing the fission cross-section of 2×10^{-24} cm^2 with the above cross-section for simple capture of 1.2×10^{-24} cm^2 we find as the total absorption cross-section for thermal neutrons 3.2×10^{-24} cm^2. Because of the large error which can affect such measurements, this may not be inconsistent with the value 5×10^{-24} cm^2 previously reported (Fermi [130]) from absorption measures, or with the value 5.9×10^{-24} cm^2 reported by Michiels, Parry, and Thompson

said “I have never yet quite understood why our measurements in those days were so poor.”[67]

The conclusive paper “to ascertain whether and to what extent the number of neutrons emitted exceeds the number absorbed” was the fourth of the series, signed by Anderson, Fermi, and Szilard.[68] As we noted in Chapter 1, it is the only paper coauthored by Fermi and Szilard, although the two scientists afterwards went on collaborating for a long time. Szilard had a key role in the collaboration; it was him indeed who procured the 360 pounds of uranium oxide necessary for an experiment on a much larger scale than those done before, and the neutron source, made of radium and beryllium.[69] Figure 5.6 contains a sketch of the experimental setup. It was a tank filled with about 130 gallons of a water solution of manganese sulphate. While water slowed down the neutrons, the manganese served to detect them.[70] For the first time, the uranium oxide is not dissolved in water but is contained in 52 cylinders arranged around the source.[71]

The number of neutrons was measured with the uranium oxide and without, and the results did not leave room to doubt; when the cylinders were in the tank, the number of neutrons increased by a 10 percent. According to Fermi, that corresponded

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(J. L. Michiels, G. Parry and G. P. Thompson, *Production of neutrons by the fission of uranium,* Nature 143 (1939), p. 760). If, instead, the total absorption is considerably larger, as reported by Whitaker *et al.* (M D. Whitaker, Ch. A. Barton, W. C. Bright and E. J. Murphy, *The cross-section of metallic uranium for slow neutrons,* Physical Review 55 (1939), p. 793), there must be some other process of absorption to account for the difference (Fermi [131], *CPF II,* p. 10).

[67] Fermi [269], *CPF II,* p. 999.

[68] Fermi [132], *CPF II,* p. 11–12.

[69] Some clarifications are in order concerning the kind of neutron sources that were used. In the previous experiments made by Fermi the source was made by radon and beryllium, which emit high energy, fast neutrons. In that case the increase in the number of neutrons observed by Anderson and Fermi could be interpreted as being due not only to neutrons emitted during the fission, but also to neutrons expelled by the target nuclei; in other words, the very energetic neutrons emitted by the radon-beryllium source could extract some neutrons from the target nuclei without inducing any fission. This problem disappeared by using a source made by radium and beryllium, as Szilard did by renting the radium, thanks to a contribution of 2,000 dollars from his friend Benjamin Liebowitz. In this case the emitted neutrons had smaller energies and could not extract neutrons by colliding with the target nuclei.

[70] Manganese, after irradiation by neutrons, gives rise to an isotope with a half-life of about three hours.

[71] As remarked by C. Bernardini and L. Bonolis (*op. cit.*, p. 177): “The problem of capturing the epithermal neutrons seemed insurmountable. Anderson said (E. Fermi [132]) that it only took Fermi about twenty minutes to find a solution. It was to concentrate the U oxide in separate blocks (heterogeneous structure) instead of distributing it homogeneously in the water (as the French in Joliot ’s group had done). This would drastically reduce the capture of the neutrons as they slowed down. This precaution, which derived from Fermi’s great mastery of the behaviour of neutrons, was a fundamental turning point in story of nuclear energy. Without it it would never have been possible to reach a self-sustaining chain reaction with natural uranium.”

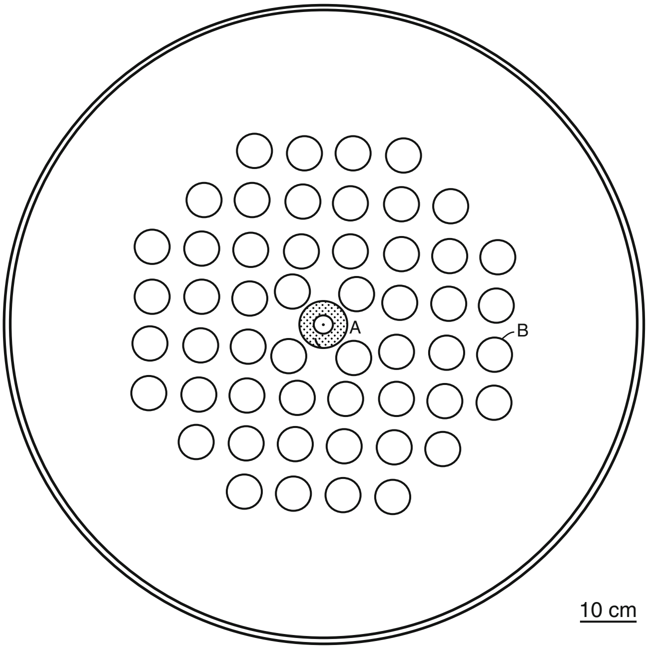

Fig. 5.6 Horizontal cross-section of the experimental setup used by Anderson, Fermi, and Szilard. *A* denotes the neutron source, made of radium and beryllium, and *B* is one of the cylinders (having a diameter of 2 inches and a height of 2 feet) containing the 200 kg of uranium oxide provided by Szilard.

> ...to an average emission of about 1.2 neutrons per thermal neutron absorbed by uranium. This number should be increased, to perhaps 1.5, by taking into account the neutrons which, in our particular arrangement, are absorbed at resonance in the non thermal region by uranium, without causing neutron emission.
> From this result we may conclude that a nuclear chain reaction could be maintained in a system in which neutrons are slowed down without much absorption until they reach thermal energies and are then mostly absorbed by uranium rather than by another element. It remains an open question, however, whether this holds for a system in which hydrogen is used for slowing down the neutrons.[72]

Thus, the summer of 1939 began with an important result, and an open problem: the theoretical feasibility of the chain reaction, and the ambivalent role played by the hydrogen in water. If on the one hand hydrogen was necessary to slow down the neutrons, on the other hand it hindered the fission, since it absorbed a sizeable number of neutrons. Fermi spent that summer in Ann Arbor, where he gave a course in theoretical physics. As reported by Anderson, during that period Fermi kept a

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[72] Fermi [132], *CPF II,* p. 13.

close correspondence with Szilard,[73] whose object was the choice of a “moderator,” namely, the substance to be used to slow down the electrons in a chain reaction. The choice of the moderator was crucial. It had to have two features: to have a light nucleus, so to slow down the incident neutrons, and a small cross-section, that is, it should not absorb too many electrons. If water was not suitable, one could try with heavy water, i.e., deuterium instead of hydrogen; it satisfied both requirements, but was difficult to produce, and very expensive. Inspecting the periodic table, the second suitable element one finds is carbon.

Fermi and Szilard had, simultaneously and independently, the idea of using a uranium-carbon structure for a chain reaction. The idea of using carbon as moderator in the form of graphite was conceived in the summer of 1939. It was a daring idea, since not much was known about the behavior of carbon under neutron bombardment, and a quite demanding one. According to Fermi’s estimates, 39 tons of carbon were needed, and 1300 pounds of uranium; Szilard’s estimates were of 500 tons of carbon, and 5 tons of uranium. The choice of graphite was not obvious. The problem was debated between the fall of 1939 and the winter of 1940 by, among others, Pegram, Szilard, and Anderson; eventually, they agreed that graphite was the best choice, also in view of its availability.

In February 1940 Fermi was in Berkeley to give a series of lectures (the Hitchcock lectures). The first experiments to estimate the absorption of neutrons by graphite started after he was back at the end of the month. Meanwhile, Briggs’ Advisory Committee met for the first time, and decided to assign Fermi and Szilard a grant of 6000 dollars. The year 1940 was basically devoted to a detailed quantitative study of the properties of graphite with respect to the slowing down of neutrons and the absorption of thermal neutrons. In Fermi’s words,

> When fast neutrons are emitted by a source inside a slowing substance two essentially different diffusion processes take place. In the first, the fast neutrons collide many times with the nuclei of the substance, thereby losing energy until they reach thermal energy. After this the second diffusion process begins; the neutrons continue to diffuse through the material but without further loss of energy until they are finally absorbed. [...] We have investigated these two processes in graphite.[74]

The two processes were conceptually different, and the paths of the neutrons were interpreted in different ways. Again in Fermi’s words,

> The length of the diffusion path in the slowing-down phase depends on the number of collisions required for reducing the initial energy of the neutron to thermal energy. The length of the path during thermal diffusion depends on the absorption cross-section of the given material for thermal neutrons.[75]

The measurements of the slowing down of the neutrons were made using a structure formed by graphite bricks, in the shape of a parallelepiped with a square

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[73] H. L. Anderson, *CPF II,* p. 15.

[74] Fermi [136], *CPF II,* p. 32.

[75] *Ibid.*

base, 36 inches wide and 96 inches high. The neutron source, made by radon and beryllium, was set at the base of the parallelepiped. The detectors were thin sheets of rhodium and discs of lead iodate distributed inside the structure at varying distances from the source.

To measure the absorption of the thermal neutrons, graphite bricks were again used to form a parallelepiped with a square base, 48 inches wide and 60 inches high. In this case the neutron source was put inside a big slab of paraffin, set at the base of the graphite pile. The detectors were again thin rhodium sheets. The theoretical model used by Fermi was the "age theory" that had been essentially developed already in Rome.[76] The results were very encouraging; while graphite slowed down the neutrons much less than hydrogen, it was also true that it absorbed them much less as well. In the fall of 1940, a consensus was reached to use graphite as a moderator for the chain reaction.

The next issue, after choosing a suitable moderator, was the actual construction of a uranium-graphite structure which could sustain a chain reaction. There were many problems, for instance, how to dispose the uranium mineral inside the structure, and which measurements were necessary to understand if the number of produced neutrons was sufficient. The first measured parameter, as reported on 17 January 1941 in the communication labeled as A-6,[77] was the average number η of neutrons produced in uranium after the capture of a thermal neutron. η however was not the only important parameter; one needed also to know, for example, the probability (say f) that a neutron is captured by the nucleus which will undergo the fission, or the probability (say p) that a neutron escapes the capture by ^{238}U. The product between these two probabilities, denoted by k and called *reproduction factor,* was very important, as it described the way neutrons reproduce after interacting with a nucleus. For example, let us suppose that we start with 1,000 electrons. If $k = 1.2$, after all neutrons have interacted with uranium nuclei, there are $1,000 \times 1.2 = 1,200$ neutrons. This second generation of neutrons after interacting with the nuclei will give rise to $1,200 \times 1.2 = 1,440$ neutrons, and so on. From this example we see that if $k > 1$ the number of neutrons increases from generation to generation and the chain reaction is self-sustained, while if $k < 1$ after a certain number of generation there are no more neutrons, and the reaction stops. Figure 5.7 shows a diagram which explains very well the meaning of the reproduction factor.[78] The communication A-6 reports a value of η equal to 1.73. It was a very encouraging result, although the estimate of a 10% error was most likely too optimistic.

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[76] See page 211 and note 37 of this chapter.

[77] Fermi [138].

[78] The diagram is taken from the notes of a series of lectures given by Fermi at the Metallurgical Laboratory in Chicago to an audience of young scientists. The notes were taken by an anonymous attendee and were never reviewed.

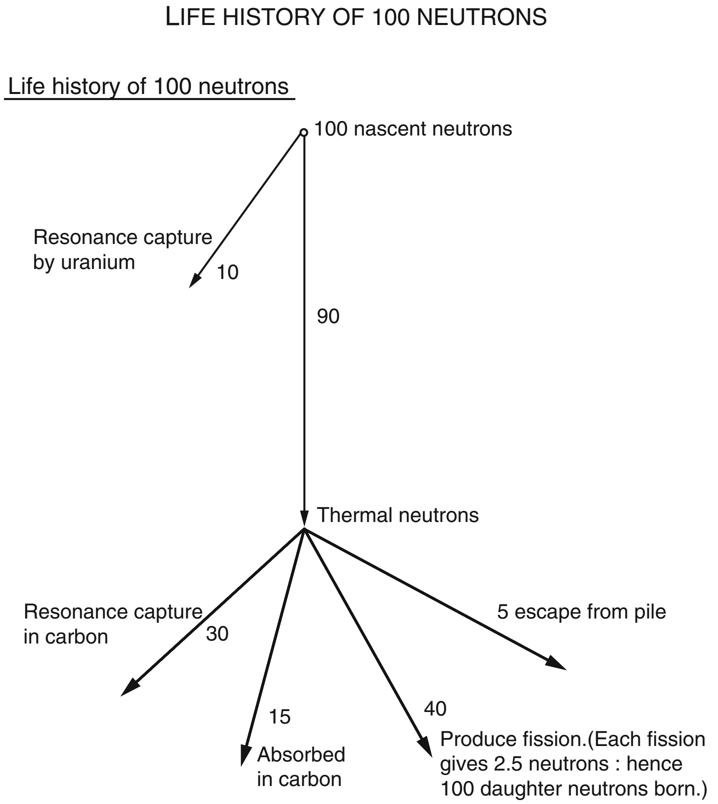

Fig. 5.7 Fermi's diagram to illustrate the "life" of a generation of neutrons inside a uranium-graphite system. 100 neutrons are born; 10 of them are captured by uranium by resonance, the remaining 90 are slowed down by graphite till they thermalize. 40 of them produce fissions, and since every fission generates 2,5 neutrons, the next generation is again formed by 100 neutrons.

New experiments, also made by the Princeton group,[79] confirmed quite quickly that a chain reaction in a system made by natural uranium and graphite was indeed possible, provided, as remarked by Anderson,[80] that the utmost care was taken to

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[79] During a conference that took place in Washington in January 1941, Wigner and Wheeler learned about the results obtained by Fermi at Columbia. As reported by Anderson (*CPF II,* p. 70), they understood that, in view of Wigner's theoretical studies on the chain reaction, they could contribute significantly to the Uranium Project. In March indeed it was decided that the experimental work on the resonance absorption was to be made in Princeton, where a cyclotron was in operation, and there were two young nuclear physicists, Robert E. Wilson and E. C. Cruetz. At that time Fermi was often commuting between Columbia and Princeton. According to Anderson, "The quantity measured in the experiment at Princeton was the ratio of the number of resonance neutrons captured per second by a uranium sphere, divided by the slowing down density, i.e., the number of neutrons per cm^3 which per second pass across the resonance levels of uranium."

[80] H. L. Anderson, *CPF II,* p. 70.

avoid undesirable neutron losses. The latter could in particular take place through the surfaces of the structure. Fermi had a solution for this problem:

> In any system of finite dimensions some neutrons escape by diffusing out of its surface. This loss of neutrons by escape can in principle be eliminated by increasing the size of the system. It was clear in 1941 that the balance of neutrons capable of sustaining a chain reaction, even if at all positive, would be so small as to make it necessary to use a system of very large size in order to eliminate most of the loss of neutrons by escape. It was important to devise methods capable of answering the following questions: (1) whether a system containing lumps of uranium distributed through the graphite in a given lattice arrangement would become chain reacting provided its dimensions were infinitely large, and (2) assuming a positive answer to the previous question, what minimum dimensions would be needed actually to achieve the chain reaction? [. . .]
>
> A brute-force method for this would be to set up a system of the given structure and keep on adding to it until a chain reaction actually is achieved or the system refused to react even when built up to enormous size. This method obviously would be exceedingly expensive both in materials and labor. Fortunately it is possible to obtain a fairly accurate answer to the two questions by using a relatively small sample of the structure under investigation. The first experiments of this type, the so-called intermediate or exponential experiments, were set up at Columbia University in the summer and fall of 1941. A lattice structure was set up containing cans filled with uranium oxide spread throughout a mass of some thirty tons of graphite. A primary source of neutrons was inserted at the bottom of this mass and the distribution of the neutrons throughout the mass was investigated in detail and compared with the theoretical expectation.[81]

The paper which, together with other aspects of the chain reaction, discussed the theory of the exponential experiments, was classified as CP-12 and is dated 12 March 1942.[82] The first exponential experiment yielded the value $k = 0.87$. That value was not so close to 1, but there was a hope to improve it by increasing the purity of the materials and optimizing the geometry of the system. As remarked by Fermi,

> It should be pointed out that this result refers to the particular lattice used. It was found by auxiliary measurements that a 4% loss in the reproduction factor k was due to the absorption in the iron cans containing the oxide. Furthermore, the oxide used was rather impure and a gain in k of a few percent can be expected by using purer oxide. Further improvements can be expected by the use of a better geometry, of compressed oxide, or of uranium metal.[83]

In the report CP-26, dated March-April the value of k was estimated at 0.918; the realization of the first nuclear reactor was approaching. Fermi's activity at Columbia during this crucial preparatory stage is described in Appendix B.5.

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[81] Fermi [223], *CPF II,* p. 546.

[82] Fermi [149]. This short report resumed in a systematic way the most important theoretical aspects of the chain reaction. It included six sections: 1. The reproduction factor in an infinite lattice. 2. Absorption of thermal neutrons by a cell. 3. Comparison between spherical and cubical cells. 4. Absorption by resonance. 5. Behavior of an infinite uranium-graphite system. 6. Theory of the exponential experiments.

[83] Fermi [150], *CPF II,* p. 136.

The next steps were strongly constrained by several events, many of which were of a political nature. Fermi left Columbia in April 1942 to join the Metallurgical Laboratory (Met Lab) in Chicago, a research institute, due to Compton's initiative, whose name hid its true objective, namely, conclude the investigations on the chain reaction that had been started at Columbia. Fermi began his work in Chicago before moving, so that for some time he commuted between New York and Chicago. The new investigations were based on an important result, i.e., a neutron reproduction factor of 0.918, very close to 1.[84] It was also clear that the objective of the American Uranium Project, on the basis of previous results on uranium 235 obtained by the British researchers, was to determine what was the most suitable fissile material for building a bomb. And indeed, as we shall see in the next section, the construction of the nuclear reactor was strongly supported by the military, who had gained full control of Uranium Project. The point was not proving the feasibility of a chain reaction using natural uranium — there were no more substantial doubts about that — but rather the possibility to use a nuclear reactor to produce plutonium, an element that, like uranium 235, was an ideal candidate for the construction of an atomic bomb.

Fermi's work in Chicago was mainly devoted to improving the parameters that measured the criticality of the atomic pile. This was mainly achieved by improving the purity of the materials, graphite and uranium.[85] During Fermi's time in Chicago about 30 exponential experiments took place, and several piles were built. In report C-207, dated 25 July 1942 Fermi announced for the first time to have reached, with pile number 9, a value of k greater than 1. It was thus possible to assess the critical dimensions of the pile, that is, the minimal dimensions of the graphite-uranium structure that could sustain a chain reaction. The value of k extrapolated from experiments referred indeed to an infinite structure, and was therefore greater than the actual factor, say k_{eff}. For instance, for the 11th prototype pile built by Fermi, a cube with an edge of about 12 feet, the value of k was between 1.012 and 1.013, but k_{eff} was 0.83. Indeed, the report CA-247, about the experiments made with pile number 11, ends with the words

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[84] As we already remarked, the possibility to have a chain reaction rests on a delicate balance; it is necessary that the number of neutrons which produce new fissions is not smaller than the number of lost neutrons. In other terms, to have a chain reaction the value of the reproduction factor k must be at least 1. For $k = 1$ the system is said to be critical, for $k > 1$ it is supercritical, and for $k < 1$ subcritical. The dimension and mass of a critical system are called critical dimension and critical mass, respectively. To better understand how the criticality of a system depends on its dimension and mass, let us consider a sphere made of a fissile material, and let us assume that the neutron loss takes place through the surface of the sphere. Thus, this is a surface phenomenon, and as such, it depends on the square diameter of the sphere. Fission on the other hand is a volume phenomenon, so that its rate depends on the cube radius; it is an effect which increases with the radius faster than the neutron loss. Therefore, there is a value of the radius for which the two effects compensate; the critical dimension and mass are those corresponding to this radius.

[85] Szilard gave an important contribution to this result. During a conversation with the managers of the National Carbon Company he realized that the graphite produced by them contained impurities of boron, a powerful neutron absorber, and convinced them to produce purer graphite.

> The effective reproduction factor defined as the reproduction factor obtained when the losses due to leakage outside of the pile of finite dimensions are included, was about $k = 0.83$.
> The critical dimensions for a cubical pile of the same internal structure as No. 11 would be about 13 m side. For a pile of the same structure of spherical shape the critical radius would be somewhat more than 7 m, corresponding to a critical mass of graphite of about 2500 tons.[86]

The construction of the first atomic pile — not an experimental prototype as the previous ones — started on 16 November 1942 in the Stagg field, under a stand in front of a terrace used to watch the squash matches. The code name of the pile was CP-1. The Stagg field was chosen by Fermi, after Compton on 14 November authorized it, as a makeup in view of the strike of the employees of the firm Stone and Webster, which had been originally entrusted of the construction of a building to host the pile in the Argonne Forest, about 15 miles from Chicago.

The pile was an ellipsoidal structure, with a polar radius of 121 inches, and an equatorial radius of 152 inches (see Figure 5.8), made by about 40,000 graphite

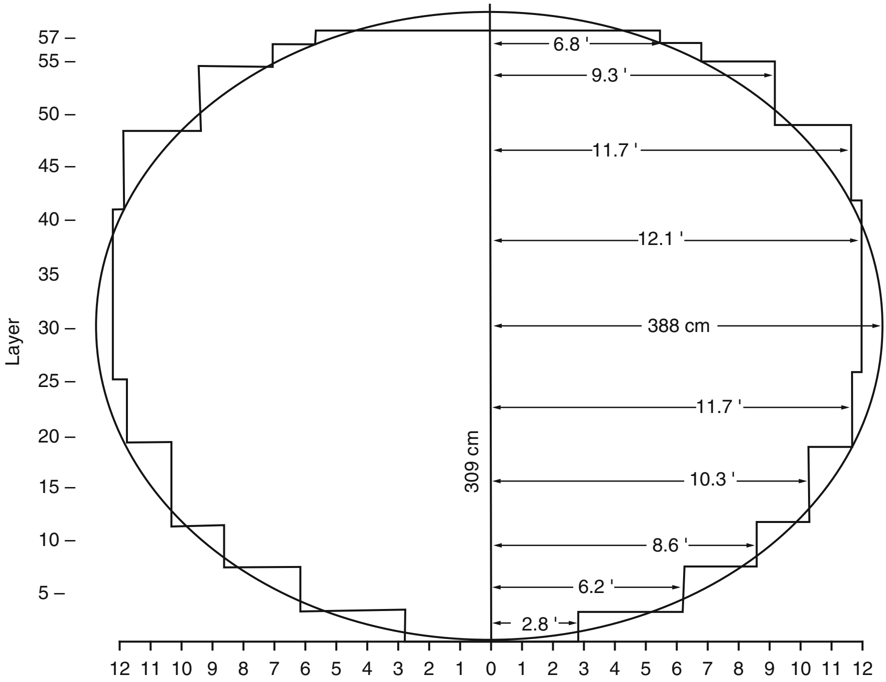

Fig. 5.8 Vertical cross-section of the CP-1 atomic pile, with evidence of the ellipsoidal shape of the structure. The numbers on the vertical axis are the number of graphite layers.

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[86] Fermi [167], *CPF II,* p. 211.

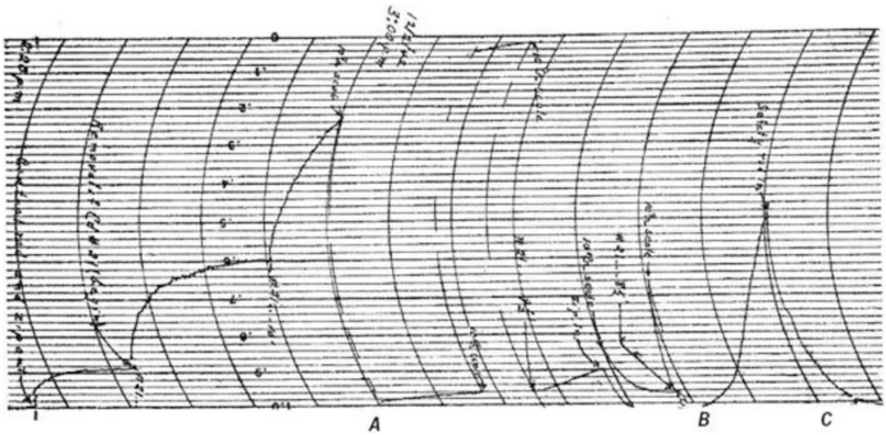

Fig. 5.9 Diagram of the first chain reaction in CP-1. The graph shows the measured neutron flux as a function of time. In the zone before point *A* the number of neutrons was increasing but tended to stabilize, and the chain reaction had not yet started. That happened at point *B*. After that the neutron flux increased exponentially without stabilizing around some value. *C* is the moment when the pile stopped working because the control bars were completely inserted into the structure.

bricks and 20,000 small uranium blocks. Uranium was partly metallic, about 6 tons, and partly in the form of uranium oxide. The construction of the pile was painstaking; about two layers per day were laid down. The structure was made by alternating two layers of inert graphite with two of active graphite layers, that is, layers that in addition to graphite also contained the uranium block. The 57th granite layer was laid down on 1 December 1942. The pile was started the next day by extracting the control bars, and became critical around 3:30 pm. The approach to criticality was monitored by measuring the neutron flux inside the structure; by means of some mathematical formulas, one could compute the value of the effective reproduction factor and estimate if it was smaller, equal, or greater than 1, that is, if the pile was working in subcritical, critical, or supercritical conditions. Figure 5.9 shows the first diagram describing a self-sustained chain reaction.

In chapter 1 we gave a detailed description of the events on 2 December 1942. We want to report here Fermi's direct recollections about that day.

