Ministero per i Beni e le Attività Culturali
Direzione Generale per i Beni Librari e gli Istituti Culturali

Comitato Nazionale per le celebrazioni
del centenario della nascita di Enrico Fermi

ENRICO Fermi
centenario
della nascita
1901 - 2001

Proceedings of the International Conference
"Enrico Fermi and the Universe of Physics"

Rome, September 29 – October 2, 2001

Accademia Nazionale dei Lincei
Istituto Nazionale di Fisica Nucleare

ENEA SIPS

Ministero per i Beni e le Attività Culturali
Direzione Generale per i Beni Librari e gli Istituti Culturali

Comitato Nazionale per le celebrazioni
del centenario della nascita di Enrico Fermi

Proceedings of the International Conference
"Enrico Fermi and the Universe of Physics"

Rome, September 29 – October 2, 2001

Accademia Nazionale dei Lincei

Istituto Nazionale di Fisica Nucleare

ENEA

SIPS

Proceedings of the International Conference
"Enrico Fermi and the Universe of Physics"
Rome, September 29 – October 2, 2001

2003 ENEA
Ente per le Nuove tecnologie, l'Energia e l'Ambiente

Lungotevere Thaon di Revel, 76
00196 - Roma

ISBN 88-8286-032-9

Honour Committee

Rettore dell'Università di Roma "La Sapienza"

Rettore dell'Università degli Studi di Roma "Tor Vergata"

Rettore della Terza Università degli Studi di Roma

Presidente del Consiglio Nazionale delle Ricerche (CNR)

Presidente dell'Ente per le Nuove tecnologie, l'Energia e l'Ambiente (ENEA)

Presidente dell'Istituto Nazionale di Fisica Nucleare (INFN)

Direttore Generale del Consiglio Europeo di Ricerche Nucleari (CERN)

Presidente dell'Istituto Nazionale di Fisica della Materia (INFM)

Presidente dell'Agenzia Italiana Nucleare (AIN)

Presidente della European Physical Society (EPS)

Presidente dell'Accademia Nazionale dei Lincei

Presidente dell'Accademia Nazionale delle Scienze detta dei XL

Presidente della Società Italiana di Fisica (SIF)

Presidente della Società Italiana per il Progresso delle Scienze (SIPS)

Direttore del Dipartimento di Fisica dell'Università di Roma
"La Sapienza"

Index

A Short Presentation of the Fermi Centennial Conference

Carlo Bernardini

Enrico Fermi was born on September 29, 1901; he died on November 28, 1954: a very short life indeed. Nevertheless, his scientific legacy is by far richer than that of most physicists in the last century. This can be easily appreciated by the extremely frequent recurrence of his name in most topics of the so called "modern" physics: Fermi coordinates, Fermi-Dirac statistics, fermions, Fermi-Thomas atom, Fermi motion, Fermi surface, Fermi energy, Fermi's golden rule, Fermi constant, Fermi theory of beta decay, fermi as a unit of length, Fermi age of neutrons, and so on (it is common to forget some in this list). This also shows that his fields of interest went from general relativity to statistical mechanics, from atomic physics to solid state, from quantum electrodynamics to nuclear physics, from elementary particles to astrophysics: actually, there is no field of modern physics in which Fermi did not contribute in a memorable way.

This, I believe, is the reason why so many distinguished people agreed to contribute to this Conference: everybody had, in some way, to pay a debt to an undisputed master of the 20th century, both the organizers and the speakers.

Italy is a nice country, beloved by visitors from abroad because of monuments, museums, climate, perhaps people; arts and literature are apparently the most congenial activities to the population. At first, it might seem that science doesn't have a central role, if any, in the Italian culture; therefore, the sudden appearance of such outstanding personalities as Galilei or Fermi (and many others, indeed) looks like a miracle. Undoubtedly, this is a good reason to examine how and why the "miracle" happened and to illustrate, particularly to Italians, that it is perfectly possible at any moment to repeat the prodigy. With this in mind, some years ago the old and glorious Società Italiana per il Progresso delle Scienze (SIPS, of which Fermi was a member) decided to ask government financial support to invite people of the international physics community in some way or other related to Fermi, to reconstruct in a public occasion both the achievements of the Scientist and the circumstances in which his activity developed.

Thanks to the above-mentioned far-sightedness of SIPS, the Ministero dei Beni Culturali (especially the general director, Francesco Sicilia, who was extremely cooperative) gave us the opportunity to organize a National Committee and I had the honor to chair it with the task to prepare a detailed proposal. I accepted in the second half of 1999, well knowing that it is extremely difficult to do “the best”. Now that the event is concluded, I can and want to say that I would never had reached any result without the invaluable help of Rocco Capasso, secretary-general of SIPS, and Luisa Bonolis, who had a special grant from the INFN (Istituto Nazionale di Fisica Nucleare) to assist the activity of the Comitato. It was decided from the very beginning that the Accademia Nazionale dei Lincei should have a role in the organization of the Conference, so that the Comitato agreed in assigning a part of the program (about one third) to a special commission of the Accademia.

During and after the Conference an Exhibition was open to the public containing some documents, pictures, films and original instruments; the Exhibition was installed in a theather in Rome, the “Teatro dei Dioscuri” near the Presidential residence at the Quirinale. The President of the Italian Republic actually was the first visitor of the Exhibition at the opening ceremony, on September 29, 2001. A large representation of the Fermi family, up to the great-granddaughter Ishbel who delighted all people present, had been there since a couple of days before, because of a ceremony we had promoted in via Gaeta 19, were Enrico was born; also, many members of the Capon family were there, the family of Laura Fermi.

The conference was, in my opinion, very satisfactory; all the aspects of the scientific activity of Enrico Fermi were considered and the peculiarities of his approach to the problems were analyzed. Here you can find the written version of the talks that, all together, constitute an important recollection of original thoughts on Fermi’s ideas, Fermi’s role, Fermi’s time. Almost all the speakers have sent their text in due time to allow a quick preparation of the Proceedings thanks to the full commitment of Diana Savelli and ENEA (Ente per le Nuove tecnologie, l’Energia e l’Ambiente) helped by Rocco Capasso of SIPS and Giovanna Dall’Ongaro (who was engaged by the Comitato in the Conference Secretariat). Professor Chen Ning Yang and professor Leon Lederman, who were not able to join the Conference because of the serious difficulties with international flights after September 11, 2001, were both so kind as to mail a short written contribution to the event: we are very grateful to them for their intention to participate in difficult moments.

A lot of open questions arise when examining Fermi's life: why was he such a precocious child? Which were the books he studied? Where came his interest in analytical mechanics from? How was he able to get a chair in theoretical physics at the age of 25 in a hostile academic surrounding?

Where did he get the idea of his statistics? How did he became quickly reknown in the international physics community and which were his relations with English, Germans and French laboratories? What was his genuine contribution to the new-born Quantum Electrodynamics and how did this prelude to the theory of beta decay? Why did he decide to convert from atomic to nuclear physics? How did he understand slow neutrons? Why did his group miss uranium fission? How was he able to realize the first nuclear reactor in such a short time? What was his contribution at Los Alamos? What was his commitment in political decisions at and after the end of WWII? What were his ideas in elementary particle physics? What the problems he contributed to in astrophysics? What his suggestions for helping Italian physics to restart after the war? How and why did he become interested in computing devices?

All these questions will find answers in the Proceedings of this Conference; this is the reason why I feel very indebted to all the speakers and want to express my gratitude to all of them and to the colleagues who chaired the sessions and conducted the discussion. I want to mention here that on July 2, 2001, the Italian Physical Society (SIF) organized a small meeting at the "E. Fermi International School" in Varenna (Como Lake) on "Fermi as a teacher"; and on October 3-6 a special meeting was organized by professor Remo Ruffini on "E. Fermi and Astrophysics" under the joint sponsorship of the Comitato Nazionale and the International Center for Relativistic Astrophysics (ICRA): both those events were quite successful and have their own separate proceedings.

Finally, besides the debt I already mentioned with Rocco Capasso and Luisa Bonolis, I want to express my great gratitude to some colleagues and friends in the Comitato who gave me invaluable help, particularly in avoiding mistakes: Giorgio Salvini, Franco Bassani, Renato Angelo Ricci and Alessandro Pascolini; I want also to remark the friendly open-mindedness of Edoardo Vesentini, President of the Accademia dei Lincei, in cooperating with the Comitato Nazionale on the common part of the program.

Giorgio Salvini

Enrico Fermi: a Guiding Light in an Anguished Century

Our twentieth century has just ended. It contained so many hopes and human contradictions. Let's try to outline it with regard of physics. At the beginning of the century, six great physicists were born, and they led us toward a new vision and understanding of matter, light, stars and particles in our universe. The great revolution occurred between 1922 and 1935. The basis of their theoretical discoveries were special relativity (1905), the atom's structure (1911), the Bohr model (1913), the universal role of *h*, the Planck's constant in thermodynamics and electromagnetism. Among the new conquers, we recall the exclusion principle and the uncertainty relations. It is sad to recall that, in this splendid phase of human history, violent barbarian wars took place, in 1914-18 and 1939-45. We recall the youth of Fermi, his coherence and character. He gained immortal fame with the Fermi-Dirac statistics; with the behaviour of neutrons in matter; with the discovery of a new type of field and forces, the weak interactions. In the United States, between 1938 and 1954, he built the first atomic pile (1942) and contributed to the study of nuclear energy for civil and military use. In 1946 he tried unsuccessfully to stop the preparation of the hydrogen bomb. In his last years of life, Enrico Fermi studied the different nature of muons and pions, and the best ways to study elementary particles. He was a great experimentalist and a great theoretician. His successes and his surprises in front of unexpected phenomena in nature make us realize to be very far from having fully understood our Universe and its general laws. Therefore we shall continue and boldly prepare the instruments which are necessary to progress and to satisfy our curiosity.

Enrico Fermi: una guida in un secolo tormentato

È da poco terminato il XX secolo, pieno di tante speranze e tante umane contraddizioni. Tracciamone un brevissimo profilo scientifico. Proprio all'inizio del secolo, tra il 1900 ed il 1902, nacquero sei grandi fisici, che ci portarono verso una nuova visione e comprensione della materia che forma la nostra Terra, e della luce, delle stelle, d'ogni radiazione in tutto il nostro Universo. La grande rivoluzione della quale sto parlando avvenne tra il 1922 ed il 1935. Alla base delle loro scoperte teoriche vi fu la relatività (1905), la struttura dell'atomo (1911), il modello di Bohr (1913), il ruolo universale di h*, la costante di Planck in termodinamica ed elettromagnetismo. Tra le nuove conquiste possiamo citare il principio di esclusione e le relazioni di indeterminazione. È triste ricordare però che in questo fecondo periodo della storia dell'uomo il mondo fu scosso da guerre barbariche e violente nel 1914-18 e nel 1939-45. Parleremo della giovinezza di Fermi, della sua coerenza e della sua personalità. Egli si guadagnò fama immortale con la teoria statistica Fermi-Dirac, con il comportamento dei neutroni nella materia e con la scoperta di un nuovo tipo di campo e di forze, le interazioni deboli. Fermi realizzò negli Stati Uniti, dove soggiornò dal 1938 al 1954, la prima pila atomica (1942) e contribuì allo studio dell'energia nucleare per scopi civili e militari. Nel 1946 cercò, senza riuscirvi, di fermare la creazione della bomba ad idrogeno. Negli ultimi giorni della sua vita Enrico Fermi studiò la diversa natura dei muoni e pioni, ed il modo migliore per comprendere le particelle elementari. Egli fu grande sperimentatore e grande teorico. La sua incessante curiosità e il suo rinnovato stupore per tutti i fenomeni naturali ci incoraggiano a proseguire le ricerche sull'Universo e le leggi che lo governano, forgiando gli strumenti necessari al progresso della scienza.*

Physics in the early 20th century

The 20th century, which held so many human hopes and contradictions, has just ended. Let's try to outline its achievements in physics.

After the Franco-German war of 1870-71, Europe enjoyed 45 years without major wars. There was suffering, society was plagued by huge differences in culture and wealth, atrocious social conflicts were in store and eventually exploded in the First World War. But the frontiers and hopes for a better future were open, and many educated people believed humankind would soon have in hand the main keys to understanding the nature of our planet and our universe.

Six great physicists were born at the very beginning of the century, between 1900 and 1902. Heirs to the achievements of Maxwell, Planck, Einstein, de Broglie and Bohr, they brought a new understanding of the matter of which our Earth is made, of light, stars, and the particles in our universe. The people I am talking about were:

- Werner Karl HEISENBERG, 1901-1976
- Wolfgang PAULI, 1900-1958
- Paul Adrien Maurice DIRAC, 1902-1984
- Ernest Pascual JORDAN, 1902-1980
- Enrico FERMI, 1901-1954
- Eugene WIGNER, 1902-1995.

The great scientific revolution of which they were among the protagonists occurred between 1922 and 1935.

Let me proceed with order to justify these statements. I shall limit myself to physics, although I know revolutions also occurred in chemistry, biology and society as a whole.

From 1890 to 1905 there were good reasons for people to be happy. The new sciences of electricity, magnetism, optics and thermodynamics seemed to have revealed the fundamental secrets of our world and how they could be used for human benefit. Freedom from manual labor – first with thermodynamic machines and a few years later with generators and electric motors – the understanding of light as an electromagnetic wave and, soon after, the large vision of electromagnetic waves and radio, which could spread information instantly around the world and promote brotherhood among its peoples, were among the greatest achievements.

Based on this new knowledge and these successes, some eminent physicists of that day thought their science had come close to a final explanation of the inorganic world. They did not know it was just on the brink of a series of fundamental theoretical and experimental discoveries: the revolution produced by the six scientists I've named.

From 1900 to 1915, physics progressed along two lines, originally nearly independent but eventually largely joined in a coherent representation.

One was the analysis of space, time and light, which led in 1905 to the theory of special relativity, essentially due to Albert Einstein, and definitively stated the relationship between mass *(m)* and total energy (E) for any particle of matter in the equation $E = mc^2$, where c stands for the speed of light in a vacuum [1].

The other was the understanding of the microscopic structure of matter. The famous experiments of Rutherford and others (1911) clarified the common structure of atoms: a central nucleus with a diameter of ten thousand billionth part of a centimeter, surrounded by a cloud of electrons [2]. The n. of electrons was found to range from one in hydrogen to 92 in the heaviest atom then known, uranium. The dimensions of this tiny solar system, with the nucleus at its center, were between 10^{-7} and 10^{-8} centimeters. The atom was seen as essentially empty, with a nucleus of enormous relative density at its center.

In 1913-14, when the six men I named were still teenagers, a new model of the atom was proposed by Bohr, followed by Sommerfeld. It incorporated the new progress in classical and relativistic mechanics and Planck's and Einstein's recent discoveries of the existence of a fundamental quantum of action, a quantity that has the dimensions of an action (energy × time) and has been indicated since then by the letter h [2].The coincidence with experimental facts was astonishing.

Another success was de Broglie's revolutionary and still absolutely true proposal (1923) that every elementary particle is also a wave: it propagates with a characteristic wavelike motion and has a wavelength equal to h/p, where p stands for the particle's momentum (mass × velocity) [2].

Physicists were thus confronted with very important results, but they did not yet have a coherent and consistent theoretical basis. They were asking themselves: "What are these things, anyway – these electrons, photons, protons – are they waves or particles?".

A new representation of the world

These elements – very sound but conceptually inadequate – were the starting point for the renewal of ideas I referred to, which led in 1925-35 to a new vision of the physical world. At first this renewal might have looked like excessive critical research – useless twists and turns of ever-unsatisfied human curiosity – but when the cracks were laid bare and the real underlying rocks of knowledge came to light, it was seen as an enormous and irreversible human step forward.

Now hang on to your hats, because the flight to a new world was such that even some great scientists, young and old, had trouble understanding it.

We are now going beyond 1913-15, the years of the soon outdated Bohr-Sommerfeld model, and beyond the theory of special relativity, which by then had been generally accepted. Scientific thought was going down two new roads, both valid but not yet merging.

One was the theory of general relativity, which, through a new analysis of gravitational forces, opens or closes and at any rate describes our universe. I am not going to speak here of this new opening, which was due to Einstein; I shall simply mention a book that elegantly underlines the fundamental value of the new ideas, Brian Greene's *The Elegant Universe* [3].

The other road was essentially opened by the six young physicists I named at the start (Heisenberg, Pauli, Dirac, Jordan, Fermi and Wigner). It is only right to join to their names those of their elders Max Born (1992-1970) and Erwin Schrödinger 1887-1961). But let me make it clear that a whole cohort of young physicists contributed to the new opening with unforgettable works.

What we can take as the starting point was a paper Heisenberg published in 1925. Let me quote Enrico Persico's exemplary presentation of it in his treatise on *The Foundations of Atomic Mechanics* [2]: "The new line was opened by W. Heisenberg with a note published in July of 1925. The fundamental idea expressed in it is that some of the quantities of the atomic model, such as the coordinates of an electron in a given instant, the duration of an orbital revolution, etc., have never been measured directly. Considering that the reasoning based on them leads to known difficulties, one can seriously doubt that these quantities have a real physical meaning and that they will ever be measured in the future. Conversely, other quantities (emitted frequencies, intensities, etc.) can be observed and measured directly. Therefore, rather than searching for a geometric mechanical model that would allow us to find the values of the observable quantities from an unobservable structure, it is better to try to interconnect the values of the observable quantities directly, without using any model".

The "Göttingen boys" (as the very young physicists at the University of Göttingen were dubbed) deserve credit for having shown in 1922-25 how to achieve this objective. The new relationships among observable quantities cannot be expressed using the ordinary methods of algebra; Heisenberg's idea was to use a mathematical algorithm that was already known but had not yet been applied in physics; that is, matrix algebra. This method was largely developed by Heisenberg, Born and Jordan, and they succeeded in finding not only the results already known from the Bohr-Sommerfeld model, but also new results that fit experimental data better.

But still more happened in that prodigious period of 1925-27. Again let me quote Persico: "The ultimate reason why it is not possible to found atomic physics on a mechanical model without a loss of logical coherence and precision was pointed by Heisenberg in a later paper (1927). In it he established the so-called uncertainty principle, which we can say is the key to all atomic physics, and which made it possible to show quantum mechanics in its true light".

One can come up with approximate images of this new wave-particle situation. But we must resign ourselves: we are confronted with a new representation of the microscopic world. The elementary quanta which constitute reality are no longer an ensemble of precise infinitesimal points, as if there were a reality that our senses cannot perceive but which is as precise as the planets and the stars of our universe. Quantum mechanics is a new vision, indeterminate in the dynamic values of each individual particle but nonetheless rigorously ordered and described in its overall structure. If we wish to explain our reality, the existence of the solid state, the properties and origin of helium, spectroscopy, and the nature of the stars, we must accept it.

We are thus confronted with a physical reality that we can largely predict and calculate. But perhaps we have not yet reached a complete understanding of quantum mechanics, hence of our world's essential structure. The great physicist Richard Feynman remarked in 1965: "At this point, many physicists have come, with much effort, to understand general relativity. But I think it is safe to say that nobody fully understands quantum mechanics". I think this is still true today.

Peace and war; molting

Let me pause for a moment in my account of these happenings from 1910 to 1930, which changed our scientific world. I'm thinking of snakes or insects that shed their skin. The snake sheds its old skin but remains its own splendid, nimble self. In the same way, during those years human knowledge experienced strong and perhaps unexpected evolution, a new sign of our capacity to progress.

But a violent, barbarous war broke out in the middle of this magnificent molting phase; from 1914 to 1918, young people from France, Germany, England, Austria, Italy and the United States slaughtered each other. No comment of mine can do justice to this coincidence between a great elevation of the human spirit and an elementary tragedy, but I cannot fail to recall here this duality of human nature. We shall return to it again when we reach the time when so many physicists, Enrico Fermi among them, were swept up in the new tragedy of World War II. But let's go back to our history, from the perspective of Fermi's life.

Enrico Fermi: the new statistics

Enrico Fermi was born in Rome in 1901. He left us too early, and still with a long program of scientific work to finish, on November 29, 1954.

In his biography of Fermi, Enrico Persico – his friend since the age of 14 – says he discovered with surprise that he had a schoolmate who was not only very smart, but had a mind completely different from those of all the best students Persico knew. He writes: “We soon discovered that we were face to face with an extraordinary genius”. I can only mention in passing the biographies written by other colleagues, E. Amaldi and F. Rasetti. They are included in *Conoscere Fermi* (Knowing Fermi), a book recently published for this centenary.

Fermi attended the Normal School of Pisa in 1918 and received his degree in 1922. During his university years he published his first papers on electromagnetism and relativity, two branches of physics that were fairly well cultivated in Italy.

This wide-ranging activity did not prevent Fermi from taking part in student life in Pisa or from taking advantage of the nearby Apuan Alps to indulge in his lifelong love of the mountains.

Soon after Fermi's graduation, the Italian physicist Orso Mario Corbino, who had a good idea of the young man's merits, sent him with two research grants to Göttingen and Leiden. Göttingen had a very active school of theoretical physics [7] operating under Max Born's leadership, and there Fermi met Dirac, Heisenberg, Jordan and Pauli – the people who opened up the new quantum physics with the matrix method already known to mathematicians.

For reasons that are not easy to understand, the exchange of ideas between Fermi and the other young people at Göttingen was not very productive.

Conversely, his stay in Leiden was very useful. Here Fermi's value was appreciated by Paul Ehrenfest, a real master of statistical mechanics. The positive results of this visit appeared when Fermi returned in 1926 to Italy, where he first took up a temporary chair of Mathematical Physics at the University of Florence, and published the statistical theory of a gas of particles which obey Pauli's exclusion principle – the particles now known as fermions.

Going back to my metaphor of molting snakes, the process had enormous consequences and proved irreversible in respect of the old ideas. It brought new discoveries, eliminated old paradoxes, and marked out new roads in physics, astronomy, cosmogony and biology. Here are a few examples from microphysics:

– the structure of hydrogen, deuterium, helium, atoms and molecules was greatly clarified by the new rules of quantum mechanics and by Schrödinger's equation, which Fermi and Heisenberg appreciated immediately [2,5];
– some aspects of the general symmetries that dominate the world of quantum mechanics and are still used today to explain the microscopic and macroscopic order of matter [2,7];
– the distribution of electrons in complex atoms, particularly in the solid state (F. BASSANI, [6]).

In this ongoing analysis of experimental facts, a new principle was announced in 1925 by the 24-year-old Wolfgang Pauli. It could only be explained by the new quantum mechanics, and made clear the structure and architecture of all atoms. This principle, known as Pauli's exclusion principle, says that two electrons cannot occupy the same dynamic position in an atom. The complete explanation can be given only by the new quantum mechanics, but it is worth noting that Pauli formulated his principle at a time when the Bohr-Sommerfeld model of the atom was still the accepted one; in fact, that model sufficed for a first enunciation.

Back in Florence in 1926, Fermi, with his great capacity for synthesis, seized on the Pauli principle and published the statistical theory of a gas of particles that obey it. The new statistical rules he produced go by the name of Fermi-Dirac statistics; in fact, Dirac discovered the same rules a year after Fermi, but he was the one who gave them their proper place in the new quantum mechanics. The particles in question are now universally known as fermions.

In 1926-27, Fermi was known in Italy to only a small group of mathematicians and physicists, but his fame grew rapidly after his stature was recognized by foreign physicists (F. RASETTI, *Biografia di Fermi* [6]).

In September of 1927, an international physics meeting was held in Como to commemorate the centenary of Alessandro Volta's death. All of the world's most eminent physicists were there, including a dozen Nobel laureates and all the inventors of quantum physics.

Arnold Sommerfeld, the great master of the Monaco School, demonstrated, together with his young collaborators, that the strange behavior of the electrons contained in metals could be immediately interpreted by the new Fermi-Dirac statistics.

It was a triumph for Fermi, and many Italians were amazed that their 26-year-old compatriot was already so well known in Germany. As the Bible says, "a prophet is not without honor, save in his own country". But it is only fair to say that in 1926, again thanks to the great Corbino's interest in him, Fermi was appointed to the new chair of theoretical physics at Rome University – the first such chair ever established in Italy.

The years in Rome at the physics department; the extraordinary properties of neutrons

This was the period when Enrico Fermi's quick mind and creative powers came into full bloom. In 1933-34 he discovered the behavior of neutrons experimentally and explained it by formulating the theory of beta disintegration of radioactive nuclei, which was soon accepted as a fundamental phenomenon of our universe.

Around 1931, Fermi and his group had realized that the future of atomic physics was rather limited: theory could explain a large part of observed phenomena, and by this time the main interest lay in the inner part of the atom, the nucleus, which is the densest part and is a hundred thousand times smaller in diameter .

Many properties of the nucleus were already known. It was clear that most nuclei in nature are stable, but others are radioactive; that is, they spontaneously turn into atoms of different elements, usually changing the value of their electric charge. The radioactive process takes place by the expulsion of an alpha particle, i.e. a helium nucleus, or an electron, i.e. the beta particle. Both phenomena are often accompanied by the emission of electromagnetic radiation in the form of gamma rays.

All this seemed to show that the nucleus, like the atom, is a compound state. It was fairly obvious in those times to think that protons and electrons – the only particles then known – were the basic constituents of nuclei. But theoretical knowledge was advanced enough to make the presence of electrons in nuclei very difficult to explain.

It was at this point (in February of 1932) that Chadwick and the Joliot-Curie couple discovered the existence of a new particle in the nucleus, dubbed the neutron, with zero electric charge and the same mass as the proton. Ettore Majorana was perhaps the first to suggest that the nucleus is composed of only neutrons and protons.

This solved all the difficulties related to the presence of electrons in the nucleus, but a new one arose: how can the nucleus emit electrons?

Pauli timidly suggested the hypothesis that the electron might be created in the same moment that it was emitted, together with another light, neutrally charged particle that Fermi later called the neutrino. How this might actually happen was a very serious problem, and Fermi was the one who solved it [6,7,8].

In fact, in the fall of 1933 Fermi presented to his group an article he had written in the early morning hours of the previous days, with full mathematical details. It was based on Pauli's hypothesis of beta ray emission, but it was a complete theory which immediately gave precise explanations of the experimental facts. The basic point lay in the assumption that a neutron can transform itself into a proton plus one electron plus one neutrino (today this is called an antineutrino): $n \rightarrow p + e^{-} + \nu$, with a new kind of interaction.

Only a few theories of modern physics have been so pregnant with results. Fermi's theory is consistent with our present knowledge. It covers not only the usual processes of beta decay (the transformation of a neutron into a proton, with the creation of an electron and a neutrino), but also various other transformations observed in those years among unstable particles[1].

The discovery of weak interactions may have been Fermi's most important contribution to the progress of theoretical physics in the 20th century. It alone would suffice to immortalize him in the history of physics. But only a few months later he made an equally important experimental discovery: radioactivity produced by bombarding nuclei with neutrons, and the particular contribution of slow neutrons.

1 In analyzing Fermi's theory, the famous physicist and historian of science A. Pais [7, pp. 417 et seq.] notes that Fermi was the first to use the second quantization of half-spin particles. Pais also remarks that the famous Fermi constant calculated in 1933 already had a value close to its present value, and that Fermi's paper pointed to the necessary existence of the heavy boson W. I agree with Pais on this point. I had the good fortune to collaborate with Carlo Rubbia, who rightly received the Nobel Prize for his discovery of heavy bosons, and in those years we knew that from the very beginning the theory of weak interaction, to be really coherent, required W and Z bosons.

Irène Curie and Frédéric Joliot had succeeded in creating radioactive nuclei by bombardment with α particles. Fermi and his group thought neutrons would be much more efficient, because their lack of an electric charge would allow them to pass the electric barrier even in the case of the heaviest nuclei.

Working feverishly, the "Via Panisperna boys" created and measured forty new radioactive isotopes. But they soon observed the unexpected effects of some substances, like water and paraffin; their simple presence around or near the bombarded element intensified its radioactivity.

In less than a day, Fermi found the explanation of this phenomenon. Neutrons slow down when they collide with the nuclei of hydrogen contained in those substances. Slow neutrons have a larger resonating cross section against many atomic nuclei. We know that slow neutrons are a fundamental key for access to nuclear energy.

Fermi's group made a whole series of discoveries. In particular, they investigated the element uranium. Without realizing what they had done, they split the uranium nucleus. Fission was demonstrated only four years later, by Otto Hahn and Fritz Strassman. One may wonder whether the history of the world would have been different had "Via Panisperna boys" discovered fission in 1935. Fermi was vexed by his failure to observe uranium fission. It was a warning that nature holds surprises and may conceal its secrets from even the finest minds. I shall return to this point.

Fermi continued to work with neutrons until he left Italy in 1938. In 1935-36, he wrote a long paper, in collaboration with Amaldi, on the diffusion of neutrons in matter and their selective absorption in various elements. An important work appeared in *La Ricerca Scientifica* in August 1936 [6,8]. In it Fermi expounded the theory of neutron slowdown and diffusion. This work was the starting point for all subsequent studies, and became the basis for the calculations regarding moderators when the first atomic pile was built, in 1940-1942.

The scientific work done at Rome University's Physics Department in those years was a very remarkable contribution to the development of physics worldwide. These results earned Enrico Fermi the Nobel Prize in 1938.

Fermi in the United States; nuclear energy and war

After his first trip to the United States, in 1930, Fermi was often invited by American universities to lecture at their summer sessions or to join their faculties on a permanent basis. Torn between his desire to remain in Italy and his desire to remove his family from the distressing environment at home, he hesitated to accept these offers. But in 1938, when the Fascist government passed anti-Semitic legislation that affected him personally – his wife, our unforgettable Laura Fermi Capon, was Jewish, and no guarantee of protection from the new laws could be believed – he made his decision and accepted an offer from Columbia University.

The trip to Stockholm to receive the Nobel Prize provided the occasion for the family's departure. They sailed straight to New York from Stockholm, and arrived on January 2, 1939 (Italy had not yet entered the war). Fermi was then in the middle of his career, and could not have expected to be involved very soon in historical and scientific events of the greatest moment, events that were the direct result of those properties of uranium that had fortuitously escaped the Via Panisperna boys.

In 1939, Otto Hahn and Fritz Strassman had discovered barium in the products created by the bombardment of uranium with neutrons. This was an unexpected discovery, and one of the greatest magnitude. It was soon established that the uranium nucleus could be split into at least two large nuclear fragments, releasing neutrons. These neutrons could in turn free other neutrons by splitting other uranium nuclei, triggering a chain reaction that would affect a whole mass of uranium. A huge amount of energy would be released from the uranium mass, and could be used for peaceful purposes, for instance to generate electricity by providing superheated steam for turbines. But if the process was instantaneous, it would release immense destructive energy; that is, the atom bomb (E. Amaldi [6]).

The decisive step in turning these dramatic new possibilities into reality was taken with the famous Chicago atomic pile. The historic goal was achieved at 2:20 p.m. on December 2, 1942, when the uranium-graphite reactor became active, meaning that the pile went critical, the chain reaction started, and energy was released. The pile was left critical for 28 minutes, with a power of around half a watt, after which the reaction was quenched so that the pile would not become too radioactive and dangerous.

This experiment, directed and controlled by Enrico Fermi in collaboration with the best physicists of that time, in particular L. Szilard, can be considered the first fundamental step in the development of atomic energy. A bronze sculpture by Henry Moore, erected at the site, commemorates the event (C. Salvetti [6]).

Fermi took part in all the subsequent efforts that led to the first experimental atomic bomb, and followed, with less direct involvement, the studies for the hydrogen bomb, a nuclear fission reaction of limitless energy triggered by the uranium bomb [9,6].

The new situation wracked the consciences of the most eminent scientists and politicians. In a letter to the president of the University of Chicago dated September, 6, 1945, Fermi outlined the possible development of the hydrogen bomb and said: “The new weapon is so destructive that in case of war between two powers equipped with atomic weapons, both the belligerents, even the victor, would have their cities destroyed.... The possibility of an honest international agreement should be studied with energy and hope. Today the possibility of such an agreement is the highest hope of the men who contributed to these developments. In their optimistic moments, they express the view that perhaps the new dangers may lead to an understanding between nations much greater than has been thought possible until now”.

In a letter written in 1949 [9], Fermi and Rabi stated that “the fact that the destruction power of this weapon is unlimited means that its very existence and the knowledge of how to build it are a real danger for all mankind. It is undeniably bad from all points of view. This is why we consider it important for the President of the United States to declare to all the world that, on the basis of fundamental ethical principles, we consider it a great mistake to develop such a weapon”.

Fifty years later, we know all too well how these weapons have developed and the terrible dangers they pose.

Let me also report, as accurately as I can, a thought of Fermi’s which I learned about in conversations with Amaldi and Bernardini. In 1954, in Varenna, Italy, a few months before his death, he said that the next century – the one we are living in now – could be really decisive for human history, which teeters between a possible absolute tragedy and the beginning of a possible new epoch of serenity and peace.

Return to fundamental scientific research

After the fall of Germany and Japan, in the summer of 1945, Fermi decided to go back to basic research and moved to the University of Chicago, where in January of 1946 he was appointed full professor of physics. He thus returned to his old life as a researcher. I shall recount only a few essential milestones in his subsequent work, dwelling in particular on his interest in Italy and on his last visit to this country, at the Varenna conference.

Italy had been devastated and disoriented by the war, as I saw for myself as a soldier from 1940 to 1943, then while in hiding in the vicinity of Milan from 1943 to 1945. It is worth noting that even though they were in hiding, the physicists of Milan (and I with them) maintained their interest in research; that is, their hope for a better future. But from the standpoint of physics history, the most important thing was a long-term project on cosmic rays conducted by three physicists at the University of Rome, Conversi, Pancini and Piccioni. Their experiment showed that the μ mesons in cosmic radiation are not the same as the nuclear mesons (pions) that would explain nuclear forces; they were particles of a different kind, totally or almost totally lacking nuclear force [10]. The existence of pions had been predicted by the Japanese physicist Yukawa in 1936, but the particles observed in cosmic radiation by the physicists in Rome (now called muons) belong to the family of leptons (which also includes electrons and other charged or neutral particles), not the family of nuclear or adronic particles.

In 1947, upon learning these results, Fermi and other theoreticians immediately realized the importance of this new research and its significance for our understanding of the properties of nuclear forces. I take pride in this research by the three Romans, though I had nothing to do with it myself, because it was born in the midst of the bombing, hidden in the cellars of a local high school, and attests to the unstoppable curiosity of human beings and their determination to know and understand.

Fermi's postwar years, from 1946 to 1954 – the year of his premature death – were years of intense activity. His achievements in this period include the first experimental analysis of nucleon (proton or neutron) excitement levels, an original theory of the origin of cosmic radiation, and an initial analysis of the possible complex structure of nuclear mesons [6,8]. Using the atomic pile as an intense source of neutrons, he studied the properties of slow neutrons. Among his results I recall the analysis of neutrons applied to the study of crystals, which marked the beginning of a new chapter in experimental crystallography.

After the construction of the Chicago cyclotron, Fermi began to study pion-proton interactions, producing initial evidence of the proton structure and its resonances. This was a fundamental field of research that was to challenge physicists for the next fifty years.

The problem of the origin of cosmic rays interested him for a few years, and in 1949 he presented his own model, based on the collision of ionized hydrogen with the clouds of ionized matter that wander through interstellar space. As Amaldi said, it is a great and elegant vision of our universe.

Fermi's new contribution to Italy

I shall now give an account of Fermi's contribution to Italy after the war, in the years of reconstruction. In 1949 he infused new zest into Italian physics research with his lectures in Milan, Rome and other cities, and took an interest in our research laboratories, which he found livelier and doing more original work than he had expected. The year before, still in the United States, he had expressed this hope for Italy [11]. In fact, on April 27th of 1948 Fermi had written to Prime Minister De Gasperi to recommend that the government budget 500 million lire for scientific research. This sum would make it possible to open new experimental laboratories and equip them with new machines. In the end, the government came up with only half that sum – enough to continue research, but not to start brand new projects.

I well remember our first meeting with Fermi, in Milan, in 1949. Since 1945 I had been working on cosmic rays. To us, living in a largely devastated country, he was a legend. His lectures in Milan, delivered in his unforgettable voice, were of great scientific and human comfort to me.

Italian researchers now wanted to move beyond cosmic rays and explore new areas. This was a phenomenon of scientific unity of which our country can still be proud. In this connection, I recollect the physicists Edoardo Amaldi and Gilberto Bernardini in particular, and of course many others as well.

In 1952, Amaldi and Bernardini managed to concentrate the funds made available to Italian universities on one national-scale problem, instead of seeing them scattered over a series of interesting but relatively minor research projects. This was the origin of the National Synchrotron Laboratories. The new facilities were to be built speedily and well at a location to be chosen through a competition open to the various regions of Italy. The location chosen through this process was the town of Frascati, just outside Rome. Studies and preparations for the new machine began in 1953.

The 1 GeV electron synchrotron went into service in 1958, with the related services and laboratories already operating. The time from start to finish was considered very short, especially since in 1953 the new site was still a field for rooted cutting, with no power lines in sight [12].

But what made this speed possible was not only the inspiration provided by our greatest teachers, but also the practical advice we had from Enrico Fermi, who reviewed our projects and discussed them with us. I want to be quite explicit about this, both as an eye-witness and to pay a debt of gratitude [13].

In August of 1954, Fermi attended a conference held at Villa Monastero in the Italian town of Varenna (R. RICCI [6]). Our synchrotron group was represented by Enrico Persico, the young researchers Fernando Amman and Carlo Bernardini, and myself.

During those unforgettable days, two lectures or scientific reports were presented on the Frascati electron synchrotron. One was Persico's explanation of the theory behind the injection of electrons into the "doughnut"; the other, which I myself presented, was on the overall design of the machine and its progress [13].

These were the last two lectures Fermi heard. He was already sick, and was to leave for the United States a few days later. He listened attentively and made comments and suggestions for which we were grateful and remain ever in his debt.

Analyzing Persico's report and recapitulating his figures, Fermi concluded that for our machine to work as well as possible, it would be most important to inject the electrons produced by the electrostatic accelerator at the maximum possible energy. At the time we were still uncertain about what would be the most suitable type of injector, and Fermi's opinion and advice were essential.

But this was not the only advice we had from Fermi in those spell-binding days. After the competition among many Italian cities and the choice of Frascati, some of the money earmarked for research was still available for new initiatives. The provinces of Lucca and Pisa looked like the best bets. Fermi examined the problem together with Edoardo Amaldi, Gilberto Bernardini and Marcello Conversi, and in the end his advice – unequivocal and precise – was: "Use the money to build an electronic computer".

Conversi immediately seized on this suggestion, and it also helps explain why Italian nuclear physicists began to work with computers first in Pisa, and later in Bologna and Rome. Let me recall that in those months Fermi had just emerged from a period of intense thinking about the functions and the future of electronic computers in scientific research (G. GALLAVOTTI, M. FALCIONI, A. VULPIANI [6]). In 1951-54 he had engaged in long discussions on the subject with J. Pasta and S. Ulam, and, based on the results obtained, he had reached the conclusion that it would be interesting to put computers to work on specific problems involving the long-term behavior of certain simple nonlinear physical systems.

Today his idea has been fully confirmed. A whole line of research, with no lack of surprises, stemmed from that first project of Fermi's; it opened the way to the birth of fertile new concepts related to the theory of complexity and chaos.

Fermi's last days

After Varenna, when Fermi suddenly went back to the United States, his colleagues realized that his health was rapidly deteriorating. To quote what Emilio Segrè reported in his book on Fermi [9]: "I found Enrico in the hospital, attended by his wife, Laura. He was perfectly aware of the situation and spoke of it with Socratic serenity. The impression I got from this visit, the painful reality and the astonishing moral strength with which he was facing it, overwhelmed me, and when I left his room after a while, I almost fainted".

Fermi survived his surgery for only a few weeks. He went back home and tried to revise the notes for his last nuclear physics course. At the hospital, he had told me that this would be his last effort, if his strength held out; in fact, his last piece of writing is a page of the book's table of contents. He died on November 29, 1954, two months after his fifty-fourth birthday.

Let me quote the great physicist Eugene Wigner, who was with Fermi during the war: "His acceptance of death was on an heroic scale" [7, p. 485].

The importance of scientific research, and a warning to nations

Enrico Fermi ranks among the last century's greatest and most devoted students of nature. Our world reveals its deepest character to a few geniuses and lucky people, as if to remind us of its uniqueness, takes us by surprise and overturns all our theoretical and mathematical intuitions.

Enrico Fermi, who was both a great theoretician and a great experimentalist, had direct experience of what we do not know and do not know that we do not know, because only experimental research can lead us through the unknown and reveal the truth.

I shall take three examples from Fermi's life and try to show how they hold a warning for all of us.

The first was the surprising result that appeared to Fermi the experimentalist in 1933, when he was studying the unexpected behavior of neutrons colliding with nuclei. From the experimental evidence, he understood that it was due to the neutrons slowing down in water and other hydrogenated substances, and to the increase in the low-energy cross section.

The second was the splitting of uranium into two or three heavy fragments. Uranium fission, announced in 1939 by Hahn and Strassman, came as a surprise after all the years of measurements made in Rome and elsewhere.

These two experimental "surprises" were the basis for our nuclear culture and nuclear energy, and for all the good and bad things they have brought.

The third was a surprise of high theoretical and mathematical value. Contrary to what Fermi had thought for a long time, a nonlinear system is not always and not simply ergodic. In other words, it can retain its initial conditions for a long time, or forever. The empirical demonstration that Fermi and his collaborators gave with the help of an advanced computer plus a fundamental theorem of Kolmogorov were of the greatest importance, and opened the way to our modern concept of chaos [6, pp. 279 et seq.].

I shall try to draw some conclusions from these and other examples. I think we are still very far from fully understanding our universe and its general laws. Rather, we are just at the beginning of scientific knowledge. This is borne out by the fact that in the last thirty years we have continuously received other splendid surprises from our most advanced research, for instance the breaking of what seemed to be the most solid symmetries, the discovery of the particles at an unexpected mass, the new superconductors, the extended black holes, the still-uncertain origin of our universe after some years of excessive confidence.

The way to understand more is to keep on doing experimental research with an open mind, and to accompany it with mathematical and theoretical meditation. A country that encourages the curiosity to learn about and explore nature, and boldly readies the instruments necessary to satisfy it, is indeed fortunate. We are only at the beginning of knowledge.

Human curiosity, in all directions, will not stop. The societies that are able to satisfy it will progress. We must defend basic scientific research and the laboratories in which it is done, because that is where our future lies. Enrico Fermi's whole life proves it.

I take the liberty of insisting on this point, because I fear that our country's understanding of the need of basic research and its willingness to support it has waned somewhat in recent years.

Of course we also need to analyze the good and the bad in the practical applications of our discoveries. We must work for their use in civilian development and fight against their use for war and abuses of power. We must figure out how they can be used to promote peace among the world's peoples, and we must ensure that our schools educate the new generations to be altruistic and to value knowledge.

It is an immense and difficult aim, but one that it is worth living for.

REFERENCES

1. C. MØLLER, *The Theory of Relativity*, Oxford: Clarendon Press, 1952: A. EINSTEIN, *The Meaning of Relativity*, Princeton: Princeton University Press, 1950.
2. E. PERSICO, *Fondamenti della Meccanica Atomica*, Bologna: Zanichelli, 1940.
3. B. GREENE, *The Elegant Universe*. Plainly, this was new, impassioned research. It remained latent from the 1940s through the '60s but has now been taken up again, seeking a synthesis between gravitational forces and weak nuclear forces. This result is still far off, but the 21st century will surely bring brand new ideas that cannot be imagined today.
4. W. HEISENBERG, *Zeitschrift für Physik*, 879 (1925).
5. W. HEISENBERG, *The Physical Principles of the Quantum Theory*, Dover Publications, 1930.
6. C. BERNARDINI and L. BONOLIS, *Conoscere Fermi*, Bologna: Editrice Compositori, 2001. Includes twenty essays by leading Italian physicists on Fermi's scientific work.
7. A. PAIS, *Inward Bound*, Oxford: Clarendon Press, 1986.
8. E. FERMI, *Collected Papers*, Chicago, University of Chicago Press, 2 vols. (around 2000 pages). Edited in collaboration with the Accademia Nazionale dei Lincei and annotated by Fermi's students and contemporaries.
9. E. SEGRÈ, *Enrico Fermi, fisico*, Bologna: Zanichelli, 1987 (2nd ed.).
10. M. CONVERSI, E. PANCINI and O. PICCIONI, in *Physical Review* 71 (1947), p. 209.
11. M. DE MARIA, *Un fisico da Via Panisperna all'America*, "Great Scientists" series, *Le Scienze* II:8, 1999.
12. G. SALVINI, ed., *L'elettrosincrotrone ed i Laboratori di Frascati*, Bologna: Zanichelli, 1962.
13. Insert dedicated to the memory of Enrico Fermi, supplement to vol. 2, series 10 of *Nuovo Cimento* 1 (1955).

Giorgio Salvini
Born in Milan in 1920, professor of physics at the Universities of Pisa (1952-55) and Rome (1955-95); now professor emeritus. From 1952 to 1960, he directed the construction of the Italian Electron-Synchrotron (1100 MeV), which went into service in Frascati in 1958 and was for several years the world's most powerful electron synchrotron.
Giorgio Salvini conducted research on extensive showers of cosmic rays and on the photoproduction of mesons, in particular eta mesons. In an international project at the European Organisation for Nuclear Research (CERN) in Geneva, he established the existence of large-mass W and Z intermediate bosons (1978-83), thereby confirming definitively the validity of the electroweak theory. At present he is engaged in research on CERN's new LHC proton accelerator. Minister of Universities and Scientific Research in 1995-96; member and Honorary President of the Lincei National Academy.

Carlo Rubbia

Fermi's Contribution to the World Energy Supply

The contributions of Enrico Fermi on the field of energy production will be reviewed, primarily on his early developments of the Nuclear Reactor. The immense consequences of such an invention will be discussed, especially for what concerns the present status of nuclear power and its future, in the light of the present concerns on emissions of conventional fossil fuels. The main problems with an extended world-wide use of nuclear power will be discussed, including possible alternatives in order to alleviate them.

Il contributo di Fermi all'approvvigionamento energetico mondiale

Verrà preso in esame il contributo apportato da Enrico Fermi al settore della produzione energetica, con particolare riferimento ai primissimi sviluppi del reattore nucleare. Si parlerà delle enormi conseguenze che tale invenzione ha avuto, con particolare attenzione allo stato attuale di sviluppo dell'energia nucleare e del suo futuro, alla luce dei crescenti timori riguardanti le emissioni da combustibili fossili convenzionali. Verranno altresì esaminati i principali problemi derivanti dall'uso dell'energia nucleare a livello mondiale, così come le possibili alternative che possono contribuire alla loro riduzione e/o risoluzione.

Fermi's work on the neutron

I shall concentrate my presentation on Fermi's work on the neutron – discovered by Chadwick in 1932 – which he initiated here in Rome in 1934. As it is well known, these studies have opened immense new horizons to Nuclear Physics, not only through the understanding of the nucleus, but also with practical applications in a vast number of domains, including the one of harnessing the immense energy asleep inside the atomic nuclei.

In January of that year, Irene Curie and Frédéric Joliot had reported the artificial production of new types of radio-elements under bombardment of α-particles. However, using helium nuclei of a few MeV of kinetic energy as projectiles, they could not split atoms with atomic number higher than 20; therefore only a part of the light elements could be transmuted. Similar results are obtained with hydrogen nuclei (protons).

Fermi and his collaborators, using neutrons, succeeded in shattering the heavier and even the heaviest elements in the periodic system. Incidentally, for this work, he was granted the Nobel prize in 1938. The neutron has qualities that make it particularly suitable as a projectile in atomic reactions. Both the helium nucleus and the hydrogen nucleus carry electric charges. The strong electric forces of repulsion developed when such a charged particle comes within reach of an atomic nucleus, deflect the projectile. The neutron being uncharged, continues on its course without suffering any hindrance until it is stopped by direct impact on the nucleus. Neutrons can thus traverse very large amounts of matter with small attenuation.

Neutrons may split the light nuclei in different elements with reactions of the type: (n,p), (n,α) and so on. However, especially for heavier elements, there is no ejection of any material part and the surplus energy disappears in the form of gamma radiation. As there is no variation in the charge, an isotope of the same initial substance is obtained, in many cases unstable, causing radioactive activation.

It was some six months after their first experiment with neutron irradiation that Fermi and his co-workers came by chance on a discovery which proved to be of the greatest importance. As recalled by Chandrasekhar, in a conversation Fermi described this discovery in this way:

> "I shall tell you how I succeeded in making the discovery which I believe is the most important of my career. We were working very hard on the induced radioactivity and the results could not be understood. One day, upon arrival at the laboratory, I thought I would like to examine the effect

> produced by a lead block placed in the neutron path. After a great effort in machining it I felt very reluctant in placing it. I said to myself "No! I do not want this piece of lead, what I want is some paraffin" I took a block of paraffin which I found at hand and I put it where it was supposed to go the lead".

It was then observed that the effect of the neutron irradiation was often strongly enhanced (by a factor up to 10^4) when the neutrons were allowed to pass through water or paraffin. Minute study of this phenomenon showed that neutrons were slowed down on impact with hydrogen nuclei present in these substances and that slowed down neutrons were much more powerful. Fermi quickly developed a simple theory in which the now well known 1/v effect of neutron capture was evidenced. Current concepts like the one of "lethargy" and the one of "Fermi's age", of the distance from creation to thermalisation in a diffusion process were developed. It was further found that the strongest effect was achieved at certain speed, which is different for each substance. This phenomenon has been compared with resonance found in optics and acustics.

Practically all elements, with the exception of hydrogen and helium, could be activated. More than four hundred new radioactive substances have thus been obtained, of which about one half due to direct capture, the rest due to decay of the activated elements. The practical applicability of his discoveries was a constant concern to Fermi, in particular the possibility of using activation isotopes as "tracers" for physical, chemical and biological processes, on which he took a patent.

The general pattern that Fermi had found in 1934 took on special interest early in 1935 when applied to the last element in the series of elements, viz. Uranium (Z=92). As it is well known today, fission is a prominent phenomenon in U-235 neutron capture. However, the extraordinary U activation associated to fission fragments was wrongly interpreted – not only by Fermi but also by Joliot-Curie in Paris and Otto Hahn and Lise Meitner in Berlin – as due to formation of additional transuranic elements, for which even the names of Ausonium (Z=93) and Hesperium (Z=94) were coined, from the names of ancient Italian populations.

The discovery of the fission process

One may argue why fission was not observed then in Rome. Amaldi recalls that at a point they put a neutron activated Thorium and Uranium sample directly inside an ionisation chamber. However, in order to remove the natural α-activity of the sample, a thin aluminium foil was added over the sample, thus "ranging out" the fission fragments, which travel only $\approx 10\ \mu g/cm^2$ in matter. Had fission be discovered then, the history of nuclear energy and the realisation of its enormous military applications would have been probably entirely different, and probably so also the course of the second World war, because of a likely much earlier realisation of nuclear weapons.

It was only about four years later that Otto Hahn and Fritz Strassmann, after a very tortuous path, identified the presence of this absolutely new phenomenon, which no one had been able to predict theoretically. Their attempts to separate chemically transuranic elements produced by neutron bombardment on U and Th indicated that one was dealing with a mixture of β-radioactive isotopes rather than with a single, chemically homogeneous, substance. In particular, it was found that there were amongst them also barium radioactive isotopes, resulting from the fission of Uranium. Until now one had observed that neutron activation produced nuclear species which differed by one or two atomic units from the target material: Barium differed from Uranium by as many as 98 atomic units!

At the beginning of 1939, the nature of the phenomenon was promptly recognised – within the Bohr liquid drop nuclear model – by Lise Meitner and Otto Frisch, as due to an extreme collective deformation with break-up of the nucleus in two smaller droplets, each of them sufficiently apart as to be affected only by the strong Coulomb repulsive force, resulting in a liberated energy of the order of 200 MeV. Because of the extreme similarity to the duplication of living cells, they called the process "fission". It is also worth recalling Bohr's disappointment because "theorists" had not predicted the process!

These observations were promptly repeated in several laboratories: in particular, as early as 15th January 1939, Joliot-Curie confirmed and published similar findings in France. The existence of fission became at this point a universally known fact.

When, soon later, it was also recognised that, together with the extraordinary energy of 200 MeV, also a few neutrons were liberated, the scientific community understood that harnessing (awaking) the immense energy asleep in the atomic nuclei through a "chain reaction" had entered the realm of things possible.

In January 1939, just after the 1938 Nobel ceremony, Fermi left definitely for the United States, where he became professor at Columbia University until 1942, when he moved to Chicago. The news of the discovery of the fission process had reached him only upon arrival in the United States. His first American paper is dated one month after his arrival and it is entitled "The fission of Uranium". It is followed by a paper with Herb Anderson and Leo Szilard on "Neutron production and absorption", in which he gives relevant cross sections and shows that the number of neutrons emitted is larger than the number absorbed, as a necessary condition for a chain reaction.