> Early in 1942 all the groups working on the production of a chain reaction were united at the Metallurgical Laboratory of the University of Chicago under the general leadership of Arthur Compton. During 1942 some twenty or thirty exponential experiments were carried out at Chicago in the attempt to improve on the conditions of the first experiment. Two different types of improvements were pursued. One consisted in a better adjustment of the dimensions of the lattice and the other in the use of better materials. Impurities had to be eliminated to a surprisingly high extent from both uranium and graphite since the parasitic absorption due to elements appearing as common impurities in uranium and graphite was responsible for the loss of an appreciable fraction of the neutrons. The problem was tackled to organize large-scale production of many tons of graphite and uranium of an unprecedented purity. Also the production of uranium in metallic form was vigorously pursued. Up to 1941 uranium metal had been produced only in very small amounts, often of

questionable purity. Uranium metal was mostly produced in the form of a highly pyrophoric powder which in several cases burst spontaneously into flames when coming in contact with air. These pyrophoric properties were only somewhat reduced by sintering the powder in to compact blocks. Some of these sintered blocks were used in exponential experiments carried out in order to obtain information on the properties of a system containing metallic uranium; while the experiments were in progress the blocks were burning so fast that they felt hot to the touch and we were afraid that they might actually burst into flames before we could go through with the experiment.

Toward the fall of 1942 the situation as to the production of materials gradually improved. Through the joint efforts of the staff of the Metallurgical Laboratory and of several industrial firms, better and better graphite was obtained. Industrial production of practically pure uranium oxide was organized and some amount of cast uranium metal was produced. The results of the exponential experiments improved correspondingly to the point that the indications were that a chain reacting unit could be built using these better brands of materials.

The actual erection of the first chain reacting unit was initiated in October 1942. It was planned to build a lattice structure in the form of a huge sphere supported by a wooden structure. The structure was to be erected in a Squash Court on the campus of the University of Chicago. Since we were somewhat doubtful whether the dimensions as planned would be sufficiently large, the structure was actually built inside a huge tent of balloon cloth fabric that in case of need could have been sealed for the purpose of removing the air in order to avoid the parasitic absorption of the atmospheric nitrogen. This precaution actually proved unnecessary.

It took a little over one month to build the structure. A large number of physicists, among them W. H. Zinn, H. L. Anderson, and W. C. Wilson, collaborated in the construction. During this time the approach to the chain reacting conditions was followed day by day by measuring the neutron intensity building up inside the pile. Some neutrons are produced spontaneously by uranium in very small numbers. When the system approaches the critical size, each of these neutrons multiplies for several generations before final absorption. Indeed, when the reproduction factor of the pile is, for instance, 99 percent, each neutron multiplies in the average one hundred generations. Consequently, the density of neutrons increases throughout the mass as the critical dimensions are approached and tends to diverge at the critical size. By watching the rise of the neutron density, one obtains, therefore, a positive method for extrapolating to the critical size.

Appreciably before the dimensions originally planned for the structure were reached, the measurements of the neutron density inside the structure indicated that the critical size would soon be attained. From this time on work was continued under careful supervision so as to make sure that criticality would not be inadvertently reached without proper precautions. Long cadmium strips were inserted in slots that had been left for this purpose in the structure. Cadmium is one of the most powerful absorbers of neutrons and the absorption of these strips was large enough to make sure that no chain reaction could take place while they were inside the pile. Each morning the cadmium strips were slowly removed, one by one, and a determination of the neutron intensity was carried out in order to estimate how far we were from the critical conditions.

On the morning of December 2, 1942, the indications were that the critical dimensions had been slightly exceeded and that the system did not chain react only because of the absorption of the cadmium strips. During the morning all the cadmium strips but one were carefully removed; then this last strip was gradually extracted, dose watch being kept on the intensity. From the measurements it was expected that the system would become critical by removing a length of about eight feet of this last strip. Actually when about seven feet were removed the intensity rose to a very high value but still stabilized after a few minutes at a finite level. It was with some trepidation that the order was given to remove one more foot and a half of the strip. This operation would bring us over the top. When the foot and a half was pulled out, the intensity started rising slowly, but at an increasing rate, and kept on increasing until

> it was evident that it would actually diverge. Then the cadmium strips were again inserted into the structure and the intensity rapidly dropped to an insignificant level.[87]

The realization of the first controlled chain reaction was of course a great success for Fermi, but also gave him the opportunity to do some new physics. As reported by Anderson,

> What thrilled Fermi most about the chain reacting pile was not so much its obvious promise for atomic energy and atomic bombs, which many others were now prepared to pursue, but an entirely new and unsuspected feature. It was a marvelous experimental tool ... [which] had a sensitivity for neutrons beyond the wildest dreams of those who had struggled so hard to make measurements before. It was, in fact, a neutron multiplier of almost unlimited power. Change the number of neutrons a little, and soon the effect would be multiplied by a million times or more. The sensitivity was, in fact, limited by even rather slight changes in the pressure and temperature of the air inside. The temperature coefficient of the reproduction factor, long an unanswered and difficult problem, was now measured precisely with the greatest of ease by the simple expedient of opening a window to allow some of the cold outside air to enter the pile. The pile became a fine device for checking the purity of the uranium, an extensive standardization was carried out, and many features of the uranium graphite lattice, inaccessible before, were studied. This was physicists' work: a new device to calibrate, measurements to make. methods to develop, limits to explore, new effects to notice, results to understand.[88]

The squash gym of the Stagg field hosted CP-1 only for a few months. During those three months of intense work the pile allowed the experimenters to gather some information that would not have been otherwise easily accessible, as, for instance, the dependence of the reproduction factor k on temperature,[89] and the measurement of the cross-section of heavy water, which, after Urey's suggestion, was again considered as a possible moderator, instead of graphite, to build a new nuclear reactor.[90] Fermi's measurements and computations led to an estimate of 6 tons as the amount of heavy water necessary to build a nuclear reactor working with natural uranium.

The most interesting results however were obtained from CP-2, the atomic pile that Fermi, after dismantling CP-1, built in the Argonne Forest, where CP-1 should have been. The measurement of the cross-sections for the neutron capture by different elements was now a relatively simple operation. The element to investigate was inserted into the pile; the neutrons absorbed by it slowed down the activity of the pile, which could be returned to its original value by extracting the control bars by some amount. That amount was proportional to the required cross-section.

Working with CP-2 in the Argonne Laboratory allowed Fermi to make a new discovery, which, a few years later, would have given rise to a new and promising

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[87] Talk at "Symposium on Atomic Energy and Its Implications," 17 November 1946, published in Proceedings of the American Philosophical Society 20 (1946), p. 20; Fermi [223], *CPF II,* p. 547–548.

[88] H. L. Anderson, *CPF II,* p. 308.

[89] Fermi [182, 183].

[90] Fermi [184, 185].

area of investigation: neutron optics. The discovery originated from the use of the "thermal column," a column of graphite set on the pile which emitted a flux of thermal neutrons, originally coming from the pile, that Fermi had already used with CP-1. The speed distribution of the neutrons coming from the graphite tower was somehow different from what was expected; the "cold" neutrons, that is, those with a smaller speed, were much more numerous than expected. It did not take much time for Fermi to find an explanation of this phenomenon. According to quantum mechanics, every particle is associated with a wave, whose wavelength is inversely proportional to the speed of the particle. Crossing the graphite crystalline lattice, neutrons give rise to diffraction phenomena whenever their wavelength is comparable with the distance between the atoms in the crystalline lattice. For these reasons, slow neutrons go through the lattice completely undisturbed. This is why, according to Fermi, "cold" neutrons were more numerous; thanks to their small speed, they were not subject to diffraction phenomena, contrary to the faster ones.[91]

As reported by Anderson, in the summer of 1943 the activity in the Argonne Forest was in full development. The group of Fermi's close collaborators split, and everybody became the coordinator of a subgroup charged of precise responsibilities; Zinn's group, for instance, took care of measuring the cross-section of various gases, Anderson's experimentally determined the contribution of the fast fission neutrons to the reproduction factor, and Marshall's compared the number of fission processes and the capture by uranium as a function of temperature.

During that period Fermi was still able to find some time for some research that we could call "free," that is, not finalized to the production of a nuclear explosive. He designed a mechanical selector of the speed of the neurons, and measured some quantities important both for neutron and reactor physics. The determination of the slow-down path of neutrons in graphite[92] and of the cross-section for the fission of uranium 235,[93] the investigation of the possibility of getting uranium 236 by irradiating uranium 235,[94] and its interest in CP-3, the heavy water reactor that was being built under Zinn's responsibility, would have been very important for Fermi's contribution to the Manhattan Project.

5.7 From CP-1 to the bomb: Eugene Farmer's deeds

John Baudino, a young officer of the military intelligence who was a lawyer as a civil, in December 1942 was about to leave for Sicily when his mission was changed; he was to go to Chicago instead. According to General Groves's orders, he was responsible of Nobel Laureate Eugene Farmer's security. Actually Baudino's

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[91] Fermi [191].

[92] Fermi [193, 196].

[93] Fermi [197, 198].

[94] Fermi [206].

task was twofold; in addition to guarantee the scientists' security, he was to monitor Farmer's conversation, as he had a very detailed knowledge of all technical and scientific aspect of the Manhattan Project. Eugene Farmer was indeed Enrico Fermi's code name. Baudino went with Fermi everywhere, complying with Groves's order, who wanted the Italian Nobel Prize to be always under control. He was Fermi's driver for his car trips, for instance, when Fermi commuted from Chicago to the Argonne Laboratories, and accompanied him during the ever more frequent train trips from Chicago to Los Alamos and Hanford, the centers where the project of building an atomic bomb was materializing.

The documentary sources about Fermi's personal involvement in the political and strategical choices about the nuclear project do not allow for precise and unambiguous conclusions. Szilard's statement about this issue is most likely the best interpretation of Fermi's attitude:

> ...and he has from that time on shown a very marked attitude of being always ready to be of service rather than considering its duty to take the initiative.[95]

However, sometimes Fermi was the promoter of rather stern initiatives. Richard Rhodes, citing the source, reports a proposal Fermi made to Oppenheimer in 1943:

> ...it appears that radioactive fission products bred in a chain-reacting pile might be used to poison the German food supply.[96]

The chosen isotope was strontium 90, which the human body absorbs together calcium, and is deposited in the bones. This idea of a radioactive poisoning however was not new. Already in 1941, in a report dated 10 December, Wigner and Smyth conclude that a pile working at the power of 100 MW would produce an amount of radioactive materials that could make a very large area unsuitable to living.[97] The projects for a radioactive war were probably regarded as an alternative to the atomic bomb, and were immediately set aside once it was clear that the realization of the bomb was a concrete possibility.

It is legitimate to wonder about the reason of Fermi's initiative, which now sounds as extremely wicked. We can only make conjectures; Rhodes, for instance, maintained that

> [his proposal was] clearly offensive in intent. He may well have been motivated in part by his scientific conservatism: may have asked himself what recourse was open to the United States if a fast-fission bomb proved impossible.[98]

Giulio Maltese has his own hypothesis:

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[95] S. R. Weart and G. W. Szilard, *Leo Szilard: His Version of the Facts,* MIT Press, Cambridge MA 1979, p. 177. Cited in R. Rhodes, *op. cit,* p. 509.

[96] *Ibid.,* p. 557. Rhodes cites a letter by Oppenheimer to Fermi, dated 25 May 1943 (Papers of J. R. Oppenheimer, Library of Congress, Box n. 33).

[97] H. De Wolf Smyth, *op. cit.,* p. 65.

[98] R. Rhodes, *op. cit.*, p. 510.

> The crudeness of the proposal was mitigated if it was regarded in the context of a war that was still far from ending, in the perspective that Germany might soon have nuclear weapons, and in consideration of the many problems that had to be solved before the United States could be in possession of them.[99]

There are however some facts that allow for one more hypothesis. In the spring of 1943 Robert Serber's "Spring Lectures" took place. They officially opened the operational phase of the Manhattan Project, and had the purpose of assessing the state of the art about the technologies needed to build the atomic bomb. As we can read in Serber's notes,

> The object of the project is to produce a *practical military weapon* in the form of a bomb in which the energy is released by a fast neutron chain reaction in one or more of the materials known to show nuclear fission.[100]

The first 30 scientists who arrived in Los Alamos and attended Serber's lectures considered in particular two serious issues about the construction of the new weapon: the detonation and the so-called pre-detonation.

In principle, realizing a system capable to give rise to a chain reaction is not complicated. It is enough that two non-critical masses of fissile material, for instance, uranium 235 or plutonium 239, are put together, so that the critical mass is reached, and the chain reaction is triggered. However, as soon as the fission starts, the material tends to expand, so that the reaction slows down and can even stop. For this reason, the two non-critical masses must be joined as quickly as possible, and the two masses must be put inside a very heavy container, which can slow down the expansion during the initial phase of the chain reaction (Figure 5.10).

This simple denotation mechanism, however, presents, in the case of plutonium, a very serious problem. Fermi intuited the problem before it emerged from Segrè experiments. As reported by Harold Agnew, a collaborator of Fermi and Anderson's at the University of Chicago in 1942,

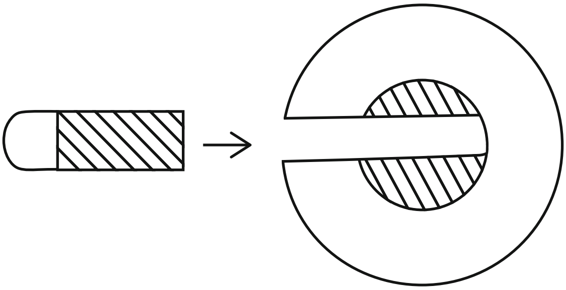

Fig. 5.10 Sketch of the detonation mechanism of a fission bomb from Steiner's talks.

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[99] Translated from G. Maltese, *Enrico Fermi in America. Una biografia scientifica 1938–1954,* Zanichelli, Bologna 2003, p. 141.

[100] http://lib-www.lanl.gov/la-pubs/00349710.pdf.

> But Enrico at a meeting said that the plutonium that we'd been working with, and it was in microgram quantities, had come from an accelerator. The plutonium that we were going to get was to come from a reactor, and it was going to be exposed for a long time to neutrons, and it may absorb a neutron, and it may be that neutron, in such a neutron-rich nucleus, that might be coming out, that we might have . . . well, I don't know if we'd called it "spontaneous fission" then, or not. But it was that phenomena that he worried about. To show that I really think that he came up with this and thought about this was the fact that the person who was assigned to find out if this were true or not was Emilio Segrè.[101]

This was Fermi's worry; if his intuition was correct, plutonium would have been useless as a nuclear explosive. This would have been a serious problem for Fermi's research; after a few months of the construction of CP-1, his activity was geared at the use of the pile as a research tool, while the administration which financed the investigations was interested in the production of plutonium to build the bomb. Therefore, Fermi's proposal to use strontium 90 could be an attempt to maintain that central importance that the pile had had till that moment.

However, the research on the pile was not saved by the poisoning of Germans food supplies. Plutonium was still considered as a possible explosive because the detonation mechanism was changed. Instead of joining two subcritical masses to obtain a critical mass, the idea is to implode a subcritical mass, so that its density increases and reaches the criticality. The idea of the implosion had already been suggested by Neddermeyer, but not because of Fermi's fears. During the April lectures, the conclusion was reached that for an efficient explosion the mass had to be greater than the critical mass. It was therefore necessary that the two masses were hurled one against the other so that to join as quickly as possible. The length of the process was estimated as one ten-thousandth of a second. However there is a problem if a wandering neutron triggers the chain reaction before the two masses have reached the critical value but have not completely joined. In this case, since the chain reaction takes places in about one millionth of a second, a much shorter time, the explosion is not efficient.

Neddermeyer's idea to avoid this pre-detonation is to build a hollow hull of fissile material, having a subcritical mass, surrounded by some casing, which in turn surrounded by a powerful traditional explosive. The idea was that the shock wave created by the traditional explosive should make the casing and the fissile material implode, so that the latter, passing from the shape of a hollow sphere to a solid one, would reach the critical density. The proposal was innovative, but did meet much approval; on the contrary, it was strongly opposed by Oppenheimer and Fermi. Their argument was the impossibility to guarantee that the shock wave had a perfect spherical symmetry, to avoid that the casing and the fissile material were fragmented and hurled in all directions. The first experiments made by Neddermeyer indeed were not encouraging. The cylindrical tubes that were supposed to implode

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[101] H. Agnew, *Fermi at Columbia, Los Alamos and Chicago,* in J. Orear *et al.* eds., *Enrico Fermi. The Master Scientist,* Cornell University, The Internet —- First University Press, p. 102.

to become solid bars, in practice deformed and twisted, and some high-speed recordings showed the insurgence of jets and asymmetries that were completely unacceptable.

The hiring of George Kistiakowsky, a Ukrainian chemist expert of explosives, and the commitment of von Neumann, Teller, and Bethe to study the phenomenon from a theoretical viewpoint show the perduring interest for the implosion technique. In the summer of 1944 these researches acquired a capital importance for the entire Manhattan Project, as implosion appeared to be the only way for detonating a plutonium bomb. This fact, which obliged Oppenheimer to completely reorganize the work at Los Alamos, was a consequence of the results obtained by Segrè in 1944. His experiments, started in the summer of 1943, showed that Fermi's worry was well founded; the plutonium arriving from Oak Ridge, and most likely, also from Hanford, contained a highly fissile isotope, plutonium 240. A few neutrons could trigger the chain reaction. With the detonation mechanism consisting in hurling two masses one against the other, a pre-detonation would be unavoidable. This discovery had a devastating effect on the projects about the bomb; another detonation mechanism was to be devised.

The only viable possibility was to switch to the implosion mechanism. Work at Los Alamos proceeded feverishly. Its final success was also due to two factors, namely, the arrival of the first IBM punched card computers, which allowed the researchers to make the complex computations involved in the behavior of the shock waves, and the adoption, after the suggestion of James L. Tuck, of the so-called "explosive lenses." Tuck was a member of the British group at Los Alamos, and was an expert in the field; he had designed the anti-tank grenade that was able to perforate the thick armored plates of the tanks. Thanks to a particular shape of the charges (which motivated the term "explosive lenses"), the explosion produced a converging shock wave. This was supposed to be capable of imploding the plutonium spherical hull. The theoretical calculations indeed showed that it was not necessary to follow Neddermeyer's initial suggestion, that is, to reach criticality by changing the geometry (from hollow hull to solid sphere). The enormous pressure applied during the implosion to a non-critical solid sphere of plutonium should have been sufficient to increase the density beyond criticality.

The first experiments about the implosion mechanism, made in early 1945, were positive. However, there was still a general problem; which neutrons should have triggered the chain reaction? One could not rely on the existence of some vagrant neutrons with the right energy to trigger the process. An "initiator" was needed, for instance, a polonium and beryllium source. Also in this case, polonium behaved worse than uranium, and also about this problem, Fermi's contribution was fundamental. However, the final solution of the initiator problem was due to Bethe. We cannot know more as the projects are still under secret. Now everything was ready for the Trinity experiment, the crowning of all efforts made at Los Alamos. From that moment the events took place as we have described in chapter 1.

It is difficult to classify Fermi's activity during wartime in terms of the local category of a "research itinerary." The documents deliver us the image of a path driven by pressing necessities, whose scientific, political, and ethical components are not easy to discern. Fermi was probably trying to come to terms with the events of those years when, as we already reported in chapter 1, he wrote to Amaldi:

> It has been a work of high scientific interest, and I am quite satisfied for having contributed to stop a war that could have gone on for months or years.[102]

Here we want to stress that the intricate process which led from the discovery of fission in 1938 to Hiroshima in 1945 was a chain of events that had the structure of a global map: fission, chain reaction, controlled reaction triggered by slow neutrons, non-controlled reaction produced by fast neutrons, uranium 235 and plutonium 239 as explosives, detonation, implosion mechanism, etc. As we have seen, Enrico Fermi contributed to all of these crucial moments, with no exclusion. We must therefore agree with Agnew when he wrote “No wonder he is thought of as the father of the atomic bomb.”[103]

5.8 The role of Great Britain: the Maud Committee

The amount of the funding, and more generally, the involvement of the national political and administrative structures are fundamental facts in the study of the researches that led to the realization of the first nuclear reactor and atomic bomb. The process was very intricate; scientific discoveries, international events, and governmental policies were deeply intertwined, and often gave rise to amazing effects. Appendixes A.2 and A.3 provide a sketchy chronology of that period. We have already given in chapter 1 a general description of the events that took place during those years. However, some specific moments turned out to be very important for Fermi’s research itinerary, and deserve to be treated in more detail. To start the discussion we need to move to England, just after the announcement of the discovery of the nuclear fission.

As in the rest of the word, the discovery of fission was considered with the utmost interest in view of the possibility of producing energy, both in controlled or explosive form. The wartime situation of Great Britain however did not allow the British establishment to support the costly and time-consuming researches necessary to overcome the difficulties that first, little encouraging results revealed. As well explained by Fakley,[104] the turning point was in March 1940 with Frisch-Peierls’s memorandum. Frisch and Peierls were both German immigrants, who had taken refuge at the University of Birmingham, and became friends.[105] Frisch

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[102] See footnote 96 in chapter 1.

[103] H. Agnew, *Fermi at Columbia, Los Alamos and Chicago, op. cit.*, p. 102.

[104] D. C. Fakley, *The British Mission,* Los Alamos Science, Winter/Spring 1983, p. 86. Available from http://www.lanl.gov/history/wartime/britishmission.shtml.

[105] O. R. Frisch and E. R. Peierls, The Frisch-Peierls memorandum, http://www.spp.astro.umd.edu/courses/honr208t/fpmemo.pdf.

had already considered the possibility to realize a chain reaction, concluding that uranium 238 could not produce an explosive reaction. But, what if uranium 238 was replaced by uranium 235? To answer this question, Frisch used an equation devised by Peierls to compute the critical mass of a fissile element. The result obtained by the two scientists was impressive; a few kilograms of uranium 235 were enough to trigger, in a time of the order of a millionth of a second, a very strong chain reaction, where temperatures of the order of some million degrees, and comparably enormous pressures, would have been produced. Frisch's estimates on the separation of the rare isotope 235 were not pessimistic, quite on the contrary, they suggested the possibility of realizing the separation in a relatively short time. As Frisch recounted, "At that point we stared at each other and realized that an atomic bomb might after all be possible."[106]

Frisch and Peierls' considerations took the form of a memorandum, as requested by Mark Oliphant.[107] It was a report divided in two parts, the second less technical than the first. At the beginning of the second part we can read:

> The attached detailed report concerns the possibility of constructing a "super-bomb" which utilises the energy stored in atomic nuclei as a source of energy. The energy liberated in the explosion of such a super-bomb is about the same as that produced by the explosion of 1,000 tons of dynamite. This energy is liberated in a small volume, in which it will, for an instant, produce a temperature comparable to that in the interior of the sun. The blast from such an explosion would destroy life in a wide area. The size of this area is difficult to estimate, but it will probably cover the center of a big city.[108]

Again in the non-technical part, Frisch and Peierls gave a sketchy description of the activation mechanism; the bomb was to be made by two parts, that at the moment of the explosion are very quickly joined, to reach the conditions necessary for the chain reaction. The conclusion of this section were very important, and were given in five points. The first three stress the potential consequences and implications of the use of the bomb.

> 1. As a weapon, the super-bomb would be practically irresistible. There is no material or structure that could be expected to resist the force of the explosion. If one thinks of using the bomb for breaking through a line of fortifications, it should be kept in mind that the radioactive radiations will prevent anyone from approaching the affected territory for several days; they will equally prevent defenders from reoccupying the affected positions. The advantage would lie with the side which can determine most accurately just when it is safe to re-enter the area; this is likely to be the aggressor, who knows the location of the bomb in advance.
> 2. Owing to the spread of radioactive substances with the wind, the bomb could probably not be used without killing large numbers of civilians, and this may make it unsuitable as a weapon for use by this country. (Use as a depth charge near a naval base suggests

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[106] O. R. Frisch, *What little I remember,* Cambridge University Press, Cambridge 1979; cited in R. Rhodes, *The Making of the Atomic Bomb, op. cit.*, p. 323.

[107] Mark Oliphant, an Australian physicist, was the director of the Physics Department of the University of Birmingham.

[108] O. R. Frisch and E. R. Peierls, *The Frisch-Peierls memorandum, op. cit.*

> itself, but even there it is likely that it would cause great loss of civilian life by flooding and by the radioactive radiations.)
>
> 3. We have no information that the same idea has also occurred to other scientists but since all the theoretical data bearing on this problem are published, it is quite conceivable that Germany is, in fact, developing this weapon. Whether this is the case is difficult to find out, since the plant for the separation of isotopes need not be of such a size as to attract attention. Information that could be helpful in this respect would be data about the exploitation of the uranium mines under German control (mainly in Czechoslovakia) and about any recent German purchases of uranium abroad. It is likely that the plant would be controlled by Dr. K. Clusius (Professor of Physical Chemistry in Munich University), the inventor of the best method for separating isotopes, and therefore information as to his whereabouts and status might also give an important clue. At the same time it is quite possible that nobody in Germany has yet realized that the separation of the uranium isotopes would make the construction of a super-bomb possible. Hence it is of extreme importance to keep this report secret since any rumour about the connection between uranium separation and a super-bomb may set a German scientist thinking along the right lines.[109]

The Frisch-Peierls memorandum pushed Henry Thomas Tizard, a prestigious British chemist, chairman of the Committee of the Scientific Survey of Air Defence,[110] to gather a group of scientists, who were asked "to advise what ought to be done, who should do it, and where it should be done."[111] The group was called "the Maud Committee";[112] it was chaired by George P. Thomson (the famous Thomson's son) and was formed by Mark Oliphant, Patrick Blackett, James Chadwick, Philip B. Moon, and John D. Cockcroft. Although the first reports sent to the American authorities by the Maud Committee were ignored, or even hidden not to have them publicized, their influence on the prosecution of the American project was determinant, also due to Lawrence's mediation. In 1941 the American Uranium Project was in serious trouble, in view of the doubts about the possibility to realize fission for military purposes in a reasonable time. From a report sent by the Maud Committee in the summer of 1941:

> We have now reached the conclusion that it will be possible to make an effective uranium bomb which, containing some 25 lb of active material, would be equivalent as regards destructive effect to 1,800 tons of T.N.T. and would also release large quantities of

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[109] *Ibid.*

[110] The Tizard Committee was the most important scientific committee of the British Ministry of Defence.

[111] R. Rhodes, *The Making of the Atomic Bomb, op. cit.*, p. 329.

[112] "Maud" was not an acronym, but a code name for the committee. There are different versions of its origin, all of which refer to a telegram, but differ as to its sender and addressee. According to Rhodes (*op. cit.,* p. 340) the telegram was sent by Lise Meitner to an English friend, and ended with the sentence "MET NIELS AND MARGRETHE RECENTLY BOTH WELL BUT UNHAPPY ABOUT EVENTS PLEASE INFORM COCKCROFT AND MAUD RAY KENT." Cockcroft thought that the three words "Maud Ray Kent" were the anagram of "radium taken"; this agreed with the news that the Germans were sweeping up the radium. In 1943, again according to Rhodes, the members of the committee learnt that Maud Ray was a Kent governess who had taught English to Bohr's children.

> radioactive substances [...] A plant to produce 2 lb (1 kg) per day [of uranium 235] (or 3 bombs per month) is estimated to cost approximately £5,000,000 [..] In spite of this very large expenditure we consider that the destructive effect, both material and moral, is so great that every effort should be made to produce bombs of this kind [...] The material for the first bomb could be ready by the end of 1943 [...] Even if the war should end before the bombs are ready the effort would not be wasted, except in the unlikely event of complete disarmament, since no nation would care to risk being caught without a weapon of such destructive capabilities.
> [...]
>
> (i) The committee considers that the scheme for a uranium bomb is practicable and likely to lead to decisive results in the war.
> (ii) It recommends that this work continue on the highest priority and on the increasing scale necessary to obtain the weapon in the shortest possible time.
> (iii) That the present collaboration with America should be continued and extended especially in the region of experimental work.[113]

The Maud report was dispatched to the U.S. via the diplomatic channel on October 3, and was delivered to the White House on October 9th. Its credibility was strengthened by Lawrence, who, after being informed on the results of the British physicists during Oliphant's visit in America, pushed Compton very much to convince him of the feasibility of the atomic bomb. Lawrence was very keen about the possible exploitation of the nuclear fission. After the May 1941 results of Seaborg and Segrè on the fissility of plutonium, he delivered, on 11 July, a memorandum which left no doubt about the strategy to follow:

> Since the first report of the National Academy of Sciences Committee on Atomic Fission, an extremely important new possibility has been opened for the exploitation of the chain reaction with unseparated isotopes of uranium. Experiments in the Radiation Laboratory of the University of California have indicated (a) that element 94 is formed as a result of capture of a neutron by uranium 238 followed by two successive beta-transformations, and furthermore (b) that this transuranic element undergoes slow neutron fission and therefore presumably behaves like uranium 235.
> It appears accordingly that, if a chain reaction with unseparated isotopes is achieved, it may be allowed to proceed violently for a period of time for the express purpose of manufacturing element 94 in substantial amounts. This material could be extracted by ordinary chemistry and would presumably be the equivalent of uranium 235 for chain reaction purposes.
> If this is so, the following three outstanding important possibilities are opened:
>
> 1. Uranium 238 would be available for energy production, thus increasing about one hundred fold the total atomic energy obtainable from a given quantity of uranium.
> 2. Using element 94 one may envisage preparation of small chain reaction units for power purposes weighing perhaps a hundred pounds instead of a hundred tons as probably would be necessary for units using natural uranium.
> 3. If large amounts of element 94 were available it is likely that a chain reaction with fast neutrons could be produced. In such a reaction the energy would be released at an explosive rate which might be described as "super bomb."[114]

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[113] R. Rhodes, *op. cit.*, p. 369.

[114] H. De Wolf Smyth, *op. cit.*, p. 64–65.

As stressed by Rhodes,

> Whenever the U.S. program bogged down in bureaucratic doubt Hitler and his war machine rescued it.[115]

In this case it was not Hitler, but the Japanese attack to Pearl Harbor which dispelled all doubts about the Uranium Project.

As far as Fermi was concerned, the construction of the CP-1 pile was the last great project he coordinated during wartime, before moving to Los Alamos to assume the role of "oracle," as we saw in chapter 1. Success arrived via the re-evaluation of the Uranium Project, which was the result of four events: Frisch and Peierls' researches in Great Britain and the Maud Committee report; Seaborg and Segrè's results on plutonium; Lawrence's intervention (he also saw the possibility of converting his cyclotron into a huge mass spectrometer to separate the uranium isotopes); and finally, the Japanese attack to Pearl Harbor. In this intricate net of events, the huge amount of resources needed to build the atomic pile was justified by the fact that it yielded the best way to produce plutonium.

5.9 The Los Alamos inheritance: toward Big Science

Los Alamos and the Manhattan Project proposed to America and the entire world the example of a new way of making science, and of the way science is perceived. It gave evidence to the public opinion of the decisive role of innovation in war, and, most of all, promoted awareness of the importance of basic research for the technological development. The organization of the investigations at Los Alamos was motivated by the emergencies of wartime, but it reverberated onto the practice of research also in peacetime, now with the support of a conspicuous funding from the government. Among all scientific disciplines, the one which was most advantaged was high energy physics, which, due to the success of the Manhattan Project, had become, also in the public imagination, the paradigm of the most advanced physics. The construction of large accelerating machines, which before the war was pursued only by a small number of university laboratories, was now the object of huge fundings. Work with those machines had to be done by large teams of researchers, with diversified professional expertise, and not by small groups as in the past.

In 1961 Alvin Weinberg, research director at the Oak Ridge National Laboratory, introduced the term "Big Science" to denote this new kind of research, as elementary particle physics or space physics, which needs huge financial resources to support the construction of large machines and the work of great groups of specialists.[116] Big Science originated at Los Alamos, and Enrico Fermi actively participated in its inception, becoming unwillingly one of its main figures.

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[115] R. Rhodes, *op. cit.*, p. 367.

[116] A. Weinberg, *Reflections on Big Science,* MIT Press, Cambridge MA 1967, p. 39.

On 28 August 1945 Enrico Fermi wrote to Amaldi “With the war over, I have accepted a position at the University of Chicago, were we have great projects for the expansion of nuclear physics.”[117] What Fermi meant was not the prosecution of researches on already established areas, of which he was already an international reference point, such as neutron physics; but rather a new area of research, which did not yet have a recognized name. As Segrè wrote, “the switch would have to be made”[118] to remain at the forefront of physical research.

“It is never too late to go beyond; it is never too late to attempt the unknown” is Gabriele D’Annunzio sentence, that we already cited in chapter 1, that Fermi liked to recall. This was to be third great turn in his scientific life. The first, in his young years, in the mid 20s, was when his interests shifted from general relativity to quantum mechanics, in the form it had in those years, namely, atomic physics; in the 30s the transition was from atomic to nuclear physics. Now, in the postwar years, it was time to switch from nuclear to high energy physics, that is, the physics of elementary particles.

It seems however that Fermi saw some difficulties in that transition, especially in connection with the structure that the organization of research was assuming. He wrote explicitly about this feeling in a letter written to Amaldi and Wick:

> [. . .] In America the situation of physics has changed very much as a consequence of war. Some changes are for the best; now that people knows that with physics one can make atomic bombs, everybody talks about sums of several million dollars with the utmost indifference [. . .] There are, of course, also some serious drawbacks; the most affecting is the military secrecy. In this connection, one hopes that a good portion of the scientific results that so far have been kept secret can be soon published, but for the moment this thing is proceeding very slowly. Another drawback is that a large part of the public opinion think that the results obtained during the war were mostly possible because of the super-organization of the scientific work. So they conclude that the best way to promote the scientific progress also in peacetime is to adopt the same super-organization. The most common opinion among the physicists is that this would a mistake. But of course, as always, the candidates to the position of super-organizer have a different opinion. Indeed, many physicists now are more involved in politics than in science, and spend their time in Washington entertaining nice conversations with senators and congressmen.[119]

The issues just hinted in this letter were proposed in a starker way during a conference held in 1947. They were rather unusual considerations for Fermi, who was not given to analyses that might reveal his convictions about the final scope of scientific research.

> The crisis through which Science has been going in the last two years [. . .] to a large extent has been due to the sudden recognition, of part of the public and the Government of the tremendous role that Science can have in human affairs. The importance of this role was already known before. But the dramatic impact of the development of the atomic bomb has brought it so vividly into the public consciousness that scientists have found themselves, unexpectedly and sometimes unwillingly, to be in the spotlight [. . .] There is

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[117] Letter by Fermi to Amaldi, in E. Amaldi, *Da via Panisperna all’America, op. cit.*, p. 160.

[118] E. Segrè, *op. cit.,* p. 166.

[119] E. Amaldi, *Da via Panisperna all’America, op. cit.*, p. 166.

> at present a great scarcity of trained research men [...] Now the enrollment of students in the scientific departments is large. I hope that very few of them are attracted by the new glamour that science has acquired. The profession of the research man again must go back to its tradition of research for the sake of uncovering new truths. Because in all directions we are surrounded by the unknown and the vocation of the scientist is to drive back the frontiers of our knowledge in all directions, not only in those that show promise of more immediate gains or more immediate applause.[120]

In chapter 1 we analyzed in detail Fermi's activity in participating to various committees, and his work in several initiatives promoted by the Administration in the early postwar years. Now we want to concentrate our attention on his research during those years.

5.10 The transition period

Fermi's itinerary on neutron physics ended with seven papers, six published in 1947, and one in 1949.[121] They were experimental works of great refinement, not because of the use of sophisticated equipment, but rather for that style which made Fermi perhaps the last physicist able to express that "Galilean dominant" we have already mentioned several times. Indeed, a consequence of the Big Science has been that, in view of the great number of specialists necessary to realize an experiment, the perfect integration and synergy between experimental and theoretical aspects in a single person has become impossible.

His coauthor Leona Marshall so commented Fermi's approach to his work:

> The series of investigations of scattering processes of slow neutrons show the main features of Fermi's often expressed philosophy of experimentation. That is, one should begin to make any straightforward measurement in a situation where there is a promising ignorance. The attempt to understand the results should in turn suggest new measurements.[122]

The experiments mentioned by Marshall began with the measurement of the phase of slow neutrons scattered by the nuclei of different elements. We have observed that when Fermi, in his first experiments with the atomic pile, discovered the "cold neutrons," he looked for an explanation of that phenomenon by means of quantum mechanics; the idea was that neutrons have wave-like properties, described by a wave function. To measure the phase of the neutrons scattered by a nucleus basically means to measure the displacement of the wave describing the scattered neutrons with respect to the wave describing the incident neutron. It was a series of important experiments, which ended the "local" period of Fermi's research on neutrons, but which, at the "global" level, opened a new line of research: neutron

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[120] Fermi, Address at the Union College on the Commencement Day of year 1947, University of Chicago Library, Enrico Fermi Collection, Box 53, Folder 8.

[121] Fermi [227–231, 234, 235].

[122] L. Marshall, *CPF II,* p. 578.

optics. The experiments used the intense neutron flux produced by the reactor (the CP-3 pile) built during wartime at the Argonne laboratories. The first paper published just after the end of the war was signed by Fermi, Sturm, and Sachs,[123] and was about the motion of slow neutrons through micro-crystalline structures.

The wavelength of the neutrons produced by the reactor was comparable with the distance among the atoms in the crystal lattice, making these neutrons the ideal particles to "look into" the structure of the crystals. It is the same principle behind the X-ray diffraction, but while this is due to electrons in the atoms, neutrons are diffracted by the nuclei. The first measurements made by Fermi and Marshall were surprising; while in the case of X-rays the incident and scattered waves always have the same phase, there are elements, such as oxygen, iron, magnesium, barium, calcium, and others, that scatter neutrons preserving their phase, while with other elements, such as lithium and manganese, the scattered neutrons have the opposite phase. The issue was very interesting. Fermi's theoretical analysis confirmed indeed that those were the only allowed possibilities; the scattered wave could only have the same phase, or could have a shift of 180 degrees with respect to the incoming wave.[124]

Together with Marshall, Fermi during that period analyzed other aspects of neutron physics, such as the dependence of the slow neutron scattering on spin; but the most important work, which ended this chapter of Fermi's research, and marked his return to theoretical physics, was a paper on the interaction between neutrons and electrons.[125] Fermi considered this as a very important problem, and indeed chose it as the topic of one of the nine lectures he gave during his 1949 trip to Italy. One should not be surprised by the existence of an interaction between electrons and neutrons, although the latter have no electric charge; indeed neutrons have a magnetic moment, and are therefore sensitive to the magnetic field created by the motion of the electrons. But Fermi was not interested in this effect, and wanted indeed to exclude it from his investigations, for instance, by making measurements with non-magnetic materials. The phenomenon he was looking for was deeper: "investigate an [intrinsic] interaction between neutrons and electrons."[126]

The results obtained with Marshall, however, were not encouraging. The same effect was studied in that period by other researchers; Rabi's group at Columbia, in particular, obtained some interesting results.[127] However, the neutron-electron interaction was given a general theoretical framework only in 1952 by Leslie L. Foldy.[128]

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[123] Fermi [226].

[124] Fermi's analysis was based on ideas he had already introduced in 1934, for instance, those of pseudopotential and characteristic length (see p. 146).

[125] Fermi [234].

[126] Fermi [240], *CPF II,* p. 721.

[127] W. W. Havens Jr., I. I. Rabi and L. J. Rainwater, *Interaction of neutrons with electrons in lead,* Physical Review 72 (1947), p. 634.

[128] L. L. Foldy, *The electron-neutron interaction,* Physical Review 87 (1952), p. 693.

5.11 The origin of cosmic rays

In the summer of 1939, when there still was some uncertainty about the choice of a moderator for the chain reaction, Fermi was busy with the Ann Arbor summer school. There he found Edoardo Amaldi, who was spending that summer in the United States, and had just formulated a theory of the ionization produced by mesons traveling through matter. There was an anomaly in the empirical data about the motion of μ-mesons in materials of different densities. The experiments showed that mesons were more absorbed by air than by a denser material, even when one consider equal masses of air and of the denser material. This was usually interpreted as a result of meson decay; indeed, in air the paths of the mesons are longer, so that they spend more time without interacting, and have time to decay. But Fermi was not happy with the explanation. In studying the decay of mesons, he thought, one cannot neglect the fact that the electric field produced by the particle interacts in a different way with the medium; according to the dielectric constant of the material, indeed, mesons produce a different ionization. This could explain the anomaly; the absorption of the particles, indeed, is higher in a material, like air, whose dielectric constant is smaller.[129] According to his calculations, the anomalous effect should take place only at high energies; however, experiments made by Bruno Rossi and his collaborators showed that mesons display an anomalous behavior also at low energies, thus providing clear evidence for their decay.[130]

Due to his research on the chain reaction and the wartime conditions, Fermi stopped his investigations on the cosmic rays in 1939. He went back to that topic in the aftermath of the war, in connection with the Conversi-Pancini-Piccioni experiment (see chapter 4). Fermi was invited to the Shelter Island conference, but could not go due to health problems. According to his own words, it was Teller who informed him of the intricate problem raised by the results of Conversi, Pancini, and Piccioni.[131] Those results rekindled in him the interest he already had during his first years in America. He immediately perceived the fundamental importance of the work of the three Italian scientists. His calculations, which he made also with the help of Teller and Weisskopf, and were made under the hypothesis that the μ-meson was to be identified with Yukawa's particle, showed that the capture time of the negative meson is of the order of 10^{-13} seconds in condensed matter, and of the order of 10^{-9} seconds in a gas. In both cases the computed time is much smaller, by several orders of magnitude, than the μ-meson half-life, which is 2×10^{-6} seconds. This definitively ruled out the possibility of identifying the meson with Yukawa's particle.

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[129] Fermi [133, 134].

[130] B. Rossi, H. V. N. Hilberry and J. B. Hoag, *The disintegration of mesotrons,* Physical Review 56 (1939), p. 837. A direct evidence of the meson decay was provided by Rasetti's experiments (F. Rasetti, *Evidence for the radioactivity of slow mesotrons,* Physical Review 59 (1941), p. 706).

[131] E. Teller, *CPF II,* p. 615.

Fermi was not only interested in the nature of cosmic rays, but also wanted to understand their origin. He proposed a theory, that, while not being definitive, "[was] valid and [..] influenced all subsequent development of this subject."[132] This crowned and concluded his research on cosmic rays. The theory stood in definite contrast with Teller's hypothesis that the cosmic rays actually were not of cosmic origin, but were rather produced by the Sun, where they are confined in a relatively small space by the strong magnetic field. This "non-cosmic" hypothesis was based on energetic considerations; indeed, Teller argued, the amount of energy of cosmic origin would be enormous. Fermi stressed this problem during the conference on cosmic rays held in Como in 1949, which he attended during his first visit to Italy after 1938. At the beginning of his contribution, he resumed the basic motivations of Teller's "non-cosmic" theory.

> If one assumes that radiation with this average density occupies all the interstellar space of the galaxy, one obtains the result that the overall energy of the cosmic radiation is of the same order of magnitude as the kinetic energy of the disordered motions of the stars. The amount of energy is so large that one might legitimately doubt whether or not it is possible to find a mechanism capable of producing cosmic radiation in such a staggering amount. For this reason Teller has recently proposed a Non-Cosmic Theory.[133]

We do not know what aspects of the theory were considered by Fermi as unsatisfactory. Subrahmanyan Chandrasekhar reported that "Fermi used to say, half jokingly, that exactly this hypothesis had suggested him the opposite point of view."[134] His idea was to recover the hypothesis of "space imbued of cosmic rays" by devising a mechanism for the acceleration of particles capable of circumventing Teller's objection. His proposal was based on the investigations of Hannes Alfvén, who during a visit to Chicago in 1948 tells Fermi of the possible existence of relatively strong "roaming" magnetic fields in the galaxy. Fermi's idea was that the particles were accelerated by the equipartition of energy. The existence of galactic magnetic field was basic to this hypothesis; it is indeed, according to Fermi, in the interaction with these magnetic fields that protons tend to a statistical equilibrium, thus reaching the same energy of the magnetic fields. Chandrasekhar's comment is very interesting:

> Fermi developed his ideas on the origin of cosmic radiation in November 1948. How completely his ideas were crystallized, already at the outset, is evident from all the essential elements of theory clearly written on the two sheets of paper reproduced here as he explained his ideas within a day or two of his first thoughts on the subject. The importance of magnetic fields and cosmic rays for the energy balance in the galaxy was realized by Fermi at once. In recognizing this, Fermi discovered a most significant fact for astronomy. His description of turbulent interstellar matter and prevailing magnetic fields as a moderator for maintaining a constant level of cosmic ray intensity is grand in its concept and inception.[135]

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[132] E. Segrè, *op. cit.,* p. 175.

[133] Fermi [238], *CPF II,* p. 667.

[134] B. Pontecorvo, *op. cit.,* p. 155.

[135] S. Chandrasekhar, *CPF II,* p. 926.

Fermi did not consider himself as an astrophysicist, but the astrophysicists were happy to deal with him, as proved by the invitation to give the 6th "Henry Norris Russell Lecture" of the American Astronomical Society, on 28 August 1953. During his lecture he proposed a revisitation of his theory in the origin of the cosmic rays, modified so to take account of the recent results on the structure of the galactic magnetic field. The conclusion of his lecture is typical of his style. As at the end of his fundamental paper on the β decay, where he had stressed the irrelevance of the details with respect to the general ideas, also in this case he observed:

> In conclusion, I should like to stress the fact that, regardless of the details of the acceleration mechanism, cosmic radiation and magnetic fields in the galaxy must be counted as very important factors in the equilibrium of interstellar gas.[136]

The last two papers dealing with astrophysics[137] may appear to be sporadic, but they are nevertheless important, in that they confirm the ampleness of Fermi's scientific culture, and highlight some important aspects of his research style. He used to say "student must solve problems, researchers must ask questions." These papers, while dealing with problems in astrophysics, seem to have originated from fundamental questions about cosmic rays. In a sense, they were "philosophical" answers to the "philosophical" questions raised by the cosmic rays. Indeed, while the presence of roaming magnetic fields allowed for a mechanism of acceleration of the cosmic rays, the presence of such magnetic fields raised serious questions about the gravitational stability of the cosmic masses.

These two papers were written jointly with Chandrasekhar. In 1952 and 1953 Fermi had regular discussion with him on issues in astrophysics. Chandrasekhar's words once more highlight Fermi's complex approach to research.

> Referring to this largely mathematical paper, several persons have remarked that it is "out of character" with Fermi. For this reason I may state that the problems which are considered in this paper were largely at Fermi's suggestion. The generalization of the virial theorem; the existence of an upper limit to the magnetic energy of a configuration in equilibrium under its own gravitation; the distortion of the spherical shape of a body in gravitational equilibrium by internal magnetic fields; the stabilization of the spiral arms of a galaxy by axial magnetic fields; all these were Fermi's ideas, novel at the time. But they had to be proved; for, as Fermi said: "It is so very easy to make mistakes in magneto-hydrodynamics that one should not believe in a result obtained after a long and complicated mathematical derivation if one cannot understand its physical origin: in the same way, one cannot also believe in a long and complicated piece of physical reasoning if one cannot demonstrate it mathematically."[138]

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[136] Fermi [265], *CPF II,* p. 976.

[137] Fermi [261, 262].

[138] S. Chandrasekhar, *CPF II,* p. 925.

5.12 A trip to the world of elementary particles

During the 50s the boundary between the "global" and "local" aspects of Fermi's research became more indefinite. In many cases, indeed, his personally itineraries determined the dynamics of entire research areas. This was the case with the sequence of events that, starting with a bold hypothesis about the compound nature of the pion, led to the discovery of the first nuclear resonance of the proton, which, as remarked by V. Weisskopf, opened "a new world, a third realm of phenomena which I like to call the subnuclear world."[139] The work which incepted this important contribution to elementary particle physics is dated 24 August 1949. It was written with Yang and was about a fundamental problem, the elementarity of the particles. As the two authors wrote,

> In recent years several new particles have been discovered which are currently assumed to be "elementary," that is, essentially, structureless. The probability that all such particles should be really elementary becomes less and less as their number increases.
> It is by no means certain that nucleons, mesons, electrons, neutrinos are all elementary particles and it could be that at least some of the failures of the present theories may be due to disregarding the possibility that some of them may have a complex structure. Unfortunately, we have no clue to decide whether this is true, much less to find out what particles are simple and what particles are complex. In what follows we will try to work out in some detail a special example more as an illustration of a possible program of the theory of particles, than in the hope that what we suggest may actually correspond to reality.[140]

The hypothesis consisted in supposing that "π-mesons may not be elementary but may be a composite particles formed by the association a nucleon with an anti-nucleon." The proposal by Fermi and Yang was based on two assumptions. The first is the existence of the anti-neutron and anti-proton, particles that until that moment had not yet been discovered. In the authors' words, "although this is an assumption that goes beyond what is known experimentally, we do not view it as a very revolutionary one." The two anti-nucleons, indeed, must bear with the corresponding nucleons the same relation occurring between electron and positron. The second consisted in assuming the existence of an attractive force between a nucleon and its anti-nucleon, capable of binding the two particles;

> since the mass of the π-meson is much smaller than twice the mass of a nucleon, it is necessary to assume that the binding energy is so great that its mass equivalent is equal to the difference between twice the mass of the nucleon and the mass of the meson.[141]

This model could explain the existence of three pions, and their negative parity.

The theory was not going to be successful. Fermi did not mention it in the two lectures on elementary particles he gave in Italy in 1949. The paper however is very important, inasmuch it concretely showed the possibility to understand the internal

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[139] V. Weisskopf, *What is an elementary particle?*, in D. Huff, O. Prewett (eds.), *The nature of the physical universe: 1976 Nobel Conference,* Wiley, New York 1979, p. 17.

[140] Fermi [239], *CPF II*, p. 675.

[141] *Ibid.*

structure of the pion, and had a great impact on the scientific community, as far as the elementarity problem was concerned, Fermi was very keen about this point, and indeed, in his two 1949 Italian lectures he was very clear about it:

> A physicist or a chemist, who knows nothing about the atomic structure, and starts exploring the nature of the chemical object and discovers a first atom, say an iron atom, might supposed that it is an elementary particle. If, after this iron atom, the chemist discovers an atom of a different nature, say a sulphur atom, and then an oxygen atom, and so on (obviously, the order in which I am mentioning these atoms is completely arbitrary), most likely the chemist's belief in the elementarity of the atom would gradually diminish, since a great number of particles would contradict the notion of elementarity. This is what is happening with the elementary particles we know today.[142]

And again, with reference to the meson theories of the nuclear forces:

> I would like to add a couple of words about the elementary particles, which, as I said, are particles without a structure, or at least, whose structure we do not know. Actually many of them show some structure, so that we can no longer consider them as elementary. For instance, in the picture I have just described, where a proton is surrounded by its own field and emits mesons, one should not consider the proton as a particle, but rather as a point with two particles, which sometimes are at a very short, but not infinitesimal, distance. The same happens also for more familiar particles, such as the electron. This is surrounded by an electromagnetic field, which in a sense is a structure, and accompanies it in all its movements. This state of affairs, that is, the physical electron, can be described as a mix of an idealized, point-like electron, and a cloud of photons which surrounds it and represents the electromagnetic field. Thus these structures, that we keep on calling elementary particles, are actually complicated objects. In trying to analyze these structures, the theory meets remarkable difficulties; for instance, the electromagnetic mass, that is, the mass due to the field surrounding the electron, turns out be infinite.[143]

This makes it clear that the project to tackle the structure of the elementary particles from a quantitative viewpoint was bound to hit the same problems already met in quantum electrodynamics, such as the infinite quantities that sometimes appear in computations. To understand the nature of this problem we shall analyze a little more closely what happens in quantum electrodynamics. The interaction between the electromagnetic field and matter, that is, between photons and electrons, is described (in the Lagrangian function of the photon-electron system) by a term which is proportional to the charge e of the electron. This quantity is known, as it enters the expression of the Lorentz force, the equation describing the classical interaction between the electromagnetic field and an electric charge. However, the presence of this interaction term makes the mathematical treatment of the quantum theory very complicated, so that one adopts a "perturbative" approach. The terms corresponding to the nth step in the perturbative development contain the nth power of the dimensionless constant e^2/hc, whose value is approximately 1/137. The fact that this constant is much smaller than 1 makes the perturbative approach meaningful, at least in principle. In the case of the meson theories, the analogous

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[142] Fermi [240], *CPF II,* p. 685.

[143] *Ibid.*, p. 692.

constant was not known from classical physics, and moreover, the value of the coupling constant g^2/hc, where g is a constant measuring the strength of the meson-nucleon interaction, is approximately 1, making the perturbative approach quite problematic.