It should also be pointed out that Fermi's interests were solicited at this time both by the muon as a possible candidate of the Yukawa particle of the nuclear force and the measurement of its lifetime by Bruno Rossi. Progressively more difficult communications did not allow the US scientists to fully appreciate the work carried out in Rome on the same subject, first by Gilberto Bernardini and Gian Carlo Wick and later by Conversi, Pancini and Piccioni.

War times

During war times, any process capable to weaken or defeat the enemy takes precedence over all possible benign uses. It has been so also in this case when the possibility of a "nuclear" bomb came to the limelight. At this point in time, Einstein wrote the famous letter to President Roosevelt, in which the military implications of the discovery are fully spelled out.

This letter marked the end of free scientific information and beginning of the military involvement. The subsequent history has indeed been made publicly known only afterwards.

In March 1940, another impulse to the reactor concept was given at the Berkeley Radiation Laboratory with the discovery of Plutonium. This element, not existing in nature, is produced by the U-238 capturing a neutron, as decay of the Np-239. Pu-239, just like U-235, being an odd isotope, was expected to be promptly fissionable.

During the years 1940 and 1941 Fermi and his colleagues performed at Columbia a number of important investigations, precursory to the demonstration in Chicago of the "chain reaction" in December 1942. At the same time, similar activities had been taking place elsewhere, and in particular in Germany, under the impulse of von Weizsäcker and Werner Heisenberg. However, the solution of the problem was not simple and in order to reach success it took all of the experience Fermi had acquired in many years of investigations. Two crucial, subtle problems had to be solved:

1) the thermalisation of neutrons in the "pile" is generally a very fast process, of the order of hundreds of microseconds. To control criticality, any mechanical device would be too slow to cope with the build-up mechanism. Fortunately, the existence of "delayed neutrons", due to neutron emission of the short lived fission fragments, amounting to about 0.7% in the case of Uranium, allows enough time for an effective control action;
2) it was known that the fission process for thermal neutrons was due to the U-235 component of natural uranium, amounting to 0.71%. Enrichment was at that time considered too difficult and expensive. However, the dominant U-238 has very strong resonances of neutron capture, leading to Np-239 and later to Pu-239. Therefore in a Uranium medium, the iso-lethargic slowdown mechanism implies neutron captures at a rate far too large to ensure criticality. Indeed all attempts by Heisenberg in Germany to achieve criticality with a *homogeneous* mixture of Uranium and graphite failed. The smart idea developed by Fermi and Szilard was to work with a *discrete* structure made of small, insulated elements of Uranium in a Graphite matrix. The fast neutrons emitted in the Uranium fission were completely thermalised inside the pure graphite, "missing" the U-238 resonances and re-entering the U only after full thermalisation.

By summer 1942 the work on sub-critical systems was so advanced – as many as 30 sub-critical assemblies were constructed – that it was decided to proceed with the test of a critical system. Purity of the ≈ 400 ton of graphite (40'000 bricks) was finally adequate. Uranium was also available in the form of 20000 bricks. The erection of the pile CP-1 took about one month and criticality was achieved on December 2, 1942.

Fermi and Szilard later took a patent on CP-1 in 1955, after presentation in 1944: it is a magnificent lecture on the physics of the nuclear reactor, which I recommend reading.

CP-1 operated on that day for 28 minutes with a peak power of 0.5 Watt. A few months later a second experimental set-up was operated at Argonne, with a power of 110 kWatt, precursory of the power reactors built by Du Pont in Hartford and destined to Plutonium production.

Peaceful applications of nuclear science grew only much later, for instance with the Conference "Atoms for Peace" in Geneva in 1958.

At that day, there were no journalists, no cameras or tape recorders in order to document the event of the "birth of the nuclear era". Everything was "top secret", within the Metallurgical Laboratory, a laboratory without metallurgists. I wonder if the thirty or so people present there could grasp – within the exaltation of the moment – the dimension and the nature of the changes that nuclear energy was about to bring to the world. Fermi commented in a later script:

> "The event was not spectacular, no fuses burned, no light flashed. But to us it meant that release of atomic energy on a large scale would be only a matter of time. The further development of atomic energy during the next three years of the war was, of course, focused on the main objective of producing an effective weapon. We hoped that perhaps the building of power plants, production of radio-isotopes for science and medicine would become the paramount objectives. Unfortunately, the end of the war did not bring brotherly love among nations. Secrecy that we thought was an unwelcome necessity during the war, still appears to be an unwelcome necessity.
> The problems posed by this world situation are not for the scientists alone but for all people to solve. Perhaps a time will come when all scientific and technical progress will be hailed for the advantages that it may bring to man, and never feared on account of its destructive power".

Production of energy at an acceptable cost

Today, sixty years after the first criticality experiment, one may try to look beyond these events to their consequences in a broader historical perspective. In this time span, the significance of science in all its aspects – not only in nuclear physics but also in biology and so on – has profoundly changed from being essentially pure knowledge to a determinant factor in the world's economy and policies. One may identify as demarcation point the famous letter of Einstein to President Roosevelt. Scientists today find themselves involved both in the economic and the political world. We must learn – also from the mistakes of the past – how to deal with this new dimension of scientific research.

The persisting and now dominant goal of nuclear science is the production of energy at an acceptable cost. In the cost one has to include not only the price of fuels and technology, but also the indirect costs to the population and the environment. So far, we have been unable to substitute conventional energy sources, which have a limited duration, with some other more permanent ones.

From the point of view of the growing energy demand, beyond the fossil era only two known energy sources are in principle capable to supply what is needed, namely nuclear energy and solar energy. In my view they must both be pursued with vigour on a planetary level. But energy from nuclei does not mean necessarily the present nuclear reactors and this for several fundamental reasons, which are as follows:

- there is no more U-235 than oil or natural gas, even at the present level of exploitation, which is about 6% of the world energy supply;
- the problem of safety, now at a probabilistic level, must be further improved. The level of an acceptable risk as perceived today by society is much lower that what it was for instance at the times of the "Cold War";
- the problem of the disposal of nuclear waste, which has already now reached large proportions and causes serious concern, at least for the component which is supposed to last millions of years. Even if we shall not be there to be made accountable, we cannot leave to future generations such an inheritance of our passage on this planet.

There are apriori three possible and well known new nuclear processes which could provide energy for many hundreds of centuries at the present level of the world's consumption. On such a time-scale, the distinction between renewable and not renewable energy is unimportant. All of them are based on breeding reactions, namely a process in which the burning (fuel) nucleus is locally generated from a natural element. These were all known at the time of Fermi's discoveries, which I have just mentioned.

One of them is fusion, and stems directly from the idea of producing nuclear reactions with charged particles. In the process, natural Lithium is transformed with the help of a neutron in the hydrogen isotope Tritium, which in turn reacts with deuterium to produce energy and the neutron.

The other two are based on transforming, again with the help of a neutron, – whose unique features have been already pointed out – either U-238 (99.3% of natural Uranium) or natural Thorium (Th-232) into Pu-239 or U-233 respectively, ensuring the "breeding" neutron from fission. Fermi saw very clearly the importance of breeding for the future of nuclear energy : "The country which first develops a breeding reactor will have a great competitive advantage in atomic energy" (Argonne, 1945). Today, almost sixty years later, I would fully subscribe his statement, however removing the word "reactor". The main problem – and I am sure Fermi understood it very well – is the one of the neutron inventory, or attaining k = 1, for which he struggled for three years from 1940 till 1942 and on which for instance Heisenberg and others had failed.

Indeed, in order to secure *both* breeding and fission, *two* neutrons are needed for each Fermi cycle, rather than one. Criticality (k = 1) becomes much harder to attain. This is why only fast neutron reactors with Plutonium and molten Sodium (Super-Phoenix) – in spite of the tremendous safety problems associated – have only a marginal chance for a realistic breeding. Natural Uranium and Thorium with thermal neutrons (whose advantage had been amply demonstrated by Fermi's work) are excluded, since they cannot meet durably the requirement of criticality ($k = 1$) in a U-235 less configuration (see point 1 above).

Extra neutrons must come from an external source. As clearly pointed out by Lawrence already sixty years ago, a high energy accelerator is the most promising complement to nuclear fission. Since the times of Fermi, accelerator technology has made enormous progress, at CERN and elsewhere, and today a sub-critical ($k < 1$), accelerator driven fission energy source has become a realistic alternative. The spallation reaction – initiated for instance by 1 GeV proton – produces as many as ≈ 50 neutrons/proton, corresponding to an energetic cost of 20 MeV/neutron, compared to the 200 MeV produced by a neutron initiated fission. Accelerator driven sub-critical systems (ADS) have also the added advantage that they can incinerate as well the long lived transuranic elements of ordinary reactor's waste and transmute their long-lived fission fragments (viz. Tc-99) into stable elements, thus solving the problem of the long lived waste.

For these new developments, and as it has happened for the critical reactor, the physics community should first lay down the underlying phenomenology with the help of specific experiments, before engineering and industry may take over. Likewise, a vast political support is necessary, in addition to scientists' enthusiasm and dedication.

Time has come to conclude. Coming back to Fermi, I would like to recall the words of Edoardo Amaldi at the commemoration of the Accademia dei Lincei in 1955, as a testimony of someone who had the good fortune of knowing him:

> "His scientific work is so powerful and so ingenious, the practical consequences of his work so important and relevant that those who did not have the chance of meeting him may be brought to a wrongful image of him. His close relatives and his friends, and those who have known him directly, know that it was very difficult to separate in Enrico Fermi the various facets of scientist, researcher, teacher and human being, since they were all so intimately together. His simplicity and his manner of being, his serenity in front of the problems of life, his absence of any disdain and strangeness of behaviour, were human qualities even more remarkable in view of the contrast with his exceptional qualities as a scientist".

Carlo Rubbia

Carlo Rubbia was born in Gorizia, Italy, on 31st March 1934. He graduated at Scuola Normale in Pisa, where he completed his University education with a thesis on Cosmic Ray Experiments. He has been working at CERN since 1961. In 1976, he suggested adapting CERN's Super Proton Synchrotron (SPS) to collide protons and antiprotons in the same ring and the world's first antiproton factory was built. The collider started running in 1981 and, in early 1983, an international team of more than 100 physicists headed by Rubbia and known as the UA1 Collaboration, detected the intermediate vector bosons. In 1984 he was awarded the Nobel Prize for Physics.

Carlo Rubbia served as Director-General of CERN from 1 January 1989 till December 1993.

From 1970 to December 1988 Rubbia spent one semester per year at Harvard University in Cambridge, Massachusetts, where he was Higgins Professor of Physics.

Since 1999 he is the President of ENEA. Carlo Rubbia is Full Professor of Physics at Pavia University, in Italy.

Alice Caton

Enrico Fermi and his Family

Alice Caton, M.A., Enrico Fermi's oldest living descendent, tells humourous family stories about her grandfather. She discusses in a personal way the dilemmas she, her family and all of our descendants must resolve in order to make full use of Enrico Fermi's scientific legacy.

Enrico Fermi e la sua famiglia

Alice Caton, la più anziana discendente di Fermi ancora in vita, ci racconta divertenti aneddoti familiari sul nonno. Con un approccio del tutto personale affronta le problematiche che lei stessa e la sua famiglia devono risolvere per sfruttare appieno il patrimonio scientifico ereditato da Enrico Fermi.

Enrico Fermi died in 1954. Two years later, his daughter Nella married and I was born in 1957. Although I never met my grandfather, he had a great impact on my life. His brilliance, the force of his personality, and the impact his work has on the world affect me both as his granddaughter and as a member of society.

Sometimes it's quite delightful to be related to Enrico. Recently I was talking with an acquaintance. When I learned he was a retired theoretical physicist, I told him who my grandfather was. If he had been an artist, it wouldn't have occurred to me to mention my relationship to Enrico. The physicist's reaction was predictable. He got very excited. "That's huge!!," he exclaimed. "Do you realize how huge that is?" Then he started telling me how important Enrico was. You know, the stuff I already knew.

My brother Paul is a math professor. When Paul was a student, he and some friends went on a long driving trip. They played a guessing game called 20 Questions. One round stands out in my brother's memory, because that time Enrico Fermi was the answer. "But," Paul said, "when I told them he was my grandfather, they didn't believe me!"

Most of the time Paul doesn't mention his relationship to Enrico. Then when his friends and colleagues find out about it, they get mad at him for not telling them. It must be hard for Paul to know what to do. When he tells them, they don't believe him and when he doesn't tell them, he gets in trouble.

Laura and Enrico's Courtship

Many of the stories I will share with you come from my grandmother's book *Atoms in the Family* about her life with Enrico. In it she tells of her courtship with my grandfather – logarithm style.

Laura and Enrico met one spring day while among a group of friends. It was 1924. She was 16 and he 22. Her friends were impressed because he was already a professor of theoretical physics in Roma. She only thought this explained why he looked rather strange.

They spent the afternoon playing soccer outside of Roma near the spot where the Tevere River splits and forms the Aniene. Enrico was the captain of Laura's team. My grandmother was not athletic and had never played soccer before. Enrico put her in goal, saying that was the easiest job.

At the height of the game, the sole of Enrico's shoe came off and was dangling from the heel, making it hard for him to run. Then he tripped. As he fell to the ground, the ball went to my grandmother. She was startled, but still managed to block the shot and won the game for their team. Laura says it is the only time she did better than Enrico, but I am not so sure.

By the time Enrico and Laura met again, two years had gone by and she had almost forgotten who he was. During summer holidays, they were at an Italian resort where many families knew each other. The children of these families, who ranged in age from very young up to young adults, went on many hikes with Enrico as their leader. When the littlest ones and the girls tired, Enrico carried their extra clothing in his pack. He was very competitive and always first to the top, despite having the largest and heaviest load.

Part of Enrico's gift as a scientist was his ability to classify scientific data and theorems so that he would have them easily at hand for his work. His need to classify did not stop at the laboratory door.

For fun, he sometimes classified people – for example by height, weight, looks or sex appeal. That particular summer he sorted people by intellectual prowess.

"People can be grouped into four classes," my grandfather declared. He went on to explain that only those with exceptional intelligence made it into class four. Laura teased him, "You mean to say that, in class four there is only one person, Enrico Fermi".

"You are being mean to me, Miss Capon. You know very well that I place many people in class four," Enrico paused and then added, "I couldn't place myself in class three. It wouldn't be fair".

After further teasing from my grandmother, Enrico protested, "Class four is not so exclusive as you make it. You also belong to it".

But Laura wouldn't let it rest. "If I am in class four, then there must be a class five to which you and you alone belong". That time my grandmother had the final word and all their friends, except Enrico, accepted her classification system.

After that, Laura enjoyed many social occasions with Enrico and his friends. She coerced her sister Anna to join them – coerced because Anna was an artist and did not find pleasure in the antics of scientific intellectuals. Anna remarked, "They are all so uninspiring. They're just logarithms". This nickname stuck. And so my grandparent's courtship proceeded among the logarithms. At some point in 1927, Enrico declared he was going to do something out of the ordinary. Something extravagant – he would either buy a car or get married.

In September 1927, he bought a car. When Laura heard of his choice, she was disappointed. She already knew that she did not meet Enrico's definition of the ideal wife: tall, blonde and athletic from country stock, with all four grandparents still alive. Laura was short, brunette, good at falling off of skis and her ancestry bordered on aristocratic. None of her grandparents were alive. That he had chosen car over wife was just further proof Enrico would remain a friend. My grandmother immediately resolved to become a career woman instead of marrying.

Soon, however, the pair were driving around Roma attracting attention in Enrico's bright yellow Bébé Peugeot. About nine months later, in the summer of 1928, they wed in a civil ceremony. The wedding went smoothly, except the groom was late. After the rest of his family had left for the ceremony, Enrico, who was only 165 cm. tall, discovered the sleeves on his new store-bought shirt were much too long. He got out a sewing needle, shortened the sleeves and went to his wedding.

Family life

In the next few days you will hear about Enrico Fermi's genius in both applied and theoretical physics. Of course, this is what he is famous for. He is not, on the other hand, known for his domestic achievements.

After my grandparents were married, Enrico told Laura she could pick the furniture as long as it had straight legs. There was a dining room table and chairs, a desk, and so on – all with straight legs. There was a straight-leg couch with no back. The back cushions simply leaned against the wall for support. The couch tended to slip away from the wall and my grandmother asked Enrico to fix it. He nailed two boards on the floor by the legs to hold it in place. Laura was horrified.

Much later, I inherited some of the straight-leg furniture and brought it from Chicago to my home in Vancouver, Canada. I found a simpler solution to the slippage problem, which is to put the front legs on a big rug.

Enrico was fiercely independent and besides that there was no one to teach him. He learned physics and mathematics on his own. In the early days of his professorship at the University in Roma, he was the one who grasped quantum theory. My grandfather tried to show his fellow researchers how both matter and energy consist of waves. They couldn't follow the line of reasoning and had to take it on faith from Enrico. And so he came to be known as the Pope.

But even the Pope can't change the weather. After my grandparents were married, there was a record breaking winter in Roma. Their apartment was so cold they couldn't get the temperature to rise above 8 degrees (C), even with the furnace going full blast. Enrico got out his slide rule and calculated the potential benefit of blocking the drafts in the windows. He concluded storm windows wouldn't make much difference, so, no storm windows. They froze. Months later, after the cold snap had long ended, Enrico revisited his calculations and discovered he had misplaced a decimal point.

While it was too late to buy storm windows, at least my grandmother's ego was soothed by the discovery her husband was objectively fallible.

Another time in the early days of their marriage, Enrico and Laura were on the way to her aunt's villa outside Firenze. The Bébé Peugeot broke down on the highway. Enrico replaced a torn fan belt with the belt from around his waist and they actually completed their journey on schedule.

Laura was helping Enrico to write a textbook for high school *(liceo)* students. My grandmother took notes which she later transcribed. He dictated, "It is evident that in a nonuniformly accelerated motion, the ratio of the speed to the time is not constant".

"It is *not* evident," my grandmother stated, without raising her eyes from her notes.

Enrico retorted, "It is to anybody with a thinking mind".

"Not to me".

"Because you refuse to use your brains," he snapped.

Exasperated, Laura suggested they consult her sister Paola. Enrico agreed.

Now Paola was the perfect one to adjudicate since she had just passed her high school (liceo) exams, and had even received a decent mark in physics, a subject she did not adore. Paola was baffled by Enrico's obvious. From then on, she was *the* arbiter of my grandparents' understandability squabbles.

When he accepted his first teaching engagement in the US, in 1930, Enrico taught himself English in two easy steps. Step one. He read 10 adventure novels in English. He only allowed himself to use a dictionary for the first 10 pages of each book. Step two. Enrico arrived at the University of Michigan. He arranged with friends to give him corrections at the end of each lecture. According to my grandmother, he never repeated a grammatical error once corrected.

Enrico turned down many offers of work from US universities during the 30's. With the rise of Nazism and fascism, things changed. Laura was Jewish and by 1938 the risk of staying in Europe outweighed my grandparents desire to stay here in Italy. Enrico won the Nobel Prize that year and it was presented in Stockholm, Sweden. My grandparents used this opportunity to leave for the United States with my mother Nella and her little brother Giulio – of course, the straight-leg furniture went along too.

From Emilio Segrè's biography of my grandfather, one learns Enrico's mother was a teacher and very intelligent. Besides being a good cook, she built her own pressure cooker. Is genius learned or passed on? I don't imagine Enrico would have had much trouble building a pressure cooker. But when it came to using the kitchen that was another story.

I think the family was living in New Jersey. Laura and the maid did the cooking, but one time they were both sick. So Enrico and my mother took over. Neither of them knew how to cook anything. Enrico asked my grandmother what would be easiest. "Boiled potatoes, with butter and salt," she told them. There was nothing to it. Or so she thought. The potatoes were a disaster. They tasted awful and were almost inedible. Enrico and Nella tested them so many times, the potatoes never had a chance to cook properly.

Once Enrico learned he was no good at housekeeping, his innovative mind went to work. At Los Alamos, there was a labour shortage, so Enrico devised a plan. Chimpanzees and gorillas would be trained to do housework for the scientists' wives. The housing office could create an Agency of Primate Distribution which would both care for and train the monkeys to scrub, vacuum, dust, greet visitors and wait tables. My grandmother complained that he never presented the idea to the housing office and so the labour shortage went on.

My mother said she had a pretty much impossible time buying gifts for her dad. However, one Christmas, she succeeded in giving him something he had never seen. It was a toy bird made of glass. Its round body was partially filled with a clear liquid and its head covered with red fuzzy stuff. If you put a glass of water in front of it, it seemed to drink perpetually and rhythmically like a pendulum, dunking it's long red beak for one drink after another.

Enrico was delighted. As usual, he determined the most straight-forward and efficient experiments to find out why the bird kept drinking. First he substituted alcohol for water. The bird sped up. Next he cut off its air supply by inverting a large glass jar over bird and water. The bird slowed down and soon stopped moving altogether.

"It must be alive", said Enrico laughing, "for it gets drunk on alcohol and smothers without air". Later the physicist Edward Teller came to visit. He too was intrigued by the bird which became the center of conversation. "What would you say if it took a step forward?" my grandfather asked Teller. "Why, I would set it back and see if it did it again", answered Teller without hesitation.

During the war, my grandfather was asked by a U.S. government agency to evaluate a particular substance and suggest applications for war work. Enrico brought the substance home and showed it to my mom and my uncle Giulio.

The stuff was soft like chewing gum and if pulled slowly could be stretched into a long thin string. But as soon as you jerked it, it cracked. You could shape it or scratch designs on it, but leave it alone and it melted into a blob. A blow with a hammer shattered it like glass and sent it flying in all directions – Enrico demonstrated carefully so that none of the material would be lost.

Nella says she asked a lot of questions and got a physics lesson from her father. The stuff actually was a liquid, like glass. Given sufficient time, either would melt into a blob – but for glass it took years instead of minutes.

Enrico was puzzling about possible applications and asked his kids for suggestions. In spite of the fun the three of them had, no one realized the obvious – it was a great toy. Today the stuff is marketed as Silly Putty and probably made the toymaker millions.

Enrico's legacy and our family

At 185 cm my brother Paul is by far the tallest in our family – 20 cm taller than Enrico was. Paul became a vegetarian when he was in grade school. He wouldn't even wear leather shoes. So our grandmother gave Paul an old pair of Enrico's tennis shoes. And they fit. Paul's friend said: "See you can fill your grandfather's shoes!". My brother wore the sneakers until they disintegrated.

I think everyone in my family grapples with Enrico's legacy in some way[1]. In the early 1960's when I was about five years old – well before I could understand what Enrico had achieved – I remember proudly announcing at school to whomever would listen, "My grandfather invented the atom bomb!"

At the same time I was feeling so proud of who I was, I also learned about the dangers of nuclear energy. My father taught me about radioactive fallout from above-ground nuclear testing – and I felt uneasy. He said it had changed the earth and its atmosphere for the worse. Soon the unease includ-

[1] My grandmother and my first cousin Rachel Fermi, Enrico's son's daughter have left a record which encouraged me to continue to explore my grandfather's legacy. In 1995, Rachel published a book of photographs of the Manhattan project called *Picturing the Bomb*. On the facing page to the book's forward, she quotes our grandmother – filling the page with large black capital letters: "But, above all, there were the moral questions. I knew scientists had hoped that the bomb would not be possible, but there it was and it had already killed and destroyed so much. Was war or science to be blamed? Should the scientists have stopped the work once they realized that a bomb was feasible? Would there always be war in the future? To these kinds of questions there is no simple answer". – Laura Fermi, in *Reminiscences of Los Alamos*, edited by Lawrence Badash.
Rachel's Foreword begins: "My grandmother asked these questions twenty-five years after the first atomic bombs were used. This book has grown out of the complexities underlying her questions, and from my own need to understand more fully the grandfather I never knew: a physicist whose work radically altered the world he was born into and helped create the world in which I now live".

ed guilt and shame as I felt so associated with the negative outcomes of the release of nuclear energy.

During that same period, I was at my grandmother's apartment in Chicago. I was still able to walk under her dining room table from Italy with the straight legs. From my child's view, the table seemed very long and forbidding.

My grandmother and a group of ladies were at the table folding flyers for the Air Pollution Control Committee. It was a lobbying group my grandmother started with some of her friends. They were pioneers in the environmental movement. They tested smoke stack emissions in Chicago. Through their lobbying efforts, local government passed air quality emission standards for the city. This led to a significant reduction in air pollution in Chicago.

On that particular day, I was allowed to help fold flyers. I got my first taste of social action and the pride which comes from making a contribution.

My childhood experience of pride and unease is not unique to me – nor even to my family. Much of what we grapple with also applies to our human family. When the first atomic bomb was detonated in a test near Los Alamos, NM, there was elation and pride in a great achievement. Soon after, the scientists also felt uneasy as they understood the destructive potential of this new technology at a deeper level.

Similarly, when World War II ended, after the dropping of atomic bombs on Hiroshima and Nagasaki, there was great pride and celebration in the total lives saved and the ending of the War. People began to learn about the positive potential uses of nuclear energy. The general public was also confronted with the dangers of nuclear power and its potential for devastation.

This past May, I was in Chicago at the memorial service of a student of my grandfather's and a friend of our family Dr. Ugo Fano. One of the most entertaining speakers was a 90 year-old professor. I was fortunate enough to find him at the reception. He shared the following anecdote with me about my grandfather:

> It was after the War, but before the hydrogen bomb was developed. I was talking with Fermi over lunch at the University of Chicago. I asked Fermi, "Did you ever have any qualms about your contribution to the development of the atom bomb, leaving aside the necessity of winning the war?" He looked at me with a face of incredulity and said something like this: "You and I are scientists. Our purpose is to explore nature. What is done with our work, is the responsibility of society".

The reply seemed in character with all that I had ever heard about my grandfather.

Here was a man who felt supremely confident in the sphere of physics, but knew his limitations in other spheres. He directed his scientific inquiry and that of his teams so as to yield fundamental results – results which have deepened our understanding of the physics of the universe. As a teacher and mentor, Enrico fostered continuing scientific inquiry and progress. In addition to all of that, he devoted some of his time to public service in the area of international policy development on nuclear energy, including taking a strong stance against the development of the hydrogen bomb.

We, Enrico's descendants, inherited the Fermi Italian straight-leg furniture. I have the living room chairs and the backless couch that still slips away from the wall. All of us alive today, and all who will come after us, are heirs to Enrico Fermi's scientific legacy. We all have a stake in it. Since the end of World War II, humanity has had knowledge of nuclear energy and its incredible potential for benefit as well as harm.

Enrico Fermi gave us a lot. And there is more to be done. Enrico Fermi's work, and the work of other scientists, exists in a world full of people who, in a certain way, are like Enrico. Remember Enrico couldn't boil potatoes nor figure out his home needed storm windows in cold weather. He, like all of us, was both brilliant *and* fallible.

We have a collective, developmental task. We must learn to integrate our scientific knowledge and our human experience to find the answers to the nuclear dilemma, and to the many other dilemmas facing us today.

My mother Nella eventually learned to boil potatoes that tasted good. Our world has yet to find the right nuclear recipe – how to harness nuclear power for the benefit of all living things.

We will need all of our human gifts to survive and flourish on this planet. From here, it looks to me like Enrico contributed all of his gifts. Now it's up to us to contribute ours. We can look back to Enrico for inspiration, if we look to ourselves for the future.

ACKNOWLEDGMENTS

I wish to thank the following family members, friends and colleagues who assisted me in various ways in writing this speech – for their humor, encouragement, and thoughtful and provocative suggestions: Joan Balmer, Tad Dick, Rachel Fermi, Piero P. Foà, M.D.*, Robert W. Fuller, Carol Layton, Debbie Mayotte, Terry Neiman, Peter Rastall, Ph.D., Hannah Salia, David Unterman, Kathy Weiner, Dr. Paul Weiner*, Elisabeth Zoffman.

References

Argonne National Laboratory (producer). (n.d.). *To Fermi with love: Commemorative two-record album on the life and times of Enrico Fermi*. Argonne, IL: Argonne National Laboratory.

*Fermi L. (1955). *Atoms in the family: My life with Enrico Fermi*. London, England: George Allen & Unwin Ltd.

Fermi R., Samra E. (1995). *Picturing the bomb: Photographs from the secret world of the Manhattan Project*. New York: Harry N. Abrams, Inc.

Holton G. (1998). *The scientific imagination*. Cambridge, MA: Harvard University Press.

Mason K.R. (1995). *Children of Los Alamos: An oral history of the town where the atomic age began*. New York: Twayne Publishers.

McEvoy J.P. (n.d.). *Heroes and villains: Enrico Fermi*. Unpublished manuscript.

Moody S., (1992, December 27). 50 years ago, man released power of the atom. *Sarasota Herald-Tribune*, pp. 1F, 6F.

*Segrè E. (1970). *Enrico Fermi: Physicist*. Chicago: University of Chicago Press.

Telegdi V.L. (n.d.). *Enrico Fermi at the University of Chicago*. Unpublished manuscript.

*Weiner N.F. (1995). *A daughter's memoir of Enrico Fermi*. Unpublished manuscript.

Wilbur K. (2000). *A theory of everything: An integral vision for business, politics, science, and spirituality*. Boston: Shambhala.

*Denotes source of anecdote(s).

Alice Caton

Alice Caton, daughter of Nella Fermi Weiner, is the oldest of Enrico Fermi's living descendants (of four grandchildren and three great-grandchildren). Ms. Caton grew up in Chicago, Illinois with her brother Paul Weiner. She has lived in Vancouver, BC, Canada for almost 20 years. Ms. Alice coaches individuals and facilitates and trains groups in strategic planning, leadership development, interest-based decision-making, communication and conflict resolution skills, cross-functional teams, business process improvement and related areas of organizational development.

Before becoming an organizational development consultant, she ran her own computer training company for 7 years, offering her services to business and government. Ms. Caton combines her project management experience on software implementation projects with a warm and insightful approach to working with clients.

Ms. Caton has a Bachelor of Arts in Theater from Oberlin College, Oberlin, OH (1979) and a Master of Arts in Applied Behavioral Science from the Leadership Institute of Seattle, Bastyr University, Seattle, WA (1999). She holds a Certificate in Conflict Resolution from the Justice Institute of B.C.

Gerald Holton

The Birth and Early Days of the Fermi Group in Rome

The presentation concentrates on the formation of the group and its early years (to 1934 inclusive), with special attention to Enrico Fermi's ability to combine his mastery of theoretical and experimental physics serendipitously with the historic situation in which he found himself with regard to the state of science and culture at the time.

La nascita ed il periodo iniziale del gruppo di Fermi a Roma

La presentazione si sofferma sulla nascita del gruppo ed i suoi primi anni di attività (1934 compreso), con particolare riferimento alla capacità di Enrico Fermi di coniugare la sua padronanza della fisica teorica e sperimentale con la situazione storica nella quale si trovò a vivere e lavorare, ed allo stato della scienza e della cultura in quegli anni.

I feel honored to have been asked to speak on the beginning and early days of Enrico Fermi's group in Rome. My main sources for this lecture are of course first of all Fermi's own *Collected Papers*, the splendid books and essays by Laura Fermi, Edoardo Amaldi, and Emilio Segrè, also the laboratory notebooks of Fermi's group that are kept at the Domus Galilaeana in Pisa, which I had occasion to visit when I spent a sabbatical leave at the University of Rome some years ago. I also had the great pleasure of meeting Enrico Fermi when – just about a year before his tragic death – he visited our Physics Department at Harvard for two weeks, giving brilliant lectures on topics ranging from the Origin of Cosmic Radiation to High Energy Nuclear Collisions.

Some years later, I felt that there should be a filmed biography of a great scientist for use in schools and colleges. Enrico Fermi was the most obvious and appealing choice. I initiated and acted as production supervisor of such a film, called "The World of Enrico Fermi". That allowed us to conduct interviews specially arranged with Professors Amaldi, Segrè and Rasetti, with Laura Fermi, whom I had come to know well, and with many other colleagues and former students of Fermi in his days in the United States, such as Agnew, Anderson, Chamberlain, Chew, Goudsmit, Morrison, Oppenheimer, Rabi, Weil, Yang and others. And that in turn caused me to research and publish on the role of Fermi's group in the recapture of Italy's place in physics. But let me add that one of the important consequences of this and other conferences on Fermi and his work during this Centenary is that they will furnish much more information for historians of science. In truth, compared to some other 20^{th} century scientists of the same high caliber, more needs to be done on this pivotal figure in modern science.

An unexpected discovery

For there is something quite special about the place of Fermi in history. We all know that in the turbulent flow of time there have arisen, on rare occasions, events that did not fit any previously made plan, but which nevertheless powerfully shaped all subsequent history. Among the most spectacular examples is of course the discovery by a captain, born in Genoa, who set sail toward Asia, but encountered instead the land that came to be called the New World. From that moment, the clock for the modern period was set. Another instance of a similar sort of serendipity was when a professor of mathematics at the University of Padua, having used his homemade spyglass for terrestrial explorations, raised it to scan the heavens, and was the first to see there the evidence, in the appearance of the Moon, Jupiter, and Venus, that the existing worldview had to be replaced again by a new one. That is when the clock for modern science suddenly came alive. And a third example was a seemingly unplanned event that took place right here in Rome, two-thirds of a century ago, also with transforming consequences, for large sections of physics, chemistry, engineering, medical research, and ultimately for politics and warfare.

I refer of course to the unexpected discovery, on a certain day in October 1934, by Fermi and members of his group, of what was later called the moderator effect, the way to turn fast neutrons into slow ones, and the startling new phenomena those neutrons could induce. Over four years later, in a paper by Otto Hahn and Fritz Strassmann – in which Lise Meitner and Otto Frisch immediately recognized the evidence for nuclear fission – Hahn and Strassmann referred to the key role in their work of "slowed-down neutrons". They did not happen to mention the Italians who had found how to make those slow neutrons. Nevertheless, it can be said that on that day in Rome, in October 1934, the clock began to tick which ever since has marked the nuclear age in world affairs.

Despite their diversities, these three examples, and others of this sort throughout history, have in common not only the initial unintention on the part of the discoverers, and the extraordinary transformations they eventually caused. They also are of the special, rare sort of research findings which do not correspond to the more usual ones. They are not merely discoveries of new facts, or verifications of predictions, or answers to old questions, or supports for an unstable theory. They are not just the addition of even another large brick to the ever-unfinished Temple of Isis. Rather, a good analogy for the sort of discoveries I have described is that suddenly and unexpectedly they open access to a blank area on the map of established knowledge, allowing an exploration of a new continent of *fruitful ignorance*. For what is most prized in science, and is most profound, is the discovery of vast ignorance, of a range of hitherto undiscovered truths, owing to the breakdown of a standard model.

Superficially, those three examples of profound discoveries might tend to support the old illusion that the course of history itself is decided by the works of great men, to use the title of Wilhelm Ostwald's famous book. That view is contrary to the other old illusion that it is history that shapes the ideas and acts of even the greats. But I call each of these two opinions by itself illusory, because any study of the actual cases soon shows that both mechanisms together are constantly at work. As the psychologist Erik Erikson put it, "an individual life is the accidental coincidences of but one life cycle with but one segment of history". Even the most unexpected, fateful chance observation has its own prehistory; and conversely, even the most turbulent event in world affairs has never been proved to have been caused by some overarching *Zeitgeist*.

From childhood to the University

With this preamble, my ground is cleared for the task assigned to me. Properly speaking, the birth of Fermi's group starts with the birth of Fermi himself. From childhood on and into his early student years, young Enrico was recognized by his teachers, acquaintances, and friends to be a prodigy. Relying largely on self-study – a mode typical of great scientists, from Kepler to Faraday to Einstein – Fermi soon became fully at home with modern physics, enjoying equally the experimental and the theoretical sides. Emilio Segrè tells us that even as a very young man Fermi turned to quantum theory, probably the first to do so in Italy, where that subject was considered a sort of no-man's land between physics and mathematics, rather than a promising research site. That part of physics was not taught in universities there, and a dissertation in theoretical physics as such would have been shocking. (Incidentally, Edwin C. Kemble, arguably the first to do quantum physics research in the United States, had found the same to be true a few years earlier, as a student at Harvard University).

At any rate, at age 21, Fermi finished his dissertation at the University of Pisa, on images obtained with monochromatic x-rays by means of a curved crystal. As elsewhere, the experimental equipment available there was largely for spectroscopy. To build a suitable source of x-rays, Fermi organized his fellow students, Franco Rasetti and Nello Carrara, to help him. It was an early indication of his leadership quality. And again, typical of his later years, Fermi was not satisfied with publishing the experimental thesis (his seventh paper, dated 1923), but before that had put into print a separate, lengthy theoretical paper on the properties and theory of x-rays. There he showed that he commanded the whole literature – including von Laue, Bragg, Moseley, Barkla, Sommerfeld, Maurice de Broglie, Debye, Scherrer, etc. – in all the many languages.

As in later years, already then he was keeping physics almost constantly in his thoughts. There is a famous story, perhaps apocryphal but believable, that one of Fermi's friends once found him pacing up and down in a room, with a preoccupied look. Concerned, his friend asked if Fermi was troubled by something. "No", Fermi replied. "I am just estimating by how much I am depressing the wooden floor as I walk along it".

Experimental x-ray studies, and even quantum physics, were by no means the only subjects then enchanting the young physicist. It is very significant, and became important for his subsequent career, that starting at age 19, Fermi's first five published papers were all on relativity theory. Most of them showed his mastery of the methods of general relativity, the theory just recently and spectacularly confirmed by Eddington's experiment. To be sure, almost all the older generation of physicists in Italy was skeptical and hostile to that theory. But like Wolfgang Pauli and Werner Heisenberg, at about the same time and at the same young age, Fermi had evidently been captivated by Herman Weyl's new book, *Raum, Zeit, Materie*, for which Einstein himself had written an enthusiastic review in 1918. Fermi contributed to relativity a theorem of permanent value (later called Fermi coordinates), and soon it was incorporated into textbooks on General Relativity. Luckily, Italy had at that time several master mathematicians working in general relativity, such as Tullio Levi-Civita and Gregorio Ricci-Curbastro. They, and other mathematicians of first rank, including Guido Castelnuovo, Federico Enriques, and Vito Volterra, began to notice Fermi's papers and support his rise.

Yet, Fermi properly realized soon that this was *not* the field in which to build his own career. From 1921 to 1925, he had no less than thirty-one publications, as reproduced in his *Collected Papers*, varying from relativity to statistics to nuclear physics; some were in experimental spectroscopy, but most in theoretical physics – even though he knew that in fact there was not a single University chair available for it in all of Italy. His persistent, wide-ranging and enthusiastic interests, his optimism and his sheer productivity were astonishing.

Since the theme assigned to me is not the brilliance of Fermi's various contributions, but the formation of his group, I can point out that we have here already met the first of the team that would soon be formed, namely the enormously talented experimental physicist and Fermi's schoolmate, Franco Rasetti. And it is also time to introduce a remarkable figure in the eventual rise of Fermi and his group, namely Orso Mario Corbino. Twenty-five years older than Fermi, he was widely known for his early work in magneto-optics, for which he had been admired by Augusto Righi of Bologna, considered the previous generation's leading physicist in Italy.

After Corbino had been called to the University of Rome, his talent as an administrator and unselfish connoisseur of talent quickly led to his becoming Senator of the Kingdom (1920), Minister of Public Instruction (1921) and Minister of National Economics (appointed in 1923, by Mussolini, although never being a member of the Fascist Party). Corbino's keen scientific mind, combined with his hope to put Italy again on the map as a center of great physics research, led him to mourn the sorry state of physics there, symbolized for him by Righi's death in 1920. He saw clearly that Italy was then unable to take advantage of the world-wide rise of opportunities in the new physics of the day. Without knowing it clearly, by 1920 Corbino was ready to discover a Fermi – just as Enrico Fermi, for his part, knowing Corbino, must have realized that without such a man there might never be a Fermi group. And yet, within a few years Corbino was able to help in the appointment, to new professorships, of two brilliant young theoretical physicists – Fermi and his childhood friend, Enrico Persico.

After Fermi's graduation from University, he returned to Rome in 1921 at age 20, living with his parents and his older sister, as he was to do for several more years, a member of a closely knit family. At the moment, he had neither a job nor prospects for one. But he made his first visit to Corbino. The two men immediately took to each other. With Corbino's help, Fermi obtained fellowships, spending unhappy months at Göttingen, and happy ones in Leyden under Paul Ehrenfest; then a couple of years in temporary posts at Florence, working with Rasetti. At last, in 1926, Fermi was appointed to the new Chair of Theoretical Physics at the University of Rome, engineered by Corbino, who was officially the director of the University's Physical Institute at Via Panisperna 89a, the building in which the top floor was in fact the flat of the family of Corbino himself.

The Roman School

And now Fermi could begin to put his and Corbino's dream into reality. But that was not going to be easy. So far, Fermi had admirers, but no followers. The outlook for building a school of bright young physicists was very dark. There was not even an Italian text on atomic physics for advanced university students; and of those students there were only a handful, because the expectation for eventual university employment was extremely poor. Serious action was called for. First, Fermi, in 1927, wrote and published that missing textbook. Also, Corbino used his influence to bring Rasetti from Florence to

Rome, eventually settling him into a professorship for spectroscopy created for that purpose. (Oh, how we all would have liked to have had a Corbino on our side. He seems to me the ideal candidate as patron saint for bright young scientists). And now Fermi and Rasetti began to recruit promising university science students for their *Istituto*.

Emilio Segrè reports that as a youth he met Rasetti first in the spring of 1927 through a mutual friend, the son of the mathematician Federico Enriques, while they were mountain climbing. Soon after, he met Fermi, who was only four years older, with the usual result that Segrè knew instantly that here was an extraordinary teacher, scientist and human being. That autumn, with Corbino again smoothing the administrative problems, Segrè transferred his studies from the engineering section to physics, thereby becoming Fermi's first pupil. And so, in his words, "The Roman School had started". Segrè in turn persuaded his friend Ettore Majorana to join the group, at least informally.

Here, an important aside is called for. These last three sentences contain several clues to the vitality and unique characteristics of the formation of the Roman School. First, Corbino was ever ready to help, in any way. Second, all of Corbino's boys, as they came to be called later, were within a few years of the same age. Third, among them there was a camaraderie in which the only trace of hierarchy was the acknowledged centrality of Fermi's brilliance. Finally, almost all members of the group were part of one social network. They typically even spent parts of their vacations together at the seaside or in the mountains. For example, in the summer of 1925, Fermi was in the mountains with the families of Levi-Civita, Castelnuovo, and Ugo Amaldi. Amaldi's 17 year-old son, Edoardo, was fascinated by the scientific talk, and ended up accompanying Fermi on a bicycle tour of the Dolomites. A bonding had begun there which, together with Corbino recruiting him from the engineering class, resulted two years later in Edoardo becoming part of Fermi's physics group at the Institute. If all this sounds too much like the interactions within a stereotypical Italian family, let us remember that this was not the way things then generally arranged themselves in physics laboratories in, say, Göttingen, or, for that matter, in Cambridge, Massachusetts.

At any rate, we see that a critical mass was being formed at the Institute in Rome. The group's younger students became more and more competent, partly through participating in experiments with Rasetti, but above all through Fermi's constant care and his informal theoretical seminars. For Fermi was an ideal teacher, so enthusiastic that – as John Marshall later recalled – he sometimes saw to it that he was the only person near the blackboard who had the chalk. Marshall added: "It was very difficult to argue with the only person who had the chalk".

Fermi's typical mode of teaching was to keep it clear and seemingly improvised, distrusting abstract theories such as the quasi-philosophical Copenhagen versions of quantum mechanics, favoring instead the visualizable approach of Schrödinger. Hans Bethe referred admiringly to Fermi's way as "enlightened simplicity". It also helped his students that Fermi at that time read omnivorously in new physics publications, interested in the whole spectrum of new ideas. Bruno Pontecorvo, in his book on Fermi, called him simply "Scienziato universale". And despite his extraordinary command and expertise, Fermi showed no trace of vanity, but rather won immediate respect through his unselfconscious charisma.

Last but not least, one must mention the famous, perhaps unique, way Fermi thought and taught about physical phenomena: Just as his experimental equipment functioned well despite being often assembled out of cannibalized pieces and put together in the least complex manner, so also did Fermi consider Nature herself put together *in the most parsimonious way*. That is to say, he recognized again and again the same scenario to be at work in completely different contexts. Thus he applied the same idea of scattering length in explaining the pressure shift of spectral lines (Document 95 in Fermi's *Collected Papers*) and in artificial radioactivity produced by neutron bombardment (Document 107) – even using the same diagrams. Or again, applying the same statistical theme to atoms on the one hand, and to neutrons on the other. Fermi's great paper on beta decay at its core treats the emission of electrons and neutrinos in nuclear events as analogous to the emission of photons from atoms in excited states (Document 80b). As Fermi's colleague at the University of Chicago (and co-author on two papers), the great astrophysicist Subrahmanyan Chandrasekhar put it (Fermi, *Collected Papers*, v. II, p. 923): "Fermi was instantly able to bring to bear, on any physical problem with which he was confronted, his profound and deep feeling for physical laws: the result invariably was that the problem was illuminated and clarified. Thus, the motions of interstellar clouds with magnetic lines of force threading through them reminded him of the vibrations of a crystal lattice; and the gravitational instability of a spiral arm of a galaxy suggested to him the instability of a plasma and led him to consider its stabilization by an axial magnetic field".

One can recognize here the way a thematic undercurrent guides some scientists' understanding of how Nature works at the fundamental level. For Einstein, the basic assumption was again and again that entirely different phenomena are aspects of one grand unity. Niels Bohr, often quoting a saying of Friedrich Schiller, thought that truths may be found "in the abyss" between contrary theories. Fermi thought of a phenomenon as exhibiting one of only a relatively small number of different basic scenarios of which Nature availed herself; of these, Fermi kept a catalogue throughout his life.

The physics of the future

To learn new skills, members of Fermi's group, already international in outlook, traveled to laboratories abroad: Rasetti to Millikan in Pasadena and later to Lise Meitner in Berlin. Segrè went to Pieter Zeemann in Amsterdam and Otto Stern in Hamburg. By transfer from other universities, more students chose to join the Rome group, including Eugenio Fubini, Ugo Fano, and Bruno Pontecorvo. By the early 1930s, they were attracted by Fermi's work, for example on the quantum theory of radiation, on statistics, above all on the theory of beta decay, the paper first published in 1933, after having been rejected by the editor of the journal *Nature* as "containing abstract speculations too remote from physical reality". Also, a good number of young physicists came from abroad, to visit and sometimes to stay for longer periods and collaborate. They included Hans Bethe, George Placzek, Felix Bloch, Rudolf Peierls, Fritz London, Edward Teller, Eugene Feenberg. And before that, there were collaboration with and visits from colleagues at other Italian universities, such as Renato Einaudi from Turin, at Persico's recommendation, but perhaps most frequently from the newly flourishing physics group in Florence under Antonio Garbasso, including Bruno Rossi, Gilberto Bernardini, Giuseppe Occhialini, Enrico Persico, Giulio Racah, and Sergio De Benedetti.

But where was the freshly hatched young Roman group itself heading in physics? Up to 1929, the largest part of their teamwork was still in spectroscopy. Starting then, it became more and more rapidly clear that remarkable changes in physics abroad signalled that the search for a promised land on which to strike gold for Italian physics would have to be reorganized. The historical development of physics itself revealed to the Fermi team what it was that these young men had been preparing themselves for in all those years of wide-ranging study and perfection of various skills. The quantum mechanics of Bohr, Heisenberg, Pauli, Dirac, Schrödinger was taking center stage in the field of theory; and on the experimental side, nuclear physics was being transformed in exciting ways, by the findings of Chadwick, Urey, Davisson and Germer, Carl Anderson, Neddermeyer and Street. The proton-neutron model of the nucleus was becoming plausible. The neutrino hypothesis of Pauli was tantalizing; and E.O. Lawrence's cyclotron was a much envied sensation.

In a speech in September 1929, Corbino showed he had already smelled out that nuclear physics was, in his words, "the true field for physics of the future". So now, the core members of the team which had patiently and often at great personal cost stuck together for years, began to re-educate themselves in a systematic study from late 1931. Amaldi led a special seminar on radioactivity, and the group learned how to build neutron sources, construct a cloud chamber, make Geiger counters. Some additional research funds became available from the Italian National Research Council (CNR). And all this without the group realizing in the least precisely what and when an opportunity would come along for using that new knowledge, to achieve the ultimate desire of the team: to make, at long last, a world-class discovery. It was a curious moment in the history of science: Here was arguably the first modern research team in physics, waiting for the signal exactly how to be put to use for a high purpose.

The first fruits

That did happen, and remarkably soon. It was helped, as so often, by an accidental discovery, published in mid-January 1934, by Irène Curie Joliot and her husband, Frédéric Joliot. They had used alpha particles from polonium, sent into a cloud chamber to bombard aluminium, thereby causing the emission of positrons from the target. Amaldi wrote, "The discovery of artificial radioactivity was due to an accidental observation by Joliot. One day, in January 1934, Joliot noticed that the emission of positrons persisted when the polonium source was taken away". One might add here that earlier, a *non*-discovery of artificial radioactivity took place in E. O. Lawrence's cyclotron laboratory in Berkeley. As Lawrence confessed in his Nobel Prize speech (for 1939, but given in 1951), "Looking back, it is remarkable that we [at Berkeley] managed to avoid the discovery", by neglecting the fact that the Geiger counters kept up their chatter after the 27-inch cyclotron had been turned off.

At any rate, with the finding by the Joliot-Curies, a huge window was suddenly opened on a new landscape of exciting ignorance. Immediately the rush was on to explore this territory, using of course alpha particle sources. But Fermi at once had the crucial intuition that unlike all others, he and his team should use a beam of neutrons instead of alpha particles to produce artificial radioactivity. It seemed to him reasonable to expect that in a neutron beam the lack of charge on such projectiles, much of them emerging at high energies from the radon-beryllium sources now available, would have a great effect on the targets, despite the admittedly still relatively weak sources.

On March 25, 1934, Fermi was able to publish the first results, in the journal of the National Research Council, the *Ricerca Scientifica*. It was the first of ten such papers, sometimes one appearing per week. From the third to the tenth of these publications, the list of authors was always given as follows: E. Amaldi, O[scar] D'Agostino (a young chemist who happened to come back to Rome on a vacation from a fellowship in Paris, but happily was pressed into service), E. Fermi, F. Rasetti, E. Segrè. Note that all core members of the "family" were listed in alphabetical order, and that, perhaps for the first time in the whole physics literature, there were as many as five authors.

As to results, Fermi typically had decided to test all available chemical elements for artificial radioactivity, going methodically up the periodic table. The team divided the labor – getting the targets, monitoring the electric circuitry of the Geiger counters, the chemical analysis, etc. – in a cooperative way. One should note that Fermi's group, from the beginning, generally tended to work together on one project – unlike the operation at, say, Rutherford's Cavendish Laboratory, where different small groups worked on different projects, whose commonality was chiefly that they represented different parts of Rutherford's wide-ranging interests.

The work in Rome was now quite frantic and tedious for some months, and a few mistakes were made. None was later more regretted than the presumed identification of transuranium radioactive products, produced by irradiating thorium and uranium with neutrons. It was the same mistake made by others at the same time, including Hahn and Meitner, and the Joliot-Curies. Altogether, apart from thorium and uranium, sixty elements were irradiated with the fast neutrons by the Roman team, thirty-five provided at least one new radioactive product, and the total of new ones identified, with their respective half lives, came to forty-four.

Here was truly excellent work, the unexpected first fruits after the long wait and preparation. The Fermi group was now widely noted. Since at the time such publications had to be first in the Italian language, I.I. Rabi at Columbia University is said to have advised, "Well, now we all have to learn Italian".

The "miracolous effect" of slow neutrons

But nobody realized the best part: that the team was now standing at the threshold of their truly startling discovery, with its long resonance into science and world history. In fact, at that point, the whole Roman group took a break for the lengthy summer vacation, getting away from hot Rome, like all sensible Romans.

They reassembled at the *Istituto* in the fall of 1934, now joined by a close family friend of Rasetti, Bruno Pontecorvo. But by mid-October things began to go wrong. Their whole experimental activity was upset by a strange inconsistency in the results of irradiation of targets during an attempt to calibrate the degrees of induced radioactivity. The inconsistency in the readings they now obtained turned out to depend on the tables used as support of the equipment. Thus, one table, made of wood, had once been the bearer of those spectroscopes of earlier day; the other, not far away, was a shelf made of stone. When an experiment on inducing radioactivity in a target made of silver was placed on the first of these tables, a markedly greater activity resulted than if the same experiment was tried on the marble support. The group famously christened it the "miracle of the two tables".