Fermi was well aware of these problems. In 1950 he wrote:

> The meson theory has been a dominant factor in the development of physics since it was announced fifteen years ago by Yukawa. One of its outstanding achievements has been the prediction that mesons should be produced in high energy nuclear collisions. At relatively low energies only one meson can be emitted. At higher energies multiple emission becomes possible.
>
> In this paper an attempt will be made to develop a crude theoretical approach for calculating the outcome of nuclear collisions with very great energy. In particular, phenomena in which two colliding nucleons may give rise to several π-mesons, briefly called hereafter pions, and perhaps also to some anti-nucleons, will be discussed.
>
> In treating this type of processes the conventional perturbation theory solution of the production and destruction of pions breaks down entirely. Indeed, the large value of the interaction constant leads quite commonly to situations in which higher approximations yield larger results than do lower approximations.[144]

Fermi's proposal was once more based on statistical considerations. It is quite interesting, following a suggestion by Allison, to compare the paper just cited with the 1927 paper about the Thomas-Fermi model,[145] where the electron distribution in atoms was computed using statistical methods, considering them as a gas surrounding the nucleus. The idea Fermi had in 1950 was analogous.

> When two nucleons collide with very great energy in their center of mass system this energy will be suddenly released in a small volume surrounding the two nucleons. We may think pictorially of the event as of a collision in which the nucleons with their surrounding retinue of pions hit against each other so that all the portion of space occupied by the nucleons and by their surrounding pion field will be suddenly loaded with a very great amount of energy. Since the interactions of the pion field are strong we may expect that rapidly this energy will be distributed among the various degrees of freedom present in this volume according to statistical laws. One can then compute statistically the probability that in this tiny volume a certain number of pions will be created with a given energy distribution.[146]

The "statistical production theory" proposed by Fermi was, as remarked by Anderson, a simplified method, useful for estimating the order of magnitude of the cross-sections of the processes involved. Fermi indeed aimed at predicting the possible outcome of an experiment. The method was discussed in detail during the Silliman Lectures, an advanced physics course Fermi gave at Yale in 1950, whose notes were published in 1952.[147] Again Anderson reported that Fermi always

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[144] Fermi [241], *CPF II,* p. 790.

[145] Fermi [43]. We discussed this model in chapter 3, page 145.

[146] Fermi [241], *CPF II,* p. 790.

[147] Fermi, *Elementary Particles,* Yale University Press, New Haven 1952.

stressed the limited purpose of his theory, and that, quite ironically, he blamed his own authoritativeness for the excessive credit that other physicists gave to the theory.[148]

Fermi's trolley (see Figure 5.11) is just one of the many ways Fermi collaborated to the realization of the synchrocyclotron, the machine able to accelerate protons up to 450 MeV which was inaugurated in Chicago in September 1951. As Anderson remarked,

> Fermi's trolley was not a thing of beauty, but he made the whole thing himself in one weekend and it worked very well for several years. He was proud to point out how much more quickly he had finished it, than if it had been made in the shop.[149]

The trolley was very useful during the scattering experiments; it could move along the edge of the magnet, and was controlled electrically, so that it could easily enter and leave the area hit by the particles accelerated by the machine. The beryllium target, which was bombarded by the protons accelerated by the synchrocyclotron, was mounted on top of the trolley. The bombardment produced a pion beam which was directed to a tank of liquid hydrogen, so that the pion-nucleon scattering could be studied.

The pion-nucleon scattering was Fermi's last great contribution to elementary particle physics (by the way, he preferred to say "fundamental particles"). It was a foundational work, not only for the main result achieved, the discovery of the first resonance,[150] but also for its contribution to the notion of isotopic spin. We have seen in the previous chapter that that concept has been crucial for understanding the properties of the "strange particles." Fermi's contribution to this fragment of the global maps was of a paramount importance, and stressed the basic role of isotopic spin in the explanation of the singular results obtained in Chicago.

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[148] H. L. Anderson, *CPF II,* p. 789.

[149] H. L. Anderson, *Meson experiments with Enrico Fermi,* Review of Modern Physics 27 (1955), p. 269.

[150] Particles have a wave-like nature, and, in particular circumstances, can compose, giving rise to a resonating compound state. For instance, a pion and a nucleon can form a resonant state which lives as an autonomous entity for a very short time, and then decays. The experiments made with the Chicago synchrocyclotron measured the scattering cross-section of pion beams of different energies colliding with liquid hydrogen (protons). A peak in the curve expressing the cross-section as a function of energy showed the existence of a resonance. In some sense, at that energy a pion and a proton gave rise to a new particle, which lived for a very short time, and then decayed into a pion and a proton. A computation established that the lifetime of that particles, called 3-3 resonance or Δ, was about 10^{-23} seconds, and its mass was about 1230 MeV. According to Pais, "The role of Δ in πN-scattering can be represented as a (real, not virtual) formation: $\pi + N \rightarrow \Delta$, followed by a decay: $\Delta \rightarrow \pi + N$. The state Δ can be assigned not only a T, a J, and a parity (even), but also a mass ~ 1230 MeV/c^2 and a lifetime $\sim 10^{-23}$ sec, as follows, respectively, from the position and width ($\sim$115 MeV) of the peak. Thus Δ has all the attributes of an unstable particle such as the neutron except for the quantitative difference of being exceedingly short-lived. It took quite a few years before physicists became comfortable with the idea that there is no real difference between a resonance and an unstable particle" A. Pais, *Inward Bound, op. cit.*, p. 487–488.

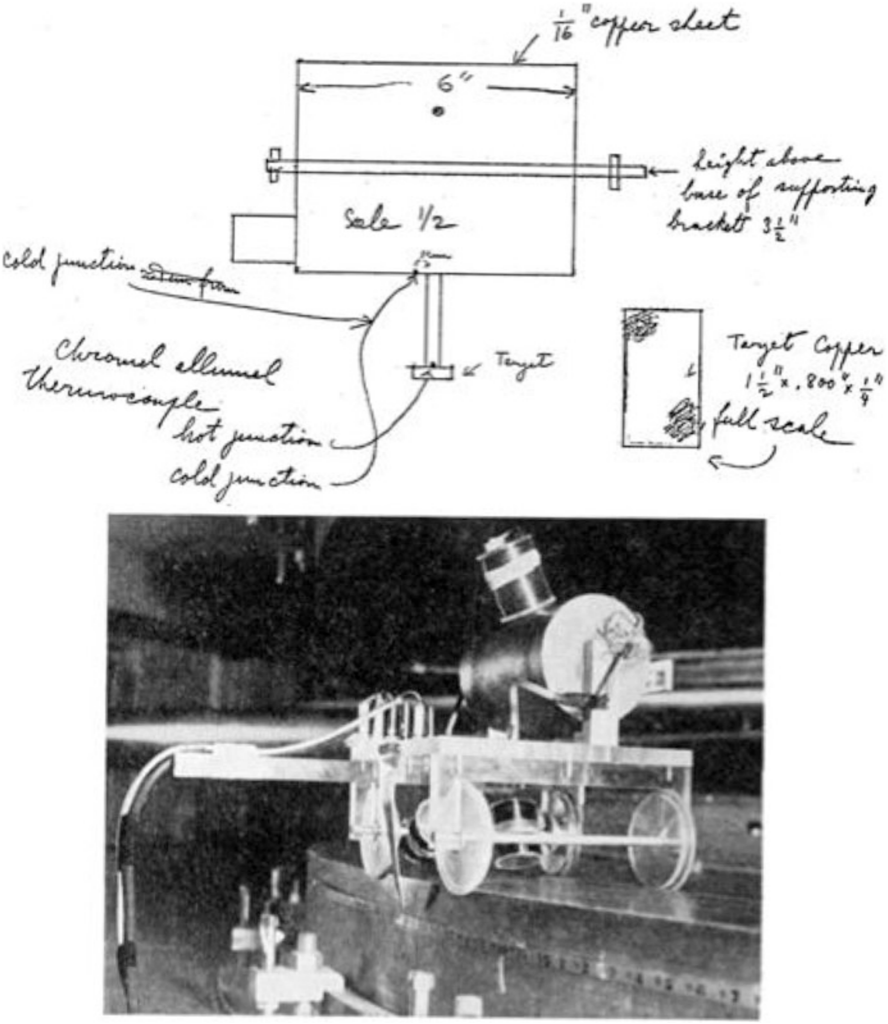

Fig. 5.11 Fermi's sketch and picture of Fermi's trolley.

The first experiments made with the synchrocyclotron were the object of three papers published in the *Physics Review*,[151] authored by Fermi, Anderson, and a number of collaborators. As we already noted, the objective was the measurement of the cross-section for the scattering of pions on hydrogen. The experiment was made first with negative, then with positive pions. Using the authors' notation, the processes analyzed were

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[151] Fermi [248–250].

1. $p + \pi^- \rightarrow p + \pi^-$
2. $p + \pi^- \rightarrow n + \pi^0 \rightarrow n + 2\gamma$
3. $p + \pi^- \rightarrow n + \gamma$

in the case of negative pions, and

4. $p + \pi^+ \rightarrow p + \pi^+$

in the case of positive pions. The first, second, and fourth case are scattering processes with or without charge exchange, while the third is the inverse of a process of photo-production of pions by the action of gamma rays on neutrons.

The most important result arrived in the morning of 21 December 1951, and was totally unexpected. By inspecting the four reactions described above, one would expect that the cross-section of negative pions on protons is greater than that of positive pions; in the first case, indeed, there are three ways in which the reaction can take place, while in the second there is only one. Fermi obtained the opposite result; the cross-section is bigger in the case of positive ions. The explanation of this anomaly is based on the conservation of isotopic spin, but Fermi was not the first to realize it. The suggestion was advanced by Keith Brueckner, who found the anomaly by comparing the experimental data from the scattering of negative pions, that Anderson had given at a conference on 27 October 1951,[152] with the data from the experiment of Sachs and Steinberger[153] at Columbia, about the scattering of positive pions. Brueckner conjectured, in the conclusions of his paper, that the peak of the cross-section could be interpreted as a resonance.

Fermi read Brueckner's pre-print on 21 December, the same morning in which he made the measurements on the scattering of positive pions, and immediately gave the correct interpretation of the results he obtained. This is how Anderson told the succession of events:

> The cross section for positive pions mounted far above the maximum found for the negatives. This seemed particularly strange at first since with the positive pions only the elastic scattering is possible. An explanation by Brueckner anticipated this result by several days, In fact, Fermi could (and did) read the preprint of Brueckner's paper [...] the very day he found the high cross section. Brueckner had seized on the idea of the isotopic spin as being an essential element in the pion nucleon interaction. Arguing that the dominant state was one with total angular momentum 3/2 and isotopic spin 3/2 all the features of the experiments could be understood at once. It took hardly more than a glance at Brueckner's paper for Fermi to grasp the idea. Twenty minutes after he left the experimental room to work through the idea by himself in his office, he emerged with this happy conclusion. "The cross sections will be in the ratio 9: 2:1," he announced. He was referring to the pi-plus elastic, the pi-minus charge exchange, and the pi-minus elastic processes in that order. A few months later when he addressed the American Physical Society at its New York meeting, he had a message to give. He had studied these pi-mesons and he could tell how they interacted with the nucleons. He had the facts about this, and also some explanation, and underlying, an important principle. In the strong interaction between the pion and

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[152] Anderson communicated the data published in Fermi [248].

[153] C. Chedester, P. Isaacs, A. Sachs and J. Steinberger, *Total cross sections of pi-mesons on protons and several other nuclei,* Physical Review 82 (1951), p. 958.

> the nucleon, the isotopic spin was conserved. Thus, an old idea, hitherto rather neglected, assumed a new importance.[154]

Fermi was well aware that the data from his experiment were just a preliminary indication of the existence of a resonance; a confirmation of that hypothesis required much more work. Detailed experiments were needed on the angular distribution of the scattered pions. This was indeed the object of the investigations of Fermi's group with the Chicago synchrocyclotron. The discovery raised enormous interest from the scientific community. As Anderson stressed,

> These experiments seemed to hold the key to the understanding of the nuclear forces, and there had been a great many speculations about the nature of pion-proton interaction. The experiments would show which of the many possible theories came closest [to] the truth. [. . .] When the Rochester High Energy Nuclear Physics Conference was held, in December of that year, everyone was eager to hear Fermi's report.[155]

Even the best theoreticians were very interested; this is confirmed by a letter of Feynman to Fermi, to which the latter answered by renewing his conviction that the isotopic spin was a "good quantum number," and expressing the need to make measurements on the cross-section for scattering angles of 90 degrees.

Fermi did not live long enough to see a final confirmation of the existence of the Δ resonance, and the ensuing discovery of many more resonances; but he could see how the hypothesis of the conservation of the isotopic spin in the strong interactions became the basic ingredient to understand the weird behavior of the strange particles, opening the way to the classification of the elementary particles based on symmetry.

5.13 Complexity: computers and nonlinear systems

There are two transversal but interconnected paths that cross Fermi's scientific activity till the end of his life. The first is about numerical calculus, the second is the statistical behavior of physical systems.

In chapter 3 we saw for the first time Fermi dealing with numerical computations, when in 1927 he was formulating the Thomas-Fermi model; the problem was to compute the potential in the atom as a function of the distance from the nucleus. In practice that required the solution of a certain differential equation. Fermi decided to tackle the problem numerically, using a Brunsviga mechanical table calculator. The work took one week.[156] Only in 1935, with the complicate calculations on the slowing down of neutrons, Fermi had to deal again with numerical computations. Segrè remembered that particular moment:

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[154] H. L. Anderson, *CPF II,* p. 835.

[155] Fermi [251], *CPF II,* p. 844.

[156] See chapter 3, footnote 160.

> He also made a number of calculations on the behavior of neutrons in hydrogenous media by what we would today call the Monte Carlo method — that is he followed in detail the fate of a neutron in its successive collisions, determining by chance (Monte Carlo) the parameters of each collision. Having followed many neutrons he could make a statistical study of the result. He did not speak about his method then, and I learned that he had used in 1935 — only many years later, in Los Alamos, from Fermi himself.[157]

A systematic use of externally programmed computers was done at Los Alamos for the first time. The first machine, ASCC or Harvard Mark I, was a gift by IBM to Harvard University in 1944, and was immediately used to make the complex computations concerning the implosion mechanism in the atomic bomb. In 1946 Mark I, which was not yet electronic, but still electromechanical, was surpassed by ENIAC (Electronic Numerical Integrator and Computer), which was systematically used for the computations for the hydrogen bomb.

During the summer of 1952 Fermi spent two months in Los Alamos, where on 15 March MANIAC (Mathematical Analyzer, Numerical Integrator, and Computer), built by Nick Metropolis, had started working. There Fermi made the complex computations in which he was interested at the time. As he observed in a joint paper with Metropolis, a conventional approach to the solution of the equations would have required one or two weeks of intense work, while with MANIAC all was done in five minutes.[158] However, Fermi at Los Alamos was not simply an end user. As Metropolis remarked,

> Fermi had early recognized the potential capabilities of electronic computers; his sustained interest was a source of stimulation to those working in the field; but it was his direct approach and complete participation that had the greatest effect on the new discipline. His curiosity extended beyond the calculation problem at hand; he raised questions about the general logical structure of computers, and his remarks were always of a penetrating nature. He was equally interested in the various experimental techniques being developed. Whenever the computer would malfunction, as it often did in those early days, he always expressed surprise and admiration that it performed 50 well. Such a sympathetic reaction was atypical and refreshing.
>
> Finally it may be mentioned that Fermi, in the summer of 1952, raised the question of the feasibility of automatically scanning and measuring, as well as analyzing, nuclear particle tracks in emulsions or photographs. Only a preliminary formulation of this problem was possible, but it was clear that Fermi had anticipated the intense efforts that would be made later.[159]

On his return from Los Alamos Fermi had become a specialist in the new computing machines. He saw the ample possibilities they afforded for physical research, and pushed hard in order that the University of Chicago was equipped with its own electronic computer. Also during his last trip to Italy, Fermi showed his increasing confidence in the possibilities offered by computers. Giorgio Salvini and Gilberto Bernardini asked him advice on how to use some conspicuous amounts of

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[157] E. Segrè, *op. cit.*, p. 86.

[158] Fermi [256].

[159] N. Metropolis, *CPF II,* p. 861.

money available to some Italian universities, Pisa in particular, that were initially destined to construct a particle accelerator (an electron synchrotron), but had become available after the decision to build the machine in Frascati. Fermi had no hesitation; he advised to build an electronic computer in Pisa.

The scientific importance of computers, not only to make computations, but also as tools capable to establish new areas of research and open new perspectives, was stressed by Fermi's last great contribution to physics. It is a paper written with John Pasta and Stanislaw Ulam and published after Fermi's death.[160] It was about a question chosen by Fermi and Ulam after long discussions.

> We discussed this at length and decided to attempt to formulate a problem simple to state, but such that a solution would require a lengthy computation which could not be done with pencil and paper or with the existing mechanical computers.[161]

The choice fell on the dynamics of nonlinear systems, and the work of the three scientists opened a new area of research. In chapter 3, examining the transition period that led Fermi from relativity to quantum mechanics, we saw that he believed to have proved that, in general, any mechanical system after a perturbation becomes ergodic (see page 128). The opinion he had in 1923 was still largely shared by the scientific community at the time of the work we are examining. So the surprise was great when he saw the results of the simulation performed on MANIAC, which described a mechanical system, formed by a chain of point-like masses, whose extrema were fixed, and were subject to elastic forces and an external perturbation; the system behaved in a non-ergodic way. According to three authors,

> This report is intended to be the first one of a series dealing with the behavior of certain nonlinear physical systems where the non-linearity is introduced as a perturbation to a primarily linear problem. The behavior of the systems is to be studied for times which are long compared to the characteristic periods of the corresponding linear problems.[162]

The first system considered by Fermi, Pasta, and Ulam was one-dimensional: a string of particles, with fixed ends, with elastic forces acting on them, including both a linear term (Hooke's law), and additional, small perturbative terms, quadratic in the distances, for instance. As Ulam wrote,

> The question was to find out how this nonlinearity after very many periods of vibrations would gradually alter the well known periodic behavior of back and forth oscillation in one mode? how other modes of the string would become more important? and how, we thought, the entire motion would ultimately thermalize, imitating perhaps the behavior of fluids which are initially laminar and become more and more turbulent and convert their macroscopic motion into heat.[163]

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[160] Fermi [266].

[161] S. M. Ulam, *Adventures of a Mathematician,* University of California Press, Berkeley–Los Angeles–London 1991, p. 225.

[162] Fermi [266], *CPF II,* p. 981.

[163] *Ibidem,* p. 226.

The result of the computer simulation turned out to be completely unexpected, as Ulam suggestively reported:

> The original objective had been to see at what rate the energy of the string, initially put into a single sine wave (the note was struck as one tone), would gradually develop higher tones with the harmonics, and how the shape would finally become "a mess" both in the form of the string and in the way the energy was distributed among higher and higher modes. Nothing of the sort happened. To our surprise the string started playing a game of musical chairs, only between several low notes, and perhaps even more amazingly, after what would have been several hundred ordinary up and down vibrations, it came back almost exactly to its original sinusoidal shape.[164]

Let us see how the simulation was done. The system consisted of 64 particles connected one to the other by nonlinear springs. The authors wanted to study the ergodic behavior of the system, to establish "experimentally" if its dynamics was governed by the principle of equipartition of energy. Let us recall some simple facts about oscillatory motions in one dimension. A single particle subject to an elastic force can oscillate in only one way, with a frequency fixed by the elastic modulus of the spring, and the mass of the particle. With two particles, again subject to elastic forces, the possible oscillatory motions (called *normal modes*) are two, with two different frequencies. The result extends to N particles subject to elastic forces; the system can oscillate in N different ways, with N frequencies that, in general, are different.

If all the energy is concentrated in one normal mode, due to the presence of the perturbation terms, one would expect an energy flow toward the other modes, so that, after some time, the available energy is evenly divided among them, with an average energy per mode given by the total energy divided by the number of modes. Let us follow the development of the simulation from the authors' words:

> Instead of a gradual, continuous flow of energy from the first modes to the higher modes, all of the problems show an entirely different behavior. Starting in one problem with a quadratic force and a pure sine wave as the initial position of the string, we indeed observe initially a gradual increase of energy in the higher modes as predicted (e. g., by Rayleigh in an infinitesimal analysis). Mode 2 starts increasing first, followed by mode 3, and so on. Later on, however, this gradual sharing of energy among successive modes ceases. Instead, it is one or the other mode that predominates. For example, mode 2 decides, as it were, to increase rather rapidly at the cost of all other modes and becomes predominant. At one time, it has more energy than all the others put together! Then mode 3 undertakes this role. It is only the first few modes which exchange energy among themselves and they do this in a rather regular fashion. Finally, at a later time mode 1 comes back to within one per cent of its initial value so that the system seems to be almost periodic.[165]

Ulam pointed out that the use of the computer was essential:

> Then the machine quickly computes in short time-steps the motion of each of these points. After having computed this, it goes to the next time-step, computes the new positions, and

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[164] *Ibidem,* p. 226–227.

[165] Fermi [266], *CPF II,* p. 981.

> so on for many times. There is absolutely no way to perform this numerical work with pencil and paper? it would literally take thousands of years.[166]

Although Fermi considered this as a "minor result," when in December 1954 he was invited to give the Gibbs Memorial Lecture at the congress of the American Mathematical Society (a very prestigious appointment), he chose to speak of this simulation. Unfortunately his illness did not allow him to do so.

The work of Fermi, Pasta, and Ulam, nowadays commonly known with the acronym FPU, started a new line of research about the foundations of statistical mechanics, and on the role of simulations in physics. In this connection, Massimo Falcioni, and Angelo Vulpiani take us back to Galileo's approach to science:

> Fermi and collaborators did not use the computer to obtain numerical details within the context of a well founded and understood theory; they instead performed a true *Gedankenexperiment,* verifying conjectures (which turned out to be wrong in the specific case) to try and shed light on a problem which was not well understood. This new way of using the computer is still with us, and we can state that numerical simulation has become a new branch of physics; together with theoretical and experimental physics, we speak now also of a computational physics. [...] The computer is not only useful for studying a given phenomenon, it can also, in a way, create the phenomenon through modelling. Simulation is somewhat like an experiment "in vitro" , in which one can choose those facets of a given phenomenon which are (or are deemed) relevant , bringing to the extreme the Galilean objective of "difalcare gli impedimenti" (removing the obstacles), which is not always possible in a real experiment.[167]

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[166] S. M. Ulam, *op. cit.*, p. 284.

[167] M. Falcioni and A. Vulpiani, *Enrico Fermi's contribution to non-linear systems: The influence of an unpublished article,* in C. Bernardini and L. Bonolis (eds.), *Enrico Fermi: His Work and Legacy,* Springer-Verlag, Berlin-Heidelberg 2001, p. 284.

Epilogue

The stereotype of a biographer is that of a narrator voice, which, based on the objectivity of the historical documents, reconstructs the intellectual or human events of the great protagonist of the story. If, in addition, it happens, as in this case, that the documentary reconstruction is guided by a consolidated historiographic model, the contribution of the biographer, his impressions, and judgement fade away, almost sterilized by the systematic application of the model.

On the other hand, by studying the documents, the biographer lives for some time with the character. My story with Enrico Fermi started three years ago, and that with the physics of his time, 25 years ago. Now I think to know many aspects of his person, perhaps difficult to document, but very clear in my perception: his style, the different nuances of his scientific "prudence," the young researcher's obedience to the academic rules, and the wisdom of the great master. And also his philosophy. According to a received view, the philosophical label to be attached to Fermi is pragmatism. But I always had a feeling, reading about his "pragmatic attitude," that this is a polite way of saying that he had no philosophy at all. If "having a philosophy" means to use a certain language and have some explicit reference points in some school of thought, then that is certainly true. But if, on the contrary, it means to look at one's own discoveries with a critical eye, assessing their value for the human knowledge, and, more generally, to be conscious of the cognitive value of the scientific enterprise, then I am sure that Fermi had a philosophy.

In some respects it was an innovative philosophy, especially for the importance it assigned to the quantitative aspects of knowledge, no less important than the qualitative ones, and, in any case, impossible to separate. Young Fermi, for instance, found very strange that the philosophical discussions about the theory of relativity were centered on length contraction and time dilation, effects that are quantitatively

G. Bruzzaniti, *Enrico Fermi*, Springer Biographies,
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negligible, without mentioning the equivalence of mass and energy, which, on the contrary, is a very important effect from the quantitative viewpoint:

> The grandiose conceptual importance of the theory of relativity as a contribution to a deeper understanding of the relationships between space and time and the often lively and passionate discussions to which it has as a consequence also given rise outside of the scientific environment, have perhaps diverted attention away from another of its results that, even though less sensational and let us say, even less paradoxical, nevertheless has consequences for physics no less worthy of note, and whose interest is realistically destined to grow in the near term development of science.
> The result to which we refer is the discovery of the relationship that ties the mass of a body to its energy.[1]

It is also quite easy to trace in Fermi's works several statements of a clear philosophical nature. In the third Donegani lecture, for instance, referring to Gamow's theory of formation of the elements, he wrote:

> So we cannot but sadly conclude that this theory is unable to explain how the elements have been formed. Perhaps this was to be expected. However, it is fitting to recognize Gamow's courage in trying to develop a theory based of very clear hypotheses; the theory failed, which means that some of its hypotheses are wrong. However, the result obtained in this way, i.e., the realization to have made a mistake, is certainly more remarkable than any result obtained with a theory so vague that, on the one hand, it is able to explain a great number of experimental phenomena, just because of the many arbitrary assumptions it contains; but on the other hand, does not show which of its parts is certainly wrong, so that it is not possible to correct the mistakes and build a new and more satisfactory theory.[2]

It is difficult not to recognize here the typical themes of Popper's philosophy. Perhaps Fermi was unaware of this, but that is not the point. The fact that he had philosophical positions belonging to a codified line of thought is irrelevant. The important thing is that Fermi's questions were problems that he believed could be answered not with a metalanguage, but rather with the language of science, into which the problems can indeed be decoded.

When I started collecting the bibliographic material for this book I found a slide used by Fermi in a 1954 lecture at Columbia (see Figure). It is the "project" of a particle accelerator, which he called the "Ultimate Accelerator," whose ring was to surround the Earth at an altitude of 1000 km. This semiserious slide seemed to me to express well the taste for paradox and provocation which is so typical of many scientists. Looking at it today, I see the metaphor of Fermi's scientific adventure: a man whose world was entirely surrounded by physics.

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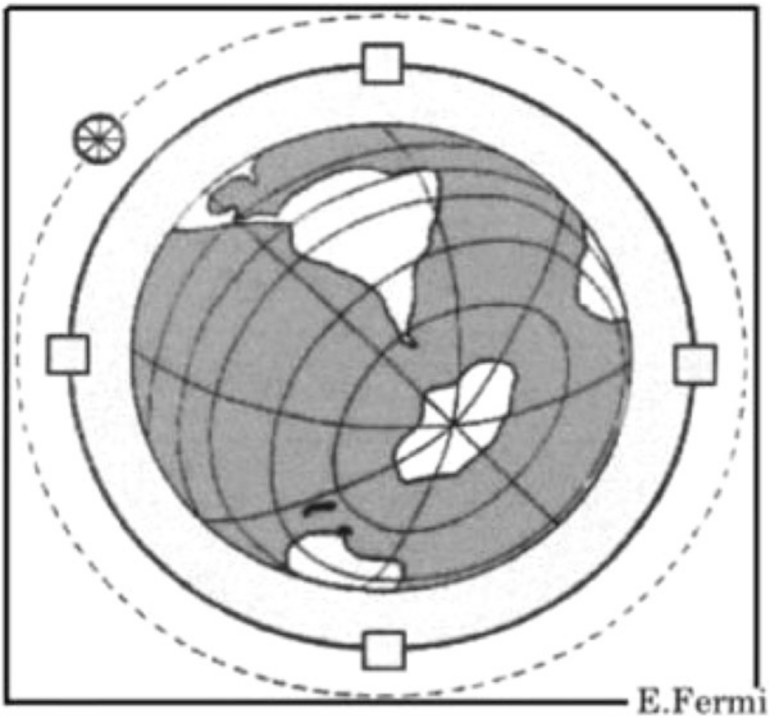
[1] E. Fermi [5], *CPF I,* p. 33. As translated in http://www34.homepage.villanova.edu/robert.jantzen/mg/fermi/2005/fermi5.pdf

[2] E. Fermi [240], *CPF II,* p. 720.



Fermi's "Ultimate Accelerator."

Erratum

Enrico Fermi

The Obedient Genius

Giuseppe Bruzzaniti

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Appendix A
Chronologies

A.1 Chronology of Fermi's life

1901. Enrico Fermi is born on 29 September in Rome, via Gaeta 19.
1911. He enrolls at the "Umberto I" high school.
1915. His brother Giulio dies.
1918. Graduates from high school one year ahead; on 14 November takes the admission test to Scuola Normale Superiore in Pisa and passes it. Enrolls in mathematics and after some time passes to physics.
1921. In January he publishes his first scientific work.
1922. On 7 July he graduates in Physics defending a thesis on X-ray diffraction. After three days he graduates from Scuola Normale (advanced degree in physics). On 20 March the March on Rome takes place. On 30 October Fermi is awarded a grant for a visit abroad.
1923. First stay abroad, at Göttingen in Max Born's institute.
1924. Charged of teaching Mathematics for Chemists at the University of Rome. On 8 May his mother dies. He is awarded a second grant for a visit abroad. Goes to Leiden. Charged of teaching mathematical physics at the University of Florence.
1925. On 2 March he is awarded "Libera Docenza" in mathematical physics.
1926. At the opening of a Mathematical Physics Professorship at the University of Cagliari, the committee in March 1926 awards the position to Giovanni Giorgi. He gets the first professorship in Theoretical Physics at the University of Rome. Moves to Rome, in the Physics Institute in Via Panisperna, where he joins the Roman physics school.
1927. His father dies on 7 May. In September, to commemorate the 100th anniversary of Alessandro Volta's death, an important physics conference takes place in Rome. The conference decrees Fermi's authoritativeness.
1928. On 19 July he marries Laura Capon.
1929. Appointed as member of the Italian Academy.

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1930. First visit to the United States, to give a summer course in theoretical physics in Ann Arbor.
1931. On 31 January Nella, his first daughter, is born. The first international conference on nuclear physics takes place in Rome.
1932. Discovery of the neutron, and little after, of the positron and deuterium. Invited in Paris at the 5th International Conference on Electricity, to give a talk on the state of the theory of the atomic nucleus.
1933. On 30 January Hindenburg appoints Hitler as Chancellor of the Reich; on 27 February the Nazis set the Reichstag on fire; on 28 February Hitler forces Hindenburg to sign a document which cancels the individual and civil rights from the Constitution. In October Fermi attends the 7th Solvay Conference, devoted to nuclear physics. In December he writes the fundamental paper on β decay.
1934. First experiments on the neutron-induced radioactivity. On 22 October the effects of slow neutrons are discovered.
1936. On 16 February his son Giulio is born. The Berlin-Rome Axe is established.
1937. On 23 January Orso Maria Corbino suddenly dies. On 20 July Guglielmo Marconi dies.
1938. In July the *Manifesto della razza* is published; the anti-semitic campaign starts in Italy; first racial laws promulgated. On 10 December Fermi receives the Nobel Prize for his researches on slow neutrons. On 24 December he sails to America with his family. In December the uranium fission is discovered.
1939. On 2 January he lands in New York. Starts working at Columbia. On 1 September the German troops invade Poland; World War II starts.
1940. On 11 June Italy enters the war. Roosevelt establishes the National Defense Research Committee, whose aim to promote war-related research.
1941. On 17 December the Japanese Air Force attacks Pearl Harbor.
1942. He moves to Chicago, together with many other physicists. He coordinates the project for the construction of an atomic pile. In June Roosevelt starts the Manhattan Project; in September its direction is entrusted to General Groves. On 2 December Fermi and his group start the first self-sustained chain reaction; the pile works for 28 minutes.
1943. In March Groves starts the Los Alamos laboratory for the construction of the first prototype of the fission bomb.
1944. In late summer Fermi moves to Los Alamos.
1945. On 12 April Roosevelt dies and is replaced by Truman. War ends in Europe on 8 May with Hitler's death. On 16 July the Trinity experiment takes place in Alamogordo, New Mexico. The first fission bomb explodes, releasing a power of 13 kton. On 6 and 9 August *Little Boy* and *Fat Man,* the first atomic bombs, are dropped on Hiroshima and Nagasaki.
1947. Fermi is appointed in the General Advisory Committee. He maintains that charge until 1950. Together with Teller and Weisskopf he interprets the Conversi-Pancini-Piccioni experiment.
1948. Works on the origin of the cosmic rays and resumes the systematic study of a new formulation of quantum electrodynamics.

1949. Together with Yang he formulates the composite pion model. In the Fall he goes to Italy, attends the Como conference, and gives a series of nine lectures in Rome and Milan (the Donegani Lectures).
1950. Participates in the design and construction of the H bomb.
1951. The first pion-nucleon resonance is discovered.
1952. Starts a collaboration with Chandrasekhar on some problems in astrophysics.
1953. He is appointed as President of the American Physical Society. Works with Ulam on the dynamics of nonlinear systems.
1954. On 18 July he is in Varenna, Italy, to deliver a series of lectures. Dies in Chicago in the early morning of 28 November.

A.2 Main events in 1939–42: from fission to the chain reaction

Year	Day and month	Scientific activity	Actions taken by the scientists	Initiatives of administrations or committees	International political events
1939	16 Feb	Anderson, Booth, Dunning, Fermi, Glasse, and Slack repeat the experiments on the fission of uranium; measurements of the cross-sections			
	16 Mar	Anderson, Fermi, and Hainstein prove experimentally that fission is associated with neutron production	Wigner, Szilard, Pegram, and Fermi informed the authorities of the possibility of a chain reaction		Hitler declares the German protectorate on Czechoslovakia
	17 Mar		Fermi, with Pegrams' reference letter, meets a delegation of the Government in Washington	First governmental funding; thanks to the intervention of one of the two scientists present in the meeting, Admiral Bowen allocates $1500 for the researches on uranium	
1939	17 May	Anderson and Fermi study neutron absorption by U^{238}			
	3 Jul	Anderson, Fermi, and Szilard realize the first experiment to prove the feasibility of the chain reaction; evidence that water is not a good moderator			

Year	Day and month	Scientific activity	Actions taken by the scientists	Initiatives of administrations or committees	International political events
	Jul-Aug	Intense correspondence between Fermi and Szilard; decision to use graphite as moderator			
	2 Aug		Einstein, after Szilard, Wigner, and Teller's request, writes the celebrated letter to Roosevelt		
	15 Aug		Einstein's letter is given to A. Sachs, but it reaches Roosevelt only in 11 October		
	1 Aug				Hitler invades Poland. World War II starts
	19 Oct			Roosevelt's answer to Einstein; L. J. Briggs chairs the new Uranium Committee (UC)	
1940	Feb	Peierls and Frisch compute the critical mass for a uranium bomb	Peierls and Frisch send Oliphant a report which describes the effects of an atomic bomb, and the techniques to assemble it; Oliphant passes the report to Tizard		

Year	Day and month	Scientific activity	Actions taken by the scientists	Initiatives of administrations or committees	International political events
	20 Feb			Allocation of the requested 6,000 $	
	2 Mar	Dumming gives an experimental proof of Bohr's hypothesis that slow neutrons only give rise to fission in uranium 235			
	Apr			The Thomson Committee is established in Great Britain to assess the feasibility of an atomic bomb,	
				the effects of its deployment, and the possibility of allocating resources to that project	
	7 Apr		Szilard, in letter to Pegram and Fermi, raises the question of secrecy		
	9 Apr				Hitler invades Denmark and Norway
	10 Apr			First meeting of the Thomson Committee	
	27 Apr			UC's second meeting; decision to stop the experiment on the uranium-carbon pile till the small-scale laboratory experiments are completed	

Year	Day and month	Scientific activity	Actions taken by the scientists	Initiatives of administrations or committees	International political events
	10 May				Hitler invades the Netherlands, Belgium, and France
	Jun	F. Simon starts the separation of isotopes by means of gas diffusion	A committee chaired by L. P. Eisenhart is established to control all publications having potential relevance to the war	The Thomson Committee changes name to “Maud Committee”	
	12 Jun			Roosevelt establishes the National Defense Research Council (NDRC), chaired by Vannevar Bush, to coordinate war-related research	
	1 Jul			NDRC takes on the responsibility for the Uranium Project and incorporates the UC; Briggs asks 140,000 $, Bush agrees for 40,000	
	25 Sep	Anderson and Fermi measure the cross-section for the absorption of slow neutrons by graphite, which turns out to be a good moderator			
	1 Nov	The construction of the pile starts at Columbia		The 40,000 $ approved in July are assigned	

Year	Day and month	Scientific activity	Actions taken by the scientists	Initiatives of administrations or committees	International political events
	Dec	The Maud Committee discusses a report by Simon on the isotope separation, and concludes that it is possible to produce uranium 235 by gas diffusion in a quantity sufficient to build an atomic bomb			
	14 Dec	Fermi, Lawrence, Pegram, and Segrè decide to start, using Lawrence's cyclotron, an investigation to isolate and study the possible decay product of neptunium, which is supposed to be fissile			
1941	Feb-Mar	Kennedy, Seaborg, Segrè, and Wahls separate ${}^{239}_{40}\mathrm{Pu}$ from the decay products of neptunium			
	Mar	Peierls uses the new data on the uranium 235 cross-section and computes a new value for the critical mass	The Maud Committee produces a report describing the importance of these results and transmits it to the United States	Briggs receives the Maud Committee report but does not show it	

Year	Day and month	Scientific activity	Actions taken by the scientists	Initiatives of administrations or committees	International political events
	17 May		After the pressures of Lawrence and the British scientists, Bush appoints a committee, chaired by Compton, to assess the military potentialities of the Uranium Project		
	18 May	Seaborg and Segrè discover that plutonium has better fissile properties than uranium 235			
	19 May		Lawrence calls Compton and stresses the urgency, in view of Seaborg and Segrè's results, to intensify the work of Fermi's group at Columbia		
	27 May		Kennedy, Lawrence, Seaborg, Segrè and Wahls write a letter to Briggs to inform him of their results about the possibilities offered by plutonium		
	22 Jun				Hitler invades the Soviet Union

Year	Day and month	Scientific activity	Actions taken by the scientists	Initiatives of administrations or committees	International political events
	28 Jun			Roosevelt establishes a new and more powerful office, chaired by Bush: the Office of Scientific Research and Development (OSRD); J. Conant replaces Bush as the NDRC chair, and Briggs' UC becomes Section S-1 of OSRD	
	30 Jun		Fermi reports at OSRD Section S-1 on the state of advancement of the pile		
	2 Jul			Bush and Conant receive a copy of the Maud Committee report	
	15 Jul		The Maud Committee approves a final report with some technical details about the atomic bomb, some proposals about its construction, and a cost estimate; the report is transmitted to OSRD	Bush receives the Maud Committee report but decides to wait before officially transmitting it	
	19 Aug			Fermi is appointed as chair of a theoretical OSRD S-1 subcommittee	

Year	Day and month	Scientific activity	Actions taken by the scientists	Initiatives of administrations or committees	International political events
	Aug-Sep	Fermi at Columbia starts to build the first atomic pile, using 30 tons of graphite, and 8 of uranium; the reproduction factor *k* however is quite small (0.83).			
	3 Sep			Churchill supports the British project for building an atomic bomb	
	3 Oct			The Maud Committee report is officially transmitted to the United States	
	9 Oct			Bush transmits the Maud Committee Report to the White House; Roosevelt approves a project along the lines of the Maud Report, extending the original American project	
	21 Oct		In a meeting called by Compton, with Lawrence, Oppenheimer, Kistiakowsky, and Conant, the Maud Report and the results obtained in America are reconsidered, under the hypothesis that the atomic bomb is feasible		

Year	Day and month	Scientific activity	Actions taken by the scientists	Initiatives of administrations or committees	International political events
	6 Dec			Bush and Conant call a meeting of the S-1 Section in Washington DC, with the objective of speeding up the construction of the bomb;	
				specific tasks are assigned to Urey (separation of the isotopes by gas diffusion) and Lawrence (separation by electromagnetic fields)	
	7 Dec				Pearl Harbor
	8 Dec				United States declare war to Japan
	11 Dec				United States declare war to Germany and Italy
	18 Dec			Section S-1 meets	
1942	Jan		Compton creates the Metallurgical Laboratory (MetLab) at the University of Chicago; Oppenheimer organizes in Berkeley a theoretical program on fast neutrons		
	Feb		Compton asks Breit to coordinate the investigations on the fast neutrons		

Year	Day and month	Scientific activity	Actions taken by the scientists	Initiatives of administrations or committees	International political events
	23 Mar			In a S-1 meeting Conant stresses the urgency to proceed in all possible ways to produce fissile material: gas separation, centrifugation, electromagnetic separation, and production of plutonium with a uranium-graphite reactor	
	Apr	Fermi, now in Chicago, starts building the CP1 pile; Seaborg starts a project for separating and purifying plutonium at an industrial scale			
	18 May		Breit leaves; Compton asks Oppenheimer to replace him		
	Jun			Roosevelt approves a budget of 85 million dollars to develop nuclear weapons	
	Jul		Oppenheimer puts up a study group in Berkeley, to study the theoretical principles of the bomb. Bethe, Teller, Van Vleck, Bloch, Serber, and Konopiski attend; a critical mass of 30 kg of uranium 235 is estimated to be necessary to get a 100 kton explosion.	The S-1 steering committee decides to follow also the project for the hydrogen bomb	

Year	Day and month	Scientific activity	Actions taken by the scientists	Initiatives of administrations or committees	International political events
			The possibility to build a combustion bomb, with hydrogen as fuel, is also discussed		
	Aug	Fermi obtains a reproduction factor *k* of 1.04; the chain reaction is possible		Colonel Marshall creates the Manhattan Engineer District (MED)	
	20 Aug	Seaborg isolates pure plutonium by a process that can be realized at an industrial scale			
	13 Sep		The need of a laboratory for the study of fast neutrons is discussed in an S-1 meeting; the code name is Project Y		
	15 Sep	Fermi gets the uranium and graphite necessary for CP-1			
	17 Sep			The command of the Manhattan Project is given to Colonel Groves, who is promoted to general on 23 September	
	18–19 Sep			Groves buys 1250 tons of high-quality uranium, and a 52,000 acre land parcel in Oak Ridge, Tennessee	

Year	Day and month	Scientific activity	Actions taken by the scientists	Initiatives of administrations or committees	International political events
	Oct			Groves entrusts DuPont with the production of plutonium	
	15 Oct			Groves asks Oppenheimer to chair Project Y	
	16 Nov	Fermi starts building CP-1 at the Staggs Field			
	2 Dec	At 3:49 pm CP-1 becomes critical and works for about 30 minutes at a 0.5 W power			
	Dec			Roosevelt approves, according to Bush's estimates, a budget of 400 million dollars for Manhattan Project; Groves and Oppenheimer visit Los Alamos in New Mexico, and choose it as the site of the Project	
1943	Jan			Groves buys Hanford Engineer Works, about 50,000 acres on the Columbia River in the state of Washington, for the production of plutonium	
				with a reactor and a separation plant	

Year	Day and month	Scientific activity	Actions taken by the scientists	Initiatives of administrations or committees	International political events
	18 Feb			A plant for the electromagnetic separation of uranium 235 is built in Oak Ridge	
	Mar			The Los Alamos laboratories are almost ready; the personnel starts to arrive. The space is not sufficient	
	Apr	The idea of implosion is considered, to avoid pre-detonation problems	Serber gives a series of lectures at Los Alamos, the “April Lectures,” which officially start the operations of the Manhattan Project		
	20 Sep	John von Neumann stresses the potential features of the implosion, and accepts to work on the problem at Los Alamos; Teller and Bethe start the theoretical analysis		Oppenheimer and Groves show interest in the implosion problem, and try to speed up the project	
	23 Sep			Oppenheimer suggests to hire Kistiakowsky to work on the implosion	
	4 Oct	The DuPont engineers conclude the design of the Hanford reactor, and the production of plutonium starts			

Year	Day and month	Scientific activity	Actions taken by the scientists	Initiatives of administrations or committees	International political events
1943	Nov	MetLab produces the first sample of metallic plutonium		Bohr, Frisch, Peierls, Chadwick, Penney, Placzek, Moon, Tuck, Bretscher, and Fuchs (many of them Maud members) arrive in the United States to participate in the construction of the atomic bomb	
	4 Nov	The Oak Ridge X-10 pile becomes critical; the first sensible amounts of plutonium (about 1 g) are obtained			
1944	Jan			A group for the theoretical study of implosion is constituted	
	Feb	The construction of the first Hanford reactor (the B-pile) starts		At Los Alamos the investigations on the deuterium fusion are reconsidered	
	Apr	The first IBM computing machines arrive at Los Alamos; Tuck suggests to use “explosive lens” to create convergent implosive spherical waves			
	28 May	First experimental test on the implosion lenses			

Year	Day and month	Scientific activity	Actions taken by the scientists	Initiatives of administrations or committees	International political events
	Jun	Bethe, Peirerls, and von Neumann work on the development of the implosion lenses		Oppenheimer replaces Neddermeyer with Kistiakowsky in the coordination of the researches on the implosion	
	Jul	The experiments with the implosion lenses start; the project of the initiator is concluded	Oppenheimer announces Segrè's results on the spontaneous fission of plutonium; the assembly of the bomb critical mass cannot be done with a cannon;		
			the realizability of the project depends now on the implosion techniques. Oppenheimer is forced to reorganize the Los Alamos laboratories		
	Aug		Fermi's family moves to Los Alamos, before him, who is still in Hanford		
	26 Sep	The Hanford reactor starts working; it contains 200 ton of uranium and 1200 ton of graphite. It works at 250 MW e produces 6 kg of plutonium per month. Fermi supervises the start of the reaction			

Year	Day and month	Scientific activity	Actions taken by the scientists	Initiatives of administrations or committees	International political events
	Dec	The tests made in Los Alamos on the implosion lenses are positive. The implosion bomb can be made			
1945	Jan	The Y-12 complex reaches a production of 204 g of uranium 235 per day; the amount necessary to build a bomb (about 40 kg) will be produced by July			
	31 Jan	Bacher reports to Oppenheimer that a beryllium-polonium initiator can be built			
	12 Apr				Roosevelt suddenly dies; he is immediately replaced by Truman
	8 May				The war with Germany ends with Hitler's suicide
	11 Jun		Franck report		
	16 Jun		Compton, Fermi, Lawrence, and Oppenheimer forward the report "Recommendations on the immediate use of nuclear weapons"		

Year	Day and month	Scientific activity	Actions taken by the scientists	Initiatives of administrations or committees	International political events
	3 Jul		Petition by Szilard and other 58 scientists against the use of the atomic bomb		
	16 Jul		Trinity experiment at Alamogordo		
	17 Jul	Second Szilard’s petition to President Truman; this time the signatures are 69			
	6 Aug			Uranium atomic bomb on Hiroshima	
	9 Aug			Second atomic bomb (plutonium) on Nagasaki	

Appendix B
Documents

B.1 Pauli's neutrino hypothesis: open letter to the Radioactive Group at the Tübingen meeting (1930)

Physics Institute
of the ETH
Zürich

Zürich, Dec. 4, 1930
Gloriastrasse

Dear Radioactive Ladies and Gentlemen,

As the bearer of these lines, to whom I graciously ask you to listen, will explain to you in more detail, because of the "wrong" statistics of the N- and Li-6 nuclei and the continuous beta spectrum, I have hit upon a desperate remedy to save the "exchange theorem" (1) of statistics and the law of conservation of energy. Namely, the possibility that in the nuclei there could exist electrically neutral particles, which I will call neutrons, that have spin 1/2 and obey the exclusion principle and that further differ from light quanta in that they do not travel with the velocity of light. The mass of the neutrons should be of the same order of magnitude as the electron mass and in any event not larger than 0.01 proton mass. — The continuous beta spectrum would then make sense with the assumption that in beta decay, in addition to the electron, a neutron is emitted such that the sum of the energies of neutron and electron is constant.

Now it is also a question of which forces act upon neutrons. For me, the most likely model for the neutron seems to be, for wave-mechanical reasons (the bearer of these lines knows more), that the neutron at rest is a magnetic dipole with a certain moment μ. The experiments seem to require that the ionizing effect of such a neutron can not be bigger than the one of a gamma-ray, and then μ is probably not allowed to be larger than $e \cdot (10^{-13}\ \text{cm})$.

G. Bruzzaniti, *Enrico Fermi*, Springer Biographies,
DOI 10.1007/978-1-4939-3533-8

But so far I do not dare to publish anything about this idea, and trustfully turn first to you, dear radioactive people, with the question of how likely it is to find experimental evidence for such a neutron if it would have the same or perhaps a 10 times larger ability to get through [material] than a gamma-ray.

I admit that my remedy may seem almost improbable because one probably would have seen those neutrons, if they exist, for a long time. But nothing ventured, nothing gained, and the seriousness of the situation, due to the continuous structure of the beta spectrum, is illuminated by a remark of my honored predecessor, Mr Debye, who told me recently in Bruxelles: “Oh, It’s better not to think about this at all, like new taxes.” Therefore one should seriously discuss every way of rescue. Thus, dear radioactive people, scrutinize and judge. — Unfortunately, I cannot personally appear in Tübingen since I am indispensable here in Zürich because of a ball on the night from December 6 to 7. With my best regards to you, and also to Mr. Back, your humble servant

signed W. Pauli

[Translated from the German by Kurt Riesselmann]

B.2 Albert Einstein's letter to Roosevelt and Roosevelt's answer (1939)

Albert Einstein
Old Grove Road
Peconic, Long Island
August 2nd, 1939

F.D. Roosevelt
President of the United States
White House
Washington, D.C.

Sir:

Some recent work by E. Fermi and L. Szilard, which has been communicated to me in manuscript, leads me to expect that the element uranium may be turned into a new and important source of energy in the immediate future. Certain aspects of the situation which has arisen seem to call for watchfulness and if necessary, quick action on the part of the Administration. I believe therefore that it is my duty to bring to your attention the following facts and recommendations.

In the course of the last four months it has been made probable through the work of Joliot in France as well as Fermi and Szilard in America — that it may be possible to set up a nuclear chain reaction in a large mass of uranium, by which vast amounts of power and large quantities of new radium-like elements would be generated. Now it appears almost certain that this could be achieved in the immediate future.

This new phenomenon would also lead to the construction of bombs, and it is conceivable — though much less certain — that extremely powerful bombs of this type may thus be constructed. A single bomb of this type, carried by boat and exploded in a port, might very well destroy the whole port together with some of the surrounding territory. However, such bombs might very well prove too heavy for transportation by air.

The United States has only very poor ores of uranium in moderate quantities. There is some good ore in Canada and former Czechoslovakia, while the most important source of uranium is in the Belgian Congo.

In view of this situation you may think it desirable to have some permanent contact maintained between the Administration and the group of physicists working on chain reactions in America. One possible way of achieving this might be for you to entrust the task with a person who has your confidence and who could perhaps serve in an unofficial capacity. His task might comprise the following:

a) to approach Government Departments, keep them informed of the further development, and put forward recommendations for Government action, giving particular attention to the problem of securing a supply of uranium ore for the United States.
b) to speed up the experimental work, which is at present being carried on within the limits of the budgets of University laboratories, by providing funds, if such funds be required, through his contacts with private persons who are willing to make contributions for this cause, and perhaps also by obtaining co-operation of industrial laboratories which have necessary equipment.

I understand that Germany has actually stopped the sale of uranium from the Czechoslovakian mines which she has taken over. That she should have taken such early action might perhaps be understood on the ground that the son of the German Under-Secretary of State, von Weizsäcker, is attached to the Kaiser–Wilhelm Institute in Berlin, where some of the American work on uranium is now being repeated.

Yours very truly,
A. Einstein
Albert Einstein

[http://www.atomicarchive.com/Docs/Begin/Einstein.shtml]

THE WHITE HOUSE
WASHINGTON

October 19, 1939

My dear Professor:

I want to thank you for your recent letter and the most interesting and important enclosure.

I found this data of such import that I have convened a Board consisting of the head of the Bureau of Standards and a chosen representative of the Army and Navy to thoroughly investigate the possibilities of your suggestion regarding the element of uranium.