To get to the bottom of it, Fermi initiated a systematic observation, starting October 18, 1934. He reasoned that perhaps the lead housing around the target affected the neutrons reaching the target in those two cases, and he observed that the interposition of a block of lead changed the activation somewhat. Thus, Fermi decided to insert a lead filter, a wedge of varying thickness, into the neutron beam. As Segrè put it later, on that day, October 22, "Persico and Bruno Rossi [were] there on a visit, kibitzing". The astonishing account of the events on the morning of that crucial day was later told by Fermi to Subrahmanyan Chandrasekhar, who published his report (Fermi, *Collected Papers*, v. II, p. 927). The essential last paragraph was repeated verbatim by others close to Fermi, such as Edoardo Amaldi and Emilio Segrè[1]. Although some here may remember the account well, allow me to quote it, because it needs more analysis.

"I will tell you how I came to make the discovery which I suppose is the most important one I have made. 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 exam-

[1] A detailed version of the events was given by Laura Fermi in her book, *Atoms in the Family* (1954, p. 98). It differs in some details, but comes to the same conclusion.

ine 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".

The result was immediately obvious: a great increase in the radioactivity induced in the target, even if the target and the paraffin filter were placed on the marble shelf. As Segrè recalled, at about noon "everybody was summoned to watch the miraculous effect of the filtration by paraffin". And in a "still extremely puzzled" state, "we went home for lunch and our usual siesta". "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" (in Segrè, *Enrico Fermi, Physicist*, p. 80).

As Amaldi reported, it was only later that the so-called 1/v law was determined, i.e., that the capture cross-section (σ_c) was inversely proportional to the speed of the neutrons at low velocities. But on that day Fermi realized that the hydrogen nuclei in the wooden table had greatly slowed some of the incident neutrons, being of about the same mass, and then had scattered them to the target, whereas the heavy nuclei in the marble in the other table could do this only very poorly. Repeating the experiment quickly by using water instead of paraffin helped prove Fermi's initial hypothesis. Moreover, the enhanced radioactivity was also observed for copper, iodine, and aluminium.

That evening, in Amaldi's home, they all met to prepare a short report of their work for the *Ricerca Scientifica*, with Fermi dictating, Segrè writing, Rasetti, Amaldi and Pontecorvo excitedly adding their comments. Amaldi's wife Ginestra, who was working with that journal, saw to it that the article would be published within two weeks, with preprints – another novelty – becoming available within days, and sent out to some forty of the most prominent researchers in the field. Soon the whole profession knew that the Roman group had reached a new frontier. It was a climactic moment for Fermi's team in Rome, and – as it turned out – for the world on its path into the uncertain future.

Intuitive intelligence

But in that story, there is a haunting puzzle. Fermi was the most rational of scientists; yet, not by accident or chance but by determined action, he had placed the crucial piece of paraffin in front of the neutron source, "with no advanced warning, no conscious, prior reasoning". Why did he, the least impulsive physicist, do this?[2] Part of the answer, it seems to me, is that that there exists a kind of intuitive intelligence which sometimes secretly guides certain brilliant minds in the early phases of their research. That concept is now rarely mentioned, least of all by scientists themselves, who tend to shy away from such a difficult-to-defend idea. But it had figured prominently in the writings of philosophers such as Baruch Spinoza, Immanuel Kant, and Henri Bergson. Arthur Schopenhauer, widely read at the time, even held that intuition is the hallmark of genius.

Einstein referred to it as "Fingerspitzengefühl", a sense or a feeling at the tips of one's fingers, and specifically referred to intuition as necessary, e.g., in his essay, *Motive des Forschens* (1918): "There is no logical path to the elementary laws, but only intuition, resting on empathy gained by experience". Henri Poincaré, in *Science and Method*, Book II, Chapter 2) noted that it is by logic that we prove, but by intuition that we discover. The scientist and philosopher Michael Polanyi wrote at length about what he called the scientist's "tacit knowledge", largely resulting from one's lengthy immersion or "in-dwelling" in the subject of research. He summarized the result in the simple sentence: "We know more than we can tell". Much earlier, Hans Christian Oersted provided for this rare gift the happy term "anticipatory consonance with nature". And a chemist and great writer, Primo Levi, wrote, "I know with my hands and my nose, with my senses" (in *The Voice of Memory,* p. 8).

So one might well understand that when Fermi's hand was reaching for the "odd piece of paraffin" instead of the lead wedge, he was guided by a speculation below the level of consciousness at that moment, but a result of an intimate knowledge of neutron physics, one built up during years of intense study, discussions and experimentation with neutronics. As Dr. Alberto De

[2] One historian of science was so astonished by the report of an action so uncharacteristic of Fermi that he even doubted the account reported by Chandrasekhar. But that idea must surely be dismissed. Chandrasekhar, who was one of the most distinguished and precision-minded scientists, even felt it necessary to start his report with a footnote: "His [Fermi's] account made so great an impression on me that though this is written from memory, I believe that it is very nearly a true verbatim account". Moreover, he published it (in 1965) when those who had "kibbitzed" that morning, on October 22, 1934 – Rossi and Persico – were still alive; and as mentioned, Fermi's co-workers, Segrè and Amaldi, endorsed Chandrasekhar's account as given above, quoting it in full.

Gregorio[3] has shown, Fermi may well have read publications in 1932-33 in which slow neutrons and effects of hydrogenous substances on neutrons were discussed, and he also had participated in the 1933 Solvay Conference which included discussions of these topics. But it is significant that nobody other than Fermi and his group entered into the crash program producing artificial radioactivity, first with fast neutrons, and then with slow ones, starting that morning when Fermi was able to draw on resources that had by then slipped below the conscious level.

In fact, Chandrasekhar's account, given above, is part of a longer piece of his, which reveals that the whole discussion with Fermi had begun precisely with a consideration of the role of "subconscious" ideas in creative work in science: Chandrasekhar wrote (p. 926, *Fermi Papers*): "I described to Fermi [Jacques] Hadamard's thesis regarding the psychology of invention in mathematics, namely, how one must distinguish four different stages: a period of conscious effort, a period of 'incubation' when various combinations are made in the subconscious mind, the moment of 'revelation' when the 'right combination' (made in the subconscious) emerges into the conscious, and finally the stage of further conscious effort. I then asked Fermi if the process of discovery in physics had any similarity. Fermi volunteered and said [there followed his account, as given above]".

There are also other accounts of Fermi's ability to dredge up, from hidden resources, answers to questions facing him. Thus, Herbert Anderson recalled that at a crucial moment during the difficult early work in 1939 at Columbia University on the possibility of a chain reaction, "Fermi asked to be left alone for 20 minutes", and emerged with a rough estimate of the effect of resonance absorption by uranium. Anderson reported that the estimate, which proved to be correct, "was largely intuitive. Fermi was never far wrong in such things...", and, what is important for the purpose of the topic of my essay, one can imagine the positive effect such talent had on Fermi's group. Elsewhere Fermi was even credited with helping reactor engineers to obtain a rough estimate of data not yet measured, such as nuclear cross-section. They did it reportedly by watching Fermi closely for an "involuntary twinkle in his eyes" while reciting to him possible cross-section values.

Science historians have struggled to understand the mechanism behind such examples of "anticipatory consonance with nature". It seems to me another case of finding ourselves cast upon the shores of a large island of fruitful ignorance.

---

[3] In a personal communication of an unpublished manuscript, for which I am grateful.

Fermi and the Fascist disaster

I am relieved that it is not part of my assignment to report on the rapidly ensuing disintegration and near destruction of the Roman group, to use Edoardo Amaldi's phrase, as well as those in Florence and elsewhere in Italy at that time. To be sure, in the early days of Fermi's ascent, some scientists had been at least indirectly helped – in hastening appointments and in the availability of some funds – by the cult of the Fascist government to revive the myth of national regeneration, on the model of ancient Rome as a center of Western civilization. Among the sixteen institutions founded after Mussolini's assumption of dictatorship in 1925, two were the new Royal Academy of Italy and the National Council of Research (CNR). Fermi's work had obtained financial support from both, although he himself was by nature and design apolitical, and felt repugnance from the regime's ideology. In turn, the government expressed annoyance with Fermi for his refusing the prestigious chair in physics left by Schrödinger in Zurich, where Fermi, acting as a proxy for Italy's science, would have been highly visible throughout the European continent.

From the mid 1930s on, a whole slew of institutions founded by the Fascist government withered, part of a general growing bleakness and collapse of civilized life. The physicists at Via Panisperna submerged themselves in hard work, hoping to use "physics as soma", on the model of Aldous Huxley's victims in his novel *Brave New World*. All this is movingly described in several essays in Edoardo Amaldi's volume of his selected historical writings – from the effect of the state's lurching toward the Ethiopian Campaign that started in October 1935, to the growing economic and political dependence on Nazi Germany, including the formation of the Rome-Berlin Axis, to Italy's participation in the Civil War in Spain, and to the institution of racist laws in Italy in July 1938, roughly along the German model. There followed the emigration of most of the group's members and, during World War II, the arrest, deportation and death of persons close to the Roman group, such as Laura Fermi's father, Augusto Capon. Among the many other such victims were several relatives of Segrè.

There had been so few years between launching upon the recovery of Italy's place in world-class physics in the mid-20s, and seeing it descend again by the end of the 30s for many years in horror and flames. That whole arc of the brave rise of Fermi and his group, their extraordinary achievements, and then the ghastly dissolution forced on them, is a symbol of the best and the worst of that tragic twentieth century.

REFERENCES

AMALDI E., *20th-Century Physics. Essays and Recollections: A Selection of Historical Writings by Edoardo Amaldi* (Singapore: World Scientific, 1998).

BUCK B., *Italian Physicists and Their Institutions, 1861-1911* (PhD dissertation, Harvard University, 1980; Supervisor, G. Holton).

FERMI E., *Collected Papers,* v. I (Chicago and London: University of Chicago Press, 1962).

FERMI E., *Collected Papers*, v. II (Chicago and London: University of Chicago Press, 1965).

FERMI L., *Atoms in the Family* (New York: American Institute of Physics, 1987).

HOLTON G., *The Scientific Imagination*, rev. ed. (Cambridge, MA: Harvard University Press), 1998. See Ch. 5, "Fermi's Group and the Recapture of Italy's Place in Physics". Translated as *L'Immaginazione Scientifica* (Torino, Italy: Giulio Einaudi Editore, 1983), "Il gruppo di Fermi e la riconquista da parte dell'Italia del suo posto nella fisica moderna", pp. 351-402.

HOLTON G., ET AL., *The World of Enrico Fermi*, film.

PONTECORVO B., *Fermi e la fisica moderna* (Rome: Editori Riuniti, 1972).

SEGRÈ E., *A Mind Always in Motion: The Autobiography of Emilio Segrè* (Berkeley, CA: University of California Press, 1993). Translated as *Autobiografia di un fisico* (Bologna: Il Mulino, 1995).

SEGRÈ E., *Enrico Fermi, Physicist* (Chicago and London: University of Chicago Press, 1970).

WEINER C., ed., Storia della fisica del XX secolo. *Rendiconti della Scuola Internazionale di Fisica "Enrico Fermi"*, Varenna, (New York: Academic Press, 1977), especially the essay by E. Amaldi.

Gerald Holton
Holton is Mallinckrodt Professor of Physics at Harvard University. Among his books (several of which have been issued in Italian translations), are Thematic Origins of Scientific Thought: Kepler to Einstein; Science and Anti-Science; The Advancement of Science and its Burdens; Einstein, History and Other Passions; and Project Physics. As part of his historical research on the work of Enrico Fermi, he co-produced a biographical film, "The World of Enrico Fermi". He is on the Editorial Committee of the Collected Papers of Albert Einstein and of learned journals, including Physis and Nuncius. Among the honors he has received are the George Sarton Medal, the R. A. Millikan and H. C. Oersted Medals, and election to such offices as President of the History of Science Society, Vice President of the Academie Internationale d'Histoire des Sciences, and to other Academies in Europe and the United States.

Fabio Sebastiani, Francesco Cordella

Fermi toward Quantum Statistics (1923-1925)

There are some uncertainties about the influences that might have led Fermi to the formulation of quantum statistics. Like already remarked by F. Rasetti, little is known about the circumstances that led the great Italian physicist to one of his most important theoretical contributions. It can be said that the "preparatory role" of two works by Fermi (the one about Stern's method for the calculation of the entropy constant of a perfect gas and the other on the quantization of systems containing identical particles) is unanimously recognized. Based on a recent historical reconstruction, it will be shown how some circumstances pushed Fermi to tackle these problems. It seems that Göttingen's environment had a strong influence on his work. We will also try to specify the time of the quantum statistics formulation.

Fermi verso la statistica quantica (1923-1925)

Sussistono alcune incertezze circa le influenze che possono aver indirizzato il percorso di Fermi verso la formulazione della statistica quantica. Come già sottolineato da F. Rasetti, poco è noto delle circostanze che hanno condotto Fermi a uno dei suoi più importanti contributi teorici. È comunque unanimemente riconosciuto il ruolo preparatorio di due lavori del giovane fisico romano, riguardanti il metodo di Stern per il calcolo della costante dell'entropia di un gas perfetto e la quantizzazione di sistemi contenenti particelle identiche. In base ad una recente ricostruzione storica, saranno mostrate le circostanze che spinsero Fermi ad affontare questi problemi. L'ambiente scientifico di Gottinga sembra aver avuto una forte influenza sui due suoi lavori preparatori. Si cerca inoltre di precisare la collocazione temporale della formulazione della statistica quantica.

The anomalous case of Enrico Fermi, in comparison with the international scientific context of the early twenties, is evident since his first works. The young scientist Fermi behaved as an outsider fairly proud of his independence, mainly because of his strong personality. The Italian scientific environment too forced him to a self-teaching method and a cut off position. Nevertheless, it must be said that Fermi never much complained about it.

In particular, the genesis of his eponymous quantum statistics gives an interesting trail to outline, at the same time, his personality and the way, generally not well known, he followed to reach a result that put a final seal on his fame at an international level. The high level of the about thirty works from 1921 to 1926 shows a precocious technical talent. Moreover, the existence of a defined scientific style and a tendency to concentrate himself on personal research programs was already clear. Furthermore, the young Fermi had great competence in various fields of physics and an uncommon interest for both the theoretical and experimental aspects.

The theoretician Fermi, although confident in old quantum physics, was not very interested in the formulation of new principles. He was rather in search of applications that descended from those principles. That explained his pragmatic use of mathematics of which, nevertheless, he had a total mastery.

The roots of this “modus operandi” should also be searched in the pre-university years, when all the main features of his scientific personality were already emerging.[1] Anyway, it was only after his two stays in Germany (Göttingen) and the Netherlands (Leyden) that Fermi started to become aware of his value and what kind of physics was produced in the ‘sacred’ places. These two stays abroad were useful to Fermi also for the many prestigious people he met there (Born, Ehrenfest, Goudsmit, Heisenberg, Kronig, Uhlenbeck, etc.). Moreover, unlike what asserted up to now (mainly by Segrè), this work will point out the scientific importance of the influence of the German stay on Fermi.

Always devoted to a major clarification and simplification of subjects, he shuns any possible technical virtuosity and not strictly necessary hypotheses. This is evident especially in the formulation of his quantum statistics.

The key point of Fermi’s discovery (typical of his way of proceeding) lies in the bold application of Pauli’s principle to the quantization of the perfect gas, that was at the time a fairly distant problem. Fermi’s approach, here

[1] Cmp. Sassi, Sebastiani (1999).

defined as 'analogy method', is present also in other significant theoretical contributions. These results are due to Fermi's great ability to profitably put in relation concepts that differ greatly.

However, Fermi's article didn't provoke any reaction, at least until Dirac came to the same results. The silence from the scientific community was mainly justified by Dirac's larger fame and powerful use of the new quantum concepts, that allowed in-depth explanations of the subject studied.[2] In his article on the new quantum statistics, Fermi also introduces an harmonic potential to contain atoms of the gas. That could be considered as an unnecessary complication, but there are also some interesting motivations for this unusual choice. In any case, later on, the harmonic potential was abandoned even by Fermi.

The years before Fermi's quantum statistics

The beginning of the twenties, as it is known, was a crucial period for physics. S. Goudsmit humorously described the physics community of the time: "The concept of the 'good old days' does not apply […] In the 1920's, by comparison, we lived in a small village with its little feuds, a Peyton Place without sex".[3]

The "Peyton Place without sex" it's a nice metaphor to illustrate the anxiety of those managing quantum physics, that were getting few gratifications and many disappointments. The few that, like Fermi in Italy, were devoting themselves to the new physics, were considered to all intents and purposes like "sinners".[4]

From January to August 1923, he stayed eight months in Germany, at Göttingen, with Max Born, without much advantage, according to Segrè.[5] Later, from September to December 1924, Fermi spent four months in Holland, at Leyden, with Paul Ehrenfest. This shorter period of study was, as for human relations, much more satisfactory. The two stays abroad had a

---

[2] "[…] playing with symmetrical and anti-symmetrical functions, Dirac derived both quantum statistics in one stroke. […] Fermi had in fact derived 'only' his statistics", cmp. BELLONI (1994), p. 107.

[3] Cmp. GOUDSMIT (1976).

[4] Actually, Fermi was almost the sole 'sinner'. Rasetti reports that: "By 1920 or even '22, quantum theory in Italy was essentially confined in Fermi's mind and there was very little outside. For instance, I first heard of the quantum theory and Planck's constant from Fermi", cmp. A.H.Q.P. transcript of a tape recorded interview by T. S. Kuhn with F. Rasetti and E. Persico, 8 April 1963, p. 8.

[5] Cmp. F.N.M., p. XXVI-XXVII.

great influence on Fermi. They can be considered as his first direct encounter with "modern" physicists and with two places extremely favourable to the scientific activity.[6]

It was also Fermi's first opportunity to get to know the culture, politics and social organisation of two countries very different from Italy's at the time: "[...] he said that he found everything so superior to what he was used to in Italy in every respect. The country is so well organised. They like good functioning in everything".[7]

As it emerges from his correspondence, Fermi never expressed his opinion on matters that did not strictly concerned physics; it was an unpleasant peculiarity of Fermi. Certainly it was a period in which events of some consideration did not lack both in Italy and in Germany![8]

During approximately the three years from the end of university and the formulation of his statistics theory, Fermi's work can be divided into four phases.

Göttingen (January 1923-August 1923)

In these eight months, Fermi went to Göttingen on a scholarship assigned him on 4 November 1922 by the Ministry of Education.

The Weimar Republic was undergoing a very bad crisis and although Fermi personally witnessed its economical disaster, still greatly admired the country's organising ability (cmp. the quoted recollections of Rasetti).[9] However, there is no trace of these events in Fermi's letters from Germany. Even more surprising, there isn't any comment on Göttingen's scientific environment, except for the quite generic statement: "The professors, especially Born, are nice people and they don't give it as if it were the Keys of the Kingdom".[10] Heisenberg, in an interview with T. S. Kuhn of 1963, remembered then that he had met Fermi at that time and that he hadn't much liked him:

---

6 Until then he was, to all intents and purposes, an autodidact; cmp. Sassi, Sebastiani (1999).

7 Cmp. A.H.Q.P. transcript of a tape recorded interview by T. S. Kuhn with F. Rasetti and E. Persico, 8 April 1963, p. 12.

8 Fermi had this attitude all his life.

9 For instance, in November 1923, one dollar was worth some milliards of marks and the exchange rate with Italian money was especially favourable, so that Fermi was able to buy a brand new bicycle, cmp. L. Fermi (1954). The political situation was also tragical and in January 1923, when Fermi arrived at Göttingen, there was the Franco-Belgian occupation of the near Ruhr zone (about 100 km west of Göttingen) as pledge for the German war debts.

10 "I professori, specialmente Born, sono persone simpatiche e non la fanno cader troppo dall'alto", cmp. stamped postcard to Persico of 30.1.1923; a photostat is in the Amal. arch. ($1E_{bis}$/n.c.).

> "I had some discussions with Fermi, but it must have been that Fermi was not – I would say – in a good period of his young life. He may have had personal problems; I don't know what the matter was. He had always been a bit shy and kept by himself, and it was not too easy to get really close to him. Still, I liked him as a rather different type of physicist. […] in Göttingen we never had a real conversation; we met in the seminar, and occasionally on the streets, but not in that way that we really got in touch".[11]

The lack of agreement with the German scientific environment is testified also by other sources.[12] Furthermore, at Göttingen Fermi could be suffering from the so-called 'swot syndrome'. Fermi, accustomed to be outstanding and considered a very talented youth in Italy, when suddenly in an 'inter pares' environment, could have had psychological problems. Anyhow, in this period Born was holding a seminar for a few people, among whom there were Fermi and Heisenberg.[13] Born seemed to Heisenberg: "as an extremely good mathematician who […] had not so much feeling about how the things in atomic physics were". But Fermi, "disliked these mathematical subtleties, proof of convergence and such […] I mean, Fermi felt, 'That's not physics'. [...] Fermi was not so pleased with [this seminar of Born]".[14]

Göttingen's interest for the new physics was partly due to the great mathematician David Hilbert who in 1912, as Tagliaferri relates,[15] decided that 'physics was too difficult for physicists'. So he asked Sommerfeld to send him some youths of great merit. Among them there were Heisenberg and Pauli. In a letter to Einstein dated April 7 1923, about three months after Fermi's

[11] Cmp. A.H.Q.P. transcript of a tape recorded interview by T. S. Kuhn with W. Heisenberg, 15 February 1963, p. 14 (We are grateful to D. Cassidy for this information).

[12] "According to Samuel Allison, many years later Fermi remembered that period with some bitterness: the professors of Göttingen considered themselves, in Fermi's opinion, as omniscients and they didn't at all care to encourage him", "Secondo Samuel Allison, molti anni dopo Fermi ricordava quel periodo con una certa amarezza: i professori di Gottinga si consideravano, secondo Fermi, onniscienti e non si preoccupavano certo di incoraggiarlo", cmp. PONTECORVO (1993), pp. 26-27. When Fermi came back to Italy and met G. Uhlenbeck in the fall of 1923, his disappointment was even more evident: "[Uhlenbeck] found he'd just come back from Germany, completely discouraged. He'd been to Göttingen for a term and had been given the works - on the lines of 'This guy can't know anything, he's small fry, he's never studied any place where it's worth studying.' The man was so thoroughly dejected that he was planning to give up physics. Uhlenbeck advised: 'Don't do that before you've had a talk with Ehrenfest. Go and see him'.", cmp. GOUDSMIT (1972). However, the idea of a Fermi intentioned to give up physics is surely exaggerated.

[13] Those present at the seminar were: Heisenberg, Jordan, Fermi, probably Hund, and at most another couple of persons.

[14] Cmp. the mentioned interview with Heisenberg, p. 6.

[15] Cmp. TAGLIAFERRI (1985), note 92, p. 336.

arrival at Göttingen, Born wrote about Heisenberg'qualities and their regret for the state of quantum physics,[16] also mentioned in several previous Heisenberg's letters to Pauli.[17]

If these were Born and Heisenberg's worries, the following assertion by Segrè, concerning the beginning of 1923 at Göttingen, sounds quite unlikely: "[…] an extremely important incubation period preluding the flowering of the new quantum mechanics that just in Göttingen had in this period one of the most important centre".[18] The impression is, instead, of a widespread disappointment and, as Tagliaferri objects: "[...] this is an unjustified inference: in 1923 the new mechanics could be a desire but it wasn't known – either in Göttingen or elsewhere – which way to follow to achieve it".[19]

Fermi was not discouraged and worked hard, also not to disappoint friends' expectations in his sojourn in Göttingen.[20]

In February 1923, only one month after his arrival, Fermi prepared a work

---

16 "[…] I worked a lot and kept a good number of pupils busy. However, the problems faced ar small: in spite of all efforts, I'm not able to penetrate the big quantum enigmas", "[…] ho lavorato parecchio e ho fatto anche lavorare un buon numero di allievi. Ma i problemi affrontati sono soltanto piccoli problemi: nonostante ogni sforzo, non riesco a penetrare i grandi enigmi dei quanti", cmp. Born (1973^2), p. 89. When Fermi arrived at Göttingen, the two most renowned Born's collaborators were taking turns. In fact Pauli, Born's assistant until the spring of 1922, went to Copenhagen as Bohr's assistant from October 1922 to September 1923, and Heisenberg was to replace him, cmp. for example Tagliaferri (1985), p. 316.

17 As a matter of fact, on 19 February 1923 writing to Pauli about the work conducted with Born, Heisenberg asserts: "[...] to summarise it all: 'it's a torture'", "[...] um alles kurz zusammenzufassen: 'es ist ein Jammer'", cmp. Pauli (1979), p. 80. Two days after, always writing to Pauli, Heisenberg ironically states : "[…] a theory can always still be wrong, if it produces something right, but can *never* be right if it produces something wrong", "Eine Theorie kann immer noch falsch sein, wenn sie etwas richtiges ergibt, aber sie kann *nie* richtig sein, wenn sie etwas falsches ergibt", cmp. Pauli (1979), p. 82. And, on 26 March of the same year, Heisenberg closed a letter to Pauli saying: "[...] we [Heisenberg and Born] don't want to get uselessly tired. Basically we are convinced that all helium models proposed until now are false as the whole atomic physics. We hope that this wonderful spring [...] would change all, but really all", "Aber wir wollen uns nicht unnötig streiten. Im Grunde sind wir beide der Überzeugung, daß alle bisherigen He-Modelle ebenso falsch sind, wie die ganze Atomphysik. Hoffen wir, daß der jetzige prachtvolle Frühling [...] alles, alles wendet", cmp. Pauli (1979), p. 86.

18 "[…] un periodo estremamente importante di incubazione preludente allo sboccio della nuova meccanica quantistica, che appunto in Göttingen aveva in quegli anni uno dei centri maggiori", cmp. F.N.M., p. XXVI.

19 "[…] quest'illazione è ingiustificata: nel 1923 la nuova meccanica poteva essere un'aspirazione, ma non si sapeva – né a Gottinga né altrove – che strada prendere per realizzarla", cmp. Tagliaferri (1985), p. 351, note 4.

20 Persico, besides waiting for scientific news from what he calls "quantum land" ("paese dei quanti", cmp. Amal. arch. ($1E_{bis}$/n.c)), when Fermi is about to return in Italy, writes to him: "And we hope that you'll bring us some Göttingen's air bottles to renew our scientific environment and guide us on a serious and organised work", "E speriamo che ci porterai alcune bombole di aria gottinghese, per rinnovare i nostri ambienti scientifici e guidarci ad un lavoro organizzato e serio", cmp. copy of the 23.06.1923 stamped postcard, ibid.

that would be later published on the Nuovo Cimento: *The adiabatic principle and systems that don't admit angular coordinates*[21] (F.N.M.12).

After nearly two months, namely in April 1923, Fermi completed a second work that he considered important. Following the rules he had given to himself,[22] he published it on the Physikalische Zeitschrift: *Proof that a normal mechanical system is usually quasi-ergodish*[23] (F.N.M.11a).

As Segrè stated in the F.N.M. introduction: "[...] Prof. Ehrenfest, who had delved deeply into the foundations of statistical mechanics, was impressed by the paper. He gave to Uhlenbeck, who was going to Rome for a while, a letter for Fermi with a number of questions and in this way Uhlenbeck met Fermi for the first time in the fall of 1924 [sic]". Actually things went differently, as the meeting between the two physicists took place one year before, namely in the autumn of 1923.[24] It's worth noting that Ehrenfest was struck by this article which he later quoted in an analytic mechanics article arrived at the Zeitschrift für Physik on 2 October 1923.[25]

The third work published by Fermi at Göttingen is: *Some theorems of analytical mechanics that are important for quantum theory*[26] (F.N.M.13) and dates back to April 1923. The same date of the previous article.[27]

On 16 April 1923 Fermi wrote to Persico: "I'm working hard on a border work among celestial mechanics, statistical mechanics and quantum theory. But I can't foresee where I'll be driven at".[28] In addition, on the 21st of April

---

21 *Il principio delle adiabatiche ed i sistemi che non ammettono coordinate angolari*, Nuovo Cimento, 25 (1923), 171-175.

22 Fermi, well aware of the Italian scientific isolation, published his most important works on foreign reviews, cmp. SEGRÈ (1970), p. 35.

23 *Beweis, dass ein mechanisches normalsystem im allgemeinen quasi-ergodisch ist*, Physikalische Zeitschrift, 24 (1923), 261-265.

24 The spreading of the error, that involved Segrè and others, presumably originated with the book by Fermi's wife; cmp. L. Fermi (1954). In the fall of 1924 Fermi was in Leyden, Holland. After all, Uhlenbeck refers that: "[...] in 1923 I also met and became good friends with Enrico Fermi", cmp. UHLENBECK (1976), p. 45.

25 Cmp. EHRENFEST (1923). It seems that Ehrenfest used with Uhlenbeck the following expression: "[...] 'I've seen an article by a young fellow and it looks pretty good; you should look him up", cmp. GOUDSMIT (1972), p. 83.

26 *Alcuni teoremi di meccanica analitica importanti per la teoria dei quanti*, Nuovo Cimento, 25 (1923), 271-285.

27 There arose the issue of the time order of the works, cleared up in the fifth note: "[…] the writer has recently demonstrated that normal mechanical systems are generally quasi ergodish", "[...] chi scrive ha recentemente dimostrato che i sistemi meccanici normali sono in generale quasi-ergodici", cmp. F.N.M., p. 93.

28 "Io sto molto lavorando ad un lavoro di confine fra la meccanica celeste, la meccanica statistica e la teoria quantistica. Ma non posso ancora prevedere dove andrò a sbattere", cmp. stamped postcard to Persico of 16.04.1923. A photostat is conserved by AMAL. ARCH. ($1E_{bis}$/n.c.).

he specified to his friend: "[…] I have three rather important publications ready […]".[29]

Rome (September 1923-August 1924)

In September 1923 Fermi came back home from Germany. On his arrival he found a country grieved by fascist violence. It was more then ever necessary for Fermi to find a job. With Corbino's help, he was assigned with teaching mathematics to chemistry students for the academical year 1923-24 in Rome.[30]

In the autumn of 1923 Fermi met Uhlenbeck and, as already mentioned, the latter was deeply impressed by Fermi's disappointment about his stay in Göttingen.[31] On 29 October 1923 the board of examiners assigned the fellowship to Fermi for the second consecutive year.[32] However, since Fermi had already taken advantage of one fellowship: "[…] it would not be possible, as laid down in article 157 of the current university regulations, to assign it to him a second time".[33] In spite of this the board of examiners, on account of "[…] the exceptional value of dr. Fermi"[34] in a departure from regulations, expressed itself favourably on a second fellowship assignment. As a matter of fact, Fermi remained in Italy until September 1924, when he went to Leyden on a fellowship grant by the Rockefeller foundation.

---

29 Cmp. Segrè (1970), p. 204. Probably the work Fermi refers to in the letter of the 16th it's inclusive of those mentioned in the letter of the 21st of April.

30 Cmp. Segrè (1970), p. 34.

31 Perhaps it's worth noting that Uhlenbeck's recollections could have been distorted, and perhaps 'projected' on Fermi, by the identity crisis the Dutch physicist was going through. In fact, when in Rome, he was devoting himself to a career of professional historian: "[...] still, even his [Fermi's] influence did not turn my back to physics. I suppose I went through what nowadays is called an identity crisis. Anyway, when I came back in June 1925, I thought that my real interest was in the study of cultural history, and that perhaps I should forget about physics", cmp. Uhlenbeck (1976), p. 45.

32 As in the previous year, Fermi in 1923-24, competed both for a scholarship abroad and for an internal one. The board of examiners stated: "Mighty and fertile is the activity of this young candidate of absolutely exceptional value and the Committee expresses the vow that to him will be given the possibility to widen the field of his knowledge in the interest of the Italian science. […] The Committee therefore, declares Fermi Enrico winner of the scholarship abroad", "Poderosa e feconda è l'attività di questo giovane concorrente di valore assolutamente eccezionale e la Commissione formula il voto che a lui sia dato modo di allargare sempre più il campo delle sue conoscenze nell'interesse della scienza italiana. […] La Commissione pertanto dichiara vincitore dell'assegno di perfezionamento all'estero il dott. Fermi Enrico", cmp. "Bollettino Ufficiale, Atti di Amministrazione", Ministero della Pubblica Istruzione, 27 March (1924), year 51, no.13, pp. 714-719.

33 "[…] non sarà possibile, a norma dell'articolo 157 del vigente regolamento generale universitario, assegnargliela una seconda volta"

34 "[…] valore eccezionale del dott. Fermi".

Fermi's didactical activity did not affect his scientific production. In fact, in December 1923 - January 1924 he prepared three very important works (F.N.M.16, F.N.M. 19 and F.N.M.17a). Two (F.N.M. 16 e 19) are fundamental to understand the evolution towards the new quantum statistics and will be considered later. The third can be defined his first article on quantum[35] and was presented on 16 December 1923 by Corbino at the Lincei, as a short essay titled: *On the probability of quantum states.*[36] In this work Fermi deals originally with a delicate quantum problem that later attracted major attention.[37] The work, that Fermi considered quite important, appeared also in a German version on the Zeitschrift für Physik (F.N.M.17b)[38] and was quoted by Planck in June 1924.[39] The German version has a 'post quem' limit, defined in February 1924;[40] in this version Fermi thanks Born for his advice concerning an imprecision in the previous Italian version. This is indirect evidence of a corrispondence between Born and Fermi, immediately after his return from Germany. In the Amaldi archive is kept the typewritten version that Fermi sent to the editor of the Zeitschrift für Physik. Above the title a note says: "Corrections to Mr. Prof. Dr. M. Born, Göttingen, Planckstrasse 21".[41] A few months later Fermi made use[42] of the results obtained to deal with some astrophysical topics that M. N. Saha had proposed in 1921.

In the summer of 1924 the political situation in Italy was very similar to that of summer 1922. The Matteotti murder on 10 June 1924 shaked the fascist regime, saved by the indifference of the Italian democratic forces. Thanks to an unpublished letter[43] it's possible to infer that Fermi, already before the summer vacations of 1924 he spent at Moena on the Dolomites, started to draw up what was later called the 'virtual quantum method'.[44] This

[35] The work on Richardson's statistical theory still constituted after all a classical attempt to study the photoelectric effect. That was a problem more simply and effectively tackled with the introduction of light quanta as Einstein did in 1905, even if the scientific community refused such an innovative approach.

[36] *Sulla probabilità degli stati quantici*, Rendiconti Lincei, 32 (1923), 493-495.

[37] In BRILLOUIN (1930), find an in-depth comparison of different treatments on the subject.

[38] *Über die Wahrscheinlichkeit der Quantenzustände*, Zeitschrift für Physik, 26 (1924), 54-56.

[39] Cmp. PLANCK (1924).

[40] That's a consequence of the fact that in F.N.M.20 (of February 1924), Fermi still uses the results he found in F.N.M.17a and not those, slightly different, he reached in F.N.M.17b.

[41] "Korrektur an Hrrn. Prof. Dr. M. Born, Göttingen, Planckstrasse 21", cmp. AMAL. ARCH. (446/n.c.). That is a proof of Born's direct interest in Fermi. Note the odd coincidence of street names!

[42] Cmp. Nuovo Cimento, 1 (1924), 153-158.

[43] Letter to Persico dated Varese 19.VIII.'24, cmp. AMAL. ARCH. ($1E_{bis}$/n.c.).

[44] It's important to note that there are many other denominations for the virtual quantum method: collision theory, impact theory, equivalent photon method, Weizsäcker-Williams' method.

article appeared on the Zeitschrift für Physik as: *On the collision theory between atoms and electrical charged particles*[45] (F.N.M.23b). This work can be considered as one of the most significant Fermi's contributions before the discovery of quantum statistics. It is based on a simple, efficient idea that uses analogy as a scientific method: utilize consolidated results in a specific area of physics to solve an apparently distant problem.[46]

Leyden (September 1924-December 1924)

From September to December 1924, Fermi stayed at Leyden on another *fellowship grant* by the International Education Board, founded by J. D. Rockefeller. V. Volterra, with the precious help of H. A. Lorentz and W. Rose (President of the Foundation), did his best for Fermi's stay in Holland. In two letters, Rose showed his interest for Fermi's case.[47]

From the correspondence with Persico it's possible to reconstruct Fermi's journey,[48] arriving at Leyden, by sea, on 12 September 1924. This second period of study abroad was, as for human relations, much more congenial to Fermi. This is evident from a letter dated 27 October 1924 to his sister Maria.[49] In Leyden Fermi made acquaintance, besides Ehrenfest, Einstein, Keesom, Lorentz, with some young physicists as G. Dicke, S. Goudsmit, R. de L. Kronig, J. Tinbergen.

---

[45] *Über die Theorie des Stosses zwischen Atomen und elektrisch geladenen Teilchen*, Zeitschrift für Physik, 29 (1924), 315-327. An Italian version appeared also on the Nuovo Cimento (F.N.M.23a): *Sulla teoria dell'urto tra atomi e corpuscoli elettrici*, Nuovo Cimento, 2 (1925), 143-158.

[46] The application of this method, here called 'analogy method', will be the keystone for the formulation of Fermi's quantum statistic.

[47] Cmp. Volt. Arch. (20/518). It results that Fermi was given a $350 check for a period of three months at Leyden. Initially Fermi should stay six months (this reduction of period was a rule except for the foundation). We are grateful to M. De Maria for this information.

[48] Cmp. Segrè (1970), pp. 205, 206. The correspondence recently emerged shows how Fermi, on 26 August 1924, was still in Moena on the Dolomites. Persico sent him the passport he needed for the Netherlands.

[49] "Leyden 27.10.1924.[…] I always feel good and, amongst big and small, I make a discovery every twenty days on the average […] everybody holds me in deep respect […] Einstein left the past week making me enthusiastic sympathy declarations […] Among my new acquaintances: prof. Ehrenfest is a very nice person, even though he wouldn't look too bad in a second-hand clothing shop in the ghetto; he takes very good care of the school and his pupils, and he has a special quality to get hold of those from whom one can expect good results for the future. I then met naturally many youngs […]", "Leyden 27.10.1924.[…] mi trovo sempre bene e, tra grandi e piccole, faccio in media una scoperta ogni venti giorni […] tutti hanno un profondo rispetto di me […]. Einstein è partito la settimana scorsa, facendomi entusiastiche dichiarazioni di simpatia […] Tra i miei nuovi conoscenti: il prof. Ehrenfest è una persona molto simpatica, benché non sfigurerebbe affatto in un negozio di abiti usati in ghetto; si occupa moltissimo della scuola e dei suoi studenti, ed ha una speciale abilità nel saper pescare fin dai primi anni quelli dai quali si possono sperare dei buoni risultati per l'avvenire. Ho conosciuto poi naturalmente molti giovani […]", cmp. E. Vinassa de Regny (1992).

Ehrenfest, together with Lorentz, was the most important person in Leyden and much of his time was devoted to teaching and forming his young pupils (he was really a talent-scout).[50]

In Leyden Fermi became part of an 'horizontal' vision of the teacher-student relation. On the contrary, Göttingen's was structured more 'vertically'.

During the Dutch period (unlike the German), Fermi made many new "discoveries" without yet completing one single article. There were many topics to write about, but writing an article would have needed a time that Fermi, at Leyden, preferred to employ otherwise.

In conclusion it can be said that the major advantage obtained by Fermi during the three months spent at Leyden, wasn't just the widening of his technical knowledge, but mainly the opportunity to collaborate with other physicists. Ehrenfest in fact, as good a teacher as he was, understood very well the psychological frustration of a pupil facing too hazy physical issues.[51]

On the contrary Born, at Göttingen, did not worry over such issues. As David Cassidy states, he had a totally different attitude: "[...] the shy and retiring theorist, plagued by hypochondria [...] the bashful Born seemed overwhelmed by the numbers of students flocking to Göttingen".[52] In a letter to Einstein dated 30 April 1922, Born writes: "[…] it's just about to start the wretched semester, true 'perturbation' of meditative work".[53]

Ehrenfest was instead very happy to work with students. He was a great teacher, thanks to his great knowledge of physics and his extremely collaborative and open attitude. Such a milieu was ideal for Fermi, very pleased to work with people more akin to his scientific style, less mathematical and more physical, like Ehrenfest. Naturally Ehrenfest soon noticed Fermi's tal-

---

50 Some hand-written drafts of Ehrenfest, dating spring 1926 or 1927 following the visit of an English-American group of students, read (note Fermi's name among the "leading students"): "[...] for not being a Dutchman I can freely express my great admiration for Dutch science and Dutch scientists [...] you get the chance of > 1/Million to get the Nobel-Prize in your life-time if you only arrange to be born as a Dutchman (Density for Leiden !) [...] Leading students (Fermi, Breit, Kronig, Gibbson) [...] small numbers, atmosphere of play (Einstein !!!) [...] not hasty, steadiness, perseverance, enormous quiet, pressing energy, big reserves of times for exigency [...] an organism not a machinery, just as science in Holland", cmp. A.H.Q.P. (60/...).

51 In a beautiful letter of January 1924, Ehrenfest talked about the young Americans he had met in America: "[...] The young [here in America] is terribly sound – hygiene, sport, school education as easy as winking until 23 years old, so that not even a soul is tickled by the devil", "[...] Die Jugend ist beängstigend gesund - Hygiene, Sport, strand-einfacher Schuldrill bis 23 sorgt, dass keine Seele vom Teufel gekitzelt wird", cmp. A.H.Q.P. (60/2).

52 Cmp. CASSIDY (1992), p. 138. Quotation reported as in BELLONI (1994), notes 39-40.

53 "[...] sta per ricominciare il benedetto semestre, vera 'perturbazione' del lavoro contemplativo", cmp. BORN (1973[2]), p. 83.

ent and there is evidence that they analyzed together Frenkel's work on electroconduction in metals.[54] Moreover, it would be interesting to verify if Fermi found out about the Bose-Einstein statistics through Ehrenfest or, at any rate, during his stay in Holland.[55]

Florence (January 1925-December 1925)

In December 1924 Fermi, just back from Leyden, went to Florence as lecturer of theoretical mechanics and electromagnetism with an annual appointment.[56] Conscientiously, he had started to prepare the lectures and the course two months before in Leyden. The Florence Physics Institute was located in Arcetri, and was directed by the physicist and Senator A. Garbasso, also Florence's mayor. Rasetti had also been there since November 1922.[57]

In December 1924, in Florence, Fermi completed two works. One had been started at Leyden in the previous October. However, it can be said that both these works and some others that followed, were carried out mostly to increase the number of his publications for university teaching qualifications (obtained on 2 March 1925). Among them we remember an experimental work conducted together with his friend Rasetti, from January 1925 to May 1925.

Thanks to Rasetti, in fact, Fermi got to know some works by Wood, Ellet, and Hanle[58] concerning the weak effects of magnetic fields on the polarisation of mercury resonance radiation. From Fermi-Kronig's correspondence in the A.H.Q.P., it's possible to reconstruct the evolution of Fermi's interest (initially a bit different). The influence of a work by Bohr on the same subject is also evident.[59] It's important to observe that this work represented Fermi's second attempt in experimental physics after several years of theo-

---

[54] Cmp. Ehrenfest-Joffé (1990), letter no. 77 on 24 November 1924: "Here when we inspected Frenkel's work on metals electroconduction [Zeitschrift für Physik, 24 (1924), 214-240] I convinced myself, even more than before, that in this work there are many ingenious ideas but also an enormous confusion. There are not only heavenly ways but also earthly ones. Perhaps me and E. Fermi (Professor in Florence, now here), that appraised the sharpness of this work, we'll find the right way and put a stop to the enormous confusion". We are grateful to R. Pisegna and to T. Jakobson for the translation from Russian.

[55] The second paragraph of Ehrenfest's (1925) contains his first note on the Bose-Einstein statistics.

[56] Cmp. Segrè (1970), p. 37.

[57] Rasetti remembers that, in this period: "[...] we were very close. We practically lived together", cmp. A.H.Q.P. transcript of a tape recorded interview by T.S. Kuhn with F. Rasetti and E. Persico, 8 April 1963, p. 12.

[58] Cmp. F.N.M. for bibliographical indications.

[59] Fermi-Kronig's correspondence is contained in A.H.Q.P. (16/4).

retical work. The first experimental contribution dated back to 1922 and was a part of the thesis on *Image formation with Röntgen rays.* The idea to use a radiofrequency field to conduct experimental research on atomic spectra was forerunner of a number of subsequent applications. It is probably for this idea that this contribution is to be appreciated.

The physicists Fermi came into contact with at Leyden exerted a positive influence on him. He kept in touch with the Dutch scientific environment also after his return to Italy, and Kronig can be considered to all intents and purposes as the link, during 1925, between Fermi and the other European physicists.

Kronig and Fermi met at Leyden and after Fermi's return to Italy there was a considerable exchange of letters between the two young physicists, less frequent after 1926. Travelling a lot and meeting many people, Kronig updated Fermi on what was going on in Berlin, Copenhagen, Göttingen, Leyden and Munich. Furthermore, Kronig spent with Fermi the August 1925 summer vacations in S. Vito di Cadore[60]. Probably that was the occasion for Fermi to discuss for the first time the new-born quantum mechanics. It's important to observe that Heisenberg, on 5 June 1925 (about two months before his famous article), wrote Kronig a letter where the non-commutative multiplication rule first appeared.[61] Also Pauli, in that period, kept in close touch with Kronig.

Fermi's quantum statistics

As it is known, Fermi or Fermi-Dirac statistics is the second quantum statistics proposed shortly after Bose-Einstein's in 1924-1925. Dirac recalls: "I worked out the basic relations for this new statistics, and I published this work. Soon after publication I got a letter from Fermi pointing out that this statistics was not really a new one; he had proposed it some time earlier [...] When I looked through Fermi's paper, I remembered that I had seen it previously, but I had completely forgotten it [...] 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 com-

---

60 Amaldi witnessed the meeting with Kronig: "Fermi was very young; he was 23 or 24 years old. A friend of his had come to the same place. That was R. de L. Kronig. [...] Kronig was a great friend of Fermi. They met when Fermi had been in the Netherlands to work with Ehrenfest. [...]", cmp. A.H.Q.P. transcript of a tape recorded interview by C. Weiner with E. Amaldi, 9 and 10 April 1969, p. 1.

61 Cmp. VAN DER WAERDEN (1967) and CASSIDY (1992).

pletely slipped out of my mind, and when I wrote up my work on the antisymmetrical wave functions, I had no recollection of it at all”.[62]

The two physicists arrived independently to the formulation of their quantum statistics, following very different approaches that, although both based on classical physics, differed greatly from[63] the drastic positions of Göttingen and Copenhagen schools. Dirac’s work[64] was presented to the Royal Society on 26 August 1926. That was more than six months after the Italian version by Fermi (F.N.M.30)[65] and more than three months after the more detailed German version (F.N.M.31).[66]

In Fermi’s case, two other previous contributions anticipated the conclusive article of 1926. The first one is a memoir presented to the Lincei in December 1923: *On Stern’s theory of absolute entropy constant of a perfect monatomic gas* (F.N.M.16).[67] The second significant contribution is an article Fermi wrote in January 1924, later published on the Nuovo Cimento: *Considerations on the quantization of systems containing identical elements* (F.N.M.19).[68]

Therefore, both the mentioned precursory works (on thermodynamical-

[62] Cmp. Dirac (1977), p. 133. The short Fermi’s letter, somewhat angry, to which Dirac refers is dated Rome 25 October 1926 and is fully reported here: “Mr. P.A.M. Dirac, St. John’s College, Cambridge. Dear Sir! In your interesting [sic] 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. I am, Sir, Yours Truly Enrico Fermi”, cmp. A.H.Q.P. (59/2).

[63] About Dirac: “he struggled to construct the new QM as a *generalization* of (and not as a *break* with) classical physics, through a systematic utilisation of the classical Hamiltonian formalism”, cmp. De Maria, La Teana (1983), p. 596. In a letter to Persico of 23 September 1925, he says of Fermi: “My impression is that during the past few months there has not been much progress, in spite of the formal results on the zoology of spectroscopic terms achieved by Heisenberg. For my taste, they have begun to exaggerate their tendency to give up understanding things”, cmp. Segrè (1970), p. 209. Dirac remembers his own impressions of the period: “I was so impressed then with the Hamiltonian formalism as the basis of atomic physics, that I thought anything not connected with it would not be much good. I thought there was not much in it [Heisenberg’s paper] and I put it aside for a week or so”, reported by Mehra in Salam, Wigner (1972), p. 31.

[64] Cmp. Dirac (1926). Previously Dirac, in an article that was not published, showed interest for the new developments of the Bose-Einstein new quantum statistics, cmp. Mehra, Rechenberg (1982), p. 109.

[65] This first formulation was presented (as a memoir) by Garbasso to Lincei on 7 February 1926: *On the quantization of the monatomic perfect gas*, (F.N.M.30), cmp. Rend. Lincei, 3 (1926), 145-149.

[66] This version, larger than the previous, appeared on 11 May 1926 on the Zeitschrift für Physik (F.N.M.31), cmp. Zeitschrift für Physik, 36 (1926), 902-912. Later on, unless different indications, one will always refer to this work.

[67] *Sopra la teoria di Stern della costante assoluta dell’entropia di un gas perfetto monoatomico*, Rendiconti Lincei, 32 (II) (1923), 395-398.

[68] *Considerazioni sulla quantizzazione dei sistemi che contengono degli elementi identici*, Nuovo Cimento, 1 (1924), 145-152.

statistical topics), can be set in the Italian 'intermezzo' of Fermi, between the German and Dutch period.

The 'Leitmotiv' that ideally links these three works (F.N.M.16, 19, 31) is the Sackur and Tetrode formula for the entropy absolute constant of an ideal monatomic gas:

$$S_0 = \frac{5}{2}R + Rln\left[\left(\frac{2\pi m}{h^2}\right)^{\frac{5}{2}} k^{\frac{5}{2}}\right]$$

This result, obtained thanks to many scientists' efforts at the beginning of the century[69], represented for Fermi a guide on the way towards the discovery of his quantum statistics.[70]

In his article of 1923 (F.N.M.16), Fermi seems to be already favourably impressed by Stern's derivation[71] of the entropy constant. This interest continued also afterwards.[72] It is natural to wonder how Fermi became interested in Stern's method and, more generally, in entropy constant.

One possible hypothesis is tightly connected with the Göttingen stay. In August 1923 Born published a very long article (nearly 250 pages) on the properties of solids: *Atomic theory of the solid state (dynamics of the crystal lattices)*.[73] In chapter 35, Born defined Stern's method. He followed almost

---

[69] For an excellent storiographical reconstruction, cmp. DESALVO (1992).

[70] Actually, Dirac's approach was based solely on the study of symmetry properties of a wave function in a system of many particles in the new ondulatory mechanics. It can be said, in fact, that Fermi's work is the last important thermodynamical statistical work written in the language of old quantum physics, whereas Dirac's is the first important statistical result obtained by new quantum mechanics.

[71] Cmp. STERN (1913) and Stern (1919). Although Stern arrived at the same formula of Sackur and Tetrode, he adopted a very different method. In 1913 he had the brilliant idea to carry out the calculus twice to find the equilibrium pressure of vapour, between solid and vapour phases. The calculus was carried out by Stern first thermodynamically and then with a kinetic model. With the thermodynamical calculus, extrapolating the high temperature limit, Stern obtained an expression containing the entropy constant S_0 for the pressure of vapour. As for kinetic calculus, it was valid only at high temperatures, and vapour pressure was obtainable without undeterminated constants. Comparing the two expressions, Stern was then able to determinate the value of S_0, reaching exactly the formula Sackur and Tetrode had found the previous year. In 1919 Stern returned on the subject, accepting part of TETRODE's method (1915).

[72] In fact a rich paragraph of his 1934 book *Molecole e Cristalli*, is dedicated to this subject, cmp. FERMI $(1966)_a$, par. 8-2. He dealt with this subject also in the 1951-52 *Notes on Thermodynamics and Statistics,* cmp. FERMI $(1966)_b$, par. 49.

[73] Cmp. BORN (1923), pp. 701-709. For more information see CORDELLA, SEBASTIANI $(1999)_d$, $(2000)_{a,b}$. For what concerns Sackur and Tetrode's works, in his article Born writes (almost exactly as Stern did in 1919) rather pessimistically: "[..] In this derivation there is a lot of arbitrariety, especially for the introduction of atoms number N", "[...] Bei dieser Ableitung bleibt vieles willkürlich, vor allem die Einführung der Atomzahl N". Born is much more enthusiastic of Stern's method: "[...] Through

exactly Stern's 1919 second formulation. Significantly, Born completed this work just when Fermi was in Göttingen.[74] Born used to have his own work checked by collaborators who liked finding mistakes or making suggestions. In 1921 E. Brody[75] was especially concerned with entropy constant. He was of great help to Born in preparing the review on solids.[76] As showed in the previous paragraph, in Germany Fermi was busy with analytical dynamics until April 1923. From this time on, there is no trace of Fermi's scientific interest. However, in F.N.M.19 Fermi made a clear reference to Brody's work of 1921. Moreover, in Göttingen in the same period, Enskog and Nordheim were working at the additivity entropy problem.[77] So it is quite likely that Fermi found, in the last four months in Göttingen, the right environment to deal with this subject.