I am glad to say that Dr. Sachs will cooperate and work with this Committee and I feel this is the most practical and effective method of dealing with the subject.

Please accept my sincere thanks.

Yours very truly,

Franklin D. Roosevelt

Dr. Albert Einstein,
Old Grove Road,
Nassau Point,
Poconic, Long Island,
New York

[http://www.atomicarchive.com/Docs/Begin/Roosevelt.shtml].

B.3 War Department release on New Mexico test, July 16, 1945

Mankind's successful transition to a new age, the Atomic Age, was ushered in July 16, 1945, before the eyes of a tense group of renowned scientists and military men gathered in the desert lands of New Mexico to witness the first end results of their $2,000,000,000 effort. Here in a remote section of the Alamogordo Air Base 120 miles southeast of Albuquerque the first man-made atomic explosion, the outstanding achievement of nuclear science, was achieved at 5:30 a.m. of that day. Darkening heavens, pouring forth rain and lightning immediately up to the zero hour, heightened the drama.

Mounted on a steel tower, a revolutionary weapon destined to change war as we know it, or which may even be the instrumentality to end all wars, was set off with an impact which signalized man's entrance into a new physical world. Success was greater than the most ambitious estimates. A small amount of matter, the product of a chain of huge specially constructed industrial plants, was made to release the energy of the universe locked up within the atom from the beginning of time. A fabulous achievement had been reached. Speculative theory, barely established in pre-war laboratories, had been projected into practicality.

This phase of the Atomic Bomb Project, which is headed by Major General Leslie R. Groves, was under the direction of Dr. J. R. Oppenheimer, theoretical physicist of the University of California. He is to be credited with achieving the implementation of atomic energy for military purposes.

Tension before the actual detonation was at a tremendous pitch. Failure was an ever-present possibility. Too great a success, envisioned by some of those present, might have meant an uncontrollable, unusable weapon.

Final assembly of the atomic bomb began on the night of July 12 in an old ranch house. As various component assemblies arrived from distant points, tension among the scientists rose to an increasing pitch. Coolest of all was the man charged with the actual assembly of the vital core, Dr. R. F. Bacher, in normal times a professor at Cornell University.

The entire cost of the project, representing the erection of whole cities and radically new plants spread over many miles of countryside, plus unprecedented experimentation, was represented in the pilot bomb and its parts. Here was the focal point of the venture. No other country in the world had been capable of such an outlay in brains and technical effort.

The full significance of these closing moments before the final factual test was not lost on these men of science. They fully knew their position as pioneers into another age. They also knew that one false move would blast them and their entire effort into eternity. Before the assembly started a receipt for the vital matter was signed by Brigadier General Thomas F. Farrell, General Groves' deputy. This signalized the formal transfer of the irreplaceable material from the scientists to the Army.

During final preliminary assembly, a bad few minutes developed when the assembly of an important section of the bomb was delayed. The entire unit was machine-tooled to the finest measurement. The insertion was partially completed when it apparently wedged tightly and would go no farther. Dr. Bacher, however, was undismayed and reassured the group that time would solve the problem. In three minutes' time, Dr. Bacher's statement was verified and basic assembly was completed without further incident.

Specialty teams, comprised of the top men on specific phases of science, all of which were bound up in the whole, took over their specialized parts of the assembly. In each group was centralized months and even years of channelized endeavor.

On Saturday, July 14, the unit which was to determine the success or failure of the entire project was elevated to the top of the steel tower. All that day and the next, the job of preparation went on. In addition to the apparatus necessary to cause the detonation, complete instrumentation to determine the pulse beat and all reactions of the bomb was rigged on the tower.

The ominous weather which had dogged the assembly of the bomb had a very sobering effect on the assembled experts whose work was accomplished amid lightning flashes and peals of thunder. The weather, unusual and upsetting, blocked out aerial observation of the test. It even held up the actual explosion scheduled at 4:00 a.m. for an hour and a half. For many months the approximate date and time had been set and had been one of the high-level secrets of the best kept secret of the entire war.

Nearest observation point was set up 10,000 yards south of the tower where in a timber and earth shelter the controls for the test were located. At a point 17,000 yards from the tower at a point which would give the best observation the key figures in the atomic bomb project took their posts. These included General Groves, Dr. Vannevar Bush, head of the Office of Scientific Research and Development and Dr. James B. Conant, president of Harvard University.

Actual detonation was in charge of Dr. K. T. Bainbridge of Massachusetts Institute of Technology. He and Lieutenant Bush, in charge of the Military Police Detachment, were the last men to inspect the tower with its cosmic bomb.

At three o'clock in the morning the party moved forward to the control station. General Groves and Dr. Oppenheimer consulted with the weathermen. The decision was made to go ahead with the test despite the lack of assurance of favorable weather. The time was set for 5:30 a.m.

General Groves rejoined Dr. Conant and Dr. Bush, and just before the test time they joined the many scientists gathered at the Base Camp. Here all present were ordered to lie on the ground, face downward, heads away from the blast direction.

Tension reached a tremendous pitch in the control room as the deadline approached. The several observation points in the area were tied in to the control room by radio and with twenty minutes to go, Dr. S. K. Allison of Chicago University took over the radio net and made periodic time announcements.

The time signals, "minus 20 minutes, minus fifteen minutes," and on and on increased the tension to the breaking point as the group in the control room which included Dr. Oppenheimer and General Farrell held their breaths, all praying with the intensity of the moment which will live forever with each man who was there. At “minus 45 seconds,” robot mechanism took over and from that point on the whole great complicated mass of intricate mechanism was in operation without human control. Stationed at a reserve switch, however, was a soldier scientist ready to attempt to stop the explosion should the order be issued. The order never came.

At the appointed time there was a blinding flash lighting up the whole area brighter than the brightest daylight. A mountain range three miles from the observation point stood out in bold relief. Then came a tremendous sustained roar and a heavy pressure wave which knocked down two men outside the control center. Immediately thereafter, a huge multi-colored surging cloud boiled to an altitude of over 40,000 feet. Clouds in its path disappeared. Soon the shifting substratosphere winds dispersed the now grey mass.

The test was over, the project a success.

The steel tower had been entirely vaporized. Where the tower had stood, there was a huge sloping crater. Dazed but relieved at the success of their tests, the scientists promptly marshaled their forces to estimate the strength of America’s new weapon. To examine the nature of the crater, specially equipped tanks were wheeled into the area, one of which carried Dr. Enrico Fermi, noted nuclear scientist. Answer to their findings rests in the destruction effected in Japan today in the first military use of the atomic bomb.

Had it not been for the desolated area where the test was held and for the cooperation of the press in the area, it is certain that the test itself would have attracted far-reaching attention. As it was, many people in that area are still discussing the effect of the smash. A significant aspect, recorded by the press, was the experience of a blind girl near Albuquerque many miles from the scene, who, when the flash of the test lighted the sky before the explosion could be heard, exclaimed, “What was that?”

Interviews of General Groves and General Farrell give the following on-the-scene versions of the test. General Groves said: “My impressions of the night’s high points follow: After about an hour’s sleep I got up at 0100 and from that time on until about five I was with Dr. Oppenheimer constantly. Naturally he was tense, although his mind was working at its usual extraordinary efficiency. I attempted to shield him from the evident concern shown by many of his assistants who were disturbed by the uncertain weather conditions. By 0330 we decided that we could probably fire at 0530. By 0400 the rain had stopped but the sky was heavily overcast. Our decision became firmer as time went on.

During most of these hours the two of us journeyed from the control house out into the darkness to look at the stars and to assure each other that the one or two visible stars were becoming brighter. At 0510 I left Dr. Oppenheimer and returned to the main observation point which was 17,000 yards from the point of explosion. In accordance with our orders I found all personnel not otherwise occupied massed on a bit of high ground.

Two minutes before the scheduled firing time, all persons lay face down with their feet pointing towards the explosion. As the remaining time was called from the loud speaker from the 10,000-yard control station there was complete awesome silence. Dr. Conant said he had never imagined seconds could be so long. Most of the individuals in accordance with orders shielded their eyes in one way or another.

First came the burst of light of a brilliance beyond any comparison. We all rolled over and looked through dark glasses at the ball of fire. About forty seconds later came the shock wave followed by the sound, neither of which seemed startling after our complete astonishment at the extraordinary lighting intensity.

A massive cloud was formed which surged and billowed upward with tremendous power, reaching the substratosphere in about five minutes.

Two supplementary explosions of minor effect other than the lighting occurred in the cloud shortly after the main explosion.

The cloud traveled to a great height first in the form of a ball, then mushroomed, then changed into a long trailing chimney-shaped column, and finally was sent in several directions by the variable winds at the different elevations.

Dr. Conant reached over and we shook hands in mutual congratulations. Dr. Bush, who was on the other side of me, did likewise. The feeling of the entire assembly, even the uninitiated, was of profound awe. Drs. Conant and Bush and myself were struck by an even stronger feeling that the faith of those who had been responsible for the initiation and the carrying on of this Herculean project had been justified.

General Farrell's impressions are: "The scene inside the shelter was dramatic beyond words. In and around the shelter were some twenty odd people concerned with last-minute arrangements. Included were Dr. Oppenheimer, the Director who had borne the great scientific burden of developing the weapon from the raw materials made in Tennessee and Washington, and a dozen of his key assistants, Dr. Kistiakowsky, Dr. Bainbridge, who supervised all the detailed arrangements for the test; the weather expert, and several others. Besides those, there were a handful of soldiers, two or three Army officers and one Naval Officer. The shelter was filled with a great variety of instruments and radios."

"For some hectic two hours preceding the blast, General Groves stayed with the Director. Twenty minutes before the zero hour, General Groves left for his station at the base camp, first because it provided a better observation point and second, because of our rule that he and I must not be together in situations where there is an element of danger which existed at both points."

"Just after General Groves left, announcements began to be broadcast of the interval remaining before the blast to the other groups participating in and observing the test. As the time interval grew smaller and changed from minutes to seconds, the tension increased by leaps and bounds. Everyone in that room knew the awful potentialities of the thing that they thought was about to happen. The scientists felt that their figuring must be right and that the bomb had to go off but there was in everyone's mind a strong measure of doubt."

"We were reaching into the unknown and we did not know what might come of it. It can safely be said that most of those present were praying–and praying harder than they had ever prayed before. If the shot were successful, it was a justification of the several years of intensive effort of tens of thousands of people — statesmen, scientists, engineers, manufacturers, soldiers, and many others in every walk of life."

"In that brief instant in the remote New Mexico desert, the tremendous effort of the brains and brawn of all these people came suddenly and startlingly to the fullest fruition. Dr. Oppenheimer, on whom had rested a very heavy burden, grew tenser as the last seconds ticked off. He scarcely breathed. He held on to a post to steady himself. For the last few seconds, he stared directly ahead and then when the announcer shouted 'Now!' and there came this tremendous burst of light followed shortly thereafter by the deep growling roar of the explosion, his face relaxed into an expression of tremendous relief. Several of the observers standing back of the shelter to watch the lighting effects were knocked flat by the blast."

"The tension in the room let up and all started congratulating each other. Everyone sensed 'This is it!'. No matter what might happen now all knew that the impossible scientific job had been done. Atomic fission would no longer be hidden in the cloisters of the theoretical physicists' dreams. It was almost full grown at birth. It was a great new force to be used for good or for evil. There was a feeling in that shelter that those concerned with its nativity should dedicate their lives to the mission that it would always be used for good and never for evil."

"Dr. Kistiakowsky threw his arms around Dr. Oppenheimer and embraced him with shouts of glee. Others were equally enthusiastic. All the pent-up emotions were released in those few minutes and all seemed to sense immediately that the explosion had far exceeded the most optimistic expectations and wildest hopes of the scientists. All seemed to feel that they had been present at the birth of a new age — The Age of Atomic Energy — and felt their profound responsibility to help in guiding into right channels the tremendous forces which had been unlocked for the first time in history."

"As to the present war, there was a feeling that no matter what else might happen, we now had the means to insure its speedy conclusion and save thousands of American lives. As to the future, there had been brought into being something big and something new that would prove to be immeasurably more important than the discovery of electricity or any of the other great discoveries which have so affected our existence."

"The effects could well be called unprecedented, magnificent, beautiful, stupendous and terrifying. No man-made phenomenon of such tremendous power had ever occurred before. The lighting effects beggared description. The whole country was lighted by a searing light with the intensity many times that of the midday sun. It was golden, purple, violet, gray and blue. It lighted every peak, crevasse and ridge of the nearby mountain range with a clarity and beauty that cannot be described but must be seen to be imagined. It was that beauty the great poets dream about but describe most poorly and inadequately. Thirty seconds after, the explosion came first, the air blast pressing hard against the people and things, to be followed almost immediately by the strong, sustained, awesome roar which warned of doomsday and made us feel that we puny things were blasphemous to dare tamper with the forces heretofore reserved to the Almighty. Words are inadequate tools for the job of acquainting those not present with the physical, mental and psychological effects. It had to be witnessed to be realized."

B.4 Fermi and the secrecy problem (1954)

A curious circumstance related to this phase of the work was that here for the first time secrecy that has been plaguing us for a number of years started and, contrary to perhaps what is the most common belief about secrecy, secrecy was not started by generals, was not started by security officers, but was started by physicists. And the man who is mostly responsible for this certainly extremely novel idea for physicists was Szilard.

I don't know how many of you know Szilard; no doubt very many of you do. He is certainly a very peculiar man, extremely intelligent (laughter). I see that is an understatement (laughter). He is extremely brilliant and he seems somewhat to enjoy, at least that is the impression that he gives to me, he seems to enjoy startling people.

So he proceeded to startle physicists by proposing to them that given the circumstances of the period — you see it was early 1939 and war was very much in the air — given the circumstances of that period, given the danger that atomic energy and possibly atomic weapons could become the chief tool for the Nazis to enslave the world, it was the duty of the physicists to depart from what had been the tradition of publishing significant results as soon as the *Physical Review* or other scientific journals might turn them out, and that instead one had to go easy, keep back some results until it was clear whether these results were potentially dangerous or potentially helpful to our side.

So Szilard talked to a number of people and convinced them that they had to join some sort of — I don't know whether it would be called a secret society, or what it would be called. Anyway to get together and circulate this information privately among a rather restricted group and not to publish it immediately. He sent in this vein a number of cables to Joliot in France, hut he did not get a favorable response from him and Joliot published his results more or less like results in physics had been published until that day. So that the fact that neutrons are emitted in fission in some abundance — the order of magnitude of one or two or three — became a matter of general knowledge. And, of course, that made the possibility of a chain reaction appear to most physicists as a vastly more real possibility than it had until that time.

Physics Today, 8 (1955), p. 12–16; *CPF II,* p. 1003.

B.5 Physics at Columbia University (the genesis of the nuclear energy project)

So physicists on the seventh floor of Pupin Laboratories started looking like coal miners (laughter) and the wives to whom these physicists came back tired at night were wondering what was happening. We know that there is smoke in the air, but after all . . . (laughter).

Well, what was happening was that in those days we were trying to learn something about the absorption properties of graphite, because perhaps graphite was no good. So, we built columns of graphite, maybe four feet on the side or something like that, maybe ten feet high. It was the first time when apparatus in physics, and these graphite columns were apparatus, was so big that you could climb on top of it — and you had to climb on top of it. Well, cyclotrons were the same way too, but anyway that was the first time when I started climbing on top of my equipment because it was just too tall — I'm not a tall man (laughter).

And the sources of neutrons were inserted at the bottom and we were studying how these neutrons were first slowed down and then diffused up the column and of course if there had been a strong absorption they would not have diffused very high. But because it turned out that the absorption was in fact small, they could diffuse quite readily up this column and by making a little bit of mathematical analysis of the situation it became possible to make the first guesses as to what was the absorption cross section of graphite, a key element in deciding the possibility or not of fabricating a chain reacting unit with graphite and natural uranium.

Well, I will not go into detail of this experimentation. That lasted really quite a number of years and required really quite many hours and many days and many weeks of extremely hard work. I may mention that very early our efforts were brought in connection with similar efforts that were taking place at Princeton University where a group with Wigner, Creutz and Bob Wilson set to work making some measurements that we had no possibility of carrying out at Columbia University. Well, as time went on, we began to identify what had to be measured and how accurately these things that I shall call "eta," f , and p — I don't think I have time to define them for you — these three quantities "eta," f , and p had to be measured to establish what could be done and what could not be done. And, in fact, if I may say so, the product of "eta," f , and p had to be greater than one. It turns out, we now know, that if one does just about the best this product can be 1.1.

So, if we had been able to measure these three quantities to the accuracy of one percent we might have found that the product was for example 1.08 plus or minus 0.03 and if that had been the case we would have said let's go ahead, or if the product had turned out to be 0.95 plus or minus 0.03 perhaps we would have said just that this line of approach is not very promising, and we had better look for something else. However I've already commented on the extremely low quality of the measurements in neutron physics that could be done at the time — where the accuracy of measuring separately either "eta," or f , or p was perhaps with a plus or minus of 20 percent (laughter). If you compound, by the well-known rules of statistics, three errors of 20 percent you will find something around 35 percent. So if you should find, for example, 0.9 plus or minus 0.3 — what do you know? Hardly anything at all (laughter). If you find 1.1 plus or minus 0.3 — again, you don't know anything much. So that was the trouble and in fact if you look in our early work — what were the detailed values given by this or that experimenter to, for example, "eta" you find that it was off 20 percent and sometimes greater amounts. In fact I think it was strongly influenced by the temperament of the physicist. Shall we say optimistic physicists felt it unavoidable to push these quantities high and pessimistic physicists like myself tried to keep them somewhat on the low side (laughter).

Anyway, nobody really knew and we decided therefore that one had to do something else. One had to devise some kind of experiment that would give a complete over-all measurement directly of the product "eta," f, p without having to measure separately the three, because then perhaps the error would sort of drop down and permit us to reach conclusions.

Well, we went to Dean Pegram, who was then the man who could carry out magic around the University, and we explained to him that we needed a big room. And when we say big we meant a really big room, perhaps he made a crack about a church not being the most suited place for a physics laboratory in his talk, but I think a church would have been just precisely what we wanted (laughter). Well, he scouted around the campus and we went with him to dark corridors and under various heating pipes and so on to visit possible sites for this experiment and eventually a big room, not a church, but something that might have been compared in size with a church was discovered in Schermerhorn.

And there we started to construct this structure that at that time looked again in order of magnitude larger than anything that we had seen before. Actually if anybody would look at that structure now he would probably extract his magnifying glass (laughter) and go close to see it. But for the ideas of the time it looked really big. It was a structure of graphite bricks and spread through these graphite bricks in some sort of pattern were big cans, cubic cans, containing uranium oxide.

Now, graphite is a black substance, as you probably know. So is uranium oxide. And to handle many tons of both makes people very black. In fact it requires even strong people. And so, well we were reasonably strong, but I mean we were, after all, thinkers (laughter). So Dean Pegram again looked around and said that seems to be a job a little bit beyond your feeble strength, but there is a football squad at Columbia (laughter) that contains a dozen or so of very husky boys who take jobs by the hour just to carry them through College. Why don't you hire them?

And it was a marvelous idea; it was really a pleasure for once to direct the work of these husky boys, canning uranium — just shoving it in — handling packs of 50 or 100 pounds with the same ease as another person would have handled three or four pounds. In passing these cans fumes of all sorts of colors, mostly black, would go in the air (laughter).

Well, so grew what was called at the time the exponential pile. It was an exponential pile, because in the theory an exponential function enters — which is not surprising. And it was a structure that was designed to test in an integral way, without going down to fine details, whether the reactivity of the pile, the reproduction factor, would be greater or less than one. Well, it turned out to be 0.87. Now that is by 0.13 less than one and it was bad. However, at the moment we had a firm point to start from, and we had essentially to see whether we could squeeze the extra 0.13 or preferably a little bit more. Now there were many obvious things that could be done. First of all, I told you these big cans were canned in tin cans, so what has the iron to do? Iron can do only harm, can absorb neutrons, and we don't want that. So, out go the cans. Then, what about the purity of the materials? We took samples of uranium, and with our physicists' lack of skill in chemical analysis, we sort of tried to find out the impurities and certainly there were impurities. We would not know what they were, but they looked impressive, at least in bulk (laughter). So, now, what do these impurities do? — clearly they can do only harm. Maybe they make harm to the tune of 13 percent. Finally, the graphite was quite pure for the standards of that time, when graphite manufacturers were not concerned with avoiding those special impurities that absorb neutrons. But still there was some considerable gain to be made out there, and especially Szilard at that time took extremely decisive and strong steps to try to organize the early phases of production of pure materials. Now, he did a marvelous job which later on was taken over by a more powerful organization than was Szilard himself. Although to match Szilard it takes a few able-bodied customers (laughter).

Well, this brings us to Pearl Harbor. At that time, in fact I believe a few days before by accident, the interest in carrying through the uranium work was spreading; work somewhat similar to what was going on at Columbia had been initiated in a number of different Universities throughout the country. And the government started taking decisive action in order to organize the work, and, of course, Pearl Harbor gave the final and very decisive impetus to this organization. And it was decided in the high councils of the government that the work on the chain reaction produced by nonseparated isotopes of uranium should go to Chicago.

That is the time when I left Columbia University, and after a few months of commuting between Chicago and New York eventually moved to Chicago to keep up the work there, and from then on, with a few notable exceptions, the work at Columbia was concentrated on the isotope-separation phase of the atomic energy project.

As I've indicated this work was initiated by Booth, Dunning, and Urey about 1940, 1939, and 1940, and with this reorganization a large laboratory was started at Columbia under the direction of Professor Urey. The work there was extremely successful and rapidly expanded into the build-up of a huge research laboratory which cooperated with the Union Carbide Company in establishing some of the separation plants at Oak Ridge. This was one of the three horses on which the directors of the atomic energy project had placed their bets, and as you know the three horses arrived almost simultaneously to the goal in the summer of 1945. I thank you. (Applause).

From the recording of a speech given at the Congress of the American Physical Society on 30 January 1954. E. Fermi [269], *CPF II,* pp. 1000–1003.

B.6 T. D. Lee. Reminiscence of Chicago days

In 1946, Fermi joined the faculty of the University of Chicago. The same year, I received a Chinese government fellowship which enabled me to come to the United States to further my study in physics.

At that time, I had only two years of undergraduate training in China. Although I did not know much English, I was already familiar with classical physics and knew some quantum mechanics. I felt well prepared for graduate study. But at that time to enter graduate school without a college degree was almost impossible, except at the University of Chicago, which was willing to take people without a formal degree provided they had read the great books of Western civilization selected by President Hutchins. However, I had zero knowledge of any of these great books. Luckily for me, I had the help of the Chicago physics department. Apparently, Fermi, William Zachariasen, John Simpson, and other professors convinced the admissions officer that I was quite knowledgeable in the oriental equivalent of such classics (Confucius, Mencius, Laotse, etc.), which she accepted. (I am grateful that years later I was told by Dr. Simpson of Fermi's role in getting me admitted to the University of Chicago. At that time, I only knew that the physics department had helped.)

Right after the war, the Chicago physics department was the best in the world. Besides Fermi, there were S. Chandrasekhar, J. Mayer, M. Mayer, R. Mulliken, J. Simpson, E. Teller, H. Urey, and W. Zachariasen (later G. Wentzel also joined the staff) on the faculty. I was indeed happy to be admitted as a graduate student at Chicago.

The first thing I did after my arrival was to read the university catalog. As I recall, it said the Department of Physics was only interested in exceptional students. It did not encourage students to take courses; however, for those who needed guidance, courses were also provided. I thought to myself that that was really the proper style of a great university. How unlike Southwest Associated University in Yunnan, where students absolutely had to take courses. Nevertheless, since Fermi was not scheduled to give any courses that quarter, I did register for quantum mechanics with Teller, electromagnetic theory with Zachariasen, and, later, statistical mechanics with both Mayers. By attending those classes I felt I was betraying the secret that I was not an exceptional student. However, that feeling was soon dissipated by my observing that there were many other students in these classes.

A few weeks later, I received a note from Fermi asking me to attend his special evening class (which was by invitation only). It was there that I had my first glimpse of Fermi in action. The subject matter ranged over all topics in physics. Sometimes he would randomly pull a card out of his file, on which usually a subject title was written with a key formula. It was wonderful to see how in one session Fermi could start from scratch, give the incisive estimate, and arrive at the relevant formula and the physics that could be derived from it. The freedom with which he moved from field to field was an inspiration to watch.

At one time, Fermi happened to pull out his index cards on group theory, which contained only titles, all listed alphabetically. He then started to lecture on Abelian group first, affine correspondence second, central of a group third, and then character of a group , and so on. Some of us were a bit confused by this unorthodox approach. Fermi said, “Group theory is merely a compilation of definitions, and alphabetical order is as good as any.” In spite of his assurance, students like myself, who were not able to apply the permutation group fast enough to change the order, had some difficulty.

Another thing which comes frequently to my mind about student life in those times is the square-dancing parties held frequently at the Fermis’ home. They were my first introduction to occidental culture. Enrico’s dancing, Laura’s punch, and Harold Agnew’s energetic calling of “do-si-do” all made indelible marks on my memory.

Soon after, I became Fermi’s Ph.D. student. Besides me, Dick Garwin was also Fermi’s student, but he was an experimental physicist (and still is an extremely brilliant one). At that time, most of the Chicago Ph.D. students in theory were supervised by E. Teller; those included M. Rosenbluth, L. Wolfenstein, and C. N. Yang.

The relation between Fermi and his students was quite personal. I would see him regularly, about once a week. Usually we had lunch together in the commons, often with his other students. After that, Fermi and I would spend the whole afternoon talking. At that time, Fermi was interested in the origin of cosmic radiation and nuclear synthesis. He directed me first toward nuclear physics and then into astrophysics. Quite often he would mention a topic and ask me if I could think and read about it and then “give him a lecture” the next week. Of course, I obliged and usually felt very good afterward. Only much later did I realize that this was an excellent way of guiding the student to be independent.

Fermi fostered a spirit of self-reliance and intellectual independence in his students. One had to verify or derive all the formulas that one used. At one point I was discussing with him the internal structure of the sun; the coupled differential equations of radiative transfer were quite complicated. Since that was not my research topic, I did not want to devote too much time to tedious checking. Instead, I simply quoted the results of well-known references. However, Fermi thought one should never accept other people’s calculations without some independent confirmation. He then had the ingenious idea of making a specialized slide rule designed to deal with these radiative transfer equations

$$\frac{dL}{dr} \approx T^{18} \qquad \text{and} \qquad \frac{dT}{dr} \approx T^{6.5}$$

(where L is the luminosity and T the temperature). Over a week’s time, he helped me to produce the magnificent six-foot-seven-inch slide rule [...], with 18 $\log x$ on one side and 6.5 $\log x$ on the other. With that, even integration became fun and I was able to complete the checking quickly and then move on to a different topic for discussion. This unique experience made a deep impression on me. Even now, sometimes when I encounter difficulties, I try to imagine how Fermi might react under similar circumstances.

In 1948, Jack Steinberger, another student in Fermi's lab, was doing experimental work on the e-spectrum in the decay of $\mu \rightarrow e + \ldots$. We talked a lot about his measurements, which indicated that μ-decay, like β-decay, involves four fermions. I became quite interested in that, and so did Rosenbluth and Yang. Are there other interactions besides β-decay that can be described by Fermi's theory? The three of us decided to make a systematic investigation.

We found that if μ-decay and μ-capture were described by a four-fermion interaction similar to β-decay, all their coupling constants appeared to be of the same magnitude. This began the universal Fermi interaction. We then went on to speculate that, in analogy with electromagnetic forces, the basic weak interaction could be carried by a universally coupled intermediate heavy boson, which I later called $W^{\pm}$ for "weak." Naturally I told Fermi of our discoveries, and he was extremely encouraging.

One serious difficulty that faced us was how to generate such a universally coupled intermediate boson from symmetry considerations. In order to have short-range interactions and to escape detection, the boson must be massive and unstable, with a very short lifetime. However, other universally coupled quanta, like photons and gravitons, are all massless and stable. In addition, because of parity conservation, it was difficult at that time to understand why there were both Fermi and Gamow-Teller interactions in β-decay. I made no progress in this direction, and procrastinated in writing it up. At the end of December 1948, Fermi called me into his office and said he had just received two reprints by J. Tiomno and J. A. Wheeler, which also discussed the universal weak interaction. He insisted that I should immediately publish whatever I had; furthermore, he would send a copy to Wheeler with a letter saying it was independently done some months earlier, which he did. I was quite touched by his thoughtfulness.

In one of the weekly afternoon sessions with Fermi, I mentioned the work by R. E. Marshak on white dwarf stars, which was based on a suggestion by H. Bethe. In the original Chandrasekhar limit, the critical mass of a white dwarf star was set to be 5.75 η^2 times the solar mass M , where η is the ratio of electrons to nucleons in the star ($\eta = 1/2$ for a helium star and 1 for a hydrogen star). Marshak found that because of the high electronic conductivity in a white dwarf, its interior temperature can be quite low; he was able to produce an acceptable solution for a white dwarf consisting of pure hydrogen. That would make the critical mass 5.75 times the solar mass M. In the course of our discussion on the Marshak paper, Fermi asked me, with his usual insight, whether anyone had studied the question of stability. Soon I found that no one had. Then, I was able to show that the Marshak solution was unstable. Consequently $\eta = 1/2$, and the critical Chandrasekhar mass limit for the white dwarf is $1.44\,M$, not $5.75\,M$. This became my Ph.D. thesis, which I completed at the end of 1949.

J. W. Cronin, *Fermi Remembered,* The University of Chicago Press, Chicago, 2004, p. 215.

Appendix C
Background material

C.1 Newtonian mechanics; inertial and gravitational mass

Newtonian physics rests on two laws: the inertia principle, which determines the motion of the bodies, and universal gravitation. The first says that the application of a force to a body produces an acceleration which is inversely proportional to a characteristic parameter of the body, called *inertial mass.* In other words, the inertial mass m_{in} expresses the resistance that a body has to be accelerated. In symbols, $F = m_{in}\, a$. The second fundamental law is the universal gravitation, according to which any two bodies in the universe attract each other with a force which is inversely proportional to the square of their distance d, and is proportional to an intrinsic characteristic of the bodies, called *gravitational mass.* In symbols, $F = Gm_{gr,1}\, m_{gr,2}/d^2$, where G is a constant.

The gravitational force, as any other force, when applied to a free body produces an acceleration that depends on the inertial mass. If we denote by M_{gr} the gravitational mass of the Earth, by m_{in} and m_{gr} the inertial and gravitational mass of a body on the Earth surface, and by R the radius of the Earth, the equation $F = m_{in}\, a$ gives $GM_{gr}\, m_{gr}/d^2 = m_{in}\, a$, that is, $a = GM_{gr}m_{gr}/m_{in}R^2$. On the other hand we know that if we let two bodies fall from the top of the Pisa tower, for instance a lead and a wooden ball (as almost certainly Galileo did not do), they reach the ground at the same time, that is, they fall with the same acceleration. If a is to be the same for all values of the mass, then the ratio m_{in}/m_{gr} must be one, i.e., $m_{in} = m_{gr}$ (more precisely, the ratio should be a constant, which however can always be set to one by redefining the constant G).

The identity of the inertial and gravitational mass is a remarkable fact, which however was not fully understood till 1907, when Einstein founded his equivalence principle on it.

G. Bruzzaniti, *Enrico Fermi*, Springer Biographies,
DOI 10.1007/978-1-4939-3533-8

C.2 Curved space: the strange worlds of Flatland and Spheriland

In 1884 Rev. Edwin A. Abbott published under a pseudonym an unusual novel: *Flatland, a Romance of Many Dimensions*. It describes a universe with two dimensions. Also its inhabitants have two dimensions and have definite geometric shapes. They are thinking beings with a rigid social structure; the circles are at the vertex of the social pyramid; they are the high priests, who control the power. The aristocracy is formed by the regular polygons, the middle class by the triangles, and so on. The inhabitants of this heightless word have no intellectual tool to perceive the third dimension. The sudden arrival of a sphere which crosses Flatland is perceived by the fact that its intersection with the plane universe changes size; first a point appears, which becomes a circle of increasing radius, to shrink again to a point, and then disappear.

Flatland's geometry is the plane Euclidean geometry, where the usual properties hold; the Pythagorean theorem holds, the sum of the interior angles of a triangles is 180 degrees, the ratio between the length of a circle and its radius is 2π, and so on (Figure C.1).

What would have happened if Abbot had situated his novel on the surface of a sphere? Would the laws of Euclidean geometry still hold in Spheriland? Before answering let us have a look at Figure C.2. On a spherical surface, the "lines" are arcs of a great circle. Rectangle triangles can have two right angles, and, as one can easily check, the length of a circle is smaller than the length of a circle on the plane having the same radius. In Spheriland, Euclidean geometry does not hold. Spherical geometry is not the only non-Euclidean geometry one can make up. While in the spherical case the ratio between the length of a circle and its radius is smaller than 2π, one could concoct a geometry where the ratio is greater than 2π (see Figure C.3).

This last case is what happens with the measurements of the observer on the rotating disc; she measures the radii of the two circles finding the same values as the standing observer, but when she measures the two circles, since her rule has

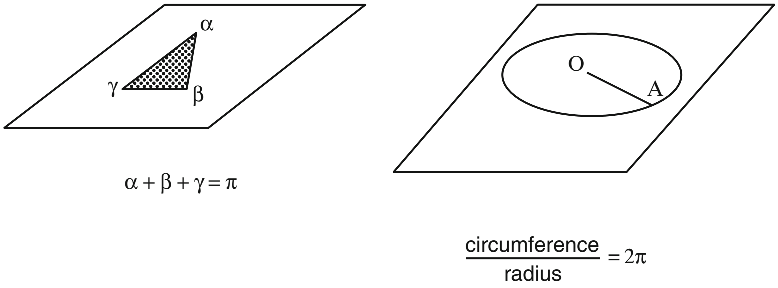

Fig. C.1 In Flatland the Pythagorean theorem for rectangle triangles holds, the ratio between circumference and radius is 2π, and the sum of the interior angles of a triangle is 180 degrees.

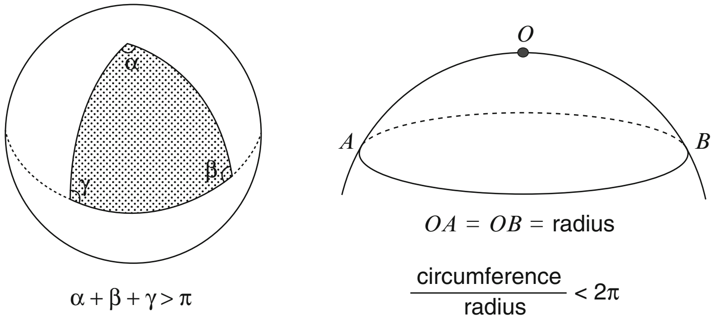

Fig. C.2 The Pythagorean theorem does not hold in Spheriland, and the sum of the interior angles of a triangle exceeds 180 degree. The ratio between a circumference and its radius is smaller than 2π.

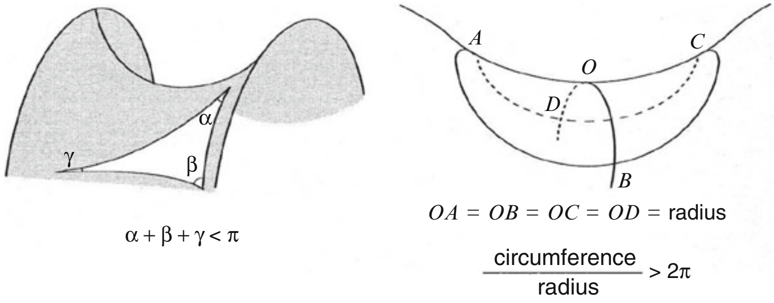

Fig. C.3 On a saddle-shaped surface the sum of the interior angles of a triangle is less than 180 degrees. The ratio between a circumference and its radius is greater than 2π.

shortened, she will find greater values than the standing observer. The difference will be bigger for larger radii, as the speed increases with the radius, and therefore also the contraction of the rule increases.

C.3 α particle scattering

The passage of particles through matter has been the first tool to investigate the atomic structure. α particles (having twice the charge of the electron, and a mass four times bigger than the hydrogen atom mass) are emitted by radioactive substances at a very high speed (about 10^7 m/s). Due to their high speed they can travel in air for several centimeters and cross thick layers of several substances, such as gold. Fermi in his textbook on atomic physics explained why α particles are an effective tool to investigate the electric structure of the atom:

> Let us now suppose that a thin beam of α particles orthogonally hits a thin leaf of some material, and let us observe the particles that cross the leaf. These are deviated by the

> collisions with the atoms in the leaf, and the incident beam will be scattered in all directions. Rutherford measured the distribution of the scattered particles. What one expects to see is very different according to the different hypotheses one can make about the structure of the positive charge in the atom. Let us consider how the collision between a particle and an atom takes place according to two extreme hypotheses:
>
> a) the positive charge is uniformly distributed in a sphere with the same dimension as the atom;
> b) the positive charge is concentrated in a point, or in region whose dimensions are much smaller than the size of the atom.
>
> Let us consider a particle that hits an atom and gets very close to its center. Under the hypothesis a), the repulsive force exerted by the positively charged sphere on the particle is proportional to the distance r from the center, and its value is therefore small near the center. On the contrary, in case b) the force is inversely proportional to r^2, and has great values when r is small. Therefore in case b) the particle will be deflected much more than in case a).
>
> Rutherford found that the distribution of the scattered particles was fully consistent with hypothesis b), and totally incompatible with hypothesis a).[1]

The following three figures show the different trajectories followed by an α particle according to the hypothesis one makes about the electric structure of the atom. Figure C.4 refers to hypothesis a), and Figure C.6 to hypothesis b). Figure C.5 shows more details about the trajectory computed according to hypothesis b); the impact factor p is the distance of the nucleus from the direction of the incident particle. As shown in figure C.6, smaller impact parameters correspond to greater scattering angles θ. In modern terms, the number of particles scattered at an angle θ is expressed in terms of a quantity that in Rutherford's times was not used, the *cross-section* $\sigma(\theta)$:

$$\sigma(\theta) = \frac{(NeQ)^2}{4m^2v^4 \sin^4 \dfrac{\theta}{2}}$$

where Ne is the positive charge of the nucleus, and Q, m, and v are charge, mass, and speed of the incident α particle, respectively.

C.4 Planck's constant and the birth of the wave-particle duality

The argument that Max Planck used to introduce in 1900 the constant that bears his name was "crazy," as remarked by Abraham Pais.[2] But Pais adds, "His reasoning was mad, but his madness has that divine quality that only the greatest transitional

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[1] E. Fermi, *Introduzione alla fisica atomica, op. cit.*, p. 69.

[2] A. Pais, *Subtle is the Lord, op. cit.*, p. 371.

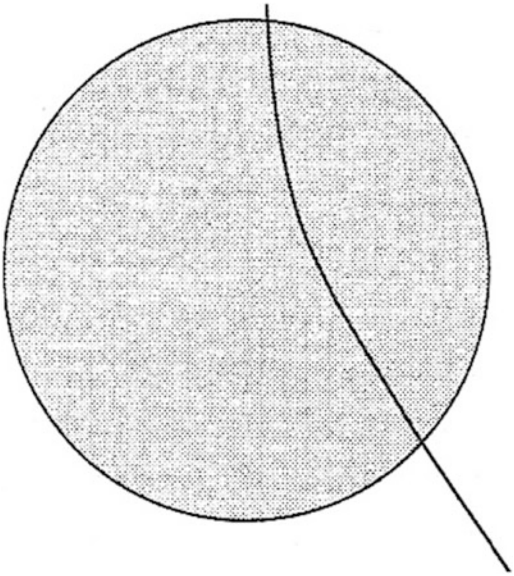

Fig. C.4 Trajectory of an α particle which crosses an atom whose positive charge is uniformly distributed over its volume.

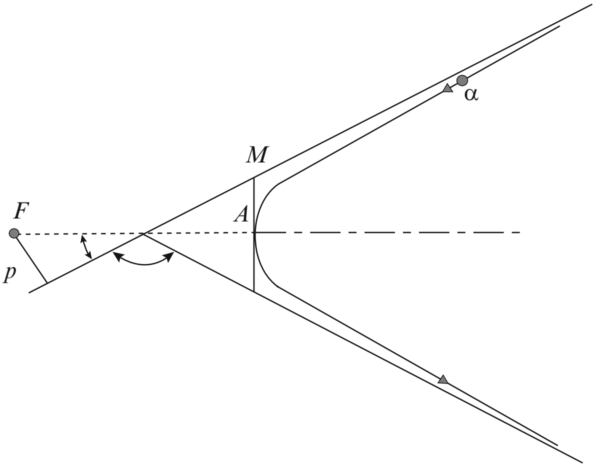

Fig. C.5 Trajectory of an α particle scattered by a charged nucleus located in F with an impact parameter p.

figures can bring to science." What was Planck's "folly"? Or better, using his words, that "desperate act" that on 14 December 1900 forced a scientist "conservative by inclination, into the role of a reluctant revolutionary"?

The answer to this question lies in a very serious problem, which in 1900 attracted Lord Kelvin's attention; the application of the most classical physical principles to the interaction between matter and radiation led to an intractable contradiction. In 1859 Kirchoff communicated a very important result to the Berlin

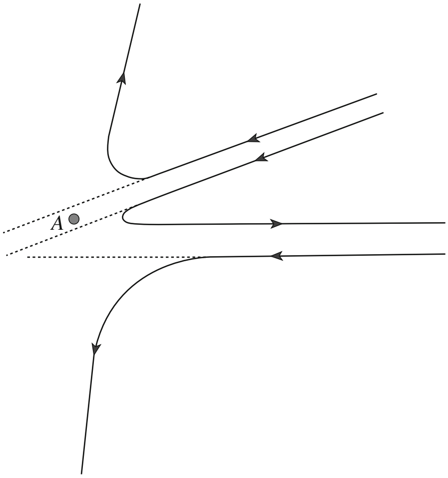

Fig. C.6 Trajectories of α particles with different impact parameter scattered by a nucleus located in A.

Academy of Science. It was known that all bodies emit and absorb electromagnetic radiation when are heated, and that the frequency of this radiation depends on temperature; a pin set on fire at about 600° C becomes incandescent and emits a reddish radiation, that is, a radiation of smaller wavelength than that emitted by boiling water. To characterize the emitting and absorbing behavior of a body, two quantities are introduced, the *emitting power* E (the energy emitted by the body at a given frequency per unit time and unit surface), and the absorbing power A (the ratio between the incident and the absorbed energies). The parameter A, therefore, takes values between 0 and 1; $A = 0$ means that the body does not absorb energy (all energy is reflected), while $A = 1$ means that the body absorbs all the incident energy. A body with the latter property is called a *black body*. Kirchoff's result was that the ratio E/A does not depend by the particular body considered, but is a universal function; for a given frequency and temperature, its value is the same for all bodies (so it coincides with the emitting power of a black body, as in that case $A = 1$).

Any attempt to compute the spectral distribution of a black body (i.e., to compute its emitting power as a function of frequency and temperature) my serious contradictions. The problem was solved by Planck on 14 December 1900. Actually he had obtained some first results in October 1900, finding a formula which fitted the experimental data very well. But the true discovery, which would have opened a new chapter of physics, took place two months later. His "folly," his "desperate act," consisted in abandoning the assumption that the atoms of the black body emit and absorb energy in a continuous way, according to the principles recognized at the time, and assuming instead that the energy exchanges could only take place by "finite elements," or "quanta," each of some energy ε. This energy was related to the frequency of the radiation ν by the equation $\varepsilon = h\nu$, where h is a constant whose value was computed by Planck, obtaining 6.63×10^{-27} erg × s, or 6.63×10^{-34}

J × s. The constant h has the physical dimensions of energy times time, a quantity called “action.” For this reason h was called “elementary action quantum.”

The celebrated formula found by Planck on that memorable day was

$$E(\nu, T) = \frac{8\pi h\nu^3}{c^3}\frac{1}{e^{h\nu/kT} - 1},$$

where h is Planck’s constant, k is Boltzmann’s constant, and c the speed of light.

Let us remark that for Planck the electromagnetic radiation was still a continuous quantity; its discrete nature was only manifest in the emission and absorption processes. Einstein was the first to extend Planck’s idea to get an explanation of the photoelectric effect, namely, the emission of electrons by a metallic surface hit by electromagnetic radiation. The empirical laws of that effect contradict classical physics, as one see that increasing the energy of the incident radiation, only the number of emitted electrons increases, and not their energy; moreover, in an absolutely incomprehensible way, the emission takes place only above a certain frequency. Einstein solved the problem with a very bold hypothesis; extending Planck’s idea, he assumed that also light is formed by energy quanta ε for which the relation $\varepsilon = h\nu$ holds. Then the photoelectric emission process becomes the result of a collision between a light quantum and an electron in the metallic surface. This makes the theory compatible with the experimental data.

So lights came to possess, together with the usual wave properties, also some properties typical of particles. This was the birth of the wave-particle duality, which found another important confirmation in the work of Arthur Compton, who in 1923 showed experimentally that the light quanta, in addition to energy, also carry momentum, and obey the laws for the collision of matter particles.

C.5 The electron spin and the exclusion principle

The failure to provide an explanation of the anomalous Zeeman effect was certainly the main reason why at the beginning of the 20s a fourth quantum number was introduced. Uhlenbeck and Goudsmit’s idea was that the difficulties lay in some unknown structural property of the electron. In particular they made the hypothesis that the electron rotates around its axis, thus having an angular momentum, and therefore a magnetic momentum. The empirical evidence of the doubling of the spectral line of the alkaline metals implied that this intrinsic angular momentum of the electron (spin) only can have two directions in space with respect to a given direction. An easy calculation shows that the absolute value of the spin must be 1/2 in units $h/2\pi$, so that, after fixing a direction in space, the value of the spin along that direction can only be 1/2 or −1/2.

From 1925, spin, together with n, l, m, was the fourth of the quantum numbers that, obeying the exclusion principle, determined the state of an electron inside an atom. In his article on the atom for *Enciclopedia Italiana,* Fermi so stated Pauli’s principle:

> It has been recognized that, if in an atom there is a electron whose motion is characterized by certain quantum numbers, there can be no other electron in the atom whose quantum numbers are the same. As quantum numbers characterizing an electron one has to consider, in addition to n and k [k stands for l], two more numbers m_1 and m_2, that characterize the orientation of the orbit and of the magnetic axis with respect to an external field. In other words, each quadruple of quantum numbers n, k, m_1, m_2 determines a place that can be occupied at most by one electron.

Moreover,

> This principle, which is based on a large number of experimental facts, lacks to date a complete theoretical justification. It does not seem indeed possible to deduce it from Sommerfeld's conditions, and also with the new [quantum] mechanics it has been so far possible only to prove that if a system initially obeys Pauli's principle, it cannot transform into a system which does not obey it; however the reason why this principle holds from the beginning is not known.[3]

C.6 The isotopic constitution of the elements

Fermi's celebrated pedagogical skill will help once more, this time to understand the notion of isotopy and its importance.

> As the chemical and spectroscopical properties of an atom are determined by the motion of its electrons, such properties will be virtually the same for two atoms having the same atomic number Z, and therefore the same nuclear charge Ze, and somehow different masses. Two atoms in these conditions are said to be isotopic because they occupy the same position in the periodic system, since the position is determined by the atomic number. It has been realized that many chemical elements are actually a mixture of a certain number of isotopes [...] More recently, Aston has proved it [...] So, he obtained the following remarkable results. The atomic weight of all elements, referred to oxygen = 16, is expressed by an integer number; the non-integer numbers that we find in the period system are due to the fact that the corresponding elements are a mixture of isotopes, each having an integer atomic weight, so that the weight that we see is an average of the atomic weights of the various isotopes. For instance, chlorine, whose atomic weight is 35.45, is a mixture of two isotopes, of weight 35 and 37, in the ratio 0.775:0.225.
>
> The problem of separating the isotopes that form a chemical element is very difficult, due to the identity of their chemical properties; only the small difference in their weights can be used for that purpose [...]
>
> Let us finally hint that the fact that the atomic weights of the atoms are integers can be interpreted with the hypothesis that nuclei are aggregates of hydrogen nuclei (also called protons) of atomic weight one, and electrons, whose weight is virtually zero. If we denote by Z_p and Z_e the number of protons and electrons in a nucleus of weight A and atomic number Z, one would have
>
> $$A = Z_p, \qquad Z = Z_p - Z_e.$$

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[3] E. Fermi, *Introduzione alla fisica atomica, op. cit.,* p. 210.

> Thus we are going back to Prout's idea that all substances are made of the same elements. On the other hand, the radioactive phenomena show that the nucleus has a complex structure.[4]

C.7 The uncertainty relations and the complementarity principle

The roots of the uncertainty relations lie in a principle that characterizes the measurement process in quantum mechanics, where by "measurement process" one means any interaction between a "quantum object" and a "classical object" (the measurement apparatus). When a measurement is made, some physical characteristic of the system under consideration is changed. While in classical physics the influence of the apparatus on the system can in principle be made arbitrarily small, in quantum physics there are limitations, due to the principles of the theory. The more precise the measurement, the more the system is perturbed. So, if we want to measure the position of an electron with a microscope, we must illuminate it with some radiation, but when we do that, by the Compton effect, we change its speed; the more precise is the position measurement, the bigger is the change in the electron speed.

This is basically the content of the indetermination relations; there are physical quantities that cannot be simultaneously measured with arbitrary precision: for instance, the position q of a particle and its speed, or equivalently, its momentum p. If Δq and Δp, respectively, denote the uncertainties in their measurements, one has $\Delta p\, \Delta q \geq h/2\pi$, where h is Planck's constant. A similar formula holds for other pairs, such as the energy of a system, and the instant of time when it is measured.

As a consequence, the notion of trajectory makes no sense in quantum mechanics, and more seriously, the determinism of classical mechanics is compromised. It is true that Schrödinger's equation provides a perfect knowledge of the time evolution of the wave function, and therefore of the state of the system, but the debate on the notions of causality and determinism in quantum mechanics is still open.

At the heart of Bohr's complementarity principle one finds the wave-particle dualism. Every time we make a measurement we record the interaction between the quantum system under consideration and the measurement apparatus, which is a classical object. Therefore, the question is not "is the electron a wave or a particle," but rather, "does the electron behave as a wave or as a particle?" This second question is easier to answer: it depends on the interaction with the measurement apparatus. In other words, the wave and particle natures of matter are *complementary* to each other. They are mutually excluding, "but only together — thus Bohr writes — they offer a natural generalization of the classical description of the objects." This means, it is worth stressing, that during the observation of a system the two aspects are mutually exclusive, but on the other hand, any theoretical description of the system must necessarily use both.

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[4] E. Fermi, *Introduzione alla fisica atomica, op. cit.,* p. 75–76.

C.8 Quantum electrodynamics

To have some grasp of the meaning of the quantization of the electromagnetic field we can use the following example, taken with some changes from Fermi's famous lectures at a summer school in theoretical physics in Ann Arbor, Michigan. Let us imagine a string with its ends fixed. There are many "elementary" ways in which the string can vibrate, which can be characterized by the number of points of the string that remain fixed during the motion. These modes of vibration are called "normal modes." Every motion of the string, however complicated, can be expressed as a (possibly infinite) sum of normal modes. Thus the study of the motion of the string is equivalent to the study of an infinite system of harmonic oscillators.

Let us consider now a free electromagnetic field. It is described by the Maxwell equations, that are linear, like the equation of the vibrating string. One can prove that what we have said about strings also holds true for every continuous system which is described by linear equations, in particular for the electromagnetic field. This, therefore, can be thought of as a set of oscillators (normal modes), which can be quantized. Every normal mode of the field can vary its energy by multiples of $h\nu$, where h is Planck's constant, and ν is the frequency of the normal mode. In other words, photons are naturally produced by the quantization of the electromagnetic field.

Things get more interesting when we consider an electromagnetic field interacting with matter. This was the problem tackled by Dirac. His work was technically quite complicated, but its basic idea was well described by Fermi.