F.N.M.16 was presented by Corbino to the Lincei on 2 December 1923. It is a short work of 3 pages and as many paragraphs. In the first two, Fermi refers briefly to the works of Sackur (1913) Tetrode (1912) and Stern (1913), (1919). Fermi shows appreciation for Stern's method and asserts that the necessity to quantize the phase space of gas, as stated by Sackur and Tetrode, appeared to him unnecessary.[78]

Stern's deduction, on the basis of Nernst's theorem, it is possible to define the only value of the chemical constant, that is the entropy constant of the gas, referred to the condensed phase at the absolute zero", "[...] Bei der sicherlich einwandfreien Ableitung von Stern kommt auch dem Einzelwerte der chemischen Konstanten auf Grund des Nernstschen Theorems ein Sinn zu, nämlich als die Entropiekonstante des Gases, bezogen auf das Kondensat beim absolutem Nullpunkt".

[74] From Born's letters to Einstein, reported in Born (1973^2), one can understand the effort that Born devoted to this work.

[75] Brody was an Hungarian physicists facing serious economic difficulties. The main reason is that, for those living on a fixed salary, as Brody, the inflation of the paper-mark was extremely heavy. Moreover, at that time it was not easy to carve out a career for an Hungarian Jewish in Germany. Cmp. Born (1973^2), p. 81.

[76] In the introduction Born writes: "[…] The more valuable help was given to me by prof. E. Brody; not only he contributed to gather the literature and elaborating some parts, but also provided, mainly through his sharp remarks, to clarify many relations and developing new methods. I'd like to thank him before everybody. Also some of my students helped me with the elaboration of texts and with corrections, especially Mr. P. Jordan, whom I thank for his many valuable comments, and Mr. K. Hermann, G. Heckmann and H. Kornfeld", "[…] Die wertvollste Hilfe bei der Arbeit fand ich durch Herrn Prof. E. Brody; er hat nicht nur zur Sammlung der Literatur beigetragen und manche Abschnitte ausgearbeitet, sondern vor allem durch scharf sinnige Bemerkungen die Klärung vieler Zusammenhänge herbeigeführt und neue Methoden entwickelt. Ihm gebührt an erster Stelle mein Dank. Auch einige meiner Schüler haben mir bei der Ausarbeitung des Textes und dem Lesen der Korrekturen in freundlicher Weise geholfen, vor allem Herr P. Jordan, dem ich viele wertvolle winke verdanke, und die Herren K. Hermann, G. Heckmann und H. Kornfeld", cmp. Born (1923), pp. V and VI.

[77] Cmp. Desalvo (1992), p. 512.

[78] "[...] That, despite the experimental testing[of Sackur-Tetrode's formula], this method seemed to be unsatisfactory, it's demonstrated by the many theoretical works produced, aiming at finding a better demonstration. Of all these attempts, Stern's is the best […] His method has the advantage of making

Fermi then devoted himself to eliminate the null point energy hypothesis from previous treatments, as Born in 1923 and Stern did in 1919: "in this work I intend to demonstrate that this unnatural hypothesis is by no means necessary […]".[79]

As stated above, in 1921 Brody was concerned with the entropy constant. Since his approach was carefully studied by Fermi, the next paragraph will outline the main features of Brody's article.

Brody's article of 1921

Brody's article of 1921: *On the theoretical determination of the chemical constant of a monatomic gas*,[80] was very important for its influence on Fermi and its concise and elegant solution to the problem of the entropy constant. As stated by Desalvo: "Brody's treatment became a standard, at least for most of the people accepting the quantization of translational motion. It represents the closest approach to the correct results in terms of the old quantum theory".[81] The work is divided into two sections: a first quantistic half *(Quantentheoretischer Teil)* and a second statistics half *(Statistischer Teil)*.

In the quantistic part, Brody takes into consideration a punctiform particle of mass m in a cubic box of side *l* with perfect reflecting walls. He refers to a system of Cartesian coordinates parallel to the sides of the box. The particle is free, supposing the gas perfect, so that its velocity components have a constant modulus. The quantum conditions for the particle periodic motions inside the box are:

$$\oint p_i dx_i = \oint m v_i dx_i \;=\; 2m|v_i| \int_0^l dx_i = 2m|v_i|\, l = n_i h \qquad (i = x, y, z)\,.$$

none of the arbitrary hypotheses on the gas that other authors need, such as gas quantization of which the reason is not clear", "[…] Che nonostante la verifica sperimentale [della formula di Sackur-Tetrode] questo modo di dedurla non sia apparso a molti soddisfacente, è dimostrato dal gran numero di lavori teorici, che furono fatti in seguito, con lo scopo di trovarne una dimostrazione migliore. Di tutti questi tentativi, quello che senza dubbio ha meglio raggiunto il suo scopo è quello di Stern [...] Il suo metodo ha il vantaggio di non fare sopra il gas perfetto nessuna di quelle poco legittime ipotesi, che sono necessarie agli altri Autori, come per esempio quella di una quantizzazione del gas stesso, della quale non si vede chiaramente la ragione", excerpts from F.N.M.16.

79 "[...] in questo lavoro mi propongo di dimostrare che questa ipotesi innaturale non è affatto necessaria […]".

80 Cmp. BRODY (1921). In note eight of this article, Brody refers that this work appeared in Hungarian as a degree thesis in 1917. The purpose of the work was to eliminate a discrepancy in a previous work of Scherrer dated 1916.

81 Cmp. DESALVO (1992), p. 506.

Therefore, the kinetic energy of the particle is:

$$E=\frac{1}{2}m(v_x^2+v_y^2+v_z^2)=\frac{h^2}{8ml^2}(n_x^2+n_y^2+n_z^2)$$

that agrees with the value that would be obtained by the solution of Schrödinger's equation for a particle in an infinite potential well.

In the statistics part, Brody suggests a substantially new reasoning. First of all he defines energy and volume as physical quantities describing the macroscopic state of the system. For the microscopic state (and that's the main assumption) instead of the coordinates and moments of the N particles, he takes into consideration the quantum numbers n_{ji} (where $j = 1, \ldots, N$ and $i = x, y, z$). In order to calculate the thermodynamic probability W, to be inserted in the Boltzmann's formula $S = k \cdot \ln W$, one has to determine in how many different ways it's possible to choose the n_{ji} microscopic states corresponding to a given macroscopic system determined by E and $l = V^{1/3}$.

Since energy can only take discontinuous values, there can be several other possible systems of values for n_{ji}. That is:

$$E\leq\frac{h^2}{8ml^2}(n_{1x}^2+n_{1y}^2+n_{1z}^2+\ldots+n_{Nx}^2+n_{Ny}^2+n_{Nz}^2)\leq E+dE.$$

The search for numbers of n_{ji} satisfying this inequality is conducted by Brody assuming the $3N$ values of $n$ji as the Cartesian coordinates of a point in a $3N$-dimensional space. To calculate W, Brody introduces, without many justifications, a further division by $N!$ so as not to consider the two microscopic states different, but distinguished only by a permutation of atoms.[82] Finally Brody obtains, as macroscopic state probability:

$$W=\frac{1}{2^{3N}N!}\frac{dV_{3N}}{dE}$$

where dV_{3N} denotes the $3N$-dimensional spherical shell whose radius are limited by the energy constraint. As Fermi stated in the article of January 1924 (F.N.M.19), to obtain W, Brody: "[…] is forced to put […] $dE = 1$, being E

[82] This procedure, needed to justify the entropy additivity and avoid Gibbs' paradox, caused an animated discussion among Planck, Ehrenfest and Schrödinger. In particular Ehrenfest and Trkal, in their well-known article of 1920, stated: "In the majority of calculations of the chemical constant, a special obscurity remains as to the way in which the "thermodynamic probability" of a gas depends on the number of molecules", cmp. Ehrenfest and Trkal (1920). The German version of the article appeared in 1921.

an energy, so that the probability is substantially defined as the number of gas quantum states […] reducing to an energy between *E* and *E*+1. In this way his probability has the dimensions, instead that of a number, of the inverse of an energy".[83] This remark significantly shows how Fermi had carefully studied this work.

Searching for a lacking principle

As already mentioned, in January 1924 Fermi wrote another thermodynamical-statistical article, later published on the Nuovo Cimento (F.N.M.19). A little more than a month after the memoir on his improvement of Stern's method.

In the introduction, Fermi recalls that Sommerfeld's quantization rules effectively described the hydrogen atom. On the other hand, they were unsuitable for the description of atoms with many electrons. To justify the need of modifying the quantum rules, for a system containing identical elements, Fermi suggested a simple example. He considered a ring with three electrons placed at the vertex of an equilateral triangle.[84]

Subsequently Fermi considered the perfect gas as a separate system of variables and intended to apply the quantization rules: "[…] making various hypotheses on the way to quantize it (to obtain a finite value for the entropy of an ideal gas it's necessary to quantize it one way or another, since the classical treatment would always lead us to an infinite value)".[85]

So let's consider, following Fermi, a perfect gas formed by N punctiform molecules contained in a vessel of volume *V*. It's possible to divide the volume V in many parallelepipedal rectangular cells, each one of sides *a*, *b*, *c*, and vol-

[83] "[...] è però costretto a porre [...] $dE = 1$, essendo *E* una energia, con che la probabilità viene in sostanza a essere da lui definita come il numero degli stati quantici del gas [...] che conducono ad una energia compresa tra *E* ed $E + 1$, di modo che la sua probabilità viene ad avere le dimensioni, anziché di un numero, dell'inverso di un'energia"

[84] Considering the electrons as ***distinguishable***, it's clear that one has to rotate the ring by a 2π angle to re-obtain the initial situation. If the electrons were instead ***indistinguishable*** it would be necessary to rotate the ring only by $2\pi/3$. Therefore, denoting with p the angular momentum of the ring, in the first case Sommerfeld's quantization rules give $(2\pi)p = nh$, while in the second case $(2\pi)p = 3nh$. This kind of argumentations on the indistinguishability concept were inspired to Fermi by two works of Compton and Breit, both quoted in F.N.M.19, cmp. A.H. COMPTON (1923) and G. BREIT (1923).

[85] "[...] facendo diverse ipotesi sopra il modo di quantizzarlo (per ottenere un valore finito dell'entropia di un gas perfetto è necessario quantizzarlo in un modo o nell'altro, poiché la trattazione classica ci condurrebbe sempre ad un valore infinito)" It's therefore evident that Fermi changed opinion. In F.N.M.16 he still considered an unsuitable hypothesis: "[...] that of a quantization of the gas of which the reason is not clear".

ume $V_C = a \cdot b \cdot c$. Fermi assumed that inside every cell there would be the same number of molecules. Therefore, there were many possibilities, because we can imagine the volume V as constituted by: N equal cells with 1 molecule, $N/2$ equal cells with 2 molecules, ...,1 cell with N molecules. Hence, one has N/x cells with $x = N \cdot V_C / V$ molecules in every cell. Assuming, as Brody did, the Cartesian coordinates parallel to the sides of the cell, the system is clearly a separate variables system. In this way, referring to Brody's results, one arrives to the following expression for the total entropy of the N molecules (we have generalised Fermi's procedure):

$$S = kN\left\{\frac{5}{2} lnT - lnP + \ln\left[\frac{(2\pi m)^{\frac{3}{2}} k^{\frac{5}{2}} e^{\frac{5}{2}}}{h^3} \cdot \frac{x}{e}\right]\right.$$

where V is supposed to be divided in N/x equal cells, each containing x molecules. So far, Fermi's considerations are quite similar to Brody's. But now, following the latter, it would be necessary to consider a single cell that contains N molecules, that is $x = N$. Then, introducing the very arguable division by $N!$ from the previous expression, it's easy to find exactly the Sackur-Tetrode formula. Yet Fermi doesn't follow this procedure and suggests a new, more subtle reasoning. As a matter of fact, the previous formula was derived assuming a particular distribution of molecules into the cells. Let P_r be the probability of this choice, Fermi asserts that the real entropy of the gas S_r is obtainable taking into consideration a further addend $-k \cdot ln\ P_r$. This assumption, that Fermi doesn't try to justify thoroughly, has actually the importance of a new thermodynamic definition of probability, namely $S = k \cdot ln(W/P_r)$. In so doing, one would obtain:

$$S_r = kN\left\{\frac{5}{2} lnT - lnP + ln\left[\frac{(2\pi m)^{\frac{3}{2}} k^{\frac{5}{2}} e^{\frac{5}{2}}}{h^3}\right] + ln(x!)^{\frac{1}{x}}\right.$$

Therefore, it's evident that the agreement with the Sackur-Tetrode's formula is reached only with $x = 1$. In conclusion, following Fermi's reasoning, the experimental testing of the Sackur-Tetrode formula requires the following. The vessel must be divided into equal N cells of volume V/N, each containing only one molecule, as it was supposed by Sackur in 1913. Of course, the 'ad hoc' character of this hypothesis is evident, but all other ways are to be excluded in the light of experimental facts. In his 1926 article on quantum statistics (where F.N.M.19 is quoted) Fermi would assert: "[…] you obtain a degeneracy of an expected order of magnitude only if you pick a vessel of so small dimensions that it generally contains only one molecule”.[86]

Further on in the 1924 article, Fermi points out that the discrepancies between S and S_r, arise precisely from the identity of two or more molecules contained in a single cell. In the case of missing identity (for example if many particles in a single cell are of different species), Fermi shows how S would provide, in this case, a correct result for the entropy.

Pursuing this line of thought, Pauli’s exclusion principle can be reached in a roundabout way. Applying Fermi’s analogy method, we can identify, for example, the helium atom with a cell into which two electrons are disposed. In that case, the previous topics suggest that there must be lack of a principle affirming that it’s impossible to find both the electrons with the same quantum numbers.[87] This “organizing” principle is obviously Pauli’s exclusion principle, but not in Pauli’s originally formulation. In fact, following Fermi, thanks to what we called here the analogy method, this result can be extended to a lot of situations (collection of quantum harmonic oscillators, etc.).

Rasetti recalls that: “[Fermi] told much later to Segrè that the division of phase space into finite cells had occupied him very much and that had not Pauli discovered the exclusion principle he might have arrived at it in a roundabout way from the entropy constant”.[88]

“Ante/post-quem” limits of Fermi’s statistics

Fermi’s article on new quantum statistics[89] was presented by Garbasso to the Lincei, in a condensed form, on 7 February 1926 (F.N.M.30), a little more than two years after F.N.M.19. On 26 March 1926, the Zeitschrift für Physik received a more detailed version of the article, then published on the German review on 11 May 1926 (F.N.M.31).[90]

The ‘ante quem’ limit of Fermi’s quantum statistics may therefore be set at the beginning of February 1926.

---

86 “[…] si ottiene una degenerazione di un atteso ordine di grandezza solo se si sceglie il recipiente di dimensioni così piccole che esso in media contenga soltanto una molecola”.

87 “Helium was only understood after quantum mechanical methods could be brought to bear, including important applications of spin and of the exclusion principle”, cmp. PAIS (1986), p. 215.

88 Cmp. Rasetti’s introduction to F.N.M.30 and 31.

89 For more information, cmp. CORDELLA, SEBASTIANI $(1999)_f$ e $(2000)_c$.

90 Cmp. Rasetti’s introduction to F.N.M.30 and 31. Otherwise AMALDI (1983), p. 253. From an immediate analysis of the Italian version of the article it’s evident that Fermi had already reached all the important results of his new statistics.

Fermi was very busy finding a post as a teacher (in February 1926 he participated unsuccessfully to an examination for a mathematical physics teaching post in Cagliari)[91]. Fermi's letter to Uhlenbeck and Goudsmit, dated 25 February 1926, stood between the two versions of the article:

> "Since I came away from Holland, I was unfortunately always very busy so that I couldn't work very much and I was forced to be satisfied with the readings of reviews. Recently I have prepared two works, one on the quantization of an ideal gas, the other on the apparition of the forbidden transitions in a magnetic field; I'll send you the abstracts as soon as I get them."[92]

As for the more important 'post quem' limit, Segrè states that Fermi, a few weeks after reading Pauli's work on the exclusion principle, was able to present at the Lincei, through Corbino,[93] an Italian version of the article.[94] However that may be, it is still in doubt when Fermi was acquainted with Pauli's article. This one appeared in the February-April 1925 number of the Zeitschrift für Physik,[95] so that Fermi could have known it at least since the spring of 1925. However, as showed in the previous chapter, during the spring of 1925 Fermi was busy carrying out experimental work with Rasetti. In July, Fermi was engaged with state examinations in Florence. Fermi spent the summer with Kronig and others in S. Vito di Cadore. That may be important because, as asserted by A. Pais, Kronig had been aware of the exclusion principle at least since January 1925.[96] Therefore a likely hypothesis is that Kronig talked to Fermi about it during the summer vacations in S.Vito. Anyway, in the various sources regarding Fermi before F.N.M.30, there is no trace of Pauli's principle, while Fermi was puzzled by the articles of Heisenberg, Born and Jordan on the new quantum mechanics.[97]

---

91 Cmp. F.N.M., p. XXVII and Segrè (1970), p. 40.

92 Cmp. A.H.Q.P. (60/6). "In der ganzen Zeit seit Ich von Holland weg kam, war ich leider immer sehr beschäftig, so dass ich habe sehr wenig Arbeiten können und habe mich hauptsächlich mit dem Lesen der Zeitschriften begnügen müssen. Ich habe neulich zwei Arbeiten gemacht, die eine über die Quantelung idealer Gase die andere über das auftreten verbotene Übergänge in einem Magnetsfelde; ich werde Euch die Separata schicken, sobald ich sie bekomme" The second work to which Fermi refers is F.N.M.32: *Sopra l'intensità delle righe proibite nei campi magnetici intensi*, Rendiconti Lincei, 3 (1926), 478-483. In the letter Fermi congratulates them for the discovery of the spin.

93 In the Rend. Acc. Lincei, Garbasso is mentioned as the member who presented Fermi's article.

94 Cmp. Segrè (1970), p. 42.

95 Cmp. Pauli (1925).

96 Cmp. Pais (1986), p. 280.

97 The T. S. Kuhn's interview to Persico and Rasetti reads: "[...] **TSK**: Did Fermi say anything that you remember about the matrix mechanics papers? **R:** Oh, Fermi tried to read them, but he could not understand them. He said, 'I cannot do any. I don't see how I can use it, how I can do any calculation with these. I don't understand what's behind it.' Oh, he read them and he was very much puzzled by

Rasetti remembers an event (also recalled by Laura Fermi) occurred when Fermi was making his gas statistics: "We were practically together from morning till evening, from discussing physics to hunting for certain geckos, a sort of lizard […]. That was precisely when he was making the gas statistics. He was probably thinking while lying down and catching the geckos with the noose".[98] In Central Italy, at the end of September geckos usually hibernate.[99]

Therefore, the 'post quem' limit of Fermi's statistics, less precisely of the 'ante quem' limit, is roughly set in mid- September 1925.

ABBREVIATIONS

A.H.Q.P. *(x/y)* = "Archive for History of Quantum Physics", microfilm *x*, section *y*.

AMAL. ARCH. *(x/y)* = "Amaldi Archive", Department of Physics, University "La Sapienza", Rome, box *x*, folder *y*.

PERS. ARCH. *(x/y)* = "Persico Archive", Department of Physics, University "La Sapienza", Rome, box *x*, folder *y*.

VOLT. ARCH. *(x/y)* = "Volterra Archive", Accademia Nazionale dei Lincei, Rome, box *x*, folder *y*.

F.N.M.*k* = FERMI (1962-1965), vol. I., the *k*th work.

REFERENCES

E. AMALDI (1983), *The Fermi-Dirac statistics and the statistics of nuclei, in Symmetries in Physics (1600-1980)*, by M.G. DONCEL, A. HERMANN, L. MICHEL, A. PAIS, Universitat autònoma de Barcelona, Seminari d'historia de les ciències.

L. BELLONI (1994), *On Fermi's route to Fermi-Dirac statistics*, European Journal of Physics, 15, 102-109.

M. BORN (1923), *Atomtheorie des festen Zustandes (Dynamik der Kristallgitter)*, in *Encykl. d. math. Wiss.*, vol. V, Leipzig: Teubner.

M. BORN (1973^2), *Scienza e vita, Lettere 1916-1955*, Torino: Einaudi (Italian edition).

M. BORN (1978), *My Life*, London: Taylor and Francis.

G. BREIT, (1923), *Note on the Width of Spectral Lines Due to Collisions and Quantum Theory*, Proceedings Natural Academy of Sciences, vol. 9, no.11, 244-245.

them. **P:** I don't remember that I ever discussed them with Fermi. **R:** I know that he showed me these papers and said, 'Now I'm trying to read them and see what Heisenberg is trying to say, but so far I don't understand it.'", cmp. A.H.Q.P. transcript of a tape recorded interview by T. S. Kuhn **(TSK)** with F. Rasetti **(R)** and E. Persico **(P)**, 8 April 1963, p. 16.

[98] Cmp. A.H.Q.P. transcript of a tape recorded interview by T. S. Kuhn with F. Rasetti and E. Persico, 8 April 1963, p. 13.

[99] M. Di Domenico, collaborator of the Zoology Department of the Istituto dell'Enciclopedia Italiana, private communication.

L. Brillouin (1930), *Les statistiques quantiques*, Paris: Les Presses Universitaires de France.

E. Brody (1921), *Zur theoretischen Bestimmung der chemischen Konstante einatomiger Gase*, Zeitschrift für Physik, 6, 79-83.

D.C. Cassidy (1992), *Uncertainity. The Life and Science of Werner Heisenberg,* New York: Freeman.

A.H. Compton (1923), *The Quantum Integral and Diffraction by a Crystal*, Proceedings of the Natural Academy of Sciences, vol. 9, no. 11, 359-362.

F. Cordella, F. Sebastiani (1999)$_a$, *Il debutto di Enrico Fermi come fisico teorico: i primi lavori sulla relatività (1921-1922/23)*, Quaderno di Storia della Fisica del Giornale di Fisica, 5, 69-88.

F. Cordella, F. Sebastiani (1999)$_b$, *Fermi a Gottinga e a Leida: gli anni che precedono la statistica quantica (1922-1925)*, Dipartimento di fisica, Università degli Studi di Roma "La Sapienza", Internal note, no.1104; Quaderno di Storia della Fisica del Giornale di Fisica, 6 (2000), 17-45.

F. Cordella, F. Sebastiani (1999)$_c$, *La corrispondenza Persico-Fermi (1922-1926)*, Giornale di Fisica, 40, 143-164.

F. Cordella, F. Sebastiani (1999)$_d$, *La genesi della statistica di Fermi*, Dipartimento di Fisica, Università degli Studi di Roma "La Sapienza", Internal note, no. 1106.

F. Cordella, F. Sebastiani (1999)$_e$, *La corrispondenza inedita Fermi-Persico (1917-1938)*, Dipartimento di Fisica, Università degli Studi di Roma "La Sapienza", Internal note, no. 1107.

F. Cordella, F. Sebastiani (1999)$_f$, *La statistica di Fermi*, Dipartimento di Fisica, Università degli Studi di Roma "La Sapienza", Internal note, no. 1110.

F. Cordella, F. Sebastiani (2000)$_a$, *I due lavori di Fermi che preludono alla statistica quantica*, Giornale di Fisica, 41, 83-101.

F. Cordella, F. Sebastiani (2000)$_b$, *Sul percorso di Fermi verso la statistica quantica*, Il Nuovo saggiatore, 16, no. 1-2, 11-22.

F. Cordella, F. Sebastiani (2000)$_c$, *La statistica di Fermi*, Giornale di Fisica 41 (2000), 131-156.

M. De Maria, F. La Teana (1983), *Dirac's 'Unorthodox' Contribution to Orthodox Quantum Mechanics (1925-1927)*, Scientia, 77, vol. 118, 595-611.

M. De Maria (1999), *Fermi: un fisico da via Panisperna all'America*, I grandi della Scienza, no. 8.

A. Desalvo (1992), *From the chemical constant to quantum statistics: a thermodynamic route to quantum mechanics*, Physis, 29, 465-537.

P.A.M. Dirac (1926), *On the Theory of Quantum Mechanics*, Proceedings of the Royal Society, A112, 661-667.

P.A.M. Dirac (1977), *Recollections of an Exciting Era, in History of Twentieth Century Physics* (Proceedings of the Internal School of Physics "Enrico Fermi", course LVII, Varenna 31 July-12 August 1972) by C. Weiner, New York: Academic Press, 109-146.

P. Ehrenfest, V. Trkal (1920), *Deduction of the Dissociation-Equilibrium from the Theory of Quanta and a Calculation of the Chemical Constant Based on this*, Proceedings Amsterdam, 23, 162-183.

P. EHRENFEST (1923), *Kann die Bewegung eines Systems von s Freiheitsgraden mehr als (2s-1)-fach-periodisch sein?*, Zeitschrift für Physik, 19, 242-245.

P. EHRENFEST (1925), *Energieschwankungen im Strahlungsfeld oder Kristallgitter bei Superposition quantisierter Eigenschwingungen*, Zeitschrift für Physik, 34, 362-373.

EHRENFEST-JOFFÉ (1990), *Nauchnaia perepiska, 1907-1933*, by N. MOSKOVCHENKO, V. FRENKEL, Leningrad: Nauka.

E. FERMI (1962-1965), *Note e Memorie (Collected Papers)*, by E. AMALDI et al., 2 voll., Roma: Accademia Nazionale dei Lincei, Chicago: University of Chicago Press.

E. FERMI (1966)a, *Molecules, Crystals, and Quantum Statistics*, New York: W.A. Benjamin (English version).

E. FERMI (1966)b, *Notes on Thermodynamics and Statistics*, Chicago: University of Chicago Press.

L. FERMI (1954), *Atoms in the Family*, Chicago: University of Chicago Press.

S.A. GOUDSMIT (1972), *Guess Work: The Discovery of the Electron Spin*, Delta, summer, 77-91.

S.A. GOUDSMIT (1976), *It might as well be spin*, Physics Today, June, 40-43.

H.S. KRAGH (1990), *Dirac: a scientific biography*, Cambridge: Cambridge University Press.

J. MEHRA, H. RECHENBERG (1982), *The Historical Development of Quantum Theory*, vol. 4, New York: Springer.

A. PAIS (1986), *Inward Bound*, New York: Oxford University Press.

W. PAULI (1925), *Über den Zusammenhang des Abschlusses der Elektronengruppen im Atom mit der Komplexstruktur der Spektren*, Zeitschrift für Physik, 31, 765-783.

W. PAULI (1979), *Wissenschaftlicher Briefwechsel mit Bohr, Einstein, Heisenberg, u. a. 1919-1929*, by A. HERMANN, K. V. MEYENN, V.F. WEISSKOPF, New York: Springer.

M. PLANCK (1924), *Zur Quantenstatistik des Bohrschen Atommodells*, Annalen der Physik, 75, 673-684.

B. PONTECORVO (1993), *Enrico Fermi. Ricordi di allievi e amici*, Pordenone: Studio Tesi (Italian edition).

O. SACKUR (1913), *Die Universelle Bedeutung des sogenannten elementaren Wirkungsquantums*, Annalen der Physik, 40, 67-86.

A. SALAM, E.P. WIGNER (by) (1972), *Aspects of quantum theory*, Cambridge: Cambridge University Press.

C. SASSI, F. SEBASTIANI (1999), *La formazione scientifica di Enrico Fermi*, Giornale di Fisica, 2, vol. XL, 89-113.

E. SEGRÈ (1970), *Enrico Fermi Physicist*, Chicago: University of Chicago Press.

O. STERN (1913), *Zur kinetischen Theorie des Dampfdrucks einatomiger fester Stoffe und über die Entropiekonstante einatomiger Gase*, Physikalische Zeitschrift, 14, 629-632.

O. STERN (1919), *Zusammenfassender Bericht über die Molekulartheorie des Dampfdruckes fester Stoffe und ihre Bedeutung für die Berechnung chemischer Konstanten*, Zeitschrift für Elektrochemie, 25, 66-80.

G. TAGLIAFERRI (1985), *Storia della fisica quantistica. Dalle origini alla meccanica ondulatoria*, Milano: Franco Angeli.

H. Tetrode (1912)a, *Die chemische Konstante der Gase und das elementare Wirkungsquantum*, Annalen der Physik, 38, 434-442.

H. Tetrode (1912)b , *Berichtigung*, Annalen der Physik, 39, 255-256.

H. Tetrode (1915), *Theoretical Determination of the Entropy Constant of Gases and Liquids*, Proceedings Amsterdam, 15, 1167-1183.

G.E. Uhlenbeck (1976), *Personal reminiscences*, Physics Today, June, 43-48.

B.L. van der Waerden (1967), *Sources of Quantum Mechanics*, Amsterdam: North-Holland Publishing Company.

E. Vinassa de Regny (by) (1992), *Una scoperta ogni venti giorni...*, Linea d'Ombra, 76, 38-39.

Fabio Sebastiani
Fabio Sebastiani is Professor of History of Physics and Director of the Museum of Physics at the University "La Sapienza" of Rome. Within the programs of the National Institute for Nuclear Physics, he carried out activities in experimental physics of elementary particles at the National Laboratories of Frascati and the CERN of Geneva. Subsequently he dedicated himself to historical researches on the genesis and development of heat theories in the 1700's and 1800's. He has carried out historical researches on the Italian period of Enrico Fermi for several years.

Francesco Cordella
Francesco Cordella is a history of physics graduate (with Fabio Sebastiani). His degree thesis "The first theoretical researches of Enrico Fermi (1921-1926)" was awarded with the 1998 prize of the National Academy of Sciences (known as that of the XL). Now he's working as Analyst Programmer with a software house of Rome.

Jan Philip Solovej

The Evolution of Fermi's Statistical Theory of Atoms

In this talk I shall give a historical review of Fermi's statistical theory of atoms with emphasis on rigorous work. The theory has had a great impact on physics and chemistry, but has long been considered far too simplistic to be of any practical interest. From a rigourous theoretical point of view however the theory has had some very important applications. I will discuss these briefly, but also argue that even from a practical point of view it may not be quite fair to deem the theory as "too simplistic".

L'evoluzione della teoria statistica degli atomi di Fermi

Delineerò un profilo storico della teoria statistica degli atomi di Fermi con particolare attenzione all'approccio rigoroso.
Tale teoria ha avuto considerevoli ripercussioni sulla fisica e la chimica, ma è stata a lungo considerata troppo semplicistica per farne discendere un qualche interesse pratico. Tuttavia, da un punto di vista strettamente teorico, ha avuto alcune importanti applicazioni. Prenderò sinteticamente in esame alcune di queste, ribadendo però che, da un punto di vista strettamente pratico, la definizione di "teoria semplicistica" non le rende giustizia.

It is a great honor to be able to give a presentation at this meeting celebrating the centennial of the Birth of Enrico Fermi. My talk is about the statistical model for atoms. Versions of this model were published independently by Fermi and Thomas in the two papers [5,22]. The model is therefore often referred to as the *Thomas-Fermi model.*

I will not talk about the early history of this model, but rather discuss some of the more recent developments.

It may be surprising to find a mathematician talking about the statistical theory of atoms, but the Thomas-Fermi theory has, in fact, been a great source of inspiration to mathematicians since the early 70s.

The virtue of the Thomas-Fermi theory is its great simplicity in comparison to the full many-body quantum mechanical description of atoms or molecules.

In fact, the model is so simple that Fermi was able to numerically calculate the atomic solution to the model on the crude hand calculators available in the late 1920s. The model can also be applied to molecules, but in that case it is more dificult to calculate solutions.

Today the theory is however often considered to be too crude to be of any real computational interest in Chemistry or Physics. With the advent of big computers one can today do calculations on much more refined models.

It should be pointed out however that one of the methods used with great success today in computational quantum chemistry is what is called density functional theory. The Thomas-Fermi theory is in fact the simplest imaginable density functional theory. The use of more elaborate such theories goes back to the works of Kohn and Hohenberg in the early 60s and for which Kohn received the Nobel prize in 1998 [9].

It is however not always the case that one can use computers to get useful answers. There are cases where the problem is simply too complicated to calculate even with a computer and there are cases where one would like a more qualitative understanding than what one gets from a long computer calculation.

An example of a problem which is too complicated for a computer is the binding energy of a macroscopic piece of material consisting of more than 10^{23} particles. As I shall explain it turns out that the Thomas-Fermi theory is very useful here. In fact it was discovered in 1976 by Lieb and Thirring [14] that one can explain the stability of ordinary matter using the theory of Thomas and Fermi. Stability here refers to the fact that matter does not collapse in an implosion caused by electrostatic forces.

In fact it was realized already in 1930 by Chandrasekhar [1] that a model similar to the Thomas-Fermi model could explain why certain cold stars known as white dwarfs did not collapse under the influence of gravity. An example where one would like a qualitative understanding is the question of the size of heavy atoms. The fact that atoms have the particular size that they do and in particular the fact that the radii of atoms varies at most by a factor 3-4 over the periodic table is the result of a delicate balance between the electrostatic forces and Fermi pressure. The size of everything around us depends on this delicate balance. It is therefore important to be able to give a simple qualitative explanation of this balance. As we shall see it can be provided by the Thomas-Fermi model.

The Thomas-Fermi model

The Thomas-Fermi model gives a description of the atomic density ρ. On the one hand, knowing the atomic density of an atom with nuclear charge Z allows one to calculate *the mean field potential*

$$\varphi(x) = Ze|x|^{-1} - \int e\rho(y)|x-y|^{-1} dy \,. \tag{1}$$

On the other hand the mean field potential allows one to calculate the density of the corresponding Fermi gas below some Fermi level μ

$$\rho(x) = 2(2\pi\hbar)^{-3} \int_{\frac{1}{2m}p^2 - \phi(x) < -\mu} d^3p \;= \gamma^{-3/2}\left[e\phi(x) - \mu\right]_+^{3/2} \tag{2}$$

where $\gamma = (3\pi^2)2/3\ \hbar^2\ (2m)^{-1}$. Here $[t]+ = \max\{t,0\}$. The two equations (1) and (2) define in a self-consistent way the density in the *statistical model.*

For a molecule with K atoms of nuclear charges $Z_1, \dots Z_K$, the mean field potential is instead

$$\varphi(x) = \sum_{k-1}^{K} Z_k e|x-R_k|^{-1} - \int d^3y e\rho(y)|x-y|^{-1}.$$

Alternatively the statistical model may be formulated from a variational principle. The equations (1) and (2) are the Euler-Lagrange equations for the Thomas-Fermi energy minimization (μ is the Lagrange multiplier for the constraint $\int \rho = N$):

$$E^{TF}(N) = \inf\left\{\mathcal{E}(\rho) : \int \rho = N, \rho \geq 0\right\}, \tag{3}$$

where

$$\mathcal{E}(\rho) := \frac{3}{5}\gamma \int d^3x \rho(x)^{5/3} - \int d^3x V(x) e\rho(x) + \frac{1}{2}\iint d^3x d^3y e^2 \frac{\rho(x)\rho(y)}{|x-y|} + U \,. \tag{4}$$

Here

$$V(x) = \sum_{j=1}^{K} \frac{Z_j e}{|x - R_j|}, \; U = \sum_{1 \leq i \leq j \leq K} \frac{Z_i Z_j e^2}{|R_i - R_j|}. \tag{5}$$

The nuclear repulsion U has been added to get the correct energy.

Mathematical results and Stability of Matter

The first to study the Thomas-Fermi theory from a mathematical point of view was E. Hille [6,7] more than 40 years after the original papers. A complete analysis establishing existence and uniqueness of the solution also in the molecular case was first given by Lieb and Simon in [13].

One of the most important facts realized about the Thomas-Fermi model is that the energy defined in (3) (with small modifications, which I shall explain below) gives a lower value (more negative) than the true quantum energy.

This is a consequence of a kinetic energy bound proved by Lieb and Thirring in [14] and a bound on the indirect Coulomb energy proved in [10] and improved in [12]. These two estimates imply that if $H_{N;K}$ is the Hamiltonian for N electrons and K nuclei and ψ is a fermionic wave function for N electrons with corresponding density ρ_ψ then the energy expectation in the state ψ, i.e., $(\psi, H_{N,K}\psi)$ satisfies the bound

$$(\psi H_{N,K} \psi) \geq \varepsilon_{\tilde{\gamma}}(\rho\psi) - 1.68^2 \int \rho_\psi^{4/3} \tag{6}$$

Here $\varepsilon_{\overline{\gamma}}$ refers to the energy defined as in (4), but with a different value for the constant γ. This is what one has been able to prove, but, in fact, it was conjectured in [15] that one does not have to change γ, i.e., that (6) holds with ε defined exactly as in (4). Establishing this fact is from a mathematical point of view one of the most challenging questions concerning the Thomas-Fermi theory and there is extensive literature on the subject.

A correction to the Thomas-Fermi theory corresponding to the last term in (6) was suggested by Dirac in [2] and is known as the Dirac exchange correction.

Another very important fact about the Thomas-Fermi theory is Teller's No-binding Theorem. It states that in the Thomas-Fermi theory atoms do not bind to form molecules. Or more precisely the Thomas-Fermi energy of a molecule is greater than the sum of the Thomas-Fermi energies of the individual atoms.

This fact was originally realized by Teller [21] and proved rigorously by Lieb and Simon [13].

Lieb and Thirring [14] used the No-binding Theorem together with (6) [or rather a somewhat similar result, since (6) was only proved subsequently] to give a very simple and extremely elegant proof of the result known as Stability of Matter. This result originally due to Dyson and Lenard [3] states that the binding energy per particle has to remain bounded even as the number of particles become arbitrarily large (e.g. of order 10^{23}).

Validity of the Thomas-Fermi model as an approximation

One may ask how well the Thomas-Fermi energy E^{TF} (N = Z), which in fact scales like $C_{TF}Z^{7/3}$, approximates the real ground state energy $E^Q = E^Q(N = Z)$ of a neutral atom of nuclear charge Z.

The answer is that the real ground state energy satisfies an asymptotic expansion of the form

$$E^Q = C_{TF}Z^{7/3} + \frac{me^4}{2\hbar^2}Z^2 + C_{Dirac/Schwinger}Z^{5/3} + o(Z^{5/3})$$

as $Z \to \infty$.

That the Thomas-Fermi model gives the leading term of order $Z^{7/3}$ was established by Lieb and Simon [13]. The next to leading term of order Z^2 was predicted by Scott [17] and proved mathematically by Hughes [8] and Siedentop-Weikard [18]. One contribution to the order $Z^{5/3}$ comes from the Dirac exchange term mentioned above. That there was another contribution to the same order was realized by Schwinger [16]. The mathematical proof was given by Fefferman and Seco [4].

The energy asymptotics can be traced to different regions in a heavy atom. In an atom with large nuclear charge Z the bulk of the electrons live a distance from the nucleus that scales like $Z^{-1/3}$, i.e., for atoms with larger and larger Z the bulk of the electrons live closer and closer to the nucleus.

These electrons contribute to the leading term $Z^{7/3}$ to the energy. The Scott term Z^2 comes from the innermost electrons living on a scale Z^{-1}. In Figure 1 we show the order of the density on the different scales of the atom.

It is however the *outermost* electrons that are of importance to chemistry.

From the point of view of an approximation to the total binding energy the Thomas-Fermi theory is too crude to say anything about energies on the chemical scale.

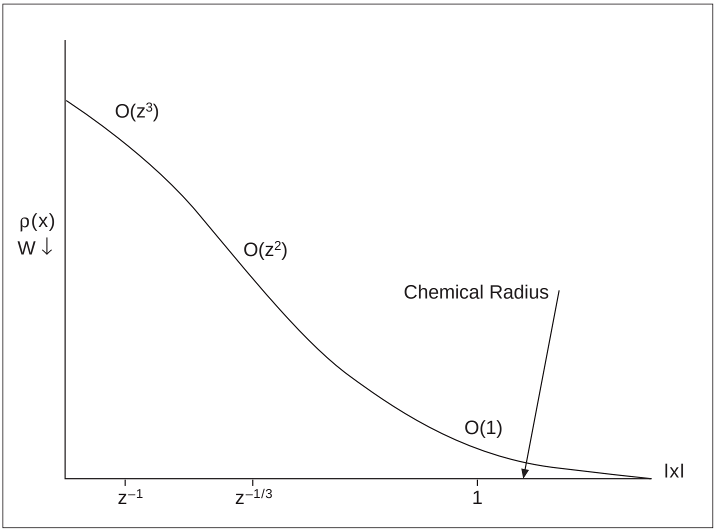

Figure 1
A picture of a heavy atom

In [20] a mathematical result was proved that indicates that the Thomas-Fermi model is in fact not such a bad approximation to the density even at the chemical radius. The precise statement is beyond the scope of this presentation. To stress the point I shall instead show what I find to be a convincing comparison of the Thomas-Fermi theory with experimental data.

The radius of an atom is not a clearly defined quantity and is certainly not directly measurable. In the paper [19] Slater pointed out, however, that one

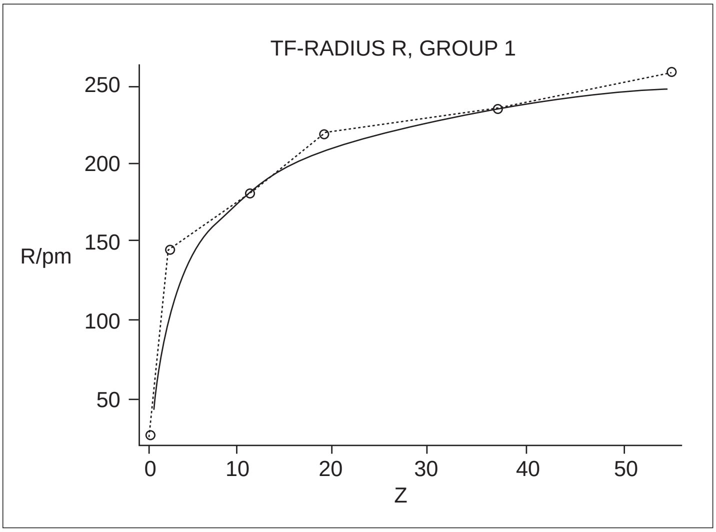

Figure 2
Comparison of Thomas-Fermi radius R with the Slater atomic radii in group 1

may assign to most atoms a radius such that the bond length between any two of these atoms as found in crystals is to very high accuracy equal to the sum of their radii.

In the Thomas-Fermi theory it is also not clear how to define a radius. For the elements in the first group in the periodic table (H, Li, Na, K, Rb, Cs, and Fr) one possibility is to say that the radius is where we find the last (valence) electron. Mathematically this amounts to defining the radius R such that $\int_{|x|>R} d^3x\rho(x) = 1$. Figure 2 shows the comparison of R defined as above calculated in the Thomas-Fermi theory (the solid curve) compared with the data given by Slater (the circled points).

REFERENCES

1. CHANDRASEKHAR S., Phil. Mag. 11, 592 (1931); Astrphys. J. 74, 81 (1931); Monthly Notices Roy. Astron. Soc. 91, 456 (1931).
2. DIRAC P.A.M., *Note on exchange phenomena in the Thomas-Fermi atom*, Proc. Cambridge Philos. Soc. 26, 376{385 (1930).
3. DYSON F.J., LENARD A., *Stability of Matter I and II*, J. Math. Phys. 8, 423-434 (1967); ibid 9, 698-711 (1968).
4. FEFFERMAN C., SECO L.A., *On the Dirac and Schwinger corrections to the ground-state energy of an atom*, Adv. Math. 107 No. 1, 1-185 (1994).
5. FERMI E., *Un metodo statistico per la determinazione di alcune proprietà dell'atomo*, Rend. Accad. Naz. Lincei 6, 602{607 (1927).
6. HILLE E., *On the Thomas-Fermi equation*, Proc. Natl. Acad. Sci. USA 62, 7-10 (1969).
7. HILLE E., *Some aspects of the Thomas-Fermi equation*, J. Anal. Math. 23, 147{170 (1970).
8. HUGHES W., *An atomic energy lower bound that gives Scott's correction*, Adv. Math. 79, 213-270 (1990).
9. KOHN W., *Nobel Lecture: Electronic structure of matter-wave functions and density functionals*, Rev. Mod. Phys. 71 No. 5, 1253-1266 (1999).
10. LIEB E.H., *A lower bound for Coulomb energies*, Phys. Lett. A 70, 444-446 (1979).
11. LIEB E.H., *Thomas-Fermi and related theories*, Rev. Mod. Physics 53 No. 4, 603-642 (1981).
12. LIEB E.H., OXFORD S., *An improved lower bound on the indirect Coulomb energy*, Int. Jour. Quantum Chem. 19, 429-439 (1981).
13. LIEB E.H., SIMON B., *The Thomas-Fermi theory of atoms, molecules and solids*, Adv. Math. 23, No. 1, 22-116 (1977).
14. LIEB E.H., THIRRING W., *Bound for the kinetic energy of fermions which prove the stability of matter*, Phys. Rev. Lett. 35, 687-689 (1975); Errata 35, 1116 (1975).
15. LIEB E.H., THIRRING W., *A bound for the moments of the eigenvalues of the Schrödinger Hamiltonian and their relation to Sobolev inequalities*, in Studies in Mathematical Physics: Essays in honor of Valentine Bargmann, edited by E.H. Lieb, B. Simon, and A.S. Wightman (Princeton University Press, Princeton), 269-303 (1976).

16. Schwinger J., *Thomas-Fermi model: The second correction*, Phys rev A 24, 2353-2361 (1981).
17. Scott J.M.C., *The binding energy of the Thomas-Fermi atom*, Philos. Mag. series 43, 859-867 (1952).
18. Siedentop H., Weikard R., *On the leading energy correction for the statistical model of the atom: interacting case*, Comm. Math. Phys. 112 No. 3, 471-490 (1987); “Upper bound on the ground state energy of atoms that proves Scott’s conjecture”, Phys. Lett. A 120 No. 7, 341-342 (1987).
19. Slater J.C., *Atomic radii in crystals*, Jour. Chem. Phys. 41 No. 10, 3199-3204 (1964).
20. Solovej J.P., *Proof of the ionization conjecture in a reduced Hartree-Fock model*, Inventiones Math. 104, 291-311 (1991); The ionization conjecture in Hartree-Fock theory, Preprint 2001.
21. Teller E., *On the Stability of molecules in the Thomas-Fermi theory*, Rev. Mod. Phys. 34, 627-631 (1962).
22. Thomas L.H., *The calculation of atomic fields*, Proc. Camb. Phil. Soc. 23, 542-548 (1927).

Jan Philip Solovej

Born: June 14, 1961, Copenhagen, Denmark; Education: Cand. Scient. (Masters), University of Copenhagen, 1985; Ph.D., Princeton University, 1989. Employment:1989-90: Visiting Assistant Professor, Dept. of Math., University of Michigan; 1990 (Fall): Post-doctoral fellow, Dept. of Math., University of Toronto; 1991 (Spring): Member, School of Math., Institute for Advanced Study; 1991-1995: Assistant Professor, Dept. of Math., Princeton University; 1995-1997: Research Professor (Forskningsprofessor), Dept. of Math., Aarhus University; 1997-present: Professor, Dept. of Math., University of Copenhagen.

Jeff Hughes

Nuclear Physics at the Cavendish Laboratory in the Thirties

Ernest Rutherford became Professor of Experimental Physics at the Cavendish Laboratory, Cambridge, in 1919. He brought with him a programme of research into radioactivity and the nature of the atom. Contrary to the "sealing wax and string" stories which surround the Cavendish, Rutherford worked with many colleagues and students using sophisticated instrumentation and theories in his quest to map the structure of the nucleus. However his programme faced uncertainty inside the lab and controversy from without. This paper explores some of the hidden history of nuclear physics at the Cavendish and its relations with other laboratories.

La fisica nucleare nel Laboratorio Cavendish negli anni Trenta

Ernest Rutherford divenne Professore di fisica sperimentale del Laboratorio Cavendish a Cambridge nel 1919. Portò con lui un programma di ricerca sulla radioattività e la natura dell'atomo. Contrario alle storie "stringhe e ceralacca" che circondano il Laboratorio Cavendish, Rutherford collaborò con diversi colleghi e studenti al mappaggio della struttura del nucleo con l'ausilio di strumentazioni e teorie sofisticate. Tuttavia il suo programma dovette affrontare incertezze all'interno del Laboratorio e controversie all'esterno. Il mio intervento verterà su alcuni eventi sconosciuti della storia della fisica nucleare nel Laboratorio Cavendish e dei suoi rapporti con altri laboratori.

Introduction

In April 1934, Ernest Rutherford wrote to Enrico Fermi to thank him for a preprint of a paper on artificial radioactivity produced by nuclear bombardment. Having commented on the interest of the the Rome group's results, he added with characteristically wry humour:[1]

> I congratulate you on your escape from the sphere of theoretical phyiscs! You seem to have struck a good line to start with. You may be interested to hear that Professor Dirac is also doing some experiments. This seems to be a good augury for the future of theoretical physics!

Rutherford wrote as the director of the world's leading centre for experimental nuclear physics research: the Cavendish Laboratory, Cambridge. One of the pioneers both of radioactivity and of investigations into the nature of the atomic nucleus, Rutherford was a towering authority in the emergent field of nuclear physics in the early 1930s. Renowned too for his attitude towards speculators and theoreticians, one can only imagine the effect of his letter on Fermi and his colleagues!

Rutherford had trained at the Cavendish under J.J. Thomson in the 1890s, and had returned to succeed his teacher as Professor and director of the laboratory in 1919 after twelve years at Manchester University, where he had both discovered and then disintegrated the nucleus. At Cambridge, he devoted his research to mapping the structure of the nucleus. With a series of gifted collaborators and co-workers using a variety of techniques – including optical scintillation counters, cloud chambers and the mass-spectrograph – the Cavendish of the 1920s and 1930s charted a path towards an understanding of the nucleus. Dominating the field of nuclear disintegration in the 1920s, the Cavendish was the 'Mecca' for nuclear studies. A series of spectacular discoveries in experimental nuclear physics in Cambridge and elsewhere in the early 1930s transformed the field, and after 1932 the Cavendish increasingly interacted with other laboratories and institutes in an atmosphere of competitive internationalism.

By the mid-1930s the Cavendish in nuclear physics was facing serious challenges to its authority, not just from the growth of nuclear physics in Europe and the United States but also within Britain itself. Key members of Rutherford's staff left for other universities where they, too, would establish nuclear physics in competition with Cambridge. More broadly, nuclear

---

[1] Rutherford to Fermi, 23 April 1934, quoted in E. Segrè, *Enrico Fermi: Physicist* (Chicago: University of Chicago, 1970) 74-75 on 75.

physics – which was increasingly *machine* physics – came under criticism within the British physics community as being irrelevant to national economic concerns and the social responsibility of science. The Cavendish found itself on the defensive. In this paper, I aim to explore some of these aspects of nuclear physics at Cambridge in the 1930s, both in the national context of British physics and in the international context of the development of nuclear physics as a discipline. In so doing, I hope to illuminate some of wider context in which the work of Fermi and his group took shape.

The Cavendish Laboratory in the 1920s

Let me begin by outlining some of the 1920s background to the development of nuclear physics. When Rutherford arrived at the Cavendish in 1919, he brought with him his new programme of experimental and theoretical research devoted to the elucidation of the structure of the nucleus. In a 1920 lecture to the Royal Society Rutherford presented his latest findings, in which he pictured the nuclei of a number of the lighter elements as consisting of various arrangements of protons and electrons. He also outlined his manifesto for future nuclear research, in which systematic nuclear disintegration experiments would be used in conjunction with information from F.W. Aston's mass-spectrograph, C.T.R. Wilson's cloud chamber and other instruments to piece together an understanding of the structure of the nucleus.

The key technique in the disintegration experiments was the optical scintillation method, in which sub-atomic fragments resulting from the disintegration of a nucleus strike a zinc sulphide screen causing minute flashes of light or scintillations. When the experiment was carried out in a darkened room, these scintillations could be counted through a microscope, yielding information about what had happened during the experiment. These experiments were very difficult to carry out, however, being laborious, time-consuming, hard on the eyes of the counters and always susceptible to corruption by radioactive contamination, lack of skill or loss of concentration by the counter, or any number of other reasons.

Very particular protocols were therefore required to ensure the integrity of these experiments and of the information which flowed from them. By strict disciplining of the experimental process, James Chadwick, Rutherford's deputy at the Cavendish, was able to achieve a certain degree of confidence in the results of the scintillation counting experiments and the conclusions based on them.