> Dirac's radiation theory is based on a very simple idea; instead of considering an atom and the radiation field with which it interacts as distinct entities, he treats them as a single system whose energy is the sum of three terms: the energy of the atom, that of the radiation field, and a small term which represent the coupling energy between the atom and the field. [...] A simple example can explain these relations. Let us consider a pendulum, which represents the atom, and a vibrating string near the pendulum, which reprints the radiation field. If there is no connection between pendulum and string, the two system oscillate independently, and the energy is just the sum of the energies of the pendulum and of the string, without any interaction term. To get a mechanical representation of this interaction term, let us connect the mass M of the pendulum with a point A of the string by means of a thin rubber band a. The presence of the rubber band will slightly perturb the motion of the pendulum and of the string. Let us suppose for instance that at the time $t = 0$ the string vibrates, while the pendulum is standing still. The vibrating string through the rubber band exercises a very small force on the pendulum, with the same period as its own oscillations. If that period is very different from that of the pendulum, the oscillations of the latter will be small; on the contrary, if the periods coincide, there will be resonance, and after a certain time, the amplitude of the oscillations of the pendulum will be considerable. This process corresponds to the absorption of radiation by the atom. If we assume that at $t = 0$ the pendulum moves, and the string stands still, the opposite phenomenon takes place; due to the strains transmitted by the rubber band, the strings stars oscillating, but only the harmonics of the strings that are close to the frequency of the pendulum reach a considerable amplitude. This process corresponds to the emission of radiation by the atom.[5]

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[5] E. Fermi [67], *CPF I,* p. 401–402.

So in the interaction between electromagnetic field and matter, we have on one side the quantized electromagnetic field, and on the other side the charged matter, i.e., electrons. Since photons are the expression of the quantization of the field, it is natural to wonder if also the electrons are expression, via some suitable mechanism, of the quantization of some field. This procedure is called "second quantization," and is formally characterized by the introduction of some special operators, called "creation" and "annihilation" operators, which when acting on the wave function of a system with n particles yield the wave function of a system with $n + 1$ and $n - 1$ particles, respectively.

Actually the wave function we are considering now is not the one entering the Schrödinger equation, as this equation does not take relativity into account. The realistic equation was written by Dirac in 1928. It is important not only because it agrees with the theory of relativity, but also because it naturally incorporates the spin of the electron. A feature of this equation is that it has solutions with negative energy. There were interpreted by Dirac in 1931 as a signal of the existence of a particle identical with electron for everything but the electric charge, which is positive: the positron.

C.9 Electromagnetic mass and electron dynamics

The idea of the electromagnetic mass of the electron has a long history. The first to consider the question was J. J. Thompson in 1881.[6] To understand the problem let us start with a simple example due to H. A. Lorentz. Let us consider a solid sphere of mass m_0 moving in an ideal fluid. One can show that the energy of the system formed by the sphere and the fluid is $E = \frac{1}{2}mv^2$, and its momentum $p = mv$, where $m = m_0 + \mu$, and μ is a parameter depending on the radius of the sphere, and the density of the fluid. In other words, one can take account of the presence of the fluid by redefining the inertial mass, adding a "hydrodynamic" mass term.

Thomson reasoned in a similar way considering a sphere of radius a over which an electric charge q is distributed, which moves with a speed v, much smaller than the speed of light. In every point of space there is an electric field E generated by the charge distribution, and a magnetic field B generated by the motion of the charged sphere. The latter is proportional to speed of the sphere. Now, any electromagnetic field has an energy U and a moment p, which is proportional to U. In the case of the charged sphere we are considering, one has $p = 2q^2/3ac$. Recalling $p = mv$, we can interpret the coefficient $2q^2/3ac$ as an inertial mass. If we compare with the electrostatic energy of the electric field produce by the particle, $U = q^2/2a$, we find the following relation between mass and energy:

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[6] J. J. Thompson, *On the electric and magnetic effects produced by the motion of electrified bodies,* Phil. Mag. 11 (1881), p. 229.

$$m = \frac{4U}{3c^2}.$$

At the end of the 19th century these results led to a very suggestive idea, namely, that the electron was pure electricity, or in other terms, that its mass was of purely electromagnetic energy.

As Max Born wrote,

> J. J. Thomson had remarked that the charge on an electron would produce an additional mass since acceleration generates a magnetic field, and he directed attention to the possibility that the whole mass of the electron might be of this electromagnetic nature, This idea was eagerly taken up by others, fascinated by the suggestion that one of the fundamental concepts of Newtonian mechanics, mass, might be not a primary but a derived quantity, and that electromagnetism lay behind mechanics.[7]

But a purely electromagnetic dynamical theory was in disagreement with the theory of relativity, according to which there is general relation between mass and energy, the celebrated equation $m = U/c$.

C.10 The adiabatic principle and OQT

Ehrenfest's adiabatic principle, together with Bohr's correspondence principle, was one of the pillars over which, at the beginning of the 20th century, physicists were trying to build some solid foundations for the Old Quantum Theory. To understand what was the question, let us once more listen to Fermi's words:

> Let us suppose that in a mechanical system the forces or the constraints are continuously modified in function of time, very slowly in comparison with the characteristic periods of the system, that is, according to Ehrenfest's expression, *adiabatically.* The adiabatic principle states that if the system at the beginning of the transformation is in a distinguished quantum orbit, it will be in that orbit also at the end of the transformation. Let us consider for instance a pendulum, and let us suppose the wire is shortened very slowly in comparison with the period of the pendulum. The frequency ν of the pendulum will slowly increase, but also the energy u will increase, in such a way that the ratio u/ν remains constant. So if initially this ratio was an integer multiple of the Planck constant h, it will remain such, and the state of the system will be a distinguished one during the whole transformation.[8]

With Ehrenfest's principle one can decide which quantities associated with a physical system can be quantized; they are the quantities that under a perturbation of the system remain constant, or change abruptly by one or more units. If the perturbation is very slow, the quantity must remain constant, i.e., it must be an *adiabatic invariant.*

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[7] M. Born, *My Life: Recollections of a Nobel Laureate,* Taylor & Francis, New York 1978, p. 133.

[8] E. Fermi [12], *CPF I,* p. 88.

The obvious problem now was how to single out the adiabatic invariants of a system. As stressed by Fermi,[9] the solution was offered by a theorem due to Burgers. To state that theorem we need to introduce a space, called *phase space*, whose dimension is $2n$ if n is the number of degrees of freedom, namely, the number of coordinates necessary to specify the position of the system in space. As coordinates in phase space one can use n position coordinates $q_1, \dots, q_n$, and the n conjugate momenta $p_1, \dots, p_n$. Now, if one is given a mechanical system whose Hamilton-Jacobi equation admits the separation of variables (i.e., it is equivalent to a set of ordinary differential equations, each having one dependent variable), then the quantities (called *action variables*)

$$J_k = \oint p_k \, dq_k, \qquad k = 1, \dots, n,$$

where the integral is extended to a period of the coordinate q_k, are adiabatic invariants.

A simple example, cited also by Fermi, is again provided by a pendulum. If m is its mass, l its length, ϕ the angle between the pendulum and the vertical direction, and ω the angular velocity, for small values of the angle ϕ the energy of the pendulum can be written as

$$E = \tfrac{1}{2} m l^2 \omega^2 + \tfrac{1}{2} m g l \phi^2.$$

If we introduce new variables $p = m\omega l$ and $q = l\phi$, we can write

$$E = \frac{p^2}{2m} + \frac{mg}{2l} q^2. \tag{C.1}$$

The pendulum we are considering has one degree of freedom, phase space has dimension 2, and can be described by two variables, for instance, p and q. Equation (C.1) can easily be recognized as the equation of an ellipse. For a pendulum (in the approximation of small oscillations), the trajectories in phase space $E =$ constant are therefore ellipses. Burgers' theorem states that the areas of these ellipses, which can be written as $\oint p \, dq$, are adiabatic invariants.

C.11 The hyperfine structures

The spectral lines of some elements are formed by a configuration of many very thin lines. Two causes have been found for this *hyperfine structure.* One, more trivial, is that a given chemical element may be of have many isotopes, and therefore

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[9] E. Fermi, *Il principio delle adiabatiche ed i sistemi che non ammettono coordinate angolari,* Il Nuovo Cimento 25 (1923), p. 171–175.

each line actually includes many thinner lines each due to a different isotope. The second, deeper cause, is that the interaction of the nucleus with the external electrons, in addition to the Coulomb attraction, also includes a contribution due to the angular momentum of the nucleus. This combines with the angular momentum of the electrons, giving rise to a total angular momentum which, according to the rules of quantum mechanics, can only take precise values. These in turn give rise to a multiplicity of energy transition, that is, to the hyperfine structure of the spectral lines.

In Fermi's words:

> As it is known, in most cases the hyperfine structure of the spectral lines is due to the interaction between the magnetic moment of the nucleus and the motion of the electrons. This interaction can be roughly described as follows. The electrons, rotating around the nucleus, give rise to a magnetic field which surrounds the nucleus; if the nucleus carries a magnetic moment, the energy will have different values according to the orientation of the nuclear axis with respect to the magnetic field produced by the electrons. Thus every energy level will break into two or more very near levels, and therefore also the spectral lines will break into some component lines. The presence of the hyperfine structure in the spectrum of an atom therefore shows that the nucleus of that atom carries a magnetic moment. It is clear that by comparing the structure of the lines of an atom with the theoretical computations one can deduce the value of the magnetic moment of the nucleus.[10]

C.12 Particle accelerators and detection techniques

C.12.1 Accelerators

The Cockroft-Walton machine. This apparatus is sketched in Figure C.7. Protons were produced in the upper section of the tube by an electrical discharge in hydrogen, and then were accelerated in the middle section. The source was kept at a positive potential with respect to the lower section, which was grounded. The middle section contained electrodes that collimated the protons, and also had the purpose of subdividing the very high voltage in smaller voltages, thus avoiding the chance of electric discharges.

The inner part of the tube was under deep vacuum. The target (lithium in the first experiments) was located at the lower end of the apparatus, and the products of the collision were detected by the scintillations produced on a screen, that were observed with a microscope. The new feature of the apparatus was the system producing the high voltage, involving an array of capacitors that were first charged in parallel, and then discharged in series.

Linear accelerators (LINAC). According to an idea of Rolf Woderöe's, their basic principle is to apply a series of small potential differences instead of a unique big voltage (Figure C.8). The charged particles move through a series of cylindrical

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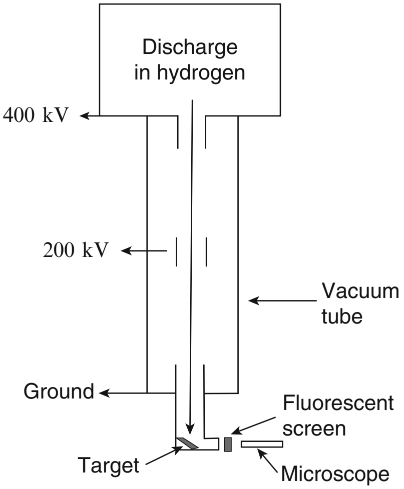
[10] E. Fermi [57a], *CPF I,* p. 336).

Fig. C.7 Outline of the Cockroft-Walton’s tube.



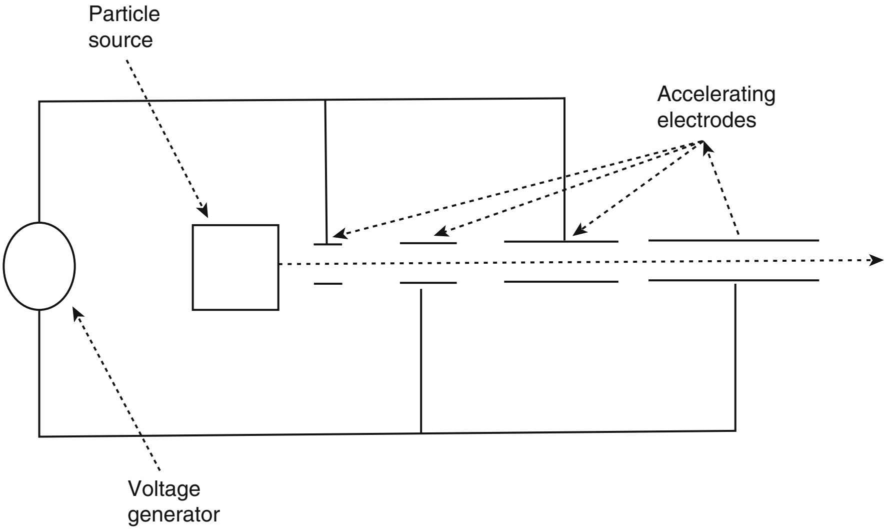


Fig. C.8 Sketch of a linear accelerator.

electrodes connected to a high-frequency alternate voltage generator. The frequency of the generator is chosen so that the particles, on leaving an electrode, always meet an electric field which accelerates them. One can note that the length of the electrodes is not constant, but increases along the accelerator. Indeed, while the speed of the particles increases, since the time spent in each cylinder must be the same if the particle is to remain in phase with the voltage, the electrodes must be longer and longer.

The cyclotron. The functioning principle of the machine is shown in Figure C.9. The figure depicts two cross-sections. The originality of Lawrence’s idea was to

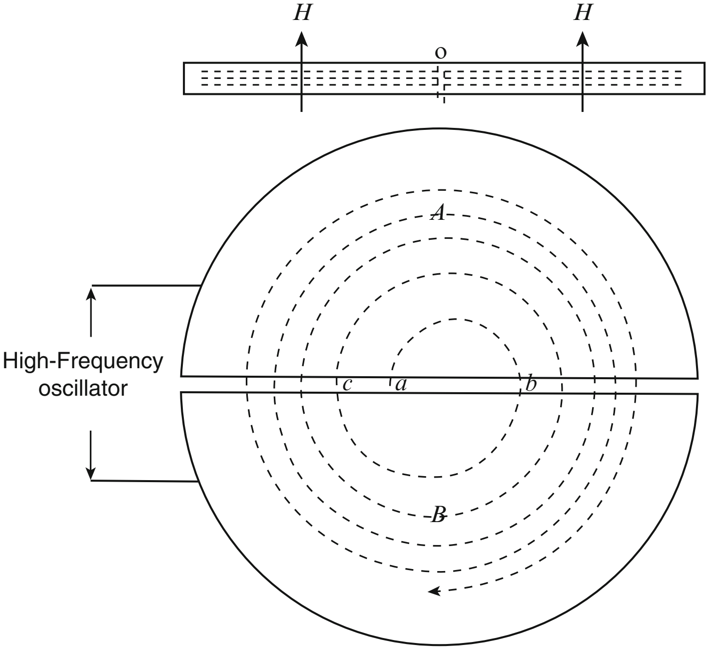


Fig. C.9 Schematic cross-sections of a cyclotron.

use a magnetic field to bend the electron trajectories, and an alternate electric field to accelerate them. In this way two of the main drawbacks of the Cockroft-Walton apparatus, the high voltages and the linear dimensions, are avoided. The semicircles *A* and *B* represent two cavities where a magnetic field *H* is present; the alternate electric field acts between *A* and *B*. Here there is no magnetic field, and in the cavities there is no electric field.

Protons are injected into the cyclotron at *a*. If *p*, *m*, and *e* are their momentum, mass, and charge, in a magnetic field *H* which is perpendicular to *p* they move along circular trajectories of radius $r = p/eH$ with period $T = 2\pi m/eH$. As the period does not depend on the speed and on the radius of the trajectory, the frequency of the electric field can be chosen so that the particles when moving in the space between the cavities always find an electric field which accelerates them. Let us for instance consider a proton which is injected at the point *a*, with a certain speed. After moving along a circular trajectory, it will leave *A* and will enter *B* at the point *b*. Here it finds an electric field which accelerates it, so that its speed and momentum increase, and it will describe now a trajectory with a greater radius. After some time (which, as we have seen, is independent of the radius of the trajectory and the speed of the particle), the proton leaves the cavity *B* at the point *c*. As the electric field was tuned to change polarity exactly after the same time interval the proton took to cross one cavity, the proton is again accelerated. Thus the proton moves along a spiral and acquires energy every time it moves between *A* and *B*, until an electrostatic deflector extracts it from the cyclotron.

The synchrocyclotron. When the energy of the proton reaches 20 MeV, the relativistic increase of the proton mass is no more negligible. To maintain the synchronism, the frequency of the electric field must diminish, and this is what happens in the synchrocyclotron.

The syncrotron. There is still a problem; with the increase of the energy the radii of the trajectories become greater, so that larger and more expensive machines are needed. To avoid this one can increase the magnetic field while the energy increases, so that the radii remain constant. This is what is done in the synchrotron.

Why larger and larger machines are built, with greater and greater energies? According to quantum mechanics, every particle has a wavelength λ given by $\lambda = h/p$, where h is the Planck constant, and p the momentum. If we use particles to explore the microscopic structure of matter, we need to use particles whose wavelength is smaller than the dimension of the structures we want to explore. If this dimension is d, we must have $\lambda < d$, namely $h/p < d$, or $p > h/d$. So, the smaller is the region we want to explore, the greater must be the momentum of the particles used, and therefore also their energy.

C.12.2 Particle detectors

Particle detectors work by detecting the energy released by the particles. The interaction between the particles and the medium making up the detector is usually the Coulomb force, and therefore only charged particles can be detected. Neutral particles can be detected only if during their passage through the medium produce charged particles. Let us analyze the most common detectors used in the period we are considering.

Scintillation detectors. The atoms of the detecting medium acquire energy from the particles which hit the detector are excited, and then release the absorbed energy as light. This gives rise to sparks which can be detected. In his studies of the radioactive phenomena, in particular his first experiments on the α particle scattering, Rutherford used detectors of this kind. The scintillating substance was zinc fluoride.

Gas detectors. A particle traveling through a gas ionizes its atoms, i.e., it removes an electron from the atom, and creates a pair electron-positive ion. The detector is a chamber containing two electrodes, cathode and anode, to which a voltage is applied; the electrodes can also be the walls of the chamber and a straight wire inside it. The ions and electrons generated by the passage of a particle give rise to an electric current, which signals the passage of the particle. If the applied voltage is of the order of some hundred volts, the electric field is strong enough to accelerate the produced electrons so that they will ionize the gas. Thus, there is a *secondary ionization,* which multiplies the number of ions. In this case the so-called *proportional counters* are needed. If the voltage is around 1000 V more phenomena take place, for instance the production of ultraviolet radiation, the induced current is no longer proportional to the primary ionization, and one has to use *Geiger-Müller counters.*

Wilson expansion chamber or cloud chamber. It allows the visualization of tracks which mark the trajectories of the particles. It is made of a cylindrical vessel with a glass slate on top which allows the observation of tracks, while the bottom is a movable piston that produces the expansion. When the piston is lowered the gas enters a metastable state; the ions produced by the passage of the charge particle perturb that state, becoming condensation nuclei and giving rise to droplets which show the passage of the particle.

Nuclear emulsion. A nuclear emulsion is a suspension of silver bromide crystals smeared on a photographic slate. Once the slate is developed, the exposed crystal are reduced to silver. An analysis with a microscope shows the trajectories of the ionizing particles as a sequence of dark dots.

C.13 Divergencies in quantum field theory and the renormalization programme

Fermi's words in his 6th Donegani lecture will help us to understand the problem of infinities in quantum field theory.

> Let us consider an electromagnetic field in the presence of some charges; to make things easy, let us say there is just one electron. The Hamiltonian of this system can be written as the sum of three terms, the Hamiltonian of the electron H_e, as if there was no electromagnetic field; the Hamiltonian H_{em} of the electromagnetic field, as the electron was not there; and a interaction term H_{int} [...] which includes terms depending on the electromagnetic field, and terms depending on the motion of the electron [...]
> In most treatments, the dynamics of the system "electromagnetic field + electron" is handled mathematically with an approximation process, consisting in considering $H_e + H_{em}$ as the unperturbed part of the problem, and H_{int} as a perturbation. This has the obvious advantage that the unperturbed problem is very easy, as the electromagnetic field is not influenced by the electron, and *vice versa.* According to the normal prescriptions of perturbation theory, the results are obtained by a series development with respect to a parameter that in this case is the famous quantity $e/\sqrt{\hbar c}$. Everything would go well, or almost, if e were infinitesimal, which however it is not, as it is the charge of the electron; replacing the numerical values one has indeed
>
> $$\frac{e}{\sqrt{\hbar c}} = \sqrt{\frac{e^2}{\hbar c}} = \frac{1}{\sqrt{137}}$$
>
> which is little less than one tenth (quite small, or quite large, according to taste!).
> In this connection, I would like to add what follows. Quantum electrodynamics was the first field theory to be developed, as the electromagnetic field is definitely the field with which we are most familiar; but between 1930 and 1940 there has been a flourishing of theories, quite similar to quantum electrodynamics, that treated other fields. One of them is the theory of β rays, which differs from quantum electrodynamics because in this case the coupling constant is really small, of the order of 10^{-10} or 10^{-12}. Another one is Yukawa's theory, the mesonic theory of nuclear forces, where unfortunately the expansion parameter is large, about equal to 1 (or maybe 1/2, or 1/3) [...]
> Going back to our main theme, I would like now to talk about some divergencies, that, as I already hinted, are the crucial issue of all these theories. The problem is the following. Let

> us consider again an electron which is only subject to forces due to the radiation field (that is, there are no other external fields). It is well known that both in classical and quantum electrodynamics the coupling between an electromagnetic field and a moving charge is equivalent to an electromagnetic mass, and it is also well known that in both theories this mass is essentially infinite.
>
> The situation in electrodynamics was, until a couple of years ago, basically the following. The practical rule for computing, according to perturbation theory, the series expansion we have earlier mentioned, was to stop as soon as one found a result different from zero, because if one dared to continue, the next term was usually divergent. Obviosly this was not a very decorous way of making a computation, but it was the practical rule we taught to the students.[11]

The renormalization techniques provide a way to handle the infinities in quantum field theory. The first successful computation was made by Bethe after Shelter Island in connection with the Lamb shift. To roughly understand what Bethe did, let us again follow Fermi's reasoning. In the case of the hydrogen atom, the H_e term in the Hamiltonian is the energy of the electron, which is the sum of the kinetic energy $p^2/2m$ and the potential energy. If we treat the interaction term H_{int} as a perturbation, then according to Fermi,

> [...] the result is the following: the first approximation yields zero, and the second approximation diverges; that is, the first approximation, the term proportional to e, vanishes, and the second approximation, the term proportional to e^2, is infinite. Usually, at this point one lost any hope, and thought "evidently, this theory is not good enough to treat these phenomena." The easy observation that was made in that conference [Shelter Island] was the following: what do we mean by the term m appearing in $p^2/2m$? Since the splitting of the Hamiltonian in three terms is based on the idea of separating quantities related to the electron only, quantities related only to the field, and the interaction, the quantity $p^2/2m$ must refer only to the electron; namely, if one could, with some strange trick, exclude the electromagnetic field, the mass m in the denominator of $p^2/2m$ should be the mechanical mass. In the computations we use for m the physical mass of the electron $m_f = m_{em} + m$, and who knows how much of this mass is mechanical, and how much is electromagnetic. This is not correct. Since the physical mass, what we measure, is the sum of the mechanical and electromagnetic masses, if we want to write [the Hamiltonian of the system] correctly, we must write it in a different way.
>
> [...]
>
> If we want to make a consistent development in powers of e we must also develop the term $p^2/2(m_f - m_{em})$ and include the part with the electromagnetic mass in the e^2 term [...] so that we have, in a way, two perturbations, one due to the term H_{int}, which gives a zero contribution in the first approximation, and an infinite term in the second approximation; but we also have a second-order term, coming directly [from the development of $p^2/2(m_f - m_{em})$]. Both perturbations contribute to the coefficient of the e^2 term, and both are infinite [...] The essential point is then that the two infinities cancel each other, and what is left is a finite result, very close to the experimental value.[12]

The renormalization techniques have become a basic tool in quantum field theory. The fact that they work so well in quantum electrodynamics, even without having a sound theoretical foundation, is most likely a hint of something deep which has not yet been understood.

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[11] E. Fermi [240], *CPF I,* p. 746–748.

[12] *Ibid.,* p. 750.

Appendix D
Enrico Fermi's bibliography

We give here the list of Fermi's publications, taken with small changes from CPF I, p. vii and CPF II, p. vii. If there is no author, it is meant that fermi was the sole author. Otherwise, authors are listed in the order they appear in the original publication; Fermi is designated by E. F.

Papers

1. *Sulla dinamica di un sistema rigido di cariche elettriche in moto traslatorio.* «Nuovo Cimento», *32,* (1921), pp. 199–207.
2. *Sull'elettrostatica di un campo gravitazionale uniforme e sul peso delle masse elettromagnetiche.* «Nuovo Cimento», *22,* (1921), pp. 176–188.
3. *Sopra i fenomeni che avvengono in vicinanza di una linea oraria.* «Atti della Reale Accademia dei Lincei. Rendiconti», *31,* (1922), pp. 21–23, 51–52, 101–103.

4a. *Über einen Widerspruch zwischen der elektrodynamischen und der relativistischen Theorie der elektromagnetischen Masse.* «Physikalische Zeitschrift», *22,* (1922), pp. 340–344.

4b. *Correzione di una grave discrepanza tra la teoria delle masse elettromagnetiche e la teoria della relatività. Inerzia e peso dell'elettricità.* «Atti della Reale Accademia dei Lincei. Rendiconti», *31,* (1922), pp. 184–87; II. *Correzione di una grave discrepanza tra la teoria elettrodinamica e quella relativistica delle masse elettromagnetiche. Inerzia e peso dell'elettricità.* «Atti della Reale Accademia dei Lincei. Rendiconti», *31,* (1922), pp. 306–309.

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11a. I. *Beweis dass ein mechanisches Normalsystem im allgemeinen quasi–ergodisch ist.* «Physikalische Zeitschrift», *24,* (1923), pp. 261–265; II. *Über die Existenz quasi–ergodischer Systeme.* «Physikalische Zeitschrift», *25,* (1924), pp. 166–167.

11b. *Dimostrazione che in generale un sistema meccanico normale è quasi ergodico.* «Nuovo Cimento», *25,* (1923), pp. 267–269.

12. *Il principio delle adiabatiche ed i sistemi che non ammettono coordinate angolari.* «Nuovo Cimento», *25,* (1923), pp. 171–175.

13. *Alcuni teoremi di meccanica analitica importanti per la teoria dei quanti.* «Nuovo Cimento», *25,* (1923), pp. 271–285.

14. *Sulla teoria statistica di Richardson dell'effetto fotoelettrico.* «Nuovo Cimento», *27,* (1923), pp. 97–104.

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