With resources in short supply in the immediate aftermath of the Great War, it had been difficult for laboratories elsewhere to take part in the exciting new programme of nuclear research. From 1923, however, Rutherford and his group became involved in an increasingly bitter dispute over the results of their nuclear disintegration experiments. Hans Pettersson and Gerhard Kirsch, two young researchers at the Institut für Radiumforschung in Vienna, began using the scintillation method to repeat the Cambridge disintegration experiments. They found that they could effect nuclear disintegration far more easily than could researchers in Cambridge. Sharp exchanges in print in *Nature, Naturwissenschaften,* the *Philosophical Magazine,* the *Proceedings of the Physical Society,* the *Zeitschrift für Physik* and elsewhere – nearly 40 papers between 1923 and 1928 – were supplemented by flurries of private correspondence between the two laboratories, in which each side attempted to point out the shortcomings in the other's practices and conclusions. All to no avail: by 1927, the controversy had reached an unpleasant stalemate, with each side claiming the legitimacy and superiority of its own practices and results.[2]

A parallel controversy between Charles Ellis of the Cavendish and Lise Meitner in Berlin over the nature of the β-ray spectrum also cast the experimental foundations of nuclear science in doubt.[3] Though Ellis and Meitner ultimately came to agree about the facts and their interpretation, movement in the Cambridge-Vienna controversy only came in December 1927 when Chadwick himself visited the Institut für Radiumforschung where he was able to show that when Cambridge conditions were imposed, the Cambridge results held. Ironically, though, one of the consequences of the Cambridge-Vienna controversy – and a third controversy with workers at Columbia University, again concerning scintillation counting – was to cast doubt on the reliability of the scintillation method, foundation of much of the previous fifteen years' work in nuclear research. Incidentally, it was the Columbia controversy which led to the introduction of nuclear physics there by George Pegram in the early 1930s, and thereby the research school to which Fermi would migrate in 1938-39.[4]

[2] R.H. Stuewer, "Artificial Disintegration and the Cambridge-Vienna Controversy", in P. Achinstein and O. Hannaway (eds.), *Observation, Experiment and Hypothesis in Modern Physical Science* (Cambridge, Mass. and London: M.I.T. Press, 1985), 239-307.

[3] C. Jensen, *Controversy and Consensus: Nuclear Beta Decay 1911-1934* (Basel: Birkhäuser Verlag, 2000).

[4] J. Hughes, "The Radioactivists. Community, Controversy and the Rise of Nuclear Physics", unpublished Ph.D dissertation, University of Cambridge, 1993, 170-204.

Transforming a discipline: making technology count

The Cavendish emerged from the controversies of the 1920s with its authority more or less intact, and encoded in the 1930 volume *Radiations from Radioactive Substances* by the Cambridge troika of Rutherford, Chadwick and Ellis (this was the book studied by the Rome group as they sought to enter the emerging field of nuclear physics). During the course of the Cambridge-Vienna controversy, it had become increasingly clear that an independent method of carrying out the disintegration experiments would be necessary to escape the regress in which the Cambridge and Vienna workers found themselves. Two key technical strategies took shape in light of this realisation. First, a number of workers in European laboratories – notably Hans Geiger and his students – invested considerable effort in the development of electrical counting methods, at least partly to provide a direct alternative to the disputed scintillation method. Aided by the development of reliable valves and sophisticated electronic circuits in connection with the burgeoning radio industry, electrical counting methods like the Geiger-Müller counter were quickly accepted in nuclear research. Though the new technique had problems of its own at first, it allowed experimenters to count many more particles than the slow, unreliable scintillation method, opening up new possibilities for experimental work. Used in conjunction with a camera, a cloud chamber and a strong magnetic field, for example, electrical counters made possible the development of detectors which could photograph the behaviour of cosmic ray particles automatically.

Second, experimentalists began to consider new ways of providing the projectiles for the disintegration experiments. During the 1920s, attempts to explore the atomic nucleus had been constrained by the kinds of probes which could be used: the alpha-particles emitted at fixed energies by naturally-occurring radioactive substances. In the late 1920s, aided by its excellent connections with the electrical industry (John Cockcroft had come to the Cavendish from the Manchester electrical engineering firm Metropolitan-Vickers, and retained strong links with them), the Cavendish Laboratory led the world in the development of machines to accelerate subatomic particles for use in atom-smashing experiments. Informed by the fresh insights into the behaviour of radiation and matter provided by the new wave mechanics of Schrödinger, Heisenberg and others, physicists began to see the nucleus not simply as a conglomeration of particles but as a complex quantum phenomenon. As quantum mechanics acquired a new legitimacy in the eyes of the experimentalists, the kinds of experiments which could be performed and the ways in which physicists thought about the nucleus both changed significantly in the late 1920s.

By about 1930, then, these transformations of technique in nuclear research were leading to the establishment of a new way of doing physics based on big machines, electrical and photographic detectors and wave mechanics. More than that, the new technologies of research were based on widely distributed skills – the same skills used by thousands of radio hams to construct and modify wireless circuits could be used in the physics laboratory to construct circuitry for an electrical particle counter, for example. This meant that many more researchers could enter the field of nuclear research, so that by 1930 not just Cambridge and Vienna but groups in Berlin, Paris, Halle, Rome, Washington, Berkeley and elsewhere were beginning to get involved in nuclear work. Though some labs were naturally better equipped than others, this expansion of the disciplinary field produced a large increase in the amount of research being done, and threw up sometimes surprising new results which could act as a focus for further investigations. Out of this combination of social and material factors would come the events of 1932 and a new disciplinary label: nuclear physics. And it was a conference organised by the Fermi group in Rome in 1931 – followed by one in London in 1934 – which set the seal on this new disciplinary identity.

From crisis to coherence: the emergence of nuclear physics 1930-1935

One surprising result thrown up in 1930 was the observation by Walther Bothe in Berlin that beryllium, when bombarded with alpha particles from polonium, produced not the expected disintegration protons but an intense form of gamma radiation. This observation was followed up by a number of the other researchers who had entered the emergent field of nuclear physics, among them Irène and Frédéric Joliot-Curie in Paris. Numerous attempts to make sense of Bothe's observation followed, but it was James Chadwick at Cambridge who made systematic experiments on the puzzling new radiation using a cloud chamber and the new electrical counters. In February 1932 he proposed that the radiation in fact consisted of uncharged particles, which he called 'neutrons.' His suggestion was quickly taken up, not least because so many laboratories now had the equipment to repeat Chadwick's results. Within days of Chadwick's suggestion, neutrons were being produced and manipulated in several laboratories. Yet there was disagreement about what exactly the neutron was: experimentalists disagreed about its mass, while theoreticians debated whether it was a proton-electron combination (as Chadwick believed) or a new elementary particle (as subsequent work seemed to show). Crucially, it was the new, widely shared material culture of nuclear physics which both revealed and sustained the neutron; and conversely it was the neutron which cemented together the new community of nuclear physicists by providing them with the shared, unifying focus of attention required for a coherent discipline.

The new technologies also raised difficulties for physicists, however. Where the 1920s had begun with small-scale, table-top experiments, the 1930s saw the development of ever-larger, ever-more powerful atom-smashing machines. Though Cockcroft and Walton were the first to succeed in using high-speed electrically accelerated particles to break an atomic nucleus apart, however, engineers and physicists elsewhere – at Berkeley, MIT and Caltech – were also working hard to produce particle accelerators of different designs, sometimes with the goal of exploring the constitution of the atomic nucleus but sometimes just for the pleasure of working with cutting-edge, large-scale electrical technology. In the early 1930s, these various groups, with their own favoured designs, were competing hard with each other to make their machines stable at the very high energies required for nuclear research. They were also competing to reach higher and higher energies, with the prize of enormous scientific credit likely awarded to the first to succeed.

In this context Cavendish physicists very quickly learned the art of 'spin doctoring' – of working with the media to present the laboratory and its work in the most favourable light possible. Their chosen vehicle was James Crowther, science correspondent of the *Manchester Guardian* (and forty years later the official centenary historian of the Cavendish). An admirer of the Soviet Union, Crowther was a close friend of the Kapitzas and of left-leaning Cavendish physicists. Favoured with inside information on the discovery of the neutron for his press reports in February 1932, Crowther was called in again within weeks to help propagate the Cavendish line on Cockcroft and Walton's work on the disintegration of nuclei using artificially accelerated protons. He gleefully told his editor "I now find I am becoming as if I were the press-agent of the Cavendish Laboratory" and sought his advice on "how I can best exploit this situation".[5] Crowther's help came at an opportune time for Cavendish physicists, for it helped them both to promote themselves and the laboratory and to stave off criticisms about the arcane and possibly irrelevant nature of

[5] J.G. Crowther to W.P. Crozier, 9 May 1932, Box 127, J.G. Crowther papers, University of Sussex.

their work. It also allowed them to control press comment very effectively: Rutherford must have sympathised with Fermi when the latter wrote in 1934 that "We have been forced to publish [the] results of a research which is actually not yet finished by the fact that the newspapers have published so many phantastic statements about our work that we found it necessary to state clearly our point of view".[6] That, at least, was not a problem in Cambridge.

Research at the Cavendish in the 1930s

In the atmosphere of competitive internationalism I have tried to describe, discoveries like that of the neutron, the positron and the deuteron offered new explanatory and exploratory tools to both experimental physicists and the burgeoning number of mathematical theorists. Work done in one laboratory was quickly replicated and pushed forward in others in this new international network, and the Cavendish now found itself as one laboratory among many, often struggling to keep up with the various lines of development. Nevertheless with its concentration of resources and experience it retained its prestige and its authority. One research student in the early 1930s – Harold Miller – was almost overwhelmed by the thought of having to leave this charmed scientific life:[7]

> There is no doubt that my time of close connection with nuclear physics has been the most thrilling since the century began – Recently the new results come out with galloping speed – The Curie-Joliot induced radioactivity and their chemical separation of radio-nitrogen is scarcely old news yet and now the Cockcroft-Walton and Oliphant firms are producing results of astounding interest daily. Cockcroft produces radio-nitrogen. Oliphant finds deutons bombarding deutons give an enormous yield of particles and now today Shire has separated the Lithium isotopes and Oliphant in one day has targets of Li_6 and Li_7 under bombardment by protons and deutons – What a life.

The research and postdoctoral students of course played a major role in the life and work of the laboratory, and felt perhaps more keenly than their seniors the constant quest for priority and credit in the spirit of competitive internationalism. Miller could also note in his diary in February 1934 that: "Today Haxel sends results on aluminium which duplicate ours and a paper

[6] Fermi to Rutherford, 15 June 1934, Rutherford papers.

[7] H. Miller diary, 1 March 1934, H. Miller papers, Sheffield University Library.

appears on magnesium with many energy changes recorded. So I felt sad and decided to have a weekend at home ...".[8] Similarly, Miller recorded the tensions within the laboratory when a senior member of staff took over an investigation from two Ph.D. students: "[Alan Nunn] May and Eric [Duncanson] are in throes of despair now waiting for emanation. They've tried the new Jolio [sic] radioactivity, got an effect from Aluminium and handed over the investigation to Ellis, who has taken all the juice, that's hard luck".[9] The situation may not be entirely unfamiliar today.

Whatever its internal tensions, the Cavendish remained a significant node in the international nuclear physics network. A constant flow of visitors and researchers helped disseminate techniques and ideas between the various laboratories, and the Cavendish maintained its reputation as an international space. According to Miller again:[10]

> Occhialini worked for a few days in our room last week. He's a funny man. He carried on discussions with Chadwick walking up and down the room at a furious rate, eager to talk all the time, Chadwick occasionally trying to pull his leg but getting his own way quietly. He had a companion who also seemed rather whimsical – who talked in a high pitched voice and seemed to be full of boyish enthusiasm whom I found out was Gamov.

Out of the mouths of babes, sucklings and Ph.D. students!

Underpinning this cosmopolitanism, of course, lay Rutherford's enormous authority. President of the Royal Society from 1925 to 1930 and ennobled as Lord Rutherford of Nelson in 1931, he occupied a commanding position not just in nuclear physics but in British science. It was entirely appropriate, then, that when the Fermi group sought to publish in English the results of their work on nuclear transformations by neutrons, Segrè and Amaldi went to Cambridge in the summer of 1934 to present the manuscript to Rutherford, who in turn communicated it to the Royal Society. When Segrè asked whether speedy publication might be possible (competitive internationalism again?), Rutherford replied "What do you think I was the President of the Royal Society for?" and laughed "with great glee"![11]

Rutherford used his authority most effectively to support the Cavendish.

---

8 H. Miller diary, 5 February 1934, Miller papers.

9 H. Miller diary, 29 January 1934, Miller papers.

10 H. Miller diary, 14 January 1934, Miller papers.

11 SEGRÈ, *Enrico Fermi: Physicist* (Chicago: University of Chicago Press, 1970), 77.

For example, he had marshalled a bequest to the Royal Society to fund a large new laboratory for Kapitza, which was on a markedly new scale. One visitor remarked that: “at Professor Kapitza’s laboratory, you [have] to ring to be admitted by a ‘flunkey’ and [are confronted not with men working in their shirt sleeves, but with Prof. Kapitza seated at a table, like the arch criminal in a detective story, only having to press a button to do a gigantic experiment”.[12] Now, in 1934, as it became clear (at least to some) that the Cavendish would need to acquire particle accelerators if it was to keep abreast of the field which it had so long dominated, Rutherford again intervened. He initiated an appeal to raise funds for the construction of accelerators comparable to those being built in the United States. Wealthy “friends of science and of Cambridge”, particularly industrialists, would be invited to give generously in support of the work of the Cavendish to enable it to keep up with developments elsewhere. In a stroke of genius, Rutherford asked Arthur Eddington, Plumian Professor of Astronomy at Cambridge University, a distinguished Fellow of the Royal Society and author of the recent best-seller *The Expanding Universe*, to write a brief account of the Cavendish and the work being done there.[13]

Eddington made a special tour of the laboratory in October 1934, and produced a 17-page booklet for circulation to potential benefactors. In it, he invoked the heritage of the Cavendish, the discoveries of J.J. Thomson and Rutherford in the world of the atom, and asked: “As little can we foresee new worlds of thought, what new control of natural forces will be opened to us by those who in years to come carry on the Cavendish tradition in a new and ampler home?”.[14] He also re-wrote history:[15]

> A period of about twelve months in 1932-1933 was an *annus mirabilis* for experimental physics. For some years previously the centre of advance had been in theoretical physics while experimental physics plodded patiently on. Then in rapid succession came a series of experimental achievements, not only startling in themselves but presenting immense possibilities for further advance. The laboratories of the world are now pressing forward in an orgy of experiment which has left the theoretical physicist gasping – though not entirely mute.

---

12 Cavendish Laboratory archives, Cambridge University Library.

13 On the Cavendish Appeal, see J. Hughes, “1932: the *annus mirabilis* of nuclear physics?” *Physics World*, 13 (7), July 2000, 43-48.

14 A. Eddington, *The Cavendish Laboratory* (Cambridge: Cambridge University Press, 1934), 3.

15 A. Eddington, *The Cavendish Laboratory*, 11.

Here is Eddington inventing what will subsequently become one of the central ornaments of the historiography of nuclear physics – the a*nnus mirabilis* of 1932. Yet he did it because the Cavendish "calls for support, that it may continue in the front rank of scientific institutions, enlarging the frontiers of Man's knowledge, leading his mind into new worlds of thought, and extending his mastery over the forces of nature".[16]

Drift at the Cavendish?: the later 1930s

As it turned out, the Cavendish Appeal was successful, resulting in the 1936 donation of £250,000 from the motor manufacturer Sir Herbert Austin. This allowed the Cavendish physicists to acquire large electrostatic generators, a cyclotron and a new building to house them. Rutherford left the planning of the new buildings and machines to younger members of staff like John Cockcroft and Mark Oliphant. In 1936 Chadwick wrote to Rutherford to congratulate him on the Austin coup, archly observing that "begging, like swindling, is only respectable on a big scale".[17] The award of the 1935 Nobel Physics Prize to Chadwick for his neutron work had been gratifying to all in the Cavendish, but already by the time of the award Chadwick had left Cambridge for his own Chair at Liverpool University. His departure was emblematic of significant change at the Cavendish, for he was one of a number of senior researchers to leave the laboratory at this time. In 1933 the cloud chamber maestro Blackett had left for Birkbeck, London (and went from there to Manchester in 1937); in 1935 Wynn-Williams, the electronics wizard, went to George Thomson's department at Imperial College, London; in 1936 Ellis left for King's College, London, and Oliphant – by this time Rutherford's lieutenant and in many ways his surrogate son and heir – left for a Chair at Birmingham University.

This efflux of talent – and, more importantly, perhaps, experience – from the laboratory had important consequences for the Cavendish. These men of course went on to establish nuclear physics elsewhere in Britain, challenging the Cavendish's national domination of the field. At the same time, there were challenges to the Cavendish from other directions. The growth of new sub-fields in physics – for example solid state physics at Bristol, x-ray crystallography at Manchester and Leeds, and electron diffraction in London – seemed to offer less esoteric and more industrially relevant lines of research.

---

16 EDDINGTON, *The Cavendish Laboratory*, 17.

17 Chadwick to Rutherford, 4 May 1936, Rutherford papers.

Indeed, in the economic context of the mid-1930s Rutherford came under heavy criticism for allowing nuclear physics to dominate at the Cavendish: returning from one high-level meeting of scientific administrators in London, he told Oliphant that "they have been at me again, implying that I am misusing gifted young men in the Cavendish to transform them into scientists chasing useless knowledge".[18]

In some ways the charge was unfair, for other kinds of research were being carried out at the Cavendish. Rutherford supported the development of a large group under Jack Ratcliffe working on wireless and the properties of the ionosphere. Following Kapitza's detention in the Soviet Union during his summer visit there in 1934 the work of the Mond Laboratory continued under Cockcroft, Shoenberg and others. Elsewhere in the Cavendish, Charles Wilson continued his work on thunderstorms, taking him back to the meteorological interests which had originally inspired his invention of the cloud chamber. Geoffrey Taylor continued his researches on fluid dynamics, many of them linked to the government's Aeronautical Research Committee and its military concerns. Some of the research students used their wireless skills to help develop communication systems for the military, and it is telling that in the mid-1930s many Cavendish graduates – even nuclear physics Ph.D.s – were hired by large electronics companies like EMI and Marconi for their specialist skills.

Nevertheless, as the Austin wing took shape and large sums of money were devoted to what many saw as abstruse technology far removed from the practical needs of the nation, the Cavendish was on the defensive. Rutherford himself seems to have had little appetite for the futuristic new developments: he commented, perhaps with a tinge of regret, that "At Cambridge, a great hall contains massive and elaborate machines rising tier on tier", reminiscent of "a photograph in the film of H.G. Wells' 'The Shape of Things to Come'".[19] But as we know, these machines were exactly the shape of things to come.

Conclusion

In October 1937, just as the Cavendish was beginning to adapt to the requirements of high-tech "atom-smashing", Rutherford died unexpectedly after a short illness. Announcing the news at an international conference in Italy in celebration of the two hundredth anniversary of the birth of Luigi

[18] Quoted in M. Oliphant, *Rutherford: Recollections of the Cambridge Days* (Amsterdam: Elsevier, 1972), 146.

[19] Quoted in A. Wood, *The Cavendish Laboratory* (Cambridge: Cambridge University Press, 1946), 48.

Galvani, Niels Bohr was in tears. British physics, and international nuclear physics, had lost an intellectual leader and a powerful spokesman. The lead at the Cavendish passed to the talents of a new generation. In Cambridge, speculation focused on who would succeed Rutherford. Though C.V. Raman privately expressed an interest to one of the electors, Chadwick was widely expected to step into his master's shoes. Yet, perhaps sensitive to criticisms about the dominance of nuclear physics, the electors chose not a nuclear physicist but the x-ray crystallographer Lawrence Bragg to become the fifth Professor of Experimental Physics and Director of the Cavendish Laboratory. The journal *Nature* approved the appointment, noting that the Cavendish was now "so large that no one man can control it all closely", and adding that "Bragg's tact and gift of leadership form the best possible assurance of the happy co-operation of its many groups of research workers".[20] Nevertheless, the place of nuclear physics at Cambridge no longer seemed assured.

By this time, in any case, the threat of war was impacting on the Cavendish and other laboratories. In the spring and summer of 1939 many of the Cavendish physicists were mobilised to work in the defensive radar chain taking shape around the eastern and southern coasts of Britain. Early that year, the laboratories at Liverpool, Birmingham and Oxford were in many ways better placed than the Cavendish to work on aspects of the most recent discovery animating nuclear physicists: nuclear fission.

ACKNOWLEDGMENTS

I am grateful to the Syndics of Cambridge University Library for permission to quote from the Rutherford papers and the Cambridge University Archives; to the University of Sussex for permission to quote from the Crowther papers; and to the University Librarian, University of Sheffield, for permission to quote from the Miller papers.

Jeff Hughes

Studied Chemistry at the University of Oxford and History of Science at the University of Cambridge. A former Research Fellow at Cambridge, he has been Lecturer in History of Science and Technology at the University of Manchester since 1993. His research interests are in the social and cultural history of radioactivity and nuclear physics. He is currently completing books on the discovery of isotopes and on the history of nuclear physics 1918-1940. He was Secretary of the British Society for the History of Science 1995-2000, and also has interests in history of science and the public understanding of science.

[20] "Prof. W.L. Bragg, O.B.E., F.R.S.", *Nature*, 141 (1938), 403.

Michel Pinault

Cooperation and Competition among Nuclear Physics Laboratories during the Thirties: the Role of Frédéric Joliot

My contribution will reach two essential aspects of cooperation and competition among nuclear physics laboratories during the Thirties, a period marked by a strong change of scale in research equipments and by the fast development of American science: on the one hand I will deal with the building by Frédéric Joliot of his first accelerators, when he feared that French physics might be overtaken because of a lack of effective laboratory instruments, and on the other hand I will come back on the beginning of the race to achieve a chain reaction, in 1939-1940, when Joliot, being first reluctant to accept a secret agreement between scientists from western countries, then became strongly involved in the scientists military mobilization. Enrico Fermi and Joliot were then in constant competition and Fermi eventually won, being the first to realise a nuclear pile in Chicago.

Cooperazione e competizione tra i laboratori di fisica nucleare negli anni Trenta: il ruolo di Frédéric Joliot

Il mio intervento verterà su due aspetti essenziali della cooperazione e della competizione tra i Laboratori di Fisica nucleare negli anni Trenta, periodo che vide una considerevole evoluzione della strumentazione scientifica, accompagnato da un veloce sviluppo della scienza negli Stati Uniti. Tratterò della creazione da parte di Frédéric Joliot dei primi acceleratori e del suo timore che la fisica francese potesse rimanere in secondo piano a causa della mancanza di strumentazione valida, ed anche di come ebbe inizio, nel 1939-1940, la competizione per arrivare alla reazione a catena, quando Joliot, inizialmente riluttante a stipulare un accordo segreto con scienziati occidentali, fu progressivamente e intensamente coinvolto nella mobilitazione militare degli scienziati. La competizione senza fine tra Enrico Fermi e Frédéric Joliot vide alla fine la vittoria di Fermi, il primo a realizzare, a Chicago, la pila nucleare.

I am going to evoke two aspects of the relations of cooperation and competition between laboratories of nuclear physics in the Thirties, at a moment when the European scientists were confronted simultaneously with the change of scale in the equipments demanded by their researches and with the fast development of the American science. This paper concerns on the one hand the construction of the first accelerators, a phase during which Frédéric Joliot tried to avoid that French physics would lag behind for lack of successful laboratory equipments, and on the other hand about the start of a run toward the chain reaction, in 1939-1940: Joliot, at first reluctant at the idea of a secret agreement among the scientists of western countries, was then involved determinedly in the war mobilization of the scientists. I'd like to add that Enrico Fermi and Joliot were then in constant competition and that Fermi triumphed by realizing, in Chicago, in 1942, the first nuclear pile.

In 1935, the year of the Nobel prize, Frédéric Joliot, 35-years-old, had been leading for several years an intense activity to acquire new and more sophisticated equipments. As France again distinguished itself in the sector of radioactivity, Joliot did not want to appear as the mere heir of a "Curie tradition" whose limits he knew[1]. He was conscious that physics moved fast and that the English and American science were to take the lead because of the their discoveries and the means they possessed. Actually the French, who had acquired a scientific advance in the field of radioactivity thanks to the sources accumulated in Marie Curie's laboratory, lagged behind the Anglo-Saxon scientists. From his part, Enrico Fermi still worked with traditional means, moreover less powerful than Joliot's, who understood that his success would be strongly conditioned by the quality of help and cooperation he would obtain. Engineer himself, he turned to manufacturers, who were not able of fulfilling his requests, and then to his colleagues abroad. A new scientific community was indeed being born at the beginning of the 30s, with Ernest Lawrence and James Chadwick, of course, but also Merle Tuve and Gregory Breit, Charles Lauristen, Robert de Graaff, John Cockcroft and Ernest Walton, Arno Brasch and Fritz Lange, Wolfgang Gentner, Manne Siegbahn and many others. These builders of laboratories and inventors of equipments, engineers as well as researchers, these engineers-physicists, were going to become "research workers of a rare quality"[2]. They planned to build their

[1] See Michel Pinault, *Frédéric Joliot-Curie,* Odile jacob, Paris, 2000, 712 p.

[2] See J.L. Heilbron and R.W. Seidel, *Lawrence and his laboratory*, University California Press, 1989, t.1, and Jeffrey Hughes, "Interactions and comparisons between France and Britain: Joliot, Chadwick and

laboratories around the instrument. They were going to transform them into workshops, supply them with more and more sophisticated installations, populate them with technicians and workers and set scientific research as a new profession, based on team work. Joliot identified himself with these "technicians-instrumentalists", of whom Aimé Cotton, before him, was one of the first representatives in France.

In June 1932 Joliot, already set forward in this direction, had entered into correspondence with Lawrence. This one had answered in detail his questions on the functioning of its invention, the circular accelerator of particles, thanking him at the same time for sending his recent notes on the neutron. He told him of an electromagnet in Bordeaux that had belonged to a broadcasting station built by American engineers during the First World War. Joliot did not obtain the license to use this installation[3]. In 1933, Joliot had attended with Wolfgang Gentner, who was then a researcher in the Curie Laboratoire, the meeting Paul Scherrer had organised for European experimenters at the Polytechnicum, in Zürich. Gentner and Joliot took advantage of the occasion to visit the Oerlikon company, which made electromagnets. There was there a tradition, an industrial know-how, associated to technical and scientific research, that many experimenters physicists knew. Joliot, having bought there the equipments for his Wilson's cloud chambers, would soon order the electromagnet for his cyclotron, as also Gentner and Scherrer did[4].

In 1933 the Joliot-Curie left the Solvay Council fearing that Lawrence, thanks to his first cyclotron, or Cockcroft with his electrostatic accelerator, would seize their ideas and make important discoveries before they did. Their discovery of artificial radioactivity, some months later, made them aware of the urgency to have these new devices at their disposal[5]. They underlined it, in their note of March 20, 1934, adding that, following their

Blackett", in M. BORDRY and P. RADVANYI (ed.), *Œuvre et engagement de Frédéric Joliot-Curie*, EDP-Sciences, Paris, 2001, p. 153-162.

3 Letter from E. Lawrence to F. Joliot, August 20, 1932, Archive Curie et Joliot-Curie. Letter from the engineer of the PTT to F. Joliot, January 4, 1933, ACJC - F 28. See F. JOLIOT, in M. NAHMIAS, *The cyclotron*, Editions de la Revue d'Optique théorique et instrumentale, Paris, 1945.

4 Interview of Charles Weiner with Wolfgang Gentner, November 15, 1971, p. 42, Archives of the American Institute for Physics. See J.L. HEILBRON, "The First European Cyclotrons", *Rivista di Storia della Scienza*, 1986, 3-1.

5 IRENE CURIE and FRÉDÉRIC JOLIOT, "Un nouveau type de radioactivité", January 15, 1934, *Comptes rendus des sessions de l'Académie des Sciences*, 1934, t.198, p. 254, "Séparation chimique des nouveaux radioéléments émetteurs d'électrons positifs", January 29, 1934, *Comptes rendus des sessions de l'Académie des Sciences*, 1934, t.198, p. 559, "I – Production artificielle d'éléments radioactifs, II – Preuve chimique de la transmutation des éléments", March 20, 1934, *Journal de Physique et Le Radium*, 1934, t. 5, p. 153, in I. and F. JOLIOT-CURIE, *Œuvres scientifiques complètes*, PUF, Paris, 1961.

discovery, Cockcroft, Gilbert and Walton had already begun working with an accelerating tube of protons. From their part, Crane, Lauristen and Lawrence's team announced the discovery, thanks to the use of accelerating tubes, of a dozen new radioelements[6].

Frédéric Joliot then proceeded at the same time in two parallel directions. On the one hand he set, in a vast available premises near Paris, a Van de Graaff high tension electrostatic generator, producing a tension of about 1.2 million volts, coupled with a Lauristen accelerating tube. On the other hand, having discovered an industrial laboratory provided with a high tension apparatus, equipped with a generator of impulses able to reach 3 million volts, he obtained the support of the dean of the Faculty of Science to set there several ions or electrons accelerating tubes. The correspondence, based on the numerous visits made by researchers and foreign technicians, keeps the tracks of the difficulties encountered in setting this installation. In it, Joliot enounced several practical suggestions on the vacuum technique or the choice of materials for electrodes, quite revealing of his doubts[7]. The relations among Joliot and Gentner who, associated to Walter Bothe, had built a Van de Graaff and defended a project of cyclotron in Germany, were then particularly close and without competitiveness. They kept mutually informed about their undertakings. Joliot wrote, in May 1937:

> "I am really very satisfied to see you pursuing splendidly your researches in Heidelberg. It is certain that at present the best production in the field of radioactivity and nuclear physics comes from your laboratory"[8].

Later, when Gentner was in charge, as officer of the German army of occupation, of Joliot's laboratory, at the Collège de France, they maintained a relation based on confidence and scientific cooperation.

But let us return to 1935. Joliot's aim was, as for the installations he tried to set up, to waste no time and try and stay on the run. To carry out his other projects, he should obtain consistent financing. Now, the funds he would have needed exceeded the subsidies of the CNRS itself. Joliot tried to avoid the obstacle by asking the Rockefeller Foundation two million francs, but he did not obtain them[9]. So, in the eventuality of winning the Nobel price

[6] Crane and Lauristen, *Physical Review*, 45, 1934, p. 431 and 49. McHenderson, Livingston and Lawrence, *Physical Review*, 45,1934, p. 428.

[7] ACJC - F 28.

[8] Letter from F. Joliot to W. Gentner, May 3, 1937, AC.JC.

[9] F. Joliot, "Project of creation of a laboratory specialized in the production of new radioelements and their biologic and physico-chemical applications", ACJC.

which would gain him the good graces of the University, in the summer of 1935 he presented to the vice-chancellor of the Academy of Paris its project of the Laboratoire de Synthèse des radioéléments artificiels. And actually, three days after the announcement of the Nobel, the vice-chancellor let him know "that the present moment (was) particularly favourable for the realization of the project"[10].

Another action in favour of Joliot was then taken at the Collège de France. Paul Langevin, holder of the chair of Experimental Physics, explained that it was desirable to promote without delay, as it was the vocation of the Collège de France, "the new science in which France had just become famous"[11]. And so, on June 13, 1936, after the victory of the Popular Front, Joliot's appointment as head of a nuclear chemistry laboratory was announced. The plan to equip it with a cyclotron was carried out without delay, thanks to the political weight of the scientists close to the Président du Conseil, Léon Blum. Also, the setting up of the Laboratoire de Synthèse Atomique, which Joliot provided with different high-tension accelerators, was largely financed by the Blum ministry in which his wife, Irene Curie, was in charge of the quite new sub-ministry of the Scientific Research[12].

Fermi who, according to Michelangelo De Maria, had decided since 1935 to obtain accelerators of particles, asked in 1937, as Joliot had two years earlier, for a 2 million francs subsidy. It amounted to twenty times the annual subsidy of the Consiglio nazionale della ricerca (CNR) of which he received the twentieth. "One notices, he wrote, that all big countries develop artificial sources". He added:

> "It is illusory to envisage an effective competition with the foreigners if one does not find in Italy the means to organize adequate researches".

Fermi had to found a way to be financed by the Istituto superiore di Sanità, connected to the Home Office, and wait for Mussolini to be personally interested in the matter for reasons of nationalist prestige, to be able to carry out his projects[13]. Unlike Fermi, or Bothe and Gentner, who did not succeded

---

[10] Charles Maurain's letter to F. Joliot, November 18, 1935, ACJC.

[11] Assembly of professors, January 26, 1936, Archives of the Collège de France.

[12] Irene Joliot-Curie's letter, as Undersecretary of State of Scientific Research, to Frédéric Joliot, September 17, 1936, AC.JC. See M. PINAULT, "The Joliot-Curie: Science, Politics, Networks", *History and Technology*, 1997, vol. 13, p. 307-324.

[13] MICHELANGELO DE MARIA, "Fermi, A physicist in the storm", *Pour la science*, February-May, 2001, p. 48, and IVANA GAMBARO, "Acceleratori di particelle e laboratori per le alte energie: Roma e Parigi negli anni trenta", in *Rivista di Storia della Scienza*, 2ª serie, giugno, 1993, p. 105-154.

in getting funds for financing their respective projects of cyclotrons by the mussolinian and nazi powers, Joliot thus benefited from a very favourable political situation.

While elaborating his cyclotron project, Joliot got much closer to Lawrence. His personality and Joliot's could certainly favour such sympathy. Lawrence embodied a "boss'style" breaking up with the dusty and stiff affectation of relations based on titles, age and hierarchy, and membership in the "establishment", of the time. The nonconformism and the enterprising mind of the Americans, well embodied by Lawrence, thus appeared to Joliot as a model he was going to confront with more or less consciously. At the same time, and partially thanks to his friendship with Lawrence, Joliot obtained a strong support from the Rockefeller Foundation. So he was allowed to recruit a biologist to work on the radioactive tracers, and financed a stay at the Collège de France for Hugh Paxton, a specialist of the cyclotron, researcher at Lawrence's laboratory, as well as a one year stay in Berkeley for Joliot's assistant, Nahmias. At the same time, Hans von Halban left for Copenhagen, to go to Niels Bohr's laboratory, where the project of a cyclotron was being developed by the the same Foundation with the collaboration – there as well – of one of Lawrence's assistants. A notebook of some pages reports the first trials of Joliot's cyclotron, between June and September 1938, before Paxton's departure for Columbia University. Joliot hoped then, as he wrote to George Pegram, president of Columbia, "to finish the trials" and "to arrive at a result before M. Paxton's departure". Since there still were lots of difficulties, Paxton, from Columbia, proposed some solutions inspired by the cyclotron, just beginning to run. Joliot tried to follow his suggestions but the beam, while allowing to produce radioelements, remained very unstable[14]. The starting on, in January 1939, of research on the chain reaction, made suddenly secondary, for Joliot, the setting up of the cyclotron.

Much has already been said on Leo Szilard's vain attempts, at the beginning of the research on the nuclear chain reaction, to convince his colleagues to establish not a general moratorium on these researches but an agreement

14 "Cyclotron trials", Notebook of laboratory, from June 24, 1938, till September 7, 1938, IPN-Orsay. G.B. Pegram's letter to F. Joliot, from Columbia University, in June 23, 1938, ACJC - F28. Hugh Paxton's letter to F. Joliot, from New York, November 14, 1938. On April 30, 1940, F. Joliot wrote to Irene, *that "the cyclotron worked and very well and (that) it prepared easily rather big quantities of iodine 8 days"* (AC.JC). See Shizue Hinokawa, "Frédéric Joliot-Curie and Cyclotron Development" in *The Journal of Humanities and Sciences*, n° 4, October, 2000, Takushoku University, p. 229-254.

of secrecy of cooperation among the researchers of three or four countries. I would like to add some remarks about the strong evolutions which underwent in the ways of cooperation and competition between laboratories.

From the discovery of the artificial radioactivity, Joliot had worried about the predictable moment when "the researchers, he had declared, will know how to carry out explosive transmutations, real chemical chain reactions". He had then asserted that "a discovery is neither moral nor immoral, it is its employment in fact that it is necessary to judge". In 1936 he declared however that "if society should continue to live according to the current rules, it would be preferable that the men of science do not reveal any more of their discoveries. They will announce them when the world will be better"[15].

Such was his state of mind. From his part, true precursor, Szilard, then emigrated in London, who had gotten interested very early at the idea of releasing nuclear energy, had deposited, on March 12, 1934, a patent to keep a right over his works in case of possible military applications of which he was afraid. At the same time, he hoped to limit to some researchers the information about the discoveries to come, until the risk to discover a new explosive disappeared[16].

But this idea went against the principles, then admitted, of the universality of science and free communication of its results. The international community of radioactivists was a small world of some dozens members. These communicated regularly, exchanging their pupils, observing and commenting each other continuously, and occasionally engaging in a running contest before assembling to confront their views[17]. The spirit of the time of Pierre and Marie Curie had profoundly affected the researchers of the Institut du Radium. They had written:

> "We published, without any reserve, all the results of our researches, as well as the processes of preparation of the radium. We gave, furthermore, to the interested all the information which they asked for"[18].

---

[15] Conference Nobel (December 12, 1935), in F. and I. JOLIOT-CURIE, *Œuvres scientifiques complètes*, op. cit., p. 549-552, and Joliot's conference at the Cercle Peuple et Culture, Grenoble, March 4, 1936, ACJC. See M. PINAULT, "Frédéric Joliot-Curie, chercheur tourmenté", *La Recherche*, n° 335, October, 2000, p. 56-61.

[16] SPENCER WEART, "Scientists with a secret", *Physics today*, February, 1976, 29-2. Leo Szilard's letter to professor Lindemann, at the Clarendon Laboratory of Oxford, June 3, 1935, in S. WEART and G. WEISS, *Leo Szilard, His version of the facts*, PUT Press, on 1980, p. 41.

[17] See DOMINIQUE PESTRE, *Physique et physiciens en France (1918-1940)*, Archives contemporaines, Paris 1984, and DANIEL J. KEVLES, *The physicists, History of a profession which changed the world*, op. cit.

[18] MARIE CURIE, *Pierre Curie*, Denoël, Paris 1955, p. 71.

Szilard's propositions of secrecy were formulated against this spirit and it was natural to accept them. Many researchers, oddly in the French progressive circles, got shaked by the disastrous and lasting divisions of the scientific community, during and after the First World war, and were afraid of its possible renewal.

The issue got moving again in January 1939. Following the discovery of the fission, certain researchers, particularly at the Collège de France, with Joliot, and at Columbia University, with Fermi, started studying the possibilities of activating a chain fission. While being engaged in a relentless competition, they had to face the responsibility they were taking by opening this "Pandora's box". It is well known that, from February 2 1939, Szilard, by then at Columbia, wrote a letter to Joliot suggesting to stop publishing on this subject[19]. But Fermi wrote as well to Joliot, the same day, without mentioning the question. He indicated that he "was engaged as, I think, all the laboratories of Nuclear physics, in trying to understand what takes place in the catastrophale destruction of uranium". At the end of January, French physicists, among whom Fernand Holweck, visited Columbia's cyclotron, not of having any message for Joliot[20]. In several letters Paxton, who had just spent one year in Joliot's laboratory before joining Columbia, did not suggest any particular attitude: on February 12, he explained "the first job that the cyclotroneers here find themselves involved in is the Uranium split business with which half the world seems to be occupied. It seems that Fermi turned Dunning toward this just as soon as the cyclotron gave a beam – which was almost a month ago"[21]. Nothing which could have alerted Joliot and his team aside from an inevitable rivalry with Fermi's group.

When, after the invasion of Czechoslovakia by the Nazi troops, a telegram from Victor Weisskopf indicated that several publications were from then on suspended by their authors, Joliot's answer was quite full of nuances:

> "I certainly do agree with the principle of an agreement, he wrote, but for it to be effective, it would be necessary to spread it among all the labora-

[19] Letter from L. Szilard to F. Joliot, New York, February 2, 1939, ACJC. Szilard became attached to the hypothesis of the chain reaction from 1933 and from this time he dreamed of "the small but real possibility of building an explosive a thousand times more powerful than the common bombs". (S. Weart, art. cit.).

[20] Let us add that a team of physicists led by Joliot to set up experiments in the Exhibition was then in New York. In their mails, they evoked their visits to the team of Columbia's cyclotron (ACJC - F30).

[21] Letter from Hugh Paxton to Ignace Zlotowski, New York, January 29, 1939. Letter from H. Paxton to Maurice Nahmias, from New York, February 12, 1939, ACJC F28.

> tories equipped to handle the matter. I would be grateful to you for announcing these considerations to the American colleagues, whom you can get in touch with”[22].

To Joliot, Szilard and Weisskopf’s idea, although not coming from the leaders of the American nuclear physics, seemed to create more problems than it could solve. Therefore, the secret risked to be aired at once if texts should circulate between both banks of the Atlantic Ocean. Rather than a secret shared between some countries, Joliot seemed to prefer a conspiracy of all the physicists from all countries, decided to hide the secret from all the governments. Now, Szilard and Fermi had already decided to alert the American government and so was also going to do Cockcroft in England.

It would take several months for Joliot to get there.

During the spring and summer of 1939, the teams continued to publish. However, Joliot decided to apply for patents, on behalf of the CNRS, of which one, “Perfection in explosive charges”, remained secret[23]. After the declaration of war, the experiments were called to remain secret. Joliot even decided “to transfer the research project to the War Office”[24]. A French military atomic program, the first one in the world, had been already defined. The issue of secrecy then became a State affair, involving the army and the special services, and the relations with foreign scientists entered this new scenario: on one side the British were allies, with whom closer contacts were taken, on the other the Germans were enemies towards whom effective precautionary measures were taken.

Informed about the German projects of purchasing Norwegian heavy water, Joliot suggested to prevent these deliveries and followed closely the German researchers: “It would be interesting – he wrote – to obtain information on the current activity of these scientists and, in particular, to know if some of them have recently left their laboratories so as to form a team working in a single place under a unique management. Such a group, if con-

---

22 Letter from F. Joliot to L. Szilard, April 19, 1939, A. AIP, Kowarski, 2-14.

23 “Mise en évidence d’une réaction nucléaire en chaîne au sein d’une masse uranifère”, handwritten corrections by Joliot and Kowarski, ACJC - C 8, *Journal de Physique et Le Radium*, October, 1939. A “pli cacheté” was deposited, October 30, 1939, at the Académie des Sciences (published in the CR, November 7, 1949).

24 Kowarski’s manuscript (ACJC), *“Novembre 1939”*. The official version, signed by the three researchers, sent to Raoul Dautry, accompanied with Joliot's letter, is dated February 13, 1940 (Archives of the CEA (Atomic Energy Commission in France) - DRI - F4 / 22-78).

firmed, would constitute a very clear indication of a German effort towards the solution of the problem which interests us”[25].

At the same time, closer contacts were taken with the British researchers. Joliot met Cockcroft several times, as a member of the service of scientific research, in the Ministry of Supply. In December 1939, the latter wrote him:

> “I was amazed by what you and the French nuclear physicists are doing. I suggested (…) that contacts should be organized as quickly as possible among the British and French physicists”[26].

A “confidential mission” arrived in London, on April 10, 1940, just when the scientific committee for the study of the questions of the uranium, the MAUD committee, was to meet for the first time[27]. There, a note probably drafted by Joliot, was read, saying that “the results of the current researches (in France) are held secret” and that “the same should be asked of all other researchers, who, in the allied Countries, are working on the same question”[28].

Meanwhile, after more than a year, Szilard wrote to Joliot, bringing up again the problem of the publications:

> “I have not discussed this matter with anyone else in America since April last year, he strangely wrote, and I don’t know what view others would take if the question was to be raised again. If, however, I should hear from you that in the meantime you have adopted some new policy of delaying publications, I could then perhaps talk to others here and find out what the general feeling is on this subject”[29].

Obviously Szilard seemed to write without having succeeded in obtaining anything, and he also seemed very isolated. Given that for more than six months French scientists had not publish because of the war, this matter did not concern them any more, but only scientists in the United States.

Nevertheless, Szilard had few chances to receive an answer. Joliot had by the time several contacts with the most important American physicists. He

---

[25] Typed note and draft from J. Allier’s hand, Archives Graf-Allier.

[26] Letters from F. Joliot to J.D. Cockcroft, Ministry of Supply, January 17, 1940, and from J.D. Cockcroft to F. Joliot, January 24 and February 18, 1940, ACJC-C 7. J.D. Cockcroft's letter to F. Joliot, December 7, 1939, ACJC - C 7.

[27] In a letter to Paul Montel, responsible for the scientific mission of the Ministry of the Armament, in London, H.J. Gough, director of the British service of the Scientific research, wrote, April 11, 1940: “It was fortunate that Mr Allier arrived one the day which has special meeting was being held to consider the same problem. We have taken of all the questions raised by Mr Allier, and will take the necessary actions”. (A. Graf-Allier).

[28] *“Confidential Note”*, not signed, undated: *“Note handed?? in London”*, id.

[29] Letter from L. Szilard to F. Joliot, April 12, 1940, ACJC - K4b.

indeed put a lot, as vice-president of the Haut comité de coordination des recherches scientifiques (HCCRS), into a now forgotten large-scale action: the organization of an International Congress of Pure and applied Sciences, in September 1940, in New York. Joliot was the inspirer of this project, which financing was assured, in France, by the Ministère de l'Armement. Only the defeat in 1940 prevented him from succeeding. The aim was to organize, in occasion of the International Exhibition of New York in 1940, a congress which would be the continuation of the "Congrès du Palais de la Découverte", organized under the presidency of Jean Perrin and Joliot, during the International Exhibition of Paris in 1937[30]. The leaders of the French scientific community had not then hidden their wish to make their colleagues aware of France's lag in research, particularly for certain new branches. In 1940, Joliot clearly defined identical objectives, at least about the industrial and military applications[31].

The foreign scientists, eager to know more about this surprising project in this restless period, contacted Joliot. Niels Bohr wrote him a fascinating and moving letter, which reveals the actual terms of the scientists' cooperation:

> "Of course, wrote Bohr, it would be wonderful if it was really possible in these critical days to meet and discuss scientific questions of actual interest and the many problems with which scientists are at the moment confronted. But, he added, I felt it difficult to promise my participation before I had more detailed information about the organization of the congress and the character and formulation of the invitation. In fact, I am afraid that an invitation, especially from a permanent committee which at the moment cannot be quite international – thus M. Thomarkin *(the agent in charge of the organization of the congress)* told me that it is their-intention to exclude the countries at war with France or not represented at the New York exhibition (as far as I remember it is not so, however, as he thinks, that Russia is not represented at the exposition) – will involve serious dangers if the whole matter is not handled with the greatest caution. (…) I believe that it is quite essential, not least in order to defend the moral cause of the free nations against mischievous propaganda, that from the very outset all precautions are taken against any possible misunderstandings and that this is made absolutely clear in the formulation of the invitation. I know you understand that I am just as interested as your-

[30] Letter from Jean Perrin to Grover Wahlen, general commissioner of the Exposition of New York, November 30, 1939, ACJC - F 30.

[31] Letter from L. W. Tomarkin, agent in charge of the organization of the congress, to F. Joliot, January 15, 1940, idem.

> self to support the upholding of international scientific relations in these critical days and as little prepared to submit to the pressure to destroy intellectual and scientific freedom. I am only afraid of any step likely to increase the difficulties of reorganizing scientific relations when once again peace is established. Indeed I hope taht such relations – contrary to what happened afler the last war – will this time prove to be a main source of that revivement of the common human spirit, which is the very aim of the present struggle for humanity and freedom. I shall therefore be very thankful if you would write to me quite openly what you and your colleagues in France are thinking of the whole matter"[32].

These issues had already been tackled in Paris. As for the participation of the scientists from non-allied countries, Joliot affirmed that "the Americans should decide on it"[33]. The American committee, chaired by Urey, included 70 leading Americans scientists, among whom the staff of Columbia University – Fermi and his team included – as well as most of the members of the advisory committee of nuclear physicists, just created in June 1940, under Urey's authority. At the request of the State department wishing American neutrality be respected, this committee invited two German scientists, Domagk and Ku, knowing already they would not come, and Otto Warburg, a "non-Aryan", according to the expression of Tomarkin, who accepted the invitation. Of the Russian side, Stern, Vavilov, Frenkel and Ioffé were invited: if Vavilov answered that he wished the success of the congress, the decision to participate lay in the hands of the Academy of Sciences of the USSR. As for Fermi's participation, the Italian ambassador indicated that he "was representing Italian Science in the United States". Three other Italians – Bottazzi, Amaldi and Rondoni – answered affirmatively, and "of their own initiative" included in their delegation Vallauri, Quagliarello and Bergami. Bohr said he could not attend the congress but Hevesy and Madsen would come. Of Switzerland, Pauli and Ruzicka answered affirmatively. Consequently, delegations from the United Kingdom and Scandinavian countries were expected. The congress was going to be really representative, particularly as regarded nuclear physics.

Finally, in the summer of 1940, Frédéric Joliot was appointed responsible, for France, of the scientific war mobilization through the CNRS-A (Applied National Centre for Scientific Research). He had organized several actions

32 Niels Bohr's letter to F. Joliot, February 16, 1940, 3 p, ACJC.

33 Project of report of the "First session of the permanent desk of the congress of pure and applied sciences", February 16, 1940, ACJC - F 30.

aiming at organizing, at the Allies level, the coordination of the scientific communities, first of France and United Kingdom, while waiting for the big forum in New York, in the autumn of 1940. As a physicist, he implemented a strategic program of nuclear researches, which his contacts with the British could accelerate. The meeting of New York, where all the researchers concerned, particularly those in the United States, United Kingdom and France, would be reunited, could even spurr him on to reach another stage in the coordination of the researches. But Joliot did not lose sight of the competition and, until the last hours of the Bataille de France, remained set on the objective to activate the first divergent chain reaction in oxide of uranium and heavy water. If such a success had been obtained before the congress of New York, the ceremony of "Celebration of the fifth anniversary of the discovery of the artificial radioactivity", during which the American committee had planned to hand to Irene and Frédéric Joliot-Curie the Barnard golden medal, awarded by the Academy of the Sciences, would then have acquired much greater significance[34].

Michel Pinault

Michel Pinault is an historian, now working on the scientific research and scientists' milieu in European societies during the XXth century. He disputed, in April 1999, his thesis, at University Paris I-Panthéon Sorbonne, on "Frédéric Joliot, la science et la société - Un itinéraire de la physique nucléaire à la politique nucléaire, 1900-1958". From this thesis a book has been published, *Frédéric Joliot-Curie* (Paris, Odile Jacob, 2000, 712 pages). He took part in the organisation of a conference on "L'Actualité de Frédéric Joliot-Curie", at the Collège de France, in Paris. The proceedings have been published: Oeuvre et engagement de *Frédéric Joliot-Curie*, Paris, EDP-Sciences, 2001, 209 pages.

Michel Pinault is a member of the editorial staff of the journal *Histoire et Sociétés - Revue européenne d'histoire sociale* (20 rue Alexandre Dumas, Paris, 75011). He is permanent professor of history and is currently teaching in a secondary school in France.

[34] L.W. TOMARKIN, "Memorandum, March 19 - April 12, 1940", id.

Ruth Lewin Sime

From Fermi to Fission: Meitner, Hahn and Strassmann in Berlin

After 1934, when Fermi suggested that the first transuranium elements had been produced, the investigation was pursued most intensively by Lise Meitner, a physicist, and the chemists Otto Hahn and Fritz Strassmann in Berlin. Their discovery of nuclear fission in 1938 was a complete surprise, and all the apparent transuranium elements were proved false. My paper focuses on the interdisciplinary nature of the work in Berlin, in particular the prevailing concepts from nuclear physics and chemistry that misguided the investigation for four years but which, in the end, made the fission discovery possible.

Da Fermi alla fissione: Meitner, Hahn e Strassmann a Berlino

Dopo il 1934, quando Fermi ipotizzò che fossero stati prodotti i primi elementi transuranici, la ricerca fu condotta in maniera intensiva a Berlino da Lise Meitner, fisica, e dai chimici Otto Hahn e Fritz Strassmann. La scoperta della fissione nucleare nel 1938 fu una grande sorpresa e tutti gli apparenti elementi transuranici furono provati falsi. Il mio intervento riguarderà la natura interdisciplinare del lavoro condotto a Berlino ed in particolare i concetti preponderanti che avevano fuorviato la ricerca per quattro anni ma che, alla fine, resero possibile la scoperta della fissione.

Uranium nuclei were split in a laboratory for the first time in 1934, here in Rome by Fermi and his group on the Via Panisperna.[1] At the time, no one understood this. Instead, Fermi and scientists everywhere believed they were creating new elements beyond uranium, and for several years the list of these new elements kept growing. When fission was discovered, in 1938, it was a surprise and a shock. It shattered assumptions about nuclear behavior and showed that all the transuranium elements were false: all were fission fragments. We can see the surprise in the events following Fermi's Nobel Prize in December 1938. The prize was awarded to Fermi for his work with neutrons, including the creation of the first transuranium elements. A year before, the Nobel physics committee had considered Fermi, but they were unsure about the transuranics; in 1938 they went ahead.[2] Fermi himself was confident. In his Nobel lecture he even referred to elements 93 and 94 by name: ausonium and hesperium. But at that moment, fission was about to be discovered in Berlin. Barium was identified just before Christmas, and the fission process was understood by New Year's Eve. The transuranium elements were gone. When Fermi sent his Nobel lecture to the printer he added a footnote to that effect, but by then ausonium and hesperium were themselves footnotes in the history of science.[3]

For four years, the world's leading nuclear physicists and radiochemists had been mis-guided by assumptions about nuclear behavior and the chemistry of heavy elements that turned out not to be true. Today I will focus on the science: I want to emphasize the interdisciplinary nature of the research, which involved nuclear physics and chemistry at every stage, difficult experiments and theory that was new. The social context also played a role, of course. For one thing, the work attracted the most prominent scientists in the field: Fermi and his group; then Lise Meitner, Otto Hahn, and Fritz Strassmann in Berlin; and Irène Curie and her coworkers in Paris, among others. Their expertise was essential, but their prominence also seems to have been an inhibiting factor, in that their conclusions were not challenged by younger people, even those with more data and better equipment. There were psychological factors too: each new element was a prize, people want-

[1] Emilio Segrè, *Enrico Fermi: Physicist*, The University of Chicago Press, Chicago (1970).

[2] Elisabeth Crawford, pers. comm., 30 July 2001; Robert Marc Friedman, *The Politics of Excellence: Behind the Nobel Prize in Science*, Henry Holt & Co., New York (2001), p. 248.

[3] Segrè, *Enrico Fermi*, pp. 98-99, 214-221; *Enrico Fermi: Collected Papers/Note e Memorie*, vol. 1, The University of Chicago Press, Chicago/Accademia Nazionale dei Lincei, Roma (1962), pp. 1037-1043.

ed them to be real. And there was a political edge: the Berlin group hoped that the international spotlight would protect them somehow in Nazi Germany. Altogether, these external factors seem to have narrowed the focus to the search for transuranium elements only, keeping people from being alert to the entire range of phenomena before them. This surely delayed the discovery, but still it did not prevent it. In the end, the nuclear physics and chemistry that misled scientists to the false transuranium elements did, finally, also lead them to recognize nuclear fission.

The work began with Fermi in the spring of 1934. He and his group were fairly new to experimental nuclear physics. In 1931 and again in 1932, Franco Rasetti had visited Lise Meitner's lab at the Kaiser Wilhelm Institute for Chemistry to learn nuclear techniques. He was there soon after the discovery of the neutron, wrote some papers, and returned to Rome with experience with neutron sources, counters, cloud chambers, and radioactive substances. In 1934, after Irène and Frédéric Joliot-Curie reported the discovery of artificial radioactivity, Fermi put this together and began systematically bombarding elements with neutrons in an effort to produce artificial radioactive species and new nuclear reactions. We know that Lise Meitner received their preprints from *Ricerca Scientifica*, repeated their experiments, and verified their results. Her interest was intense, and that was true of nuclear physicists everywhere.[4]

When Fermi and his group reached uranium and found several new activities, he cautiously proposed that the uranium nucleus had captured a neutron and begun a sequence of beta decays, producing element 93 and 94 – the first artificial elements. This was sensational news. One Italian newspaper hailed it as an example of Italy's restored scientific grandeur under the Fascists, another wrote that Fermi presented a vial of element 93 to the Queen of Italy.[5] Such stories ranged from dubious to completely untrue. But scientists were also fascinated.

At this point, in the summer of 1934, Meitner asked Otto Hahn to join her for their first collaboration in many years; she realized that "one could not get ahead…with physics alone; an outstanding chemist like Otto was needed to get results". Hahn joined Meitner late in 1934. They made a formidable

4 RUTH LEWIN SIME, *Lise Meitner: A Life in Physics*, University of California Press, Berkeley (1996), p. 161-163; SEGRÈ, *Enrico Fermi*, pp. 68, 73; Rasetti to Meitner, 18 March 1933.

5 LAURA FERMI, *Atoms in the Family: My Life with Enrico Fermi*, The University of Chicago Press, Chicago (1954), p. 91.

team, which got stronger when Strassmann, a young analytical chemist, joined them in 1935.[6] The uranium investigation had shifted from Rome to Berlin.

The search for the transuranium elements was framed from the start by two guiding assumptions, from physics and from chemistry. Physicists had always observed that nuclei were quite stable: when nuclear reactions did occur, the changes were small. Fermi's neutron results were consistent with this. With light elements, he found that a neutron might knock out a proton or an alpha particle, but nothing bigger. With heavier elements, the reaction was always neutron capture, followed by beta decay to the next higher element. So it was reasonable to assume that the new beta activities from uranium would be elements beyond uranium.

And there was theoretical support for the idea of small nuclear changes. In 1928 George Gamow formulated a successful theory of alpha decay, in which the nucleus is quantized and only small particles – protons or alpha particles – had a finite probability of escaping. That year Gamow proposed also another theory, in which subnuclear particles are bound together like molecules in a drop of water. The liquid-drop theory accounted for nuclear stability and the known nuclear mass defects. In the mid-1930s Niels Bohr and Fritz Kalckar developed the theory of the compound nucleus, also based on a liquid drop, which was useful for nuclear reactions.[7] No theory predicted, and no physicist imagined, anything as disruptive as nuclear fission.

The chemists also contributed a false assumption in predicting the expected chemical behavior of the transuranium elements. We now know that the actinides, including uranium, are homologous to the rare-earth elements, but in the 1920s and 1930s, uranium was considered a transition element (figure 1)[8] and so the elements beyond uranium were also expected to be transition elements. Like the physics assumption, the chemistry prediction was inductive: the known elements up to U are chemically very similar to the transition elements above them so it was assumed that the elements beyond U would have the chemistry of Re, Os, Ir, Pt, etc.

It is interesting to note that at the time there were some questions about the placement of these elements. In the early 1920's, Bohr had established the relationship between chemical behavior, periodicity, and electronic struc-

---

6 Sime, *Lise Meitner,* chapter 7.

7 Roger H. Stuewer, *The Origin of the Liquid-Drop Model and the Interpretation of Nuclear Fission*, Perspectives on Science, 2 (1994), pp. 76-129.

8 J. W. van Spronsen, *The Periodic System of Chemical Elements: A History of the First Hundred Years* Elsevier, Amsterdam (1969), p. 160.

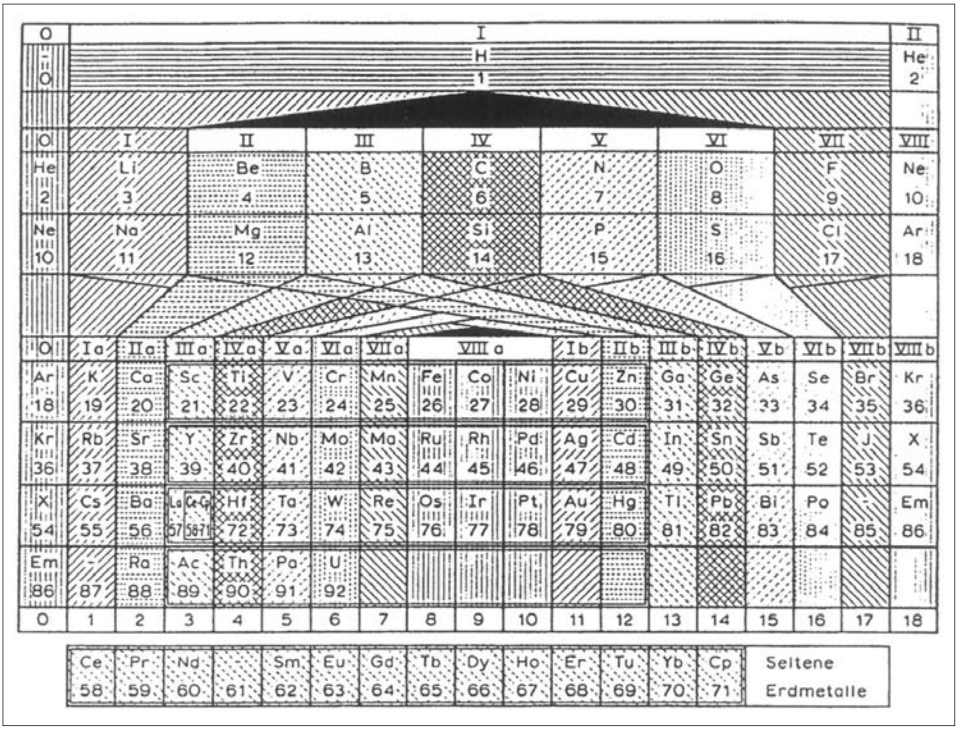

Figure 1
Periodic system of von Antropoff, 1920s and 1930s. The lanthanides, or rare-earth elements, were grouped separately, but Th, Pa, and U were ciassified as transition elements

Source: Spronsen, Periodic System (note 9).

ture. One of his great successes was his proposal of a 4*f* sublevel, which incorporated and correctly placed the rare earths into the periodic table. Bohr also predicted the existence of a second rare-earth series in a 5*f* sublevel, but he could not predict where the 5*f* would begin. In his table (figure 2)[9] the box with the dotted lines places the second group of rare-earths somewhere beyond uranium. Bohr did this because the spectroscopic data were inconclusive and the chemical evidence for uranium as a transition element was very strong.

Nevertheless, the start of the 5*f* series was still an open question.[10] But the chemists who were searching for transuranium elements simply regarded uranium as a transition element and extrapolated from there. Hahn and

[9] SPRONSEN, p. 156.
[10] SPRONSEN, pp. 317-320.

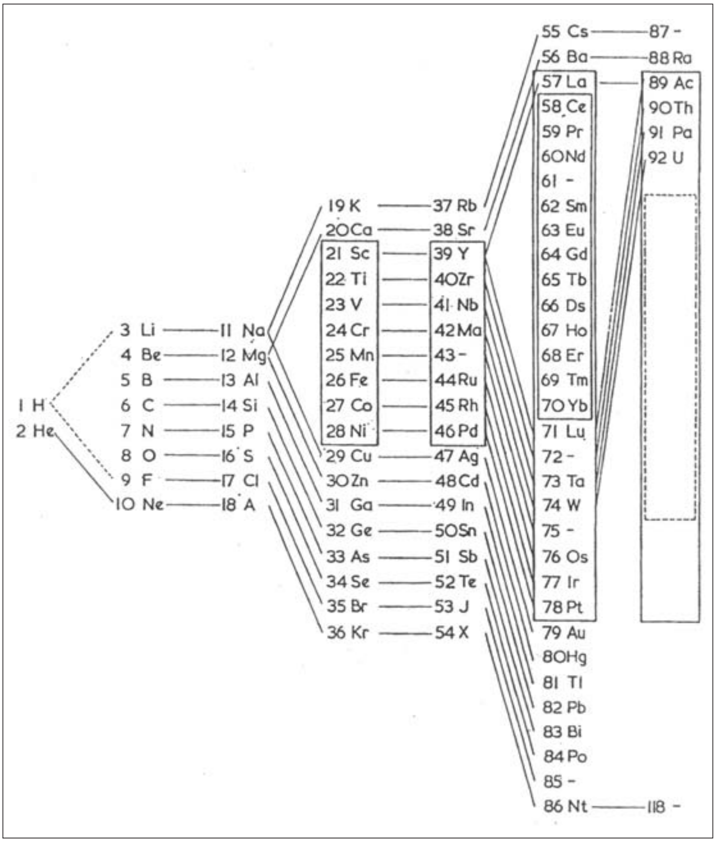

Figure 2
Bohr's periodic system, 1922. Bohr predicted that the second rare-earth group (dotted lines) would begin somewhere after uranium

Source: Spronsen, Periodic System (note 9).

Meitner may have been especially inclined to do so because their discovery of protactinium in 1918 was based on its chemical similarity to tantalum.[11]

Uranium was doubly deceiving: it behaves like a transition element although it isn't one, and its nucleus appears to be stable even though it is on the verge of disintegrating explosively. This was bad luck, the more so because the false assumptions from nuclear physics dovetailed with those from chemistry. The only public challenge came from Ida Noddack in 1934. Noddack was an inorganic chemist and the co-discoverer of rhenium. She questioned Fermi's chemical separations and noted that no one had excluded the possibility of the uranium nucleus breaking into large pieces.[12] Much

[11] For protactinium, see SIME, *Lise Meitner*, chapter 3.

[12] IDA NODDACK, "Über das Element 93", *Zeitschrift für Angewandte Chemie*, 47 (1934), pp. 653-655.

later Edoardo Amaldi speculated on why her suggestion was not looked into, but he had little explanation.[13] According to Emilio Segrè, Fermi later remembered that the mass defect data was misleading,[14] but the curve from 1935 (figure 3),[15] has the familiar minimum, suggesting that it is energetically feasible for large nuclei like uranium to split in two. No one saw the implications of it. It seems that physicists regarded the nucleus as a stable unit and that was that.[16]

For much of the investigation, the experimental approach was constrained by the limits of the radiochemistry. The neutron sources were weak, and so the new beta activities were not much stronger than the natural radioactivity from uranium and its decay products. Fermi chemically separated the new activities from uranium by precipitating them with transition metal compounds, which supported the notion that these were transuranium elements. This approach also structured the investigation in Berlin. In early 1935, Hahn, Meitner, and Strassmann improved Fermi's separation and began disentangling the activities in the precipitate. For the next three years, with few

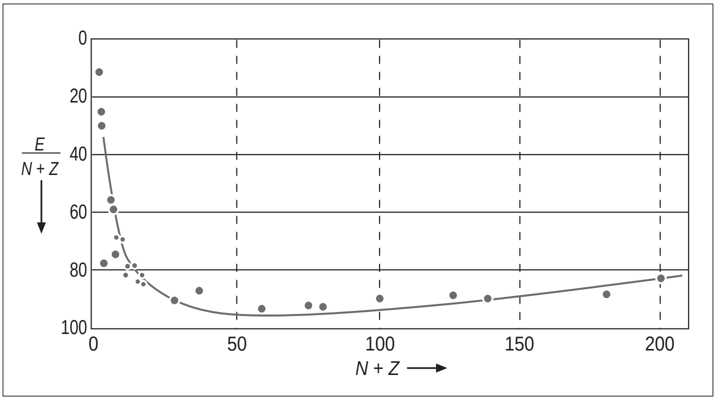

Figure 3
Mass defect curve, 1935. The dots represent Aston's mass spectrographic data. The semi-empirical curve calculated by Carl Friedrich von Weizsäcker is based on Gamow's liquid-drop theory

Source: Stuewer, "Origin of the Liquid-Drop Model", p. 96 (note 8).

---

[13] E. AMALDI, "From the discovery of the neutron to the discovery of nuclear fission", p. 277 in *Physics Reports*, 111, pp. 1-332. Amaldi noted that the Fermi group was scientifically conservative and thus reluctant to consider something entirely new, and he noted that Noddack herself did not pursue it. See also SIME, *Lise Meitner*, pp. 271-273.

[14] SEGRÈ, *Enrico Fermi*, p. 76.

[15] STUEWER, p. 92, 96.

[16] SPENCER R. WEART, "The Discovery of Fission and a Nuclear Physics Paradigm", pp. 103-104 in WILLIAM R. SHEA, ed., *Otto Hahn and the Rise of Nuclear Physics*, D. Reidel Publishing Co., Dordrecht (1983), pp. 91-133. "One still imagined the nucleus as a unit…Theory served best when it simply suggested that the experiments were getting into something unexplained."

1. U + n [fast/thermal] → $_{92}$U (10″) → $_{93}$EkaRe (2.2′) → $_{94}$EkaOs (59′) → $_{95}$EkaIr (66h) → $_{96}$EkaPt (2.5h) → $_{97}$EkaAu?

2. U + n [fast/thermal] → $_{92}$U (40″) → $_{93}$EkaRe (16′) → $_{94}$EkaOs (5.7h) → $_{95}$EkaIr?

3. U + n [slow] → $_{92}$U (23′) → $_{93}$EkaRe?

Figure 4
The "transuranium" elements, 1937. In 1937 Meitner, Hahn, and Strassmann assigned the radioactive species they found to three different reaction processes. In processes 1 and 2, the sequence of beta decays was assigned to elements 93, 94, etc., ali with mass 239 (Half-lives are in parentheses; EkaRe denotes the expected position of element 93 below Re, etc.). Later it was recognized that processes 1 and 2 are in fact fission processes, but process 3 was correctly interpreted at the time as a typical resonance capture of slow (25 ev) neutrons to form ^{239}U, which in 1940 was shown to produce element 93 (^{239}Np)

Source: Meitner, Hahn, and Strassmann, "Umwandlungsreihen" (note 17)

exceptions, they worked on the precipitate and ignored the filtrate, which contained uranium, its decay products, and quite a lot more.

We remember that fission involves a long sequence of beta decays; parallel sequences of isotopes; and elements from all groups in the periodic table, including transition elements. By 1937, the Berlin group had assembled their findings into three processes (figure 4):[17]

- in process 1, it appeared that U-238 captured fast or thermal neutrons, followed by a sequence of beta emitters, which they assigned to elements 93, 94, etc.
- process 2 is parallel to process 1, the same elements but different half-lives.
- process 3 is clearly different. Here, U-238 captures only slow neutrons, and there are no further beta decays.

Later, after the discovery of fission, it became clear that only process 3 was what it appeared to be. Processes 1 and 2 result from fission. From all the possible fission products, the scientists were selecting out just those with the chemistry of transition elements, the ones they thought they were looking for.

This diagram represents the combined efforts of radiochemistry and physics. The chemists were very confident. Hahn repeatedly wrote that there could be "no doubt" that these were transuranium elements: the genetic sequences (93 decaying to 94 and so on) fit the expected chemistry of EkaRe, EkaOs, etc.,

[17] L. Meitner, O. Hahn, and F. Strassmann, "Über die Umwandlungsreihen des Urans, die durch Neutronenbestrahlung erzeugt werden", *Zeitschrift für Physik*, 106 (1937), pp. 249-270.

so well it seemed it just had to be true. Meitner measured reaction cross-sections, neutron energies, and irradiation conditions; as the physicist she was responsible for interpreting all the results. And here there were problems. How could just one isotope, U-238, be the starting point for three different processes? Why did the capture of just one neutron create such great instability that it took many beta decays to alleviate it? How to explain triple isomerism and worse, the *inherited* isomerism of processes 1 and 2?

Meitner knew that process 3 was the most normal: a resonance capture of slow neutrons to form U-239, which was chemically identified as uranium and which necessarily decays to element 93. If the Berlin team had detected this "93" and determined its chemical properties, they would have known that the "93" in processes 1 and 2 were not right. But they didn't do it. Their neutron sources were too weak and, as Hahn later wrote, they were not very interested:[18] after all, they already had found several transuranium elements and that, of course, was what they were looking for.

At the time no one contested this, although everyone was aware of the problems.[19] Irène Curie and her co-workers in Paris were the Berlin team's chief competitors, and they verified it. Some physicists tried to find reaction mechanisms physically but they covered their ionization chambers to screen out the natural decay of uranium and never detected the large ionization bursts from fission fragments. In Berkeley, Philip Abelson used the cyclotron as a neutron source – it was orders of magnitude more intense than the ones in Europe and gave far more activities, but he too verified the Berlin results. Later he attributed it to the "high reputation and prestige" of the Fermi group.[20] Glenn Seaborg, also at Berkeley, regarded Hahn's 1933 book on *Applied Radiochemistry* as his bible; he avidly followed the Berlin publications and accepted the results.[21] And in 1938 Lawrence Quill discussed the difficulties in a 70-page article in *Chemical Reviews*, but he did not question processes 1 and 2. Instead, he agreed that the elements up to 97 were transition elements, and that the 5*f* sublevel would not begin before element 98.[22]

---

[18] OTTO HAHN, *A Scientific Autobiography*, Willy Ley, transl. and ed., MacGibbon & Kee, London (1967), p. 175.

[19] For overview, see SIME, *Lise Meitner*, chapter 6.

[20] PHILIP H. ABELSON, "Discovery of Neptunium", pp. 51-53 in L. R. MORSS and J. FUGER, eds., *Transuranium Elements: A Half Century*, American Chemical Society, Washington, D.C. (1992), pp. 50-55.

[21] GLENN T. SEABORG, *Nuclear Milestones*, W. H. Freeman and Company, San Francisco (1972), p. 5; SEABORG in HAHN, *Autobiography*, p. ix.

[22] LAWRENCE L. QUILL, "The Transuranium Elements", *Chemical Reviews*, 23 (1938), pp. 87-155.

Later, after the fission discovery, Hahn would say that physics had misled the investigation by insisting on small nuclear changes; he never acknowledged the mistaken assumptions of chemistry.[23] I have shown elsewhere that Hahn had political motives for separating himself from Lise Meitner and claiming fission for chemistry. And one can argue that the limiting factor in Berlin was the radiochemistry: they separated out the precipitate with the supposed transuranics, and almost never looked at the filtrate.

In an interview in 1963, Meitner said: "I really think our misfortune was that we didn't search the filtrate... The chemists absolutely didn't want to. I pestered them to do it while I was there because I was so disturbed by it".[24]

The breakthrough came from Paris, where Irène Curie had devised a method for measuring the uranium activities *without* separation. Early in 1938, she and Pavel Savitch reported a strong new activity with uncertain chemistry. By the time Hahn and Strassmann looked into it, it was October 1938. Meitner, who was of Jewish origin, had escaped from Germany a few months before and had gone to Stockholm, but she and Hahn corresponded constantly. Hahn and Strassmann separated the Curie activity and decided it was an isotope of radium because it followed a barium carrier (figure 1). The reaction conditions were the same as in Processes 1 and 2, and again there were several isomers.

It is at this point that nuclear physics and radiochemistry were able to solve the problem. It happened because chemists were now in familiar territory, working with elements whose chemistry and radiochemistry were known. When their findings conflicted with the physicists' assumption of small changes, the discrepancy was obvious and could be resolved.

We know from Meitner's letters that she was doubtful about the radium result. From theoretical considerations she and other physicists were convinced that slow neutrons could not make uranium eject even one alpha particle – and certainly not two. In November 1938 Meitner met Hahn in Copenhagen and, according to Strassmann, she "urgently requested" that they scrutinize the radium very intensively one more time.[25] In Berlin, Hahn and Strassmann began new experiments that led directly to the finding of barium a few weeks later.

---

[23] Sime, *Lise Meitner*, chapters 10, 11, 12.

[24] Lise Meitner interview by Thomas Kuhn, 12 May 1963: American Institute of Physics Oral History Project, Tape 65a, transcript pp. 19-20.

[25] Fritz Krafft, *Im Schatten der Sensation: Leben und Wirken von Fritz Strassmann*, Verlag Chemie, Weinheim (1981), pp. 208, 210.

To verify the radium, Hahn and Strassmann used Marie Curie's method of fractional crystallization, a classic radiochemical procedure, to separate the radium from its barium carrier. When there was no separation, they knew that their "radium" was barium. Hahn informed Meitner about the barium, but he was mystified and asked her "for some sort of fantastic explanation". Meitner responded instantly: "A major breakup seems very difficult to me...but one cannot unconditionally say: it is impossible".[26] Within a week she and her nephew Otto Frisch, also a physicist, devised the first theoretical interpretation of the fission process, calculated the energy released, understood that the transuranium elements were fission fragments, and realized that only process 3 led to element 93.

The barium was reported by the chemists,[27] and the theory by the physicists[28] – separately, in different journals, in different languages. I have argued that this separation was artificial and unjust, the result of Meitner's forced emigration and the politics of the time. The separation did not reflect the science: it excluded physics from the discovery of barium. This artificial separation was reinforced when Hahn later denied the role of physics and of Lise Meitner, and it was reinforced further by a Nobel Prize – a badly mistaken Nobel prize – in chemistry that went to Hahn, alone.[29]

As a postscript, let me note that once fission was discovered, the interdependence of nuclear physicists and chemists was essentially over. The chemists were left with nothing but fission fragments – the false transuranics that had inspired such confidence turned out to be a messy mixture of light elements from all over the periodic table.[30] Actually the chemists were left with less than nothing, since they still expected transuranium elements to be transition elements, and this prevented them from detecting the real element 93 for more than a year.[31] In 1940 McMillan and Abelson found this 93

---

[26] SIME, *Lise Meitner*, pp. 233, 235.

[27] O. HAHN and F. STRASSMANN, "Über den Nachweis und das Verhalten der bei der Bestrahlung des Urans mittels Neutronen entstehenden Erdalkalimetalle", *Naturwissenschaften*, 27 (1939), pp. 11-15.

[28] L. MEITNER and O. R. FRISCH, "Disintegration of Uranium by Neutrons: A New Type of Nuclear Reaction", *Nature*, 143 (1939) pp. 239-240.

[29] ELISABETH CRAWFORD, RUTH LEWIN SIME, MARK WALKER, "A Nobel Tale of Wartime Injustice", *Nature*, 143 (1996), pp. 393-396; "A Nobel Tale of Postwar Injustice", Physics Today, 50:9 (September 1997), pp. 26-33; Friedman, *Politics of Excellence*, pp. 232-250.

[30] H. MENKE and G. HERRMANN, "Was waren die 'Transurane' der dreißiger Jahre in Wirklichkeit?", Radiochimica Acta, 16 (1971), pp. 119-123.

[31] EMILIO SEGRÈ, *A Mind Always in Motion: The Autobiography of Emilio Segrè*, University of California Press, Berkeley (1993), pp. 152-153.

(neptunium) and showed that its chemistry was more like uranium than rhenium, the first evidence of the 5*f* series.[32]

Fission belonged to nuclear physics, and it took off rapidly, with remarkable developments in experiment and theory. In contrast to the chemistry, the physics data of the previous years was entirely valid. For example, Niels Bohr immediately used the reaction cross-sections that Meitner had measured in 1937 to deduce that the fissile isotope of uranium was U-235 and not U-238. Fermi, at Columbia, was instantly involved as well, all his experience with neutrons coming into play. Now we have returned to Fermi and I will end my talk, since I know this next period in Fermi's work will be covered by other speakers.

Ruth Lewin Sime
A native of New York City who taught undergraduate chemistry at Sacramento City College for many years, she has been concerned with attracting women and minorities to the physical sciences. Sime's interest in history of science began when she taught a women science course and discovered that surprisingly little was known of Lise Meitner's life and work. Her biography "Lise Meitner: A Life in Physics" appeared in 1996 and has been translated into several languages. Recently Sime retired from teaching to work on a study of Meitner's colleagues Otto Hahn and Max von Laue during the National Socialist years and the postwar period.

[32] Glenn T. Seaborg and Walter T. Loveland, *The Elements Beyond Uranium*, John Wiley & Sons, New York (1990), pp. 8-11, 65ff.; Abelson, "Neptunium", pp. 53-55.

Ugo Amaldi

Slow Neutrons at Via Panisperna: the Discovery, the Production of Isotopes and the Birth of Nuclear Medicine

The paper sent to La Ricerca Scientifica in March 1934 by Enrico Fermi titled "Radioattività indotta dal bombardamento di neutroni - I" was the first of a long series published on the same subject with his collaborators E. Amaldi, O. D'Agostino, B. Pontecorvo, F. Rasetti and G. Segrè. These papers describe the production of about fifty new artificial radioactive isotopes and contain four major discoveries. The first part of this contribution describes the first two of them: the initial observation by Fermi and the discovery in October 1934 of the very large effects produced by slow neutrons. The second part addresses a non-scientific question: why that day Fermi suddenly decided to place, between the neutron source and the material to be bombarded with neutrons, a piece of paraffin instead of the lead block he was machining? With the help of a recent paper by Alberto De Gregorio, one can guess the unconscious thoughts that may have induced Fermi to make the move that brought to the second discovery. The third part discusses the actions that went on in Rome from 1935 to 1938 to secure the production of radioactive isotopes to be used in medicine. In these developments – together with Fermi, Rasetti and Amaldi – two figures are most important: Giulio Cesare Trabacchi and Domenico Marotta, the leaders of the physics and chemistry laboratory of the Istituto di Sanità Pubblica. The presentation is focused on the proposal made by Marotta, with the support of Fermi, of the construction of the 1 MeV Cockcroft-Walton electrostatic accelerator and the discovery, made in Palermo in 1937 by Segrè and Perrier of technetium, the element that is used in 90% of all modern nuclear medicine examinations.

Neutroni lenti a via Panisperna: la scoperta e la produzione degli isotopi e la nascita della medicina nucleare

La relazione inviata da Enrico Fermi alla Ricerca Scientifica *nel marzo del 1934, intitolata "La radioattività indotta dal bombardamento di neutroni", fu la prima di una lunga serie di pubblicazioni sulle stesse tematiche, realizzate con la collaborazione di E. Amaldi, O. D'Agostino, B. Pontecorvo, F. Rasetti e G. Segrè.*

Queste pubblicazioni descrivono la creazione di circa cinquanta nuovi isotopi radioattivi artificiali e contengono quattro importantissime scoperte. La prima parte della relazione riguarda le prime due, e riporta le osservazioni iniziali fatte da Enrico Fermi e la scoperta, nell'ottobre 1934, degli imponenti effetti provocati dai neutroni lenti, mentre la seconda affronta un importante quesito scientifico, ovvero cosa spinse Enrico Fermi ad apporre, tra la sorgente neutronica ed il materiale da bombardare, un pezzo di paraffina anziché il blocco di piombo al quale stava lavorando. Una recente pubblicazione di Alberto De Gregorio può essere d'ausilio alla comprensione dei processi inconsci che potrebbero aver condotto Fermi alla seconda scoperta. La terza parte prende in esame l'arco di tempo che va dal 1935 al 1938, nel quale il gruppo romano concentrò la sua ricerca sulla produzione di isotopi radioattivi a scopi medici. Due nomi risaltano particolarmente, assieme a quelli di Fermi, Rasetti ed Amaldi: quelli di Giulio Cesare Trabacchi e Domenico Marotta, direttori dei laboratori di fisica e di chimica dell'Istituto di Sanità Pubblica. Questo contributo discute la proposta, fatta da Marotta e sostenuta da Fermi, della costruzione dell'acceleratore elettrostatico Cockcroft-Walton da 1 MeV e sulla scoperta fatta a Palermo nel 1937, da Segrè e Perrier, del technetium, l'elemento impiegato nel 90% delle moderne diagnostiche mediche.

Artificial radioactivity produced by neutrons

The long stream of experiments initiated by the first paper on neutron radioactivity, published by Enrico Fermi in March 1934, led to four major discoveries:

(i) the radioactivity induced by neutrons,
(ii) the radioactivity induced by neutrons slowed down by collisions with light nuclei, in particular hydrogen,
(iii) the law of the inverse of the velocity, with which slow neutrons are absorbed in nuclei with the emission of gamma rays,
(iv) the existence of strong selective absorption bands and the effect of chemical bonds on the phenomenon.

In the first part of this contribution I will discuss only the first two discoveries, so as to devote the second part to the much less known parallel engagement of Fermi and collaborators in trying to secure the abundant production of new isotopes for medical and industrial applications.

The first two papers on the discovery of the radioactivity induced by neu-

Figure 1
Enrico Fermi with his daughter Nella in 1931

trons are signed by Fermi [1]. Already in the second, one he acknowledges the contribution of Amaldi and Segrè in carrying out the experiment. How this happened is described by Edoardo Amaldi in a Physics Report published in 1984, exactly fifty years after the events [2]. In a Section that bears the title 'Fermi's discovery' he writes [3]:

> "After the papers of Joliot and Curie were read in Rome, Fermi, at the beginning of March 1934, suggested to Rasetti that they should try to observe similar effects with neutrons by using the Po_a+ Be source prepared by Rasetti. About two weeks later several elements were irradiated and tested for activity by means of a thin-walled Geiger-Müller counter but the results were negative due to lack of intensity".
>
> "Then Rasetti left for Morocco for a vacation while Fermi continued the experiments. The idea then occurred to Fermi that in order to observe a neutron induced activity it was not necessary to use a Po_α + Be source. A much stronger Rn_α + Be source could be employed, since its beta and gamma radiations (absent in Po_α + Be sources) were no objection to the observation of a delayed effect. Radon sources were familiar to Fermi since they had been supplied previously by Professor G.C. Trabacchi (of the Laboratorio Fisico dell'Istituto di Sanità Pubblica) for use with the gamma-ray spectrometer".
>
> "All one had to do was to prepare a similar source consisting of a glass bulb filled with beryllium powder and radon. When Fermi had his stronger neutron source (about 30 millicurie of Rn) he systematically bombarded the elements in order of increasing atomic number, starting from hydrogen and following with lithium, beryllium, boron, carbon, nitrogen and oxygen, all with negative results. Finally, he was successful in obtaining a few counts on his Geiger-Müller counter when he bombarded fluorine and alumini-

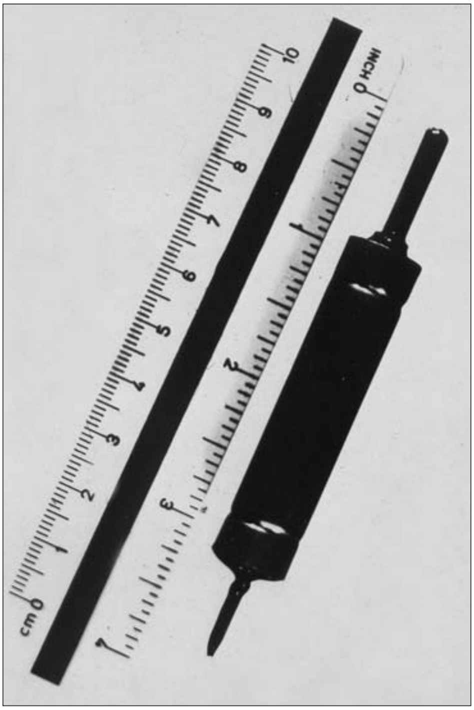

Figure 2
One of the Roman Geiger counters, made by Edoardo Amaldi from an aluminium pill box

um. These results and their interpretation in terms of (n,(γ) reaction were announced in a letter to *Ricerca Scientifica* on March 25, 1934. The title: *Radioattività indotta da bombardamento di neutroni* – I indicated his intention to start a systematic study of the phenomenon which would have brought to the publication of a series of similar papers".

"Fermi wanted to proceed with the work as quick as possible and therefore asked Segrè and me to help him with the experiments, as it appears also from the acknowledgement at the end of his second Letter to the Editor of the *Ricerca Scientifica* where he reported preliminary results obtained in a number of other elements (Si, P, Cl, Fe, V, Cu, As, Ag, Te, Cr, Ba)".

"A cable was sent to Rasetti asking him to come back from his vacation. The work immediately was organised in a very efficient way. Fermi, helped a few days later by Rasetti, did a good part of the measurements and calculations, Segrè secured the substances to be irradiated and the necessary equipment and later was involved in most of the chemical work. I took care in the construction of the Geiger-Müller counters and of what we now call electronics. The division of the activities, however, was not rigid at all and each of us participated in all phases of the work. We immediately realised that we needed the help of a professional chemist. Fortunately we succeeded almost immediately in convincing Oscar D'Agostino, […] (who) had held a fellowship in Paris in the laboratory of Madame Curie. […] The results obtained during the first two weeks were summarised by Fermi in a letter to *Nature*".

Figure 3
From the left, Edoardo Amaldi, Franco Rasetti and Emilio Segrè during a walk in the surroundings of Rome

Since then, the Rome group worked actively together and by summer 1934 about fifty new radioactive isotopes had been found and three articles had been published in *La Ricerca Scientifica* [4], where at the time Ginestra Giovene – who was an astronomer before becoming in 1933 Edoardo's wife – worked as assistent editor. Through her, the reprints were made available within days, so that they could be sent by mail to a selected list of prominent physicists: the Rome group was the first to use 'preprints' to spread its results rapidly.

Because of the organisation of the work and the use of preprints the Rome group, as discussed by Gerald Holton [5], was the first team of physicists acting as it has become customary after the Second World War.

In summer 1934, a manuscript summarising the work done in Rome was brought by Amaldi and Segrè to Lord Rutherford in Cambridge [6]. At the first encounter, Segrè asked whether it would be possible to obtain prompt publication in the Proceedings of the Royal Society. Many years later he wrote: "I imprudently recommended prompt publication, whereupon he answered, whether in jest or annoyance I could not tell 'What do you think I am the President of the Royal Society for?'" [7]. Amaldi adds: "Unfortunately our understanding of Rutherford English at the time was imperfect and we could not follow most of his remarks, many of which must have been humorous because he laughed from time to time and only then took the pipe out of his mouth" [8].

The effect of slow neutrons

The way to the second discovery was opened when, on October 18th, 1934, Edoardo Amaldi started a systematic investigation to clarify the miraculous properties of some wooden tables, on which the induced radioactivity was larger than the one measured with the same apparatus mounted on a marble table.

The Physics Report contains many more interesting, and by now well known, details [9]:

> "In the paper published by Fermi's group in the Proceedings of the Royal Society the activity of the various artificial bodies had been classified only qualitatively. […] Therefore, around the middle of September 1934, we decided to try to establish a quantitative scale of activities which for the moment could be in arbitrary units. This work was assigned to me and B. Pontecorvo (b. 1913), one of our best students, who had taken the degree *(laurea)* in July 1934 and after the summer vacations had joined the group. We started by studying the conditions of irradiation most convenient for obtaining well reproducible results. For this type of work we used the activity of 2.3 min half-life of silver".

"We immediately found, however, some difficulty because it became apparent that the activation depended on the conditions of irradiation. In particular in the dark room, where usually we carried out the neutron irradiation, there were certain wooden tables near a spectroscope that had miraculous properties. As Pontecorvo noticed accidentally silver irradiaed on those tables gained more activity than when it was irradiated on the usual marble table in the same room".

"These results, daily reported to Fermi and the others, were friendly, but at the same time strongly, criticised by Rasetti who, in a teasing mood, insinuated that I and Pontecorvo were unable to perform 'clean and reproducible measurements'".

"In order to clarify the situation I started a systematic investigation. In the note book B1, where the data of that period are recorded, these measurements were started on October 18, 1934. Page 3 [contains] the summary of a typical series of measurements made inside and outside a lead housing *('castelletto')*, the walls of which were 5 cm thick. […]"

"On the morning of October 22 most of us were busy doing examinations and Fermi decided to proceed in making the measurements. Bruno Rossi from the University of Padua and Enrico Persico from the University of Turin were around in the Istituto di Via Panisperna and

Figure 4
Ginestra Giovene Amaldi had a degree in astronomy and, after working at "La Ricerca Scientifica", in 1936 wrote with Laura Capon Fermi "Alchimia del nostro tempo" (Alchemy of our time), the first Italian book on modern physics for the layman. After the war she wrote alone many other popular science books, of which the best known has been translated in four languages

> Persico was, I believe, the only eyewitness of what happened. At the moment of using the lead Fermi decided suddenly to try it with a wedge of some light element and paraffin was used first. The results of these measurements are recorded on pages 8 and 9 of the same note book B1. They are written by Fermi at the beginning and towards the end by Persico. Towards noon we were all summoned to watch the extraordinary effect of the filtration by paraffin: the activity was increased by an appreciable factor".

On the same day other well-known episodes took place. Starting with the measurements – performed in the water of a fountain – and ending with the excitement of the authors of the letter written to *La Ricerca Scientifica* in the apartment of the Amaldi's. Emilio Segrè described the scene as follows: "Fermi dictated while I wrote. He stood by me; Rasetti, Amaldi and Pontecorvo paced the room excitedly, all making comments at the same time. The din was such that when we left, Amaldi's maid discreetly asked whether the evening guests were tipsy. Ginestra Amaldi handed the paper to her boss at *La Ricerca Scientifica* the following morning" [7].

Why paraffin?

The paper published by *La Ricerca Scientifica* was the first step along a trail which brought to the discovery of other new radioactive species, to the confirmation of the $1/v$ law for the reaction cross section, to the experimental definition of the various neutron 'groups', to the discovery of chemical effects and to the experimental evidence of the presence of neutron resonances.

Leaving aside these very important scientific consequences of the initial discovery, I shall concentrate in this Section on a non-scientific question: why did Enrico Fermi suddenly decide to choose a block of paraffin?

Many years later Fermi himself described what happened to Subrahmanyan Chandraseckhar who reported the episode with the following words [10]:

> "Others with greater competence have written about Fermi's fundamental contributions to physics. But his own account of the critical moment when the effect of the slowing down of neutrons on their ability to induce nuclear transformations was discovered is perhaps worth recording. I described to Fermi Hadamard's thesis regarding the psychology of invention in mathematics, namely, how one must distinguish four different stages: a period of conscious effort, a period of 'incubation' when various combinations are made in the subconscious mind, the moment of 'reve-

lation' when the 'right combination' (made in the subconscious) emerges into the conscious, and finally the stage of further conscious effort. I then asked Fermi if the process of discovery in physics had any similarity. Fermi volunteered and said (his account made so great an impression on me that though this is written from memory, I believe that it is very nearly a truly verbatim account): 'I will tell you how I came to make the discovery which I suppose is the most important one I have made. 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. 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. I said to myself: 'No, I do not want the piece of lead here; what I want is a piece of paraffin'. It was just like that with no advance warning, no conscious prior reasoning. I immediately took some odd piece of paraffin and placed it where the piece of lead was to have been'".

Figure 5
The Institute of Via Panisperna in Rome. On the top floor lived the Corbino family

To complete the picture, in his book on Fermi Bruno Pontecorvo described what happened immediately after the first observation [11]:

> “The results were most surprising: the silver activity was hundreds of times greater than the one previously measured. Fermi stopped the confusion and agitation of his collaborators pronouncing a famous sentence that, they say, he repeated eight years later at the start-up of the first nuclear reactor: ‘Let us go for lunch’. … [In the discovery of the effect of slow neutrons] some accidental circumstances and the depth and intuition of a great mind, both played a crucial role. When we asked Fermi why he had used paraffin instead of lead, he smiled and teasingly said ‘C.I.F.’, that in Italian can be read ‘Con Intuito Formidabile’ (with formidable intuition). If the reader would conclude that Fermi was immodest he would be grossly wrong. He was direct, very simple and modest, but he was well conscious of his qualities. To this point I can add that, after lunch when he came back to the Institute and explained very clearly the effect of the paraffin block - thus introducing the concept of the slowing down of neutrons - with total sincerity he told us: ‘What a stupidity to have discovered this effect by chance without having being capable of predicting it’ ”.

Figure 6
Senator Orso Mario Corbino

C.I.F. was an expression invented and used in the Rome group, as reported by Emilio Segrè in his autobiography: “C.I.F. (Con Intuito Formidabile) [was] a joking acronym we used for statements by Fermi that were true, but that he could not prove” [12].

Immediately after the discovery Fermi proposed the correct explanation of the phenomenon by combining three arguments. Firstly, the neutrons *slow down* through elastic collisions with the hydrogen nuclei of paraffin or water. Secondly, the *reaction* cross section on nuclei *decreases* with the velocity of the neutron, so that slow neutrons are more effective than fast neutrons in producing radioactive substances. Thirdly, the reaction cross section for all the processes in which the neutron is absorbed is *much smaller* than the neutron-proton elastic cross section, so that the slowing down phenomenon can take place.

At that time, the terms "slow neutrons" and "fast neutrons" were used by some physicists and many thought that the elastic cross section against proton was *decreasing* with an *increase* of the neutron velocity. Instead, everybody was expecting that the *reaction* cross section of the neutrons would *increase, and not decrease, with their velocity* since intuitively larger energies in an inelastic collision produce more and not less damage. Fermi guessed that the opposite was true and this indeed was the fully unexpected physics result of that momentous day.

However any reasoning built up *a posteriori* does not account for the mental jump by which Fermi told himself "what I want is a piece of paraffin". Recently Alberto De Gregorio has carefully examined the experimental facts that were surely known to Fermi at that moment and could shed light on the sudden decision to use paraffin and not something else [13]. In the first part of his paper he traces the experiments that in the years 1932 and 1933 had convinced the physicists working with neutrons that these particles are more scattered and absorbed by paraffin than by lead. For this it is enough to quote here the Joliot-Curies who wrote [14]: "The radiation emitted by a Po + Li source is much more absorbed, for the same mass per square centimeter, by paraffin rather than by lead, at variance with what happens to the gamma rays emitted by polonium".

In connection with the knowledge that Fermi had of these results, De Gregorio points out the importance of the detailed discussions that took place at the Solvay Conference on *Structure et propriétés des noyaux atomiques*. This Conference was held in Bruxells from October 22 to October 29, 1933 – exactly one year before the discovery [15]. There J. Chadwick discussed the available knowledge on the collisions between neutrons and atomic nuclei by specifying [16]: "It seems that, in general, slow neutrons are more easily scattered than fast neutrons. […] I found that the collision radius of hydrogen varies with the velocity of the neutrons. […] Some experiments with slow neutrons seem to indicate that the collision radius [i.e. the elastic cross section] continues to increases when the neutron velocity decreases".

He even went so far as to derive – from the fact that slow neutrons behave as waves and their wavelength is much larger than the nuclear radius – a formula in which the elastic cross-section is *inversely proportional to the square* of the velocity. This formula, reproduced in the proceedings, had been published by him previously.

At the same Conference the Joliot-Curies presented a paper titled *Rayonnement pénétrant des atomes sous l'action des rayons alfa*, where the results discussed above were presented. As underlined by De Gregorio, the discussion following this presentation is very illuminating due to the quality and the number of the interventions: Meitner, Chadwick, Perrin, Heisenberg, Fermi, de Broglie, Bothe, Lawrence, Gamow, Rutherford, Peierls, Bohr, Pauli… . Heisenberg expressed doubts about the inverse square law and Fermi, after recalling the hypotheses needed for deriving Chadwick's formula, said "the experimental cross sections are many times smaller than what is predicted by the formula".

De Gregorio concludes his paper by writing: "Fermi's intervention in Bruxells proves the interest of the Roman physicist for the behaviour of neutrons. We can definitely state that, already at the end of 1933, he was aware both of the increase of the [neutron-proton] scattering cross section when the energy decreases and of the larger efficiency of paraffin with respect to lead in the slowing down and in the absorption of neutrons. […] The sudden decision of October 1934 would thus have been the result of a subconscious elaboration of what was already known to the Italian physicist. Such a reconstruction would, among other things, confirm Hadamard's thesis on the psychology of inventions in mathematics. [… The hypothesis of a previous unconscious elaboration of known information] is supported by the very fact that Fermi [in answering to Chandrasekhar] thought to his 'sudden' decision to use paraffin as an example that could confirm the hypothesis of the great French mathematician".

Of course this chain of arguments does not reduce Fermi's merits for both

1. the experimental discovery of an unexpected phenomenon, that followed six months of concentrated work performed with his collaborators, and
2. the prompt interpretation of the observations, which required the new hypothesis of an increasing reaction cross section when the neutron velocity decreases.

The patent

Leaving the main course of the Roman discoveries, I now wish to discuss the activity that went on in Rome to secure the production of radioactive isotopes for medical and industrial applications. The beginnings of this development can be traced back to Orso Mario Corbino, the Director of the Institute and Fermi's mentor. As described by Laura Fermi in *Atoms in the family*, the first scene went as follows [17]:

> "One morning, a couple of days after [the discovery], Corbino came to the laboratory; although he did not actively participated in research, he kept informed and often gave good advice. He had followed the younger men's work step by step, and on that morning also he asked to be told what they were at. They were preparing to write a more extensive report on their experiments, they answered. Corbino became incensed. 'What? Do you want to publish more than you have already?' he asked in a swift rush of words, helping the oral expression with brisk gestures, as all Sicilians do. 'Are you crazy? Can't you see that your discovery may have industrial applications? You should take a patent before you give out more details on how to make artificial radioactive substances!' ".

The boys of Via Panisperna had not thought about this possibility and at the beginning they considered it completely at variance with what respectable scientists do of their discoveries. But Corbino was a very practical man connected with many industries and had a lot of influence on the people around him. Thus on October 26, only four days after the discovery, Amaldi, D'Agostino, Fermi, Pontecorvo, Rasetti, Segrè, and Trabacchi – the 'Divine Providence' who had provided the radon for the neutron sources – jointly applied to obtain a patent for their process to produce artificial radioactivity with slow neutrons. The Italian patent n. 324 458 was later extended to other countries. Its history is very interesting, but there is no space for it in this contribution.

Figure 7
From the left, Oscar D'Agostino and Giulio Cesare Trabacchi, the "Divine Providence"

As often said by Edoardo Amaldi, the patent was a really unexpected outcome of their research. In the 1984 *Physics Report* he wrote [18]: "We were extremely pleased and amused, not so much because a patent could result, sometime in the future, in a financial benefit for the 'inventors', but rather because a work, carried out with great energy and dedication, only for its intrinsic merits, had, unexpectedly, brought us to applications, which, in addition, would be mainly of a scientific and medical nature".

The medical applications were of immediate interest for Giulio Cesare Trabacchi (the 'Divine Providence'), who was the leader of the small but relatively rich *Laboratorio Fisico della Sanità Pubblica*. This Laboratory was created by Corbino, who in the years 1921-1922 had been Minister of Education in the last democratic government before Mussolini took power. Since then the Laboratory was occupying four offices of the building of Via Panisperna. The radon for the neutron sources, used by Fermi and collaborators, was extracted from the radium belonging to the public health service and kept in the basement of the same building.

The first Italian accelerator

After the patent was deposited, it must have been very natural for Trabacchi to get interested in the medical utilisation of the new isotopes the person who was his colleague as leader of the *Laboratorio Chimico della Sanità Pubblica*: Domenico Marotta (1886-1964) [19]. Marotta was a chemist and a great science manager. He had been for a few years one of the main promoters of the creation of the *Istituto di Sanità Pubblica*, whose building was funded in 1929 by the Rockfeller Foundation and inaugurated by Mussolini on April 21st, 1934. The Physics Laboratory, led by Trabacchi, and the Chemistry Laboratory, led by Marotta, became two of the five laboratories forming the new Institute. At the beginning of 1935 Domenico Marotta was nominated Director of the Health Institute and Trabacchi and his small group moved from Via Panisperna to the new building on *Viale Regina Margherita* [20,21].

Marotta had very clear ideas about the importance of fundamental research in the life of the new Institute, and greatly valued the collaboration of Fermi who was known worldwide and could also have political influence since he was the youngest member of the *Accademia d'Italia* created by Mussolini. After many discussions with Trabacchi and Fermi, on October 21, 1935, exactly one year after the discovery of slow neutrons, Domenico Marotta wrote to Fermi to obtain a written statement on the possible practical uses of the new radioactive substances. The reply, dated October 23, 1935 contains the following statement [22]:

> "I believe that it is reasonable to predict that, in a near future, it will be possible to currently produce artificial radioactive bodies having activities equal or greater than the ones of the radioactive sources now used in ther-

Figure 8
Domenico Marotta during the visit at the Istituto Superiore di Sanità of her Highness Maria Josè, Princess of Piedmont

> apy. Thus the artificial sources will be at least equivalent, and probably less costly, than radium. Given the variety of elements in which, with the new methods, artificial radioactivity can be produced, there is also the possibility – which can only be checked with extensive dedicated studies – that some elements will be found that are particularly convenient as far as their chemical and physiological properties are concerned".

Since it was obvious that more intense neutron sources were needed for both fundamental research and new medical applications, during the year 1936 the subject was widely discussed in Rome between Fermi, Marotta, Trabacchi and Amaldi. At that time the other junior members of the group were far away: Rasetti was for one year at Columbia University, Segrè had been appointed professor in Palermo and Pontecorvo, after devoting himself for some time to theoretical physics with G.C. Wick, went to Paris to work with the Joliots. Since one of the main subjects of discussion was the choice between an electrostatic accelerator and a cyclotron, it was decided that direct information should be collected on the matter. Thus in summer 1936 Edoardo Amaldi left for the United States, sharing his time between Columbia University and the Carnegie Institute of Washington, where Merle Tuve and collaborators had constructed a new type of Van de Graf electrostatic accelerator. Before going to the States Amaldi reported about the work done in Rome at the international *"Probleme der Atomkernphysik"* held in Copenhagen from June 14 to June 20, 1936. In listening to the other presentations he got even more convinced of the fact that, in order to be scientifically competitive, more intense neutron sources were mandatory.

In the following months, Trabacchi requested for his laboratory a one million volt electrostatic accelerator with a current of 10 milliampere, most probably because he was convinced that a cyclotron was too expensive. On October 16, 1936, Marotta wrote again to Fermi indicating his decision to engage his Institute in the production of radioactive isotopes [23]. The phrasing of the letter indicates that he considered the electrostatic accelerator requested by Trabacchi insufficient and that he was pushing in the direction of a much more powerful cyclotron. Marotta wanted also to be sure that Fermi was ready to collaborate, in case the needed funds would have been found.

Fermi's reply was reproduced by Marotta in the document sent on December 20, 1936, to the Cabinet of the Minister [24]. In this memorandum the idea of the cyclotron is not even mentioned, one – and not ten – milliaperes are quoted and the following excerpts of the reply by Fermi are reproduced.

> "Having an apparatus of up to one million volt, the best method to produce neutrons consist in bombarding with nuclei of heavy hydrogen a target of either beryllium or lithium. With a beryllium target and one million volt, the yield is 1.5 10^{11} neutrons for a current of one milliampere of deuterium ions. […] With a current of one milliampere one can produce artificial radioactive substances having an activity of up to two Curies. […] With short irradiations one could use iodine (with a lifetime of 25 minutes), for intermediate irradiations manganese (lifetime: 2.5 hours), arsenic for long irradiations (26 hours) or other substances, as cobalt and iridium, that have months long lifetimes. On top of the applications to cancer therapy, sizeable quantities of artificial radioactive substances could be used as tracers in chemistry and biochemistry research".

For the one million volt accelerator Marotta requested 300,000 lire as investment money and 100,000 lire per year as running budget. Being a very capable science manager Marotta added also the following economic argument:

> "The apparatus could produce eight curie-hour per day that, at the present market prices, have a commercial value of 2,500 lire. Thus the running costs would be covered with only 40 irradiation days per year. Given the large demand of radioactive substances this minimum will certainly be passed and not only the facility will be profitable, but also the cost of the products could be reduced, so that their use will be extended with benefits for humankind".

As underlined by G. Battimelli [25], the arguments put forward were medical but certainly another reason, not written in the memorandum to the Health Minister, was very clear to him and to all the physicists working in Rome. An accelerator would have allowed the group to compete in nuclear fundamental research with foreign laboratories, that already had accelerators. They knew very well that to this end a cyclotron would have been better than a Cockcroft-Walton, but the cost would have been much larger. The fear to loose everything by asking too much is probably at the basis of Marotta's choice to single out the electrostatic accelerator for the request to the Ministry.

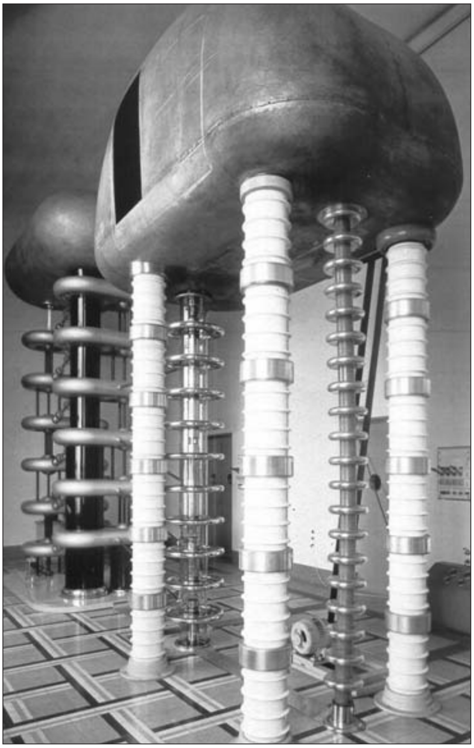

Figure 9
The Cockcroft-Walton was built on the top floor of the Istituto Superiore di Sanità

Waiting for the funds, a prototype 200 keV Cockcroft-Walton was constructed; it accelerated particles in June 1937, as reported in a paper signed by Amaldi, Fermi and Rasetti [26]. Shortly afterwards Amaldi was appointed to the chair of Corbino, who had prematurely died. The funds for the accelerator were available and the construction had just started when, at the end of 1938, Fermi left for the States directly after receiving in Stockholm the Nobel prize for the discovery of the radioactivity induced by neutrons. With this momentous event, the Rome group ceased to exist. In the following year Amaldi and Trabacchi completed – with the new junior members of the laboratory

Mario Ageno and Daria Bocciarelli – the 1.1 MeV Cockcroft-Walton of the Health Institute [27]. The first Italian accelerator was at the level of the ones running at that time at the National Bureau of Standards, the National Physical Laboratory, the Physikalische Technische Reichsanstalt and the Hôpital St. Antoine. Shortly after, Francis Aston [of the Cavendish Laboratory] and Otto Hahn [the discoverer of fission] visited the physics laboratory of the Health Institute and the accelerator and declared that “this was the most beautiful laboratory they had ever seen” [28].

The accelerator was still running when, twenty years later, I entered as a junior fellow the Physics Laboratory of the Istituto Superiore di Sanità (ISS).

The birth of nuclear medicine

On April 29, 1938, six months before receiving the Nobel prize, in the crowded auditorium of the *Istituto di Sanità Pubblica* Fermi addressed the staff on ‘Prospects of application of artificial radioactivity’ [29]. After a presentation on the history of artificial radioactivity – illustrated with a simple experiment that used a Geiger counter and a piece of rhodium – he showed a picture of a cyclotron and a drawing of the Cockcroft-Walton, which was

Figure 10
This picture was taken at the Istituto Superiore di Sanità after Fermi’s speech.
From left to right: G.C. Trabacchi, E. Fermi and D. Marotta

at that time under construction on the sixth floor of the same building. To quantify the advantage of this costly apparatus he said that the accelerator would produce the same neutron flux as a radon-beryllium source based on some kilograms of radio, one thousands times more than the one gram they had been using in Rome.

In the concluding part he shortly mentioned the therapeutic uses of the new isotopes and added:

> "Independently of these possibilities, the use of sizeable quantities of artificial radioactive substances will open the way, I hope, to many interesting studies in biology and chemistry in which the radioelements will be used as 'tracers'. […] By mixing radioactive phosphor with the phosphor contained in the aliments one can follow the behaviour of this element in a living being, as has been already proved in the beautiful experiments started by Hevesy in Copenhagen and continued by Segrè and Camillo Artom in Palermo".

In 1938 the use of radioactive isotopes in medicine for diagnostic purposes was in its infancy. In 1923 Georg von Hevesy had for the first time used a natural radioisotope of lead to study the metabolism in plants. In 1925 Hermann Blumgart and Soma Weiss had studied with radioisotopes the velocity of circulating blood. In 1936 the cyclotron, built by Ernest Lawrence in Berkeley, had been used by John Lawrence to produce phosphorus-32 for medical treatments. As mentioned by Fermi, the same isotope was employed in 1936 for metabolic studies of the mouse by Segrè and Artom, who was professor of physiology in Palermo. In 1937 in Berkeley Joseph Hamilton had used radioactive sodium to study the human metabolism of food.

The scarcity of applications fully justify the 'I hope' in the sentence 'it will open the way to many interesting studies' pronounced by Fermi, who was always very careful before making strong statements. His care was further justified by the fact that the few artificial radioisotopes utilised at that time had been produced with reactions initiated by the high energy particles accelerated by the Lawrence cyclotron and *not* by slow neutrons.

Of course Fermi could not know that at the time of his public lecture the radioactive element that is now used in more than 90% of all nuclear medicine diagnostics tests had already been produced and discovered by no less than his collaborator Emilio Segrè. Let us read how Segrè himself describes in his autobiography the discovery of 'technetium' [30]:

"In February 1937 I received a letter from Lawrence containing more radioactive stuff. In particular, it contained a molybdenum foil that had been part of the cyclotron deflector. I suspected at once that it might contain element 43. […] For this investigation I enlisted the cooperation of Carlo Perrier, who had more experience in chemistry than I. First we separated the activity we were studying from all known elements. […] Next we established several of the chemical properties of element 43. […] We had two radioactive isotopes: technetium 95, with a period of 61 days, and technetium 97, with a period of 90 days. […] In this work we had discovered the first chemical element created by man".

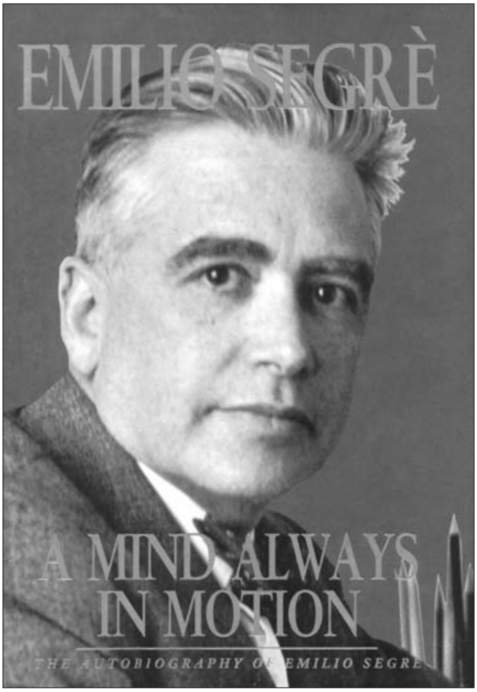

Figure 11
The cover of the autobiography of Emilio Segrè, published in 1993 by the University of California Press, Oxford

This was no little achievement since previously many had prematurely claimed the discovery of the element having 43 electrons, that was given in turn the following names: ilmenium, davyum, lucium, nipponium and masurium [31]. Due to this long story of failures Segrè and Perrier decided not to name the new element till everybody would have agreed on the solid foundations of their claim. The name technetium, justified by the fact that this was the first artificial element ever produced, was proposed by them only ten years later [32].

The masurium story is related to the missed discovery of fission and a short detour is fully justified.

Going back to 1934, it is well known that in Rome many new activities were found when bombarding thorium and uranium. In a first letter, published in May, it is written [33]: "Using chemical operations, an attempt was made to determine whether the element, which [is formed in an uranium target and] disintegrates with a period of 13 min, is an isotope of one of the heavy elements. […] This complex of conclusions, which we are trying to corroborate by means of further experiments, give rise to the spontaneous hypothesis that the active substance of uranium might have atomic number 93 (homologous with rhenium)".

The interpretation of the Rome group was discussed by von Grosse and Agruss [34] and by Ida Noddack [35]. In the paper of the first two authors the hypothesis of the creation of different transuranic elements was supported with chemical data, while the second contained the much-quoted sentence: "One can think that by bombardment of heavy elements with neutrons, these nuclei break in many large pieces, which are isotopes of known elements, but not neighbouring of those irradiated".

After the discovery of the effect of paraffin in October 1934, the Rome group irradiated with slow neutrons uranium and thorium finding many new activities that were sensitive to the presence of a light material capable of slowing down the neutrons and seemed to confirm the previous interpretation based on transuranic elements. This was supported also by two papers written by O. Hahn and L. Meitner [36]. Why the Rome group did not consider the proposal by Ida Noddack? About this very puzzling question Amaldi wrote the following [37]:

> "The work by Ida Noddack was not taken seriously by any one of the people working in the field. She sent her paper to Fermi and both, she and her husband, communicated repeatedly their point of view to O. Hahn during the years 1934-1935 and 1936. [I seem] to remember some discussions among the members of our group, including Fermi, in which the ideas of 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 [and] 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 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 called 'masurium'".

Thus element 43 enters twice in the subject treated in this paper. Its false discovery induced the best experimenters of the time not to pay the due attention to Noddack's proposal and its true discovery opened the way to the modern diagnostic procedures in nuclear medicine.

Going back to nuclear medicine, there is a final twist to the link made among the work on slow neutrons done in Rome in 1934-1935, the talk given in 1938 by Fermi at the *Istituto di Sanità* and the use of isotopes in medicine.

As already stressed, most examinations of current nuclear medicine (as bone imaging, the study of renal functions and the control of myocardial perfusion) use the metastable isotope Technetium 99m, that has a convenient lifetime of 6 hours, so that it could not be found by Segrè in his molybdenum foil. The utilisation of this isotope became practical when, in the late 1950's, a new method for producing it was developed at Brookhaven National Laboratory. In this 'generator system' a long-lived radioactive parent is packaged, from which the short-lived daughter isotope is separated. Thus ^{99m}Tc is separated in any nuclear medicine department from molybdenum 99, that is contained in an easily shipped generator. In the framework of this presentation, the interesting aspect is that ^{99}Mo is at present produced in nuclear reactors *by slow neutrons* absorbed by the stable nuclide ^{98}Mo and not by cyclotrons.

Since the reactors used are ageing and no new one is being built, various plans exist in the world to resort to high current sector cyclotrons for the future medical needs. But instead of using the reactions induced by nuclei accelerated at a few MeV, that created in Berkeley the first technetium isotopes, these cyclotrons would give technetium 99m by neutron capture. The needed large *neutron fluxes* could be created in a fission target bombarded by protons, as proposed by Carlo Rubbia with the Energy Amplifier.

This development would thus close the barely visible line that connects the discoveries in nuclear physics that took place in Rome, Berkeley and Palermo in the 30's with nowadays medical physics.

I am very grateful to Alberto De Gregorio for comments and suggestions of improvements.

References

1. E. Fermi., *Ric. Scient.* 5(1), 283 (1934). *Ibid.* 5(1), 330 (1934).
2. E. Amaldi, *From the discovery of the neutron to the discovery of nuclear fission*, Physics Report 111 (1-4) (1984) 1 – 331.
3. Ref. 2, p. 124.

4. E. Amaldi, O. D'Agostino, E. Fermi, F. Rasetti, and E. Segrè, *Ric. Scient.* 5(1), 452 (1934). Ibid. 5(1), 652 (1934). Ibid. 5(2), 21 (1934).
5. G. Holton, *Fermi's Group and the Recapture of Italy's Place in Physics*, in *The Scientific Imagination*, Case Studies, Cambridge: Cambridge University Press, Part 2, Section 5, 1978.
6. E. Amaldi, O. D'Agostino, E. Fermi,. F. Rasetti, and E. Segrè, Proc. Roy. Soc. A146, 483 (1934).
7. E. Segrè, *Enrico Fermi Physicist*, Chicago: The University of Chicago Press, 1970, p. 92.
8. Ref. 2, p. 132.
9. Ref. 2, p. 151.
10. *Enrico Fermi Collected Papers*, E. Amaldi et al. eds., Accademia dei Lincei and University of Chicago Press, Rome-Chicago, 1965, Vol II, p. 926-927.
11. B. Pontecorvo, *Enrico Fermi*, Edizioni Studio Tesi, Pordenone, 1993, p. 82.
12. E. Segrè, *A mind always in motion*, University of California Press, Berkeley - Los Angeles - Oxford, 1993, p. 151
13. A. De Gregorio, *Caso e necessità nella scoperta da parte di Fermi delle proprietà dei neutroni lenti*, Il Giornale di Fisica, 2002.
14. I. Curie and F. Joliot, *Journal de Phys.* 4, 21 (1933).
15. *Structure et propriétés des noyaux atomiques*, Rapports et discussion du 7me Conseil de Physique tenu à Bruxelles du 22 au 29 octobre 1933, Gauthiers-Villars, Paris, 1934.
16. J. Chadwick, Ref. 15, p. 106.
17. L. Fermi, *Atoms in the Family*, University of Chicago Press, Chicago, 1954, p. 100.
18. Ref. 2, p. 156.
19. *Domenico Marotta nel 25° anniversario della morte*, Rend. Acc. Naz. Scienze, 23, 77-247 (2000).
20. L. Cerruti, *Domenico Marotta. Dai Laboratori di Sanità pubblica alla fondazione dell'Istituto*, Ref. 19, p. 112-114.
21. G. Battimelli, *Le origini del laboratorio di fisica*, Ref. 19, p. 149-160.
22. Letter by Fermi to Marotta, 23 October 1935, files *'Laboratorio di Fisica - Istituto Superiore di Sanità'*, Central Archives of the State, Rome.
23. Letter by Marotta to Fermi, 16 October 1936, files *'Laboratorio di Fisica - Istituto Superiore di Sanità'*, Central Archives of the State, Rome.
24. Memorandum by Marotta to the Cabinet, 20 December 1937, files *'Laboratorio di Fisica - Istituto Superiore di Sanità'*, Central Archives of the State, Rome.
25. G. Battimelli, *Le origini del laboratorio di fisica*, Ref. 19, p. 157.
26. E. Amaldi, E. Fermi, and F. Rasetti, *Ric. Scient.* 8(2), 40 (1937).
27. M. Ageno, E. Amaldi, D. Bocciarelli, and G.C. Trabacchi, *Rend. Ist. Sup. Sanità* 3, 201 (1940).
28. G. Battimelli, *Le origini del laboratorio di fisica*, Ref. 19, p. 1589.
29. The text of the address, that has the Italian title *Prospettive di applicazioni della radioattività artificiale*, appears in the first volume of the Rendiconti of the Institute, 421-432 (1938).

30. E. SEGRÈ, Ref. 12, p. 115-116.
31. E. SEGRÈ, Ref. 12, p. 308.
32. C. PERRIER and E. SEGRÈ, *Nature* 159, 24 (1947).
33. E. AMALDI, O. D'AGOSTINO, E. FERMI, F. RASETTI and G. SEGRÈ, *Ric. Scient.* 5(1), 677 (1934).
34. A. v. GROSSE and M. ANGRUSS, *Phys. Rev.* 46, 241 (1934).
35. I. NODDACK, "Über das element 93", *Angewandte Chemie*, 47 and 391 (1934).
36. O. HAHN and L. MEITNER, *Naturwiss.*, 23, 37 and 230 (1935).
37. Ref. 2, p. 277-278.

Ugo Amaldi
He initially worked for the Italian National Health Institute's Physics Laboratory on nuclear and radiation physics. In the 1970s, Ugo Amaldi served as Senior Scientist at the European Organisation for Nuclear Research (CERN) in Geneva, where he conducted many experiments in particle physics. At present he teachs Medical Physics at the University of Milano Bicocca. Among other activities, he headed for 15 years the DELPHI project, which built and managed a large-scale detector that collected data on the LEP electron-positron collider from 1989 to 2000.
He has over 400 scientific publications to his credit, including a score of high-school physics textbooks, the earliest of which he co-authored with Edoardo Amaldi. He holds honourary doctorates from the Universities of Helsinki, Lyon, Uppsala and Valencia. As president of the TERA Foundation, he is now working to introduce adron therapy techniques in Europe for the treatment of deep, radio-resistant tumors.

Giovanni Battimelli

Funds and Failures: the Political Economy of Fermi's Group

Political protection and academic patronage, offered by Corbino and Marconi, were not enough to secure the experimental researches on nuclear physics done by Fermi and his younger collaborators in the early thirties; although it was certainly not yet the "big science" of the afterwar years, and has been depicted by Segrè as "string and sealing wax physics", still that kind of work required also adequate funding and financial support. The institutions who were supposed to provide that money (mainly the newly reconstituted Consiglio Nazionale delle Ricerche, where the Physics Committee was firmly controlled by supporters of the "new physics" and had Fermi as secretary) were actually unable to do that adequately. Fermi's work was in fact made possible by means provided through the direct intervention of the Istituto di Sanità Pubblica, an institution which had in principle nothing to share with fundamental nuclear research, but was deeply linked, through its Physics Laboratory, to Corbino and the Physics Institute of via Panisperna. Tracing the details of that side of the "political economy" of the Fermi group allows to get a clearer picture of the support given to fundamental physics in Italy in that period, and of its limitations, and ultimately makes it possible to understand why Fermi's design to create in Italy a national laboratory for research in fundamental physics, endowed with the best equipment available in the mid-thirties (a cyclotron) to keep it competitive with the great research centers abroad, eventually failed.

Finanziamenti e fallimenti: l'economia politica del gruppo Fermi

La protezione politica ed accademica offerta da Corbino e Marconi non erano sufficienti ad assicurare la prosecuzione della ricerca sperimentale condotta da Fermi ed i suoi più giovani collaboratori nei primi anni trenta. Sebbene non si trattasse ancora della "big science" del dopoguerra, e sia stata descritta da Segrè come fisica "spago e ceralacca", essa necessitava ciononostante di congrui sostegni e finanziamenti adeguati.

Le istituzioni a ciò preposte (nella fattispecie l'allora appena ricostituito Consiglio Nazionale delle Ricerche, il cui Comitato per la Fisica era saldamente controllato dai sostenitori della "nuova fisica" e aveva Fermi come segretario), non furono in grado di adempiere adeguatamente a tale funzione.

Il lavoro di Fermi fu difatti reso possibile dall'intervento diretto dell'Istituto di Sanità Pubblica, un'istituzione che in linea di principio non aveva nulla a che fare con la ricerca nucleare fondamentale, ma che era strettamente legata, attraverso il suo Laboratorio di Fisica, a Corbino ed all'Istituto di Fisica di via Panisperna.

La ricostruzione dettagliata di questo lato dell'"economia politica" del gruppo di Fermi ci permette di avere una visione più chiara del sostegno dato alla fisica fondamentale in quegli anni in Italia e dei suoi limiti, nonché delle ragioni del fallimento del progetto di Fermi di creare un laboratorio nazionale di ricerca per la fisica fondamentale, dotato delle migliori strumentazioni esistenti all'epoca (un ciclotrone), al fine di poterlo rendere competitivo con i maggiori centri di ricerca stranieri.

Funds: "a fabulous wealth"

The Nobel prize for physics awarded to Enrico Fermi in 1938 came as the most visible recognition of a long string of brilliant achievements in both theoretical and experimental research. These, in turn, were the outcome not only of the outstanding intellectual abilities and professional skills of the Italian physicist, but of a planned and organized effort to raise the standards of fundamental physical research in Italy that turned out to be highly successful, eventually leading to what has been defined "the recapture of Italy's place in physics".[1] The fundamental role played in this context by Orso Mario Corbino's scientific vision, for the development of the scheme, and of his patronage, both in academic circles and outside, to turn it into a successful enterprise, has been widely recognized, by the actors of the story and by later historians as well.[2] It has equally been stressed the importance of the election of Fermi, as the only physicist, among the members of the newly founded *Accademia d'Italia* in 1929; through the *Accademia* and the connected circles came to Fermi not only prestige but also support for his and his team's work, in the form of grants for travels abroad and financial means to organize scientific events (the 1931 nuclear physics conference in Rome, the first international gathering in the field, was held under the aegis of the Accademia and lavishly sponsored by the *Fondazione Volta*, an institution where Corbino had an influential voice).

Less attention has been generally paid, on the contrary, to the problem of the sources and amount of direct financial support to the researches of Fermi's "boys" in via Panisperna. There is little or no talk of money, in the recollections of the golden days of the thirties provided by the members of the team, or in subsequent historical investigations. The general consensus seems to be that the matter wasn't a really serious one, the limited support available being more than sufficient for what was, after all, as Emilio Segrè put it in a way that has become part of the image and legend of the via Panisperna saga, "a string and sealing wax physics".[3] Segrè recalls that when, after Fermi's initial success with fluorine, it was decided to "irradiate all the

---

1 G. HOLTON, "Fermi's group and the recapture of Italy's place in physics", in G. HOLTON (ed.), *The scientific imagination. Case studies*, Cambridge 1978, pp. 155-198.

2 Among the several accounts of the activities of Fermi's group in via Panisperna, particular attention is devoted to discuss and elucidate the personality and the influence of Corbino by C. Tarsitani, "Tradizione e innovazione nella fisica italiana tra le due guerre: il caso del gruppo Fermi", *Critica marxista* 6 (1981), pp. 79-120.

3 "You see, it was a different type of physics. It was done on a few tables with string and sealing wax. It was extremely simple. It cost very little"; quoted in G. HOLTON, *Fermi's group…* , p. 194.

substances we could lay our hands on", for which "we needed a little money", this was provided by "a phone call by Fermi to the National Research Council (that) got us about $1000 with no strings attached".[4] In later writings he reported the same amount of money (converted into 20,000 lire) as having been granted and spent for the acquisition of chemical substances for the 1934 experiments, stressing "the extremely small cost of this research".[5] Segrè also gives a figure of about 50,000 lire, granted by the National Research Council (*Consiglio Nazionale delle Ricerche*, CNR), as the sum spent for researches at the via Panisperna institute by Fermi's group in the two previous years, commenting on it as being "a considerable amount of money for that time".[6] Further information is provided by Franco Rasetti: referring to the work done up to the end of 1933, he states that "these developments were made possible by a grant from the *Consiglio Nazionale delle Ricerche*, which had raised the research budget of the department to an amount of the order of $2000 to $3000 per year; a fabulous wealth when one considers that the average for physics departments in Italian universities was about one-tenth of that amount".[7] In a speech delivered in 1937, the same Rasetti gave an estimate of the total expenditure for the researches in nuclear physics under Fermi's direction in the four previous years as being "less than 150,000 lire".[8]

These rough estimates fit quite well together, and the overall picture emerging from the different comments is a consistent one: a few tens of thousands lire per year are indeed small money if compared with what will be the typical investment needed for experimental research in nuclear physics in later years, while constituting at the same time "a fabulous wealth" if comparison is made with the general situation of financial support to physics in Italy at the time, when the annual budget of a physics institute oscillated

---

4 E. Segrè, "Fermi and Neutron Physics", *Reviews of Modern Physics* 27, 3 (1955), pp. 257-263, quot. p. 259.

5 E. Segrè, preface to papers 84a to 110, in E. Fermi, *Note e memorie (Collected Papers)*, vol. I, Accademia Nazionale dei Lincei , Roma, and The University of Chicago Press, 1962, p.640; E. Segrè, *Nota biografica*, ibid. p. XXXV; E. Segrè, *Enrico Fermi, Physicist*, The University of Chicago Press, 1970, p. 78 of the Italian edition (Zanichelli, Bologna 1971). The conversion rate of dollars into lire grew from about 12 lire per dollar in the early thirties to about 20 lire per dollar at the end of the decade.

6 E. Segrè, *Enrico Fermi, Physicist*, The University of Chicago Press, 1970, p. 75-76 of the Italian edition (Zanichelli, Bologna 1971).

7 F. Rasetti, preface to paper 78, in E. Fermi, *Note e memorie (Collected Papers)*, vol. I, Accademia Nazionale dei Lincei , Roma, and The University of Chicago Press, 1962, p. 548.

8 F. Rasetti, *Progressi recenti della fisica nucleare*, SIPS – Scienza e tecnica, supplemento agli Atti della Società Italiana per il Progresso delle Scienze, 1937, 337.

between a few thousands lire for the small universities to twenty- to thirtythousand lire in the main centers of research, such as Rome or Bologna. Support to physical research began arriving from CNR in sensible measure following the 1927 reform of the Council, when firm political control over it was finally established through the destitution of former President and founder Vito Volterra, an outspoken antifascist, and his replacement with the more reliable Guglielmo Marconi; the reorganization of the Council that went along with such measures meant also that, for the first time since its foundation in 1923, the Council's budget was raised to an amount that, although still hopelessly small if compared with that of analogous bodies abroad, allowed the various Committees to actually do something for the active support of research and scientific projects. The CNR budget, which had stayed for years at the ridiculous 1923 level of 175,000 lire, grew to half a million in 1927, jumped to a million and a half in 1930 and kept increasing throughout the thirties to a peak of 25 millions lire in 1939.[9]

The main supporters of the "new physics" in the universities also held key positions in the relevant CNR bodies: while Corbino was called by Marconi to preside over the Committee for Scientific Radiotelegraphy, the Physics Committee was headed (from 1927 to his death in 1933) by Antonio Garbasso, the director of the physics institute in Florence, and had Fermi as its secretary. They all saw in the renewed institution a privileged channel through which secure financial support to the more modern, promising trends in physical research, and actively managed to successfully steer a large part of the available funds in that direction.

The records of the CNR Directory and of the Physics Committee show that indeed a large fraction of the annual budget of the latter, and some significant contributions coming directly from the main central body, went to the research groups in Florence and Rome. Altogether, what these official records allow to calculate as the contribution offered by CNR to support Fermi' researches is in excellent agreement with Rasetti's estimate mentioned earlier.[10]

---

[9] On CNR in general see G. PAOLONI, R. SIMILI (eds.), *Per una storia del Consiglio Nazionale delle Ricerche*, 2 Vol., Laterza, Roma-Bari 2001; in particular, for the main lines of development in the period concerned, see in Vol. 1 the essays by R. SIMILI, *La Presidenza Volterra* (pp. 72-127) and *La Presidenza Marconi* (pp. 128-172), and by G. PAOLONI, *Organizzazione e risorse di un ente in formazione* (pp. 201-223).

[10] On the CNR Physics Committee in these years see G. BATTIMELLI, M. DE MARIA, *La fisica*, in G. PAOLONI, R. SIMILI (eds.), *Per una storia del Consiglio Nazionale delle Ricerche*, quot., Vol. 1, pp. 281-311. The Committee budget was (in lire) 64,000 in 1931, 47,000 in 1932, 67,000 in 1933, 115,000 in 1934, 98,000 in 1935, 167,000 in 1936.

These sums, though constituting "a fabulous wealth" when compared to the average situation, should not lead to attribute to the new areas of research in Italian physics at the time more weight than they actually had managed to obtain. It may be interesting to notice that in 1930, on a much more traditional field, a special contribution of 150,000 lire was granted by the CNR Directory to the director of the National Institute of Optics in Florence, Vasco Ronchi, who had advanced the request for the purpose of buying high precision optical equipment.

Applied science always was the focus of attention in CNR, and large research centers such as the *Istituto Nazionale di Ottica* got the larger share of the cake. Fermi was perfectly aware of this and, as a consequence, of the importance of establishing in Italy a large national laboratory for physics able to go beyond the intrinsic limits of a university institute. He did a first move in that direction soon after his election to the *Accademia d'Italia*, when he felt that the repeated statements by Mussolini on the need to give a strong support to science gave him a chance to push his project successfully. Fermi went to talk to Mussolini himself on January 1930, submitting him a proposal for the transformation of the via Panisperna institute into a National Physics Institute, with special status in the university system and corresponding special financial support from the State (he envisaged a specific contribution of 200,000 lire per year).

The project was forwarded by Mussolini to the Minister of National Education Balbino Giuliano, who sternly refused to take it into consideration on the basis of the disrupting effects that such a move would produce on the whole university system.[11] It was the first failure on the road towards the creation of a national laboratory for physics, a goal that figured eminently throughout the successive years in the CNR agenda, and that never materialized.

"La Divina Provvidenza"

Whatever the limits and the shortcomings of CNR's action on the larger scale, they didn't prevent Fermi and his "boys" to secure for themselves the means needed to do excellent physics. With a skillful combination of "frugal-

[11] The documentation is in Archivio Centrale dello Stato, Presidenza del Consiglio dei Ministri, Gabinetto, 1928-30, folder 5-1-10527, and is reproduced in the Addenda to the second Italian edition of E. Segrè, *Enrico Fermi, fisico*, Zanichelli, Bologna 1987, p. 245-247.

ity and improvisation" (to use Holton's words) and a good use of the CNR money they could build with their own hands or buy the necessary equipment. Not all of it, however. They could build by themselves the primitive but efficient Geiger-Müller counters to detect radiation, and buy the substances to be irradiated by their neutron sources, but there was a key element of their instrumentation that they couldn't build by themselves nor buy, but had to be acquired by some other channel. Radioactive sources, the only really expensive piece of their equipment, came into their hands by lucky coincidence:

> "The start of the neutron experiments was facilitated by the very lucky circumstance that Prof. G.C. Trabacchi in the "Laboratorio della Sanità Pubblica", which was located in the same premises as the Physics Institute in Via Panisperna in Rome, had on hand more than one gram of radium and the plant with which to extract the radium emanation and prepare radon-beryllium sources. His wholehearted cooperation was invaluable…"[12]

Trabacchi's "invaluable" cooperation can actually be accurately evaluated. His contribution to the success of Fermi's investigations, which came under the form of one gram of radium, was worth about a million lire. Though the generosity and friendly attitude of Trabacchi towards Fermi and his team has been widely and repeatedly recognized by all of them in several occasions, no one has ever bothered to translate the meaning of this generosity into crude numbers.[13] These numbers put into a rather different perspective the role actually played by Trabacchi in the story; they allow to better appreciate to what extent, far from being just a benevolent old fellow, he fully deserved the nickname of "La Divina Provvidenza". They help to explain why Trabacchi, though he didn't take active part in the experimental work on artificial radioactivity, was included in the group of "inventors" that applied for the first patent on neutron bombardment in October 1934, or why he is the only individual whose support Fermi explicitly and thankfully acknowledged in his 1938 Nobel lecture: he was the one who had made the whole thing possible.

---

[12] E. SEGRÈ, preface to papers 84a to 110, in E. FERMI, *Note e memorie (Collected Papers)*, vol. I, Accademia Nazionale dei Lincei , Roma, and The University of Chicago Press, 1962, pp. 639-640.

[13] The only reference to the actual value of Trabacchi's radium can be found in Laura Fermi's memories of the days in via Panisperna; she relates an episode in the Institute, when Rasetti showed her and Ginestra Amaldi the laboratory and commented "In the safe behind these glass tubes there is the gram of radium of the Divine Providence. It is worth about 670,000 lire", which is a surprisingly accurate figure, surprisingly ignored in all later, less anecdotal accounts of the story. L. FERMI, *Atoms in the Family*, The University of Chicago Press, 1954.

Establishing state control over radioactive substances and creating a specific scientific body to that effect had been a project pushed by Corbino back in 1923, in his capacity as Minister of National Economy during the first Mussolini government. An Office for Radioactive Substances had been created (usually called *Ufficio del Radio*), which in 1925 changed name and status, becoming the *Laboratorio Fisico* under the responsibility of the General Direction of Public Health, at the time a branch of the Ministero dell'Interno. The Laboratory was located in the basement of the Physics Institute in via Panisperna; Giulio Cesare Trabacchi, a physicist and former assistant of Corbino in that same Institute, was named director already in 1924 (he kept office until 1958).[14] The task of the Laboratory was to exercise control over the radioactive material in possession of the State, to distribute it among the various medical institutions and supervise its use for therapeutical purposes, to deal with the technical problems related to the calibration and safe handling of such material, and more generally to perform any kind of scientific research in the field of physics connected with the task of safeguarding public health. It was a small office hosted inside a large institute, yet it disposed of means that the larger institute couldn't even dream of. The Direction of Public Health was not the National Education; as a branch of the Ministero dell'Interno, it could afford large expenditures, and radium was expensive. Though its price had dropped in the afterwar years to stabilize around a thousand lire per milligram, the total amount of radioactive material handled by Trabacchi's laboratory was still worth "a fabulous wealth". From the original 500 mg in 1924, the laboratory came to manage in 1936 about four grams of radium; its cost was approximately four millions lire. Already in 1928 Trabacchi could sign an order to the Union Minière du Haut Katanga for the acquisition of 1041 mg of radium minerals, for an amount of slightly less than a million lire: a sum vastly superior to the total budget of CNR for that same year.[15]

In view of his institutional duties, radioactivity meant to Trabacchi sample dosimetry and calibration, radiological screens, protection from radiation, preparation of sources for therapeutical uses. For the boys upstairs, it meant beta decay, nuclear structure, artificial nuclear processes induced by neutron

---

14 G. Battimelli, *Le origini del laboratorio di fisica*, in *Domenico Marotta nel 25° anniversario della morte* (Proceedings of the Conference, Rome July 9, 1999), Vol. 117, Memorie di Scienze Fisiche e Naturali, Rendiconti della Accademia Nazionale delle Scienze detta dei XL, ser. V, vol. XXIII (1999), pp. 149-160.

15 Documents from the folders "Laboratorio di Fisica", Istituto Superiore di Sanità, Archivio Centrale dello Stato, Rome.

bombardment; by a "lucky circumstance" the investigation of the related fundamental physical properties required the use of those very sources that they couldn't afford to buy given the limited means at disposal, and that Trabacchi handled liberally in his laboratory in the basement, for reasons that had in principle nothing to do with research in fundamental physics. The link provided by Corbino made the rest; fully aware of the significance of Fermi's work, Trabacchi compensated with his sources for the inadequate support given to fundamental research by the institutions that were supposed to do so. It was a lucky circumstance indeed, that allowed the completion of the outstanding work on nuclear structure and neutron properties that would grant Fermi the Nobel Prize; it was, however, a circumstance that couldn't indefinitely compensate for the lack of investment in the field, as it became soon clear when a leap forward in instrumentation and scale was required by the advancements in nuclear science and the related technological developments.

Failures: the laboratory that never materialized

The cyclotron and the different types of high-voltage accelerators developed throughout the thirties, and by the second half of the decade the machine invented and perfected by E.O. Lawrence at Berkeley had established itself as the best tool for producing beams of high-energy particles to be used for research in the fast growing field of nuclear physics. Up to 1935 physicists visiting Berkeley tended to give a skeptical judgment about the merits of the cyclotron; it still was seen as a not very reliable, complicated contraption, giving beams of feeble intensity compared with those obtainable by van de Graaff or Cockcroft-Walton HV accelerators, and having the added disadvantage of requiring unfamiliar electronic technology for its operation, not to mention – last but not least – the fact that it was definitely more expensive than HV machines. Already in 1936, however, the balance began to shift; the 27-inch cyclotron started operating in Berkeley (and operating in a regular way), while the following year Lawrence introduced an improved, 37-inch version. These machines were now capable of producing beams whose intensities were not different from those given by HV accelerators, but at definitely higher energies. The growing interest in beams of energetic deuterons or neutrons for the production of artificial isotopes in the "high" region of the periodic system established in a few years the cyclotron as an almost indispensable tool for any active nuclear physics laboratory. In 1939 Lawrence put into operation the 60-inch version of the machine, and by that time cyclotrons were working, or under construction, in several american laboratories and in Cambridge, Copenhagen, Liverpool, Paris and Stockholm in Europe; towards the end of the decade, "a laboratory without a cyclotron could no longer compete in the interdisciplinary nuclear science invented in Berkeley".[16]

The need to move from natural radioactive sources to accelerators was felt by Fermi and his team as early as 1935. Thanks to money provided by the Fondazione Volta, the Rome physicists travelled to the States during the summers of 1935 and 1936 to learn about the different kinds of accelerators being developed, and make up their minds on which machine to prefer. Rasetti went to Pasadena and Berkeley in 1935, and Amaldi visited the East Coast laboratories at Columbia and at the Carnegie Institution in 1936, while at the same time Segrè was studying the cyclotron in Berkeley.[17] Rasetti's lack of enthousiasm about Lawrence's cyclotron gave way the following year to Segrè's admiration for the performances of the 27-inch version, that had more than doubled the beam intensity and energy. A few months later, Segrè would discover in Palermo element 43 analyzing material irradiated by that same cyclotron, sent to him from Berkeley, and express his thanks to Lawrence referring to the cyclotron as "a sort of hen laying golden eggs".[18]

A first chance to raise the financial means needed to build an accelerator came through the direct intervention of the director of the *Istituto di Sanità Pubblica*. The *Istituto* had been created in 1934, putting together the different laboratories previously attached to the *Direzione Generale della Sanità*, and the chemist Domenico Marotta had been appointed director in July 1935. In that same year the *Istituto* with its laboratories moved to its new headquarters, an imposing building erected with the financial support of the Rockefeller Foundation just across the street from the site where the new *Città Universitaria* was being completed. Trabacchi moved with his laboratory to the new site; the Physics Institute would do the same less than two years later, leaving via Panisperna to find accomodation in its present location at the *Città Universitaria*. Marotta saw a promising road for the devel-

[16] J.L. Heilbron, "The First European Cyclotrons", *Rivista di Storia della Scienza*, 3 (1986), 1-44, quot. p. 7.

[17] F. Rasetti, *Sorgenti artificiali di neutroni (Stati Uniti, agosto-ottobre 1935-XIII)*, Viaggi di studio promossi dalla Fondazione Volta III, 1936, pp. 77-79; E. Amaldi, *Istituti di fisica negli Stati uniti (luglio-ottobre 1936-XIV)*, Viaggi di studio promossi dalla Fondazione Volta IV, 1938, pp. 7-10.

[18] I. Gambaro, *La scoperta del tecnezio*, Atti dell'VIII Congresso Nazionale di Storia della Fisica (Napoli 12-17 ottobre 1987), 1987, pp. 187-200; E. Segrè, *A Mind always in Motion – The Autobiography of Emilio Segrè*, University of California Press, Berkeley 1993.

opment of the scientific activity of his *Istituto* through the collaboration with Fermi on the ground of artificial radioactivity. He could dispose of the means needed to start an ambitious program of production of radioactive substances, a program that would highly profit from being backed by the scientific authority of Fermi, and Fermi could rely on the support of Marotta and the means provided by the Sanità to try to achieve that leap forward in instrumentation that university and CNR would likely not make possible. As soon as October 1935 Marotta started to look for ways of interaction with Fermi, asking "his opinion on the possibility that artificial radioactive substances may be of practical use to substitute Radium in medical applications, which for Public Health would be of invaluable interest". Fermi's positive answer clearly showed his interest in future joint projects on the subject.[19] These exchanges and others that followed gave birth to the idea of building at the physics laboratory of the *Sanità* an accelerator for the production of artificial radioactive substances. The proposal, formally advanced by Trabacchi but certainly discussed previously in detail between him, Marotta and Fermi, was forwarded by Marotta to the *Ministero dell'Interno* in December 1936. To justify such a request Marotta stressed the advantages for his Istituto, and for the country at large: the possibility of producing substances for therapeutic uses on a relatively large scale, the prospect of not having to rely on foreign sources for the acquisition of radium, the increased safety due to the controlled source of the radioactive materials, the added prestige coming to Italy "in a field where our country already acquired a brilliant standing thanks to the researches of the Academician Fermi".[20] No mention was made of a further benefit, which was however obviously present to Marotta, and to Fermi as well: the new machine would have provided the physicists with a badly needed tool to keep investigating the fundamental properties of the nucleus.

The choice for the *Sanità* accelerator fell on the more conventional, high-voltage machine. Marotta's request was for a 1 MV Cockcroft-Walton: in his proposal he asked for 300,000 lire for the construction of the apparatus, and around 100,000 more lire per year for its operation. Though the estimated cost was sensibly inferior to that of a cyclotron, it was nonetheless a sum

[19] D. Marotta to E. Fermi, October 21, 1935; E. Fermi to D. Marotta, October 23, 1935; folders "Laboratorio di Fisica", Istituto Superiore di Sanità, Archivio Centrale dello Stato, Rome.

[20] D. MAROTTA, *Appunto per l'on. gabinetto – Oggetto: fabbricazione di sostanze radioattive artificiali*, Amaldi Archive, Dipartimento di Fisica, Università "La Sapienza", box 21 E.

largely exceeding the total investment made by CNR in nuclear physics in the previous years, and a good order of magnitude larger than the average budget of a university physics institute at the time. A 200 kV prototype was realized in June 1937 by Amaldi, Fermi and Rasetti in the new Physics Institute, while the final 1 MV version was completed two years later in the basement of the *Istituto di Sanità*; Fermi had by that time already left Italy for good.[21]

It was however clear to Fermi that the generous support of the *Istituto di Sanità* and its physics laboratory could not substitute for what he perceived as absolute priority for the development of physical research in Italy: the creation of a national laboratory for physics, independent from any single university institute and endowed with the best available equipment. Remembering his ill-fated attempt of 1930 to turn the physics institute in Rome into such a special laboratory, he resolved to push the project addressing himself to CNR. The move was carefully prepared: at the end of 1936 Fermi wrote to the directors of some of the more prestigious European laboratories, inquiring about size, equipment and personnel dotation, and budget. On the basis of information received by Cockcroft, Joliot and Scherrer,[22] he submitted to CNR a proposal for the creation of a national laboratory modeled on the most advanced research institutions abroad.[23] In the accompanying letter to the CNR Directory, Fermi made clear the nature of the changes affecting the field of nuclear research: if "radioactive technique could up to now largely use as primary sources the natural radioactive substances", so that "the ordinary means of a university physics laboratory could, with limited external support, be adequate for the development of research", this was no longer going to be true.

> "In the main foreign countries, beside the technique of natural sources, a new one has been developed, based on artificial sources obtained through bombardment by accelerated ions… It is clear that these circumstances

---

[21] E. Amaldi, E. Fermi, F. Rasetti, "Un generatore artificiale di neutroni", *Ricerca Scientifica* 8(2), 1937, 40; E. Amaldi, D. Bocciarelli, F. Rasetti, G.C. Trabacchi, "Generatore di neutroni a 1000 kV", *Ricerca Scientifica* 10, 1939, 623.

[22] P. Scherrer to E. Fermi, January 16,1937; J. Cockroft to E. Fermi, January 21, 1937; F. Joliot to E. Fermi, January 25, 1937; Amaldi Archive, Dipartimento di Fisica, Università "La Sapienza", box 1E.

[23] For a study of parallel developments on the matter in Rome and Paris see I. Gambaro, *Acceleratori di particelle e laboratori per le alte energie: Roma e Parigi negli anni Trenta*, Rivista di Storia della Scienza 1, II ser. (1993), pp. 105-154. On Fermi's and later Italian efforts on accelerators see G. Battimelli, I. Gambaro, Un laboratorio per le alte energie alla vigilia della seconda guerra mondiale, Atti del XIV e XV Congresso Nazionale di Storia della Fisica, Conte, Lecce 1995, pp. 475-487; G. Battimelli, I. Gambaro, *Da via Panisperna a Frascati: gli acceleratori mai realizzati*, Quaderni di Storia della fisica 1 (1997), pp. 319-333.

> make it hopeless to think of an effective competitiveness with laboratories abroad, unless even in Italy a way is found to organize these researches on an adequate basis, for which it seems highly unlikely that the resources of a single university institute may be sufficient. I therefore take the liberty of suggesting the opportunity for the Consiglio Nazionale delle Ricerche to take the initiative for the creation of a National Institute for Radioactivity."[24]

That the proposed laboratory was to be meant essentially for the benefit of fundamental physics emerged clearly from the research targets that Fermi listed for such an Institute: alongside with "the study of the properties of new radioactive bodies" and the "application of artificial radioactive substances as indicators for the study of chemical reactions", first and foremost came the "innumerable unsolved problems related to nuclear structure and neutron properties". To the proposal Fermi appended a detailed (though a little conservative) budget estimate: 300,000 lire for equipment, and 230,000 lire per year for ordinary administration. Though in the proposal it was not explicitly stated what kind of equipment Fermi envisaged for the laboratory, it can be confidently assumed that he was considering a cyclotron. This, at least, was obvious in the summer of 1937, when he went to Stanford and visited Berkeley: in August he wrote to the director of the Physics Institute in Rome about "the reasonable prospect of building a cheap cyclotron".[25]

How "cheap" a cyclotron could be was translated in crude numbers by Rasetti not much later, in an address delivered at the annual meeting of the Italian Society for the Advancement of Sciences (Società Italiana per il Progresso delle Scienze, SIPS). Talking on "Recent advancements of nuclear physics", Rasetti went on, in the last part of his speech, to discuss the new means developed for nuclear research, high tension accelerators and the cyclotron:

> "This last apparatus has proven itself to be the more powerful, and presently there are, functioning or in an advanced stage of contruction, 12 exemplars in the United States, one in France, two in England and one in Denmark. Why don't we build one here at home? It will not be necessary for me to explain the reasons to the audience, who knows well the not astronomical numbers of the budgets of university institutes, when I say that the construction of a cyclotron requires about 80 tons of iron, 8

[24] E. Fermi to CNR, January 27, 1937; Archivio Centrale dello Stato, CNR, box 105, folder "Istituto di fisica dell'Università di Roma".

[25] E. Fermi to A. Lo Surdo, August 5,1937; Amaldi Archive, Dipartimento di Fisica, Università "La Sapienza", box 1E.

> tons of copper, and the installation of a short-wave oscillator with a power comparable to that of the most powerful broadcasting station in Italy. Or when I say, more synthetically, that the cost of a cyclotron today is around a million lire".[26]

Rasetti went on to say that, while Fermi's discoveries had been made possible even with limited means (here he gave the figure, mentioned earlier, of 150,000 lire for the total cost of these researches over a period of four years), this time was over. And, to give a more optimistic tone to the conclusions of his talk, he ended with an appeal to CNR and to the highest authority of the country:

> "For further progress we need the collaboration of several researchers and a powerful organisation of laboratories, such as only a special body like CNR could develop. The interest of the Chief for science and its applications, that led to the creation of this highly effective body, gives us the right to be confident that these problems, just as endless others faced and solved by the Regime, will find in a short time a solution worthy of imperial Italy".

Most likely, Rasetti was not all that confident in the effectiveness of CNR, or in the interest of the Chief in the matter. He had no reason to be. Pressed by more immediate concerns of application of science to the contingent problems of autarchy and impending war, CNR did not pay any serious attention to Fermi's proposal for a national laboratory. In May 1937, a sum of 30,000 lire was granted to Fermi for the completion of the 200 kV prototype accelerator at the physics institute. The death of Guglielmo Marconi in July, following that of Orso Mario Corbino, suddenly deceased in January, deprived Fermi of most of the support he could count on in the Council. A final decision on Fermi's proposal was taken only in June 1938: considering that "for the creation of an Institute of Radioactivity… much larger and more conspicuous means would be required that those approximately estimated by S.E.Fermi", the Presidency of CNR resolved not to take into consideration the creation of such an institute, leaving to the Directory the decision whether to grant Fermi an annual budget "to organize researches in the field of radioactivity".[27]

---

[26] F. Rasetti, *Progressi recenti della fisica nucleare*, SIPS – Scienza e tecnica, supplemento agli Atti della Società Italiana per il Progresso delle Scienze, 1937, 337.

[27] The relative documents are in Archivio Centrale dello Stato, CNR, box 105, folder "Istituto di fisica dell'Università di Roma".

Conclusion

The untimely death of Corbino in January 1937 prevented him from witnessing the final disruption of Fermi's group, and the emigration of several of his members. It is not clear what his perception was of the deteriorating climate and of the downhill slope physical research in the country had started sliding on. According to Amaldi, he didn't seem to worry about that issue in the last months of his life:

> "Around 1936 he used to say that, given the international reputation gained by Fermi and his school, it was no longer necessary for him to take care of the further development of nuclear physics: it was in excellent hands, and its future could therefore be given for granted".[28]

The usually far-sighted Corbino was on that occasion deluding himself. Nuclear physics was "in excellent hands" as far as its practitioners were by now firmly installed in academic positions, and had made a name for themselves thanks to the excellent work done under Fermi's guidance. To give its future for granted, however, one had to count on the fact that such excellence would be maintained in the future thanks to adequate support. This was not likely going to be the case; evidence pointed in the opposite direction. The Nobel prize awarded to Fermi for his outstanding researches on nuclear physics came shortly after the main national state agency in charge of supporting science had let him know that the means required to keep doing that kind of research were not available, and will not be for time to come. There is no doubt that the promulgation of the racial laws in the second half of 1938 was a key factor in forcing Fermi to take the decision to emigrate, but they didn't arrive as a sudden and unexpected blow from the outside world to perturb what would otherwise be a rising parable of scientific production. The parable was already declining, and the racial laws were the last element leading to a final move that was already contemplated on the basis of strictly scientific considerations. Fermi's failure to create a large physical laboratory to keep the competitive status that Italian nuclear science had acquired in previous years points to the failure of Italian scientific institutions, pressed by needs of immediate application and constrained by chronic shortage of funds, to give adequate support to fundamental science. This, in turn, reflects the failure of the Italian ruling class (or, at least, of that part of

[28] E. AMALDI, *Corbino, Orso Mario*, Dizionario Biografico degli Italiani, Vol. 28, Istituto della Enciclopedia Italiana, Roma 1983, pp. 760-762.

the Italian ruling class that finally emerged to power through the inner tensions of late fascism) to fully appreciate science's role in society, and illustrates the impoverished and distorted way in which the relation between science and modernization was then perceived and pursued.

Giovanni Battimelli
Giovanni Battimelli is associate professor at the Physics Department of the University "La Sapienza" in Rome. He has done extensive research on different aspects of late XIXth and XXth century physics, and on the development of Italian scientific institutions since 1870. In collaboration with M. De Maria and G. Paoloni, he has edited the writings by E. Amaldi on the history of physics; recently he has co-authored volumes on the history of CNR (Consiglio Nazionale delle Ricerche) and INFN (Istituto Nazionale di Fisica Nucleare).

Sam Schweber

Fermi and Quantum Electrodynamics (QED)

Between 1928 and 1932 Fermi wrote several fundamental papers elucidating in a readily vizualizable and intuitive way the physics that results from the interaction of charged particles with the electromagnetic field when both are quantized. I will review these contributions with special emphasis on his 1932 *Reviews of Modern Physics* article from which an entire generation of physicists learned QED.

Enrico Fermi e l'elettrodinamica dei quanti (QED)

Tra il 1928 ed il 1932 Fermi scrisse diverse relazioni fondamentali in uno stile chiaro ed immediato sulla fisica risultante dall'interazione di particelle cariche con un campo elettromagnetico quando entrambi siano quantizzati. La mia relazione verterà specificatamente sul suo articolo pubblicato nel 1932 sulla rivista "Reviews of Modern Physics", dal quale una intera generazione di fisici apprese la QED.

Introduction

In 1926, shortly after the publication of Schrödinger's wave mechanics papers, papers he had carefully studied and mastered, Fermi wrote a short article entitled "Arguments pro and con the hypothesis of light quanta". In it he indicated that "at the present time the state of science is such that one can say that we lack a theory that gives a satisfactory account of optical phenomena." He listed the experiments that were convincingly explained by light being assumed to be constituted of (particle-like) photons such as the photoelectric effect and the Compton effect, and those that were readily understood in terms of the wave theory, namely, interference and diffraction.

The challenge, Fermi stated, was to elucidate the processes involved in the interaction of light with matter at the atomic level and to give intuitive explanations of optical phenomena at this microscopic level. (Fermi 1926) Fermi met the challenge. In a series of papers from 1929 to 1932 he formulated a relativistically invariant description of the interaction between charged particles and the electromagnetic field which treated both particles and electromagnetic field quantum mechanically.

He first devised a simple, readily interpretable, Hamiltonian description of charged particles interacting with the e.m. field, then indicated how to quantize this formulation and thereafter showed how to exploit perturbation theory to describe quantum electrodynamic phenomena.

There were others who tackled these same problems, but none of their formulations – Heisenberg and Pauli's papers in particular – had the simplicity, Anschaulischkeit, yet thoroughness of Fermi's approach. What Fermi accomplished with his papers was the following:

1) He provided a simple, vizualizable way to describe the interactions between photons and charged particles. It was the formulation from which an entire generation learned how to think about quantum electrodynamical effects in atomic phenomena, and on which Heitler based his 1936 edition of *The Quantum Theory of Radiation* which taught physicists how to calculate cross-sections for quantum electrodynamic processes. It was the point of departure for Feynman in 1939 when addressing quantum electrodynamics in his Lagrangian formulation, and the source of Weisskopf's insight that the Lamb shift could be interpreted as the effect of the zero point energy vacuum fluctuations of the electromagnetic field on the motion of the electron in the hydrogen atom. In addition, Fermi showed how to handle the gauge conditions in QED when the electromagnetic field is described by vector and scalar potentials – and his

treatment was the point of departure for fixing gauges in quantized gauged theories.

2) He indicated under what circumstances the intuitive picture of a photon as a massless, spin 1 particle-like entity that moves with velocity c was appropriate. And
3) in a paper with Bethe he helped secure the perturbation theoretic picture that depicts the interaction between charged particles as stemming from the exchange of photons.

I believe that these QED studies were important, if not necessary, preparations for Fermi being able to formulate in 1933 his theory of β-decay and that in the first of these papers he could assert that electrons *do not* exist as such in nuclei before β-emission occurs

> but that they, so to say, acquire their existence at the very moment when they are emitted; in the same manner as a quantum of light, emitted by an atom in a quantum jump, can in no way be considered as pre-existing in the atom prior to the emission process.
> In this theory, then, the total number of the electrons and of the neutrinos (like the total number of light quanta in the theory of radiation) will not necessarily be constant, since there might be processes of creation or destruction of these light particles. [Fermi 1934]

Fermi's paper on β-decay constitutes the birth of quantum field theory as applied to elementary particle physics.

Rendiconti della Accademia dei Lincei

In his preface to the articles on QED in Volume I of *The Collected Papers of Enrico Fermi* (Fermi 1962) Edoardo Amaldi indicated that Fermi started studying the quantum theory of radiation during the winter of 1928-29. He mastered the two 1927 papers by Dirac that had laid the foundations of the subject (Dirac 1927a, b). He familiarized himself with the papers of Jordan and Klein (1927)and of Jordan and Wigner (1928) in which the wave-mechanical description in 3N dimensional configuration space of a system of N identical non-relativistic Bosons and Fermions was recovered by "quantizing" an appropriate Schrödinger equation considered as a field system. And at the time he also read and was impressed by Jordan and Pauli's 1928 paper in which they had formulated a quantization procedure for the free electromagnetic field in a relativistically invariant manner.

Recall that in his first paper, Dirac (1927a) dealt with the problem of the interaction of an atom with the radiation field and described photons as particles of zero rest mass obeying Bose-Einstein statistics. The Hamiltonian was taken to be

$$H = H_0 + V \tag{1}$$

with

$$H_0 = H_p + \sum_r W_r b^*{}_r b_r \tag{2}$$

where H_p is the hamiltonian of the atom, and the b's are creation and annihilation operators for the photons

$$\begin{aligned} b_r &= e^{-i\theta_r/h} N_r^{1/2} \\ b_r^* &= N_r^{1/2} e^{+i\theta_r/h} \end{aligned} \tag{3}$$

with

$$N_r = b_r^* b_r \tag{4}$$

the number operator for photons of energy W_r. The b operators satisfy the Bose commutation rules

$$[b_r, b_s^*] = \delta_{rs} \tag{5}$$

Dirac took the interaction between the photons and the atom to be of the form

$$V = \sum_{rs} v_{rs} b_r^* b_s \tag{6}$$

Since this Hamiltonian conserves the number of photons it cannot describe the spontaneous emission of photons nor their absorption. However Dirac assumed zero energy photons (i.e. those with r=0) to be special as they are unobservable. He characterized the vacuum as a state with an infinite number of zero energy and zero momentum photons, and he stipulated that in any physical state there are an infinite number of such photons. Photon emission was then considered as a transition from the vacuum state to a state of a single photon with finite momentum and energy; photon absorption consisted of the reversed transition.[1] In fact, he imposed the limit $N_0 \to \infty$ in such a way that

$$v_{r0} (N_0 + 1)^{1/2} e^{-i\theta_r/h} \rightarrow v_r \tag{7a}$$

$$v_{0r} (N_0)^{1/2} e^{+i\theta_r/h} \rightarrow v_r^* \tag{7b}$$

The interaction term thus became

$$\sum_{r\neq 0} (v_r b_r^* + v_r^* b_r) + \sum_{r\neq 0} \sum_{s\neq 0} v_{rs} b_r^* b_s \tag{7c}$$

and the terms linear in the b_s and b_s * could then account for the emission and absorption of photons, including spontaneous emission. In the last brief section of his paper, Dirac turned to the interaction of an atom with the electromagnetic field as described "from the wave point of view." He adopted the Coulomb gauge in which the radiation field is described by a transverse (divergenceless) vector potential which is considered to be a non-commuting variable and showed the equivalence between the two approaches if in the wave formulation the interaction is taken to be

$$\frac{e}{mc} \mathbf{p} \cdot \mathbf{A}(\mathbf{q}) \tag{7d}$$

and the Fourier expansion coefficients of $\boldsymbol{A}$ are appropriately chosen.

Amaldi tells us "that the method used by Dirac did not appeal to Fermi, who preferred, as he did very often, to recast the theory in a form mathematically more familiar to him" (Amaldi in Fermi 1965, p. 305). It was probably not so much the mathematical aspects of Dirac's formulation that didn't appeal to Fermi, but the physical approach. Although in Dirac's approach the notion of a photon was apparent, its connection to the quantization of the radiation field was not clear for his formulation did not rest upon a well defined quantization procedure for the electromagnetic field; nor was it obviously relativistically invariant; nor could it deal with the problem of field reaction during the act of emission of a photon – a problem that Fermi had addressed after he had read Schrödinger's papers (Fermi 1927). Also, as initially formulated Dirac's papers dealt only with the interaction of a single charge with the e.m. field, and it was not clear how retardation effects could be taken into account in the interaction between charges. But it is evident from the problems that Fermi addressed that he had noted Dirac's desiderata for any acceptable quantum electrodynamics:

1) it must correctly take into account the fact that electromagnetic "forces are propagated with the velocity of light instead of instantaneously";
2) it must describe "the production of an electromagnetic field by a moving electron";
3) it must describe "the reaction of this field on the electron". And
4) the formulation must "satisfy all the requirements of the restricted principle of relativity." (Dirac 1927a, p. 243-4)

In his first article on "quantum electrodynamics" Fermi stressed that he wanted to formulate the equations of motion of classical electrodynamics in such a way that "they can readily be translated into a quantum form". His approach was to describe the em field (assumed to be contained in a large cavity of volume Ω) in terms of a vector and scalar potential that satisfied:

$$\nabla^2 V - \frac{1}{c^2}\frac{\partial^2 V}{\partial t^2} = -4\pi\rho \tag{8}$$

$$\nabla^2 \boldsymbol{U} - \frac{1}{c^2}\frac{\partial^2 \boldsymbol{U}}{\partial t^2} = -4\pi \boldsymbol{j} \tag{9}$$

These potentials were then Fourier analyzed

$$V(\boldsymbol{X}, t) = \sqrt{\frac{8\pi}{\Omega}}\, c \sum_s Q_s(t) \cos\left(\frac{2\pi\alpha_s \cdot \boldsymbol{X}}{\lambda_s} + \beta_s\right) \tag{10}$$

$$\boldsymbol{U}(\boldsymbol{X}, t) = \sqrt{\frac{8\pi}{\Omega}}\, c \sum_s \boldsymbol{q}_s(t) \sin\left(\frac{2\pi\alpha_s \cdot \boldsymbol{X}}{\lambda_s} + \beta_s\right) \tag{11}$$

In these equations, α_s is a unit vector in the direction of propagation of the *s* th wave, and β_s is a phase factor.

The equations of motion (8) and (9) expressed in terms of the Fourier coefficients Q_s and q_s take the following form when it is assumed that the charge and current densities' ρ and j arise from point particles located at $\boldsymbol{X}_i$ at time t and moving with velocity $d\boldsymbol{X}_i/dt$:

$$\frac{d^2 Q_s}{dt^2} + 4\pi^2\nu_s^2 Q_s = \sqrt{\frac{8\pi}{\Omega}}\, c \sum_i e_i \cos\left(\frac{2\pi\alpha_s \cdot \boldsymbol{X}_i}{\lambda_s} + \beta_s\right) \tag{12}$$

$$\frac{d^2 \boldsymbol{q}_s}{dt^2} + 4\pi^2\nu_s^2 \boldsymbol{q}_s = \sqrt{\frac{8\pi}{\Omega}}\, c \sum_i e_i \dot{\boldsymbol{X}}_i \sin\left(\frac{2\pi\alpha_s \cdot \boldsymbol{X}_i}{\lambda_s} + \beta_s\right) \tag{13}$$

Charge conservation requires that

$$\nabla \cdot \boldsymbol{U} + \frac{1}{c}\frac{\partial V}{\partial t} = 0 \tag{14}$$

which translates into the requirement that

$$2\pi\nu_s \chi_s + \frac{dQ_s}{dt} = 0 \tag{15}$$

where χ_s is the longitudinal component of q_s, i.e. the component of q_s along the direction of propagation α_s.

With Eqs. (12) and (13), that is, with the translation of the description of the dynamics of the electromagnetic field into that of (coupled) harmonic oscillators, Fermi achieved one of his stated objectives. It allowed him to readily formulate quantization rules for the e.m. field and to give an "anschaulisch" – intuitive and vizualizable representation of the interaction of the quantized e.m. field with quantum mechanically described electrons, atoms and molecules. Fermi's first paper on QED concluded with writing down the Hamiltonian that yields the equations of motion (13) and (14), and those of the charged particles (assumed to be non-relativistic):

$$\begin{aligned} H = {} & \sum_i \frac{p_i^2}{2m_i} + \sum_s \frac{1}{2}(\omega_{s1}^2 + \omega_{s2}^2 + \omega_s^2 - P_s^2) + \\ & \sum_s 2\pi^2\nu_s^2\,(w_{s1}^2 + w_{s2}^2 + \chi_s^2 - Q_s^2) + \\ & \sqrt{\frac{8\pi}{\Omega}}\, c \sum_{is} e_i Q_s \cos\left(\frac{2\pi\alpha_s \cdot \mathbf{X}_i}{\lambda_s} + \beta_s\right) - \\ & \sqrt{\frac{8\pi}{\Omega}} \sum_{is} \frac{e_i}{m_i}\,(\alpha_s\chi_s + \mathbf{A}_{s1}w_{s1} + +\mathbf{A}_{s2}w_{s2}) \cdot \mathbf{p}_i \\ & \sin\left(\frac{2\pi\alpha_s \cdot \mathbf{X}_i}{\lambda_s} + \beta_s\right) \end{aligned} \tag{16}$$

where the ω_s and the P_s are the conjugate variables to the w_s, χ^s and Q_s. In writing down (16) a decomposition of the q_s along direction perpendicular (As1 and As2) and parallel to the wave s (α_s) has been made:

$$\mathbf{q}_s = \alpha_s\chi_s + \mathbf{A}_{s1}w_{s1} + \mathbf{A}_{s2}w_{s2} \tag{17}$$

Fermi noted that Eq. (16) yields the correct equations of motion and to order v/c reduces to the familiar Hamiltonian for interacting non-relativistic point charges:

$$H = \sum_i \frac{\mathbf{p}_i^2}{2m_i} + \sum_i e_i V_i - \sum_i \frac{e_i}{m_i c}\mathbf{p}_i \cdot \mathbf{U}(\mathbf{q}_i) \tag{18}$$

Since the theory had now been expressed in terms of canonical variables, these could now be promoted to non-commuting operators satisfying the usual commutation rules. In configuration space these canonical momenta *p* are equivalent to ih ∂/∂q and thus it was clear what operations to attribute to the Hamiltonian operator in the Schrödinger equation describing the em field-charged particle system:

$$H\Psi = -\frac{h}{2\pi i}\frac{\partial\Psi}{\partial t} \tag{19}$$

Pauli after reading Fermi's paper wrote Pascual Jordan that he only perused a recent article by Landé fleetingly ("sehr flüchtig") but that "he studied Fermi's *Rendiconti della Accademia dei Lincei* (Mai 1929) electrodynamics article very carefully ("genau"). It does not depend at all on Heisenberg and my article and is methodologically interesting although it doesn't produce any new results" (Pauli 1979, p. 523).

A further methodological interesting thing that Fermi did was expounded in his second paper on QED. There he showed explicitly that the Hamiltonian does indeed yield the desired equations of motion. Furthermore, that the gauge condition (14) can be stated as follows:

$$2\pi\nu_s\chi_s - P_s = 0 \tag{20}$$

that its time derivative is equal to

$$\omega_s - 2\pi\nu_s Q_s + \frac{c}{2\pi\nu_s}\sqrt{\frac{8\pi}{\Omega}}\sum_i e_i \cos\left(\frac{2\pi\alpha_s \cdot \boldsymbol{X}_i}{\lambda_s} + \beta_s\right) = 0 \tag{21}$$

(21)

(corresponding to the equation div $E = 4\pi\rho$) and that by virtue of the equations of motions

$$\left(\frac{d^2}{dt^2} + 4\pi^2\nu_s^2\right)\left(2\pi\nu_s\chi_s - P_s\right) = 0 \tag{22}$$

These are classical results. When the theory is quantized, i.e. when the classical variables are promoted to non-commuting operators, the gauge condition (14) cannot be taken as an operator identity since

$$2\pi\nu_s\chi_s - P_s$$

does not commute with the Q_s and the q_s. In order to circumvent this difficulty Fermi made the weaker demand that Eqs. (20) and (21) restrict the possible states of the system. In fact Fermi exhibited explicitly the states Ψ of the system that satisfy the subsidiary conditions:

$$\left(2\pi\nu_s\chi_s - P_s\right)\Psi = 0$$

$$\left(\omega_s - 2\pi\nu_s Q_s + \frac{c}{2\pi\nu_s}\sqrt{\frac{8\pi}{\Omega}}\sum_i e_i \cos\left(\frac{2\pi\alpha_s \cdot \mathbf{x}_i}{\lambda_s} + \beta_s\right)\right)\Psi = 0 \tag{23}$$

They are

$$\Psi = \left[\prod_s \exp\left(\frac{2\pi i}{h}\chi_s\left(\omega_s - 2\pi\nu_s Q_s + \frac{c}{2\pi\nu_s}\sqrt{\frac{8\pi}{\Omega}}\sum_i e_i \cos\left(\frac{2\pi\alpha_s \cdot \mathbf{x}_i}{\lambda_s} + \beta_s\right)\right)\right)\right] \Phi\left(t, \mathbf{x}_i, \sigma_i, w_{s1}, w_{s1}\right) \tag{24}$$

Fermi then showed that Φ satisfies the following equation

$$R\Phi = -\frac{h}{2\pi i}\frac{\partial\Phi}{\partial t}$$

$$R = \sum_i \frac{\mathbf{p}_i^2}{2m_i} - \sum_i \frac{e_i}{m_i c}\mathbf{p}_i \cdot \mathbf{A}(\mathbf{q}_i) + \sum_i \frac{e^2}{2mc^2}\mathbf{A}^2(\mathbf{q}_i) + \frac{1}{2}\sum_{ij}\frac{e_i e_j}{r_{ij}}$$

$$\text{with} \qquad \nabla \cdot \mathbf{A} = 0 \tag{25}$$

thus justifying the transverse Coulomb gauge. The Coulomb term contains a self-interaction term which is infinite since the charges are assumed to be point-like. Fermi excluded the terms i = j as they make an infinite *constant* contribution to the Hamiltonian.

Between the submission of his first QED paper and the writing of this second paper on QED Fermi had read Pauli and Jordan's article on a relativistically invariant formulation of the quantum theory of the charge-free electromagnetic field (Pauli 1929). In order to state his theory in a relativistically invariant form Fermi described the charged particles by Dirac equations so that the Hamiltonian in (25) now read as follows:

$$R = \sum_i \left(c\alpha_i \cdot \mathbf{p}_i + \beta_i m_i c^2\right) + \sum_i e_i \alpha_i \cdot \mathbf{A}(\mathbf{q}_i) + \frac{1}{2}\sum_{ij}\frac{e_i e_j}{r_{ij}} \tag{26}$$

In all his QED papers from 1929 to 1932 Fermi described matter as particles obeying either Schrödinger or Dirac equations rather than by quantized fields – à la Jordan-Klein, Jordan-Wigner or Heisenberg and Pauli. Using quantized fields to describe the electrically charged matter – an approach with which Fermi was acquainted – would have made no difference in the non-relativistic case, but was so fraught with difficulties in the relativistic case that much of the Anschaulichkeit of Fermi's approach would have been dispelled.

Early in 1929 Fermi was informed of the work of Weisskopf and Wigner on the theory of line width. This was of great interest to him because very shortly after the advent of wave mechanics he had unsuccessfully addressed the problem of the lifetime of an excited state of an atom and that of the natural line width of the emitted radiation (Fermi 1927). Fermi clearly mastered that paper. In fact, as we shall see Weisskopf and Wigner's paper was the key for Fermi to being to explain interference and other undulatory light phenomena from a microscopic viewpoint. Amaldi tells us that while Fermi was carrying out his researches

> he taught his results to several of his pupils and friends including Amaldi, Majorana, Racah, Rasetti and Segrè. Every day when work was over he gathered the various people... around his table and started to elaborate before them, first the basic formulation of quantum electrodynamics and then, one after the other, a long series of applications of the general principles to particular physical problems. A striking feature of Fermi's method of working on a theoretical problem in public (so to speak) and of teaching at the same time, was the way in which he could say out loud what he was thinking, proceeding at a steady unhesitating pace; never going extremely fast, but never failing to make progress. (Amaldi in Fermi 1962, p. 305)

Gulio Racah in particular, proved to be an extremely valuable assistant in the project. Fermi gave him the problem of explaining interference phenomena quantum electrodynamically, and in two papers submitted to the *Rendiconti* in the spring of 1930 gave a QED treatment of Lippman fringes. (Racah 1930a, b).

Fermi lectured on his findings in the course that he gave in Paris in April 1929 and his lectures are summarized in the *Annales de l'Institut Henri Poincaré*. In them he elaborated on the physical content of his approach. He pointed out that the description of the electromagnetic field as coupled, forced, linear harmonic oscillators allowed an intuitive ("anschaulich") view of electromagnetic processes. He first considered the interaction of a single charged particle with the em field. In the case that its motion is prescribed, i.e. when **v** = d**X**/dt is specified, the equation analogous to (13) corresponding to the Fourier components of **A** (with div **A** = 0) can be integrated. Its solution will contain terms corresponding to the free motion of the oscillator – i.e. pure radiation – and a term stemming from the "force" term, the **v**·**A** term. That part of the solution corresponds to the e.m field (photons) "attached" to the charge, that travel with velocity **v.** For the case **v** = constant, these attached photons give rise to the Biot-Savart field arising from the charged particle's motion.[2]

The principal thrust of the Paris lectures was to establish QED as a readily *usable* theory. To do so Fermi indicated how perturbation theory could be recast as a technique that allowed transition amplitudes to be analyzed and calculated in an almost algorithmic manner. Recall that until the spring of 1929 no one had given a fully quantum mechanical formulation of electrodynamic processes. The computation of the cross-sections for the photoelectric effect, for Compton scattering, ... all depended on either semi-classical or correspondence principle approaches. What Fermi did was to indicate how all these processes could be given a fully quantum mechanical treatment. He demonstrated how perturbation theory should be handled to derive the cross sections for the processes. His point of departure was the Schrödinger-Dirac perturbation theory in which the wave function for the system which satisfies the Schrödinger equation:

$$i\hbar\partial_t\Psi = (H_0 + V)\Psi \tag{27}$$

is expanded in terms of solutions of the unperturbed Hamiltonian

$$H_0\,\Phi_k = \epsilon_k\,\Phi_k \tag{28}$$

that is,

$$\Psi(t) = \sum_k a_k(t)\Phi_k e^{-i\epsilon_k t/\hbar} \tag{29}$$

The amplitude $a_k(t)$ for finding the system in the state Φ_k then satisfy

$$i\hbar\frac{da_k(t)}{dt} = \sum_l (\Phi_k, V\Phi_l)\, a_l(t)\, e^{-\frac{i}{\hbar}(e_k - e_l)t} \tag{30}$$

How to use these equations to compute the a_k for various physical processes is the subject matter of the lectures that Fermi gave at the University of

Michigan Summer School in Ann Arbor during the summer of 1930. It is to these lectures that we now turn. But before doing so it is appropriate to recall the comments Bethe made at the Enrico Fermi memorial symposium at the Washington meeting of the APS on April 29, 1955 shortly after Fermi's death:

> 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. It came after a number of quite complicated papers and before another set of quite complicated papers on the subject, and without Fermi's enlightening simplicity I think many of us would never have been able to follow into the depths of field theory. I think I am one of them. (Bethe 1955)

The Michigan Summer School Lectures

After introducing his formulation of QED – as outlined in his *Rendiconti della Accademia dei Lincei papers* – Fermi turned to the applications and implications of his formalism. The first problem that he addressed was that of the line width of the radiation emitted by an atom. Weisskopf and Wigner had indicated the solution (Weisskopf 1930) and Born had evidently communicated to Fermi their results. The Weisskopf-Wigner solution played an important role in the subsequent examples treated by Fermi in his lectures.

Fermi's description of the problem of the spontaneous emission of radiation is as follows: At time t=0 the atom is an excited state and there are no photons present. After a certain time the atom makes a transition to its ground state and emits a photon[1]: To compute the amplitude that at time t the atom is still in its exited state. We will use a more modern notation to indicate what Fermi did. The clarity of his approach will then not be obscured by the cumbersomeness of his notation.[2] We shall denote the initial state of the system – with the atom in an excited state and no photons present – by Φ_{E0}, a time-independent eigenstate of H_0. Similarly, $\Phi_{Gk\epsilon}$ denotes an (unperturbed) state with the atom in its ground state and one photon of momentum **k**, polarization present. The state vector in the inter-

[1] Fermi assumed that the atom has in fact but two states.

[2] Fermi used the p and q variables of harmonic oscillators to operate on the wave functions describing the state of the radiation field which made for complicated notation.

action picture satisfies the equation

$$i\hbar\partial_t\Phi(t) = H_I(t)\,\Phi(t) \tag{31}$$

Fermi approximated the solution of the problem by assuming that the system can be described solely in terms of the amplitudes

$$a_{E0}(t) = \left(\Phi_{E0},\, \Phi(t)\right) \tag{32a}$$

$$b_{Gk\varepsilon}(t) = \left(\Phi_{Gk\varepsilon},\Phi(t)\right) \tag{32b}$$

and that $\Phi(t)$ could be approximated by

$$\Phi(t) = \left[1 - \frac{i}{\hbar}\int_{-\infty}^{t} H_I(t')\,dt'\right]a(t)\,\Phi_{E0} \tag{33}$$

Thus *a(t)* satisfies the equation

$$\begin{aligned} i\hbar\frac{da_{E0}(t)}{dt} &= \left(\Phi_{E0},\, H_I(t)\Phi(t)\right) \\ &\approx \left(\Phi_{E0},\, H_I(t)\left[1 - \frac{i}{\hbar}\int_{-\infty}^{t} H_I(t')\,dt'\right]\Phi_{E0}\right)a(t) \end{aligned} \tag{34}$$

which then immediately yields

$$\begin{aligned} &\frac{1}{a_{E0}}\,\frac{da_{E0}(t)}{dt} = \\ &-\frac{i}{\hbar c}\int_{-\infty}^{t} dt'\int d^3r\int d^3r'\, e^{ik.(x-x')-ic|k|(t-t')}\left(j(\boldsymbol{x},t)\,j(\boldsymbol{x}',t')\right)_{EE} \\ &\qquad = -\Gamma - \frac{i}{\hbar}\Delta E \end{aligned} \tag{35}$$

ΔE, the level shift due the emission and reabsorption of a photon is as it stands divergent – and was omitted both by Weisskopf and Wigner and by Fermi. For times long compared to atomic frequencies *a(t)* is thus assumed to be given by

$$a_{E0}(t) = e^{-\Gamma t} \tag{36}$$

We have omitted a fair amount of nitty-gritty details. In the above one has to account for taking the time integral from -∞ to t rather than from 0 to t. The justification given for this is that the initial condition for the atom to be in the excited state will depend on the mechanism for getting it into that state – and is a rather complicated matter. The solution exhibited above, i.e. the Weisskopf-Wigner approximation – is only valid for times t large compared to all atomic frequencies after the atom has settled down to a “quasi-stationary” state of radiative decay. The integration from -∞ to 0 presumably smoothes out the preparation issue and thereafter the integration to t (with t large compared to the natural frequency of the atom) t is to effect this quasi-stationarity.[3] Fermi was of course aware of all these difficulties and that the Weisskopf-Wigner approximation could not be readily justified. The fact that Fermi could circumnavigate all the difficulties led Wigner to comment:

> Fermi disliked complicated theories and avoided them as much as possible... His article on the Quantum Theory of Radiation in the *Reviews of Modern Physics* (1932) is a model of many of his addresses and lectures: nobody not fully familiar with the intricacies of the theory could have written it, nobody could have better avoided those intricacies...
>
> (Wigner in Segrè *1970, p. 55*)

But once one accepts the Weisskopf-Wigner solution for $a(t)$, the amplitude for finding the atom in its ground state and one photon being present is easily calculated

$$
\begin{aligned}
i\hbar \frac{db_{Gk\varepsilon}(t)}{dt} &= \left(\Phi_{Gk\varepsilon}, H_I(t)\Phi(t)\right) \\
&\approx \left(\Phi_{Gk\varepsilon}, H_I(t)\left[1 - \frac{i}{\hbar}\int_{-\infty}^{t} H_I(t')\,dt'\right]\Phi_{E0}\right) a(t) \\
&= \left(\Phi_{Gk\varepsilon}, H_I(t)\Phi_{E0}\right) a(t)
\end{aligned}
\tag{37}
$$

so that for $t \gg 1/\Gamma$ $b(t)$ becomes equal to

$$
b_{Gk\varepsilon}(t) = \frac{i}{\hbar}\frac{\left(\Phi_{Gk\varepsilon}, H_I\,\Phi_{E0}\right)}{\omega_s - \omega_k + i\Gamma}
\tag{38}
$$

[3] See Dyson 1952, after whom the above treatment is patterned, and Low 1952.

Fermi's second example is the propagation of photons in vacuo. He considered two atoms A and B. A is located at the origin and B at a distance r away from atom A. Initially A is an excited state and it is assumed that its lifetime is very short so that the photon is emitted from A "at a very definite time" (and at a very definite place). It is further assumed that B is in ground state and that the mean life of the state to which B is excited by photon absorption is very long (Again Fermi assumed that both atoms are two level systems). Since $1/\Gamma_A$ is very short the line emitted from A is very broad and Fermi considers it as part of the continuous spectrum, whereas B absorbs a very sharp line. The system is described by the amplitudes a, b, and c where

$$a_{EG0}(t) = (\Phi_{EG0}, \Phi(t)) \tag{39}$$

Φ_{EG0} is an eigenstate of H_0 corresponding to atom A being in an excited state, atom B being in ground state, and no photons being present; b_{GE0} *(t)*

$$b_{GE0}(t) = (\Phi_{GE0}, \Phi(t)) \tag{40}$$

is the amplitude for finding at time t atom A in the ground state, B in the excited state and no photons present; and similarly,

$$b_{GGk\epsilon}(t) = (\Phi_{GGk\epsilon}, \Phi(t)) \tag{41}$$

is the amplitude for finding at time t both atoms in the their ground states and one photon of momentum k polarization ϵ. Fermi approximates $\Phi(t)$ as follows:

$$\Phi(t) = \left(1 - \frac{i}{\hbar}\int_{-\infty}^{t} H_I(t')\,dt'\right)\Phi_{EG0}\, a_{EG0}(t) + \sum_{k\epsilon} b_{GGk\epsilon}\Phi_{GGk\epsilon} \tag{42}$$

Clearly

$$i\hbar\frac{da_{EG0}(t)}{dt} = \left(\Phi_{EG0}, H_I(t)\Phi(t)\right) \approx \left(\Phi_{EG0}, H_I(t)\left[1 - \frac{i}{\hbar}\int_{-\infty}^{t} H_I(t')\right]\Phi_{EG0}\right) a_{EG0}(t) \tag{43}$$

so that neglecting the level shift if the initial excited state and the effect of atom B on the lifetime of A

$$a_{EG0}(t) = e^{-\Gamma_A t}$$

Similarly

$$\begin{aligned} i\hbar \frac{da_{GGke}(t)}{dt} &= \left(\Phi_{GGke}, H_I(t)\Phi(t)\right) \\ &\approx \left(\Phi_{GGke}, H_I(t)\left[1 - \frac{i}{\hbar}\int_{-\infty}^{t} H_I(t')\,dt'\right]\Phi_{EG0}\right) a_{EG0}(t) \\ &= (\Phi_{GGke}, H_I(t)\Phi_{EG0})\, e^{\frac{i}{\hbar}(\omega_S^A - \omega_k + i\Gamma_A)t} \end{aligned} \tag{44}$$

so that for $t >> 1/\Gamma_A$

$$a_{GGke}(t) = \frac{(\Phi_{GGke}, H_I \Phi_{EG0})}{(\omega_S^A - \omega_k + i\Gamma_A)} \tag{45}$$

Finally the amplitude $b_{GE0}(t)$ satisfies

$$\begin{aligned} i\hbar \frac{da_{GE0}(t)}{dt} &= \left(\Phi_{GE0}, H_I(t)\Phi(t)\right) \\ &= \sum_{ke} \left(\Phi_{GE0}, H_I(t)\,\Phi_{GGke}\right)\left(\Phi_{GGke}, \Phi(t)\right) \\ &= \sum_{ke} \left(\Phi_{GE0}, H_I(t)\,\Phi_{GGke}\right) a_{GGke}(t) \\ &= -\frac{i}{\hbar}\sum_{ke} \frac{\left(\Phi_{GE0}, H_I\Phi_{GGke}\right)\left(\Phi_{GGke}, H_I\Phi_{EG0}\right)}{\omega_S^A - \omega_k + i\Gamma_A}\, e^{i(\omega_S^B - \omega_k)t} \end{aligned} \tag{46}$$

Fermi then showed that if you assume the atoms localized at the origin and at r that when r is very large compared to the wave length of the emitted radiation, $c/\omega_S{}^A$,

$$a_{GE0} = 0 \text{ for } r/c > t$$

and is proportional to $1/r$ for $t > r/c$. Thus the theory "correctly" attributes the velocity c to the propagation of a photon and also gives the "correct" dependence of the intensity (which is proportional to $|b_{GE0}|^2$) on distance, namely that it decreases as $1/r^2$. The calculation thus gave support to the picture of a photon as a particle like entity traveling between emission and absorption by widely separated atoms with velocity *c*. It also moderated the Landau and Peierls (1930) view that one could not give a configuration space treatment of photons.

The next problem Fermi treated in the his RPM article, the theory of Lippman fringes, depended on the analysis he had given for the two-atom problem. He could again use atom A as a localized source for the emitted photon, and atom B for the detector. There were several reasons for two of the other examples treated, namely Thomson and Compton scattering. On the one hand Fermi wanted to further illustrate how perturbation theory yielded these by then familiar results, and on the other indicate how QED recovered the classical Thomson limit for the scattering of long wavelength radiation off free electrons. In particular, he wanted to point to the fact that in the non-relativistic approximation, it was the $\mathbf{A}^2$ term that was responsible for the effect. He would come back to this observation when deriving the Klein-Nishina formula and indicate the role of the negative states in the recovering the Thomson limit. We will not discuss these matters except to say that after Dirac had introduced his relativistic equation to describe the electron, Fermi carefully studied its properties[4] and followed all the developments relating to the difficulties and novel properties associated with the negative energy states. Thus in the his Michigan Summer School lectures and in the his RMP paper Fermi included an extensive discussion of the Dirac equation. By the time of the submission of the RMP paper Dirac had formulated his hypothesis that all the negative energy states were filled – and that holes in the negative energy sea corresponded to *protons.* At Michigan Fermi spoke of Tamm's and of Oppenheimer's work pointing out the difficulty with that interpretation – namely that the hydrogen atom would be unstable since protons and electrons could annihilate via two photon emission – and stressed the necessity of including negative energy states contributions in the order to derive the Klein-Nishina formula and be able to recover the Thomson limit from it.

We next shall look at a problem that Fermi treated with Bethe in 1932, namely the derivation of the interaction potential between two charged particles. Kragh (1991) and Roqué (1991) have given thorough expositions of the background to this paper, namely the theory of Møller scattering and the controversies over the derivations by Breit and others of the electron-elec-

[4] Thus his second *Rendiconti* paper already described the particles by the Dirac equation.

tron interaction potential that included both magnetic and retardation effects:

$$V = \frac{e^2}{r}\left[1 - \frac{\alpha_1 \cdot \alpha_2}{2} - \frac{(\alpha_1 \cdot \mathbf{r})\,(\alpha_2 \cdot \mathbf{r})}{2r^2}\right] \tag{47}$$

Suffice it to say here that Møller made use of correspondence arguments – still in the vogue in the Copenhagen at the time – to derive the scattering matrix element for the transition from the initial two-electron state to the final one after the scattering and that Breit had made heavy use of the Heisenberg-Pauli formalism whose consistency was in the question and whose clarity was not always readily discernable. There was also the question of the dependence of the result on the particular gauge that is adopted, whether one works in the Lorentz gauge in the which both transversal as well as longitudonal and time-like photons are exchanged and the initial state of the charged particles are free-particle states, or one works in the radiation gauge where only transverse photons are exchanged and the Coulomb interaction is part of the unperturbed Hamiltonian. The aim of Bethe and Fermi was to indicate the relation between the two approaches, and more importantly, how perturbation theory could be used to generate transparent results. Again transcribing their perturbation theoretic treatment into modern terminology, they consider the change in the time of the two-electron amplitude:

$$a_I(t) = \left(\Phi_I\,,\,\Phi(t)\right) \tag{48}$$

where Φ_I is the initial state (an eigenfunction of H_0), a two-electron state specified by appropriate quantum numbers and the photon vacuum. One readily derives that to lowest order of perturbation theory the amplitude that at some later time t the two electron system is in the state Φ_F (again a two electron state and no photons present) is given by

$$\begin{aligned} a_F(t) &= \left(\Phi_F\,,\,\Phi(t)\right) \\ &\approx -\frac{1}{\hbar^2}\left(\Phi_F\,,\int_{-\infty}^{t} dt' \int_{-\infty}^{t'} dt'' H_I(t)\, H_I(t')\,\Phi_I\right) \end{aligned} \tag{49}$$

which is to be compared with the amplitude

$$\left(\frac{-i}{\hbar}\right)\left(\Phi_F, \int_{-\infty}^{t} dt' V(t')\Phi_I\right)$$

that would be computed on the assumption that in the Schrödinger picture the two electrons obey the equation

$$i\hbar\partial_t \phi^s(t) = \Big((c\alpha_1 \cdot \mathbf{p}_1 + \beta_1 m_1 c^2) + (c\alpha_2 \cdot \mathbf{p}_2 + \beta_2 m_2 c^2) + V\Big)\phi^s(t) \quad (50)$$

and interact via a potential V. A comparison of (49) and (50) [after taking the appropriate photon vacuum expectation value in Eq. (49)] yields the Breit interaction for V stemming from one-photon exchange. It is clear from their derivation that they considered the force between the charged particles as arising from the exchange of the photons between them.

Some technical remarks

There is one aspect of the derivations of the description of physical processes in the Fermi QED papers that I have not commented upon. It will be recalled that in the decomposition of the scalar and vector potential Fermi assigned a different phase to each the degrees of freedom. These different phases will therefore be present in the computed answers to the various processes that Fermi analyzed. The simplicity of the final expressions resulted from an averaging over these phases. The physical meaning of these phase averaging is not readily apparent.

It will be recalled that in the section 3 of the Dreimännerarbeit Jordan rederived Einstein's result for the mean square fluctuations of the electromagnetic energy at frequency ν in the thermal equilibrium at temperature T in the a volume v of a cavity

$$<(E_v(\nu) - <E_v(\nu)>_T)^2>_T = \left(h\nu\rho(\nu,T) + \frac{c^3}{8\pi\nu^2}\rho(\nu,T)^2\right) v \quad (51a)$$

$$<(E_v(\nu)>_T = v\rho(\nu,T) \quad (51b)$$

Jordan actually only treated a one-dimensional system: the transverse vibrations of a string of length L, described by an amplitude $u(x,t)$

$$u(x,t) = \sum_{k=1}^{\infty} q_k(t) \sin k \frac{\Pi}{L} x \tag{52a}$$

$$q_k(t) = \frac{2}{L} \int_0^L dx\, u(x,t) \sin k \frac{\Pi}{L} x \tag{52b}$$

The $q_k\ (t)$ are the "coordinates" describing the system and by virtue of the equations of motion their time dependence is given by:

$$q_k(t) = a_k \cos(\omega_k t + \phi_k)$$
$$\omega_k = k \frac{\Pi}{L} \tag{53}$$

Jordan then computed the mean square fluctuations of the energy in the subinterval (o, a) of the string. To arrive at Einstein's result Jordan had to take time averages of the q_k*'s* and claimed that this time averaging was equivalent to a phase averaging over the phases Φ_K. In some sense the phase averaging enforces the locality of the description requiring the fields at different space-time points to be independent degrees of freedom *with independent quantum fluctuations.*

Wightman (1996)has suggested that the departures from Einstein's formula if the phase averaging is not taken represents the quantum mechanical version of the interference effects discussed by Ornstein and Zernike (1919) and by Ehrenfest (1925)[5]. The justification of the phase averaging must therefore have recourse to an analysis of the measuring apparatus and the extent to which it is sensitive to possible frequency dependent phase correlations.

The electromagnetic mass paper

Bethe visited Rome in 1931 and in 1932 as a Rockefeller Foundation fellow. He later recalled that:

> Between the two visits work in the field theory had gone on and Fermi, like so many other of the great theorists, had tried to explain away the divergences of Quantum electrodynamics.

Fermi's paper (Fermi 1931) was a response to a paper on the self-energy problem that Heisenberg had published the year before in the *Zeitschrift für Physik*. In his introductory statement in his paper Heisenberg commented that:

[5] See Wightman 1996 for a detailed history of this controversy.

> In classical theory, the field strengths E and H become arbitrarily large in the neighborhood of the point-charge e, so that the integral over the energy density $(1/8\pi)(E^2 + H^2)$ diverges. To overcome this difficulty, one therefore assumes a finite radius r_0 for the electron in classical electron theory. The radius is related to the mass m of the electron in the order of magnitude relation $r_0 \sim e^2/mc^2$: the integral over the energy density is then of the order mc^2. In quantum theory, not only this radius r_0 but possibly another length $\lambda_0 = h/mc$, which is characteristic of the electron, plays a role in the self-energy in a superficial consideration in terms of the correspondence principle, one would suspect that the self energy of the point-like electron must also become infinite in quantum theory.
> In, fact Oppenheimer (1930) and Waller (1930) have indeed shown that a perturbation method which proceeds in powers of e does not yield finite values for the self-energy [of a point-like electron].

But instead of considering a finite sized electron, Heisenberg suggested

> that one divide space into cells of the finite size r_0^3, and that one replaces the present differential equations by difference equations... [T]he self energy of an electron would be finite in such as lattice world. [But] the statement that a smallest length exists is no longer relativistically invariant, and no way is presently known to harmonize the requirement of relativistic invariance with the fundamental introduction of a smallest length. In the meantime it therefore seems more correct *not* to introduce the length $r0$ into the foundation of the theory but to hold fast to relativistic invariance.

Heisenberg then proceeded to analyze the self-energy of an electron moving with a speed nearly that of light, in which case its rest mass can be neglected, and thus "we always calculate... with $m = 0$." Under those circumstances Heisenberg believed that the self energy must remain finite on dimensional ground.

The one particle Hamiltonian Heisenberg worked with is given by:

$$H = \alpha . \left(\boldsymbol{p} + \frac{e}{hc} \Phi \right) + \int dV \frac{1}{8\pi} \left(\boldsymbol{E}^2 + \boldsymbol{H}^2 \right)$$
$$\nabla \times \Phi = \boldsymbol{H}$$
$$\nabla . \boldsymbol{E} = 4\pi e \, \delta(\boldsymbol{x} - \boldsymbol{q}) \tag{54}$$

with p and q and Φ and E satisfying the usual commutation rules.

The total momentum operator fro the system is given by

$$\boldsymbol{G} = \left(\boldsymbol{p} + \frac{e}{hc} \Phi \right) + \int dV \frac{1}{2} \left[\boldsymbol{E} \times \boldsymbol{H} - \boldsymbol{H} \times \boldsymbol{E} \right] \tag{55}$$

Heisenberg then noted that the electron coordinates can be completely eliminated from the Hamiltonian by using the total momentum so that it becomes:

$$H = c\alpha \cdot \boldsymbol{G} - \alpha \cdot \left(\frac{1}{4\pi} \int dV \frac{1}{2} \left[\boldsymbol{E} \times \boldsymbol{H} - \boldsymbol{H} \times \boldsymbol{E} \right] \right) + \frac{1}{8\pi} \int dV \left(\boldsymbol{E}^2 + \boldsymbol{H}^2 \right) \qquad (56)$$

For an electron under the influence of no force Heisenberg claims that the equation

$$H = c\alpha \cdot \boldsymbol{G} \qquad (57)$$

must hold. He therefore looked whether solutions wherein

$$\left[\int dV \, \alpha \cdot \left[\boldsymbol{E} \times \boldsymbol{H} - \boldsymbol{H} \times \boldsymbol{E} \right] + \int dV \left(\boldsymbol{E}^2 + \boldsymbol{H}^2 \right) \right] \Psi = 0 \qquad (58)$$

exist – and came to the conclusion that

> [t]he one electron could thus be treated correctly without an infinite self energy if there were solutions of vacuum electrodynamics without a zero-point energy. Unfortunately, such solutions do not exist... A solution of the basic equations...has therefore not been found for the time being; it is also not probable that one will achieve a solution without substantial modification of the quantum theory of wave fields. *The purpose of this paper was to show that the difficulties of field theory do not come directly from the infinite self-energy of the electron but that rather the foundations of field theory still require modification.* (emphasis added)

In his paper written slightly later, Fermi (1931) also investigated the problem of the self-energy of an electron. Fermi was cognizant of the fact that the divergences resulted from the point character of the charges which was reflected in the local nature of the stipulated interaction: $p_i \cdot A(x_i)$ or $\upsilon_i \cdot A(x_i)$ and in the form of the Coulomb interaction. Fermi undertook to explore the consequences of assuming that the charge on an "elementary" particle was extended – fully aware that this destroyed the relativistic invariance of the theory. Fermi's model is similar to Lorentz's: electrons were objects with finite extension. Moreover, Lorentz made a distinction between electromagnetic mass, m_{ele}, and material mass, i.e. mechanical mass, m_0, the electromagnetic mass being the inertia that the charged particle had by virtue of its charge.

The total mass, identified with the experimentally determined mass, is

$$m_{exp} = m_0 + m_{ele}$$

Lorentz thought that all the mass of the electron was electro-magnetic, i.e. that $m_0 = 0$. Fermi in his investigation echoed these views.

Fermi made the charge frequency (scale) dependent to reflect its distributed nature. His argument for doing so was as follows: if the electron has a finite radius its various parts will present the same phase as far as the wave lengths that are large in the comparison with the electron radius are concerned. On the other hand, for wave lengths of the order of or smaller than the electron's radius different interior points will react with different phases. The electron thus interacts differently with radiation of different frequency – effectively it presents a smaller charge for high frequencies – with the observed charge some kind of average charge. Fermi thus in the Hamiltonian makes the charge of each particle frequency dependent: $e_i = e_i(\nu)$.

Following Heisenberg, Fermi when characterizing the one particle state as having a charge e, momentum p and energy E, required that this state vector must be an eigenfunction of the total *momentum* operator, G, (i.e of field + of particle + momentum stemming from the interaction between charge and field) with eigenvalue p. He then investigated the chiral case, [i.e. the formulation when the mechanical mass in the Dirac equation for the charged particle is set equal to zero], to see whether he could find states Φ which satisfied the requirements

$$\mathbf{G}\,\Phi = \mathbf{p}\,\Phi \tag{59}$$

and

$$\mathbf{H}\,\Phi = \mathrm{E}\,\Phi \tag{60}$$

The joint requirement can be satisfied since G and H commute with one another: $[G, H] = 0$.). In fact, Fermi explicitly exhibited the state vectors that satisfied the equation $G\,\Phi = p\,\Phi$.

He then pointed out where Heisenberg had gone wrong. Heisenberg's assumption that in the full quantum electrodynamical description the force free electron must satisfy the Dirac equation

$$H = c\boldsymbol{\alpha}\cdot\boldsymbol{G} \tag{61}$$

was incorrect. As a result of its interaction with the [quantized] electromagnetic field the equation the electron obeys should be taken to be

$$H = c\alpha \cdot \boldsymbol{G} + \beta m_{em} \tag{62}$$

where m_{em} is the electromagnetic mass the electron acquires by virtue of the interaction. Fermi therefore inquired whether there are solutions that satisfy

$$H\Phi = c\sqrt{\mathbf{p}^2 + m_{em}^2 c^2}\, \Phi \tag{63}$$

with m_{em} to be determined. For the chiral case, Fermi formulated a perturbation theoretic approach, and to lowest order found an approximate solution for which

$$m_{em} = \sqrt{\frac{4\hbar}{c^5}\int_0^\infty e^2(\nu)\,\nu\, d\nu} \tag{64}$$

Fermi commented that by virtue of the dependence on h the generation of the electromagnetic mass was a strictly quantum mechanical phenomenon. Note that the chiral symmetry is broken!

Fermi thus discovered an early case of anomalous or quantal symmetry breaking, namely that the symmetry of the classical theory need not survive quantization. The introduction of the frequency dependent charge was a regularization procedure that allowed the symmetry breaking to be exhibited.

As far as we have ascertained, Fermi's paper fell on deaf ears. During the 1930s – except for Kramers' papers in 1938 – the relativistic invariance of the formulation took precedence over structural modeling and calculations. Thus there is no reference to either Heisenberg's 1930 self-energy paper nor to Fermi's 1931 paper in Weisskopf 1934 papers wherein he calculated the self-energy of the electron in hole theory and ascertained that to order $\alpha = e^2/hc$ the self-energy diverges logarithmically.[3]

Fermi and the Lamb Shift

Shortly after finishing his QED and β-decay researches Fermi turned to nuclear physics and devoted most of his activities to his neutron work.

Although he did not publish any further theoretical papers during the 1930s dealing with these subjects it is clear that he kept abreast of theoretical developments in the these fields. There are two occasions that I know which elicited his interest in the QED. In the 1947 after Bethe had completed and circulated his calculation indicating that a major part of the Lamb-Retherford experimental result on the 2s-2p level shift in the hydrogen could be explained as a non-relativistic quantum electrodynamical effect, the task at hand became to carry out a relativistic calculation using the full hole theoretic formalism to justify Bethe's introduction of a cut-off at mc^2. French and Weisskopf, who had started such a calculation before the Shelter Island conference at which Lamb had presented his data, continued their calculation, but now made use of Kramers'ideas on mass renormalization that had been discussed at Shelter Island and thus simplified somewhat the subtraction of infinities. At Cornell, Bethe assigned the problem to one of his graduate students, Scalettar. Similarly, Lamb at Columbia started on a hole theoretical calculation during the early part of the summer of 1947, and was soon joined by Norman Kroll. Fermi, who was spending the summer 1947 at Los Alamos, upon receiving a copy of Bethe's preprint explored a relativistic calculation. His first step was to understand the Bethe calculation – which he redid, in the collaboration with Uehling, who was also visiting Los Alamos, but they obtained an expression for the Lamb shift which was 4/3 times larger than Bethe's formula.

> The factor 4/3 [was] due... to the inadequacy of our assumption that the [intermediate] states can be described by plane waves

Fermi wrote Uehling after speaking to Bethe. Furthermore,

> A point that is not explained in the Bethe's paper but which he explained to us in the Los Alamos is the procedure for justifying that the recoil of the light quantum can be disregarded. This can actually be done by using an only slightly more complicated sum rule and I do not understand why Bethe did not follow this more complete procedure in the writing his paper since it would have made the result more convincing.

Fermi continued

> The point that still is quite unsatisfactory is of course the upper limit of the logarithm in the Bethe's formula (11). Apparently several people (Bethe, Weisskopf and Schwinger) have tried unsuccessfully to carry out a relativistic calculation of this upper limit. Also Teller and I tried the same and we believe that we have a method that seems to be practical though probably far from simple. This method consists in the describing the [intermediate] state n as plane waves plus a first approximation [Coulomb] correction which is necessary and sufficient to correct for the factor 4/3 discussed above.

Fermi gave the problem "of the electromagnetic energy level shift in the relativistic case" to Marvin L. Goldberger who was a graduate student at Chicago at the time. Goldberger wrote Bethe in the early October 1947 to ask him whether "our work is sufficiently different to warrant both Mr. Scalettar and me to work on the problem. Clearly, if our work is merely repetition of his, we will drop our program." The approach Goldberger was to employ was the Fermi-Teller proposal to use "for the intermediate state [in the hole theoretic generalization of the

$$\sum_m \frac{|p_{mn}|^2}{E_n - E_m}$$

term in the Bethe formula the first order Coulomb perturbation of the plane waves... [since] with this device the problem appears to be not too difficult." Bethe promptly answered him and informed him that

> We are using a very similar method to yours which effectively amounts to a Born approximation on the intermediate state. However, the calculation is by no means simple even with this method... In the some calculations which I did in the August, I was able to...demonstrate the convergence of the result. Moreover, I found that the result is similar to the non-relativistic case. Scalettar is now checking my arguments and especially calculating explicitly the result in the order to obtain the numerical value. There are approximately twenty different terms which have to be integrated...
> Because of the considerable complication of the calculation I should find it desirable that the calculation be done at several places ndependently. You may be able to find a simple method. The main reason against further duplication is that in the addition Scalettar, also Weisskopf and Lamb are engaged in the similar calculations.

Evidently Goldberger dropped the problem.

The second occasion for which Fermi came to review developments in QED after the war was in the lectures he delivered in Rome and in Milan in the fall of 1949 at the invitation of the Fondazione Donegani. The Rome lecture was devoted entirely to the recent developments in QED. In it Fermi reviewed Lamb and Retherford's experiment that established that the $2P_{1/2}$ and $2S_{1/2}$ energy levels in atomic hydrogen were not degenerate (as stated by the Dirac equation for an electron in a Coulomb field) but were separated by 0.033 cm^{-1}. He then outlined the Bethe calculation for the Lamb shift, carefully going over the physics involved in mass renormalization, and proceeded to explicate what Tomonaga, Schwinger, Feynman and Dyson had accomplished. (Fermi 1950).

A third review of QED by Fermi is contained in his Silliman lectures on "Elementary Particles" which he delivered at Yale in the winter of 1950. His opening remarks were a succinct overview of the state of affairs in QED and QFT:

> Perhaps the most central problem in theoretical physics during the last twenty years has been the search for a description of the elementary particles and their interactions. The radiation theory of Dirac and the subsequent development of quantum electrodynamics form the present basis for our understanding of the electromagnetic field and its associated particles, the photons... The field theories of other elementary particles are patterned on that of the photon. The assumption is made that for each type of elementary particle there exists an associated field of which the particles are the quanta. In addition to the electromagnetic field an electron-positron field, a nucleon field, several meson fields, etc., are also introduced.
>
> The Maxwell equations that describe the macroscopic behavior of the electromagnetic field have been known for a long time. It is therefore natural to assume that these are the basic equations one should attempt to quantize in constructing a quantum electrodynamics. This has been done with considerable measure of success. In the past two or three years the last remaining difficulties associated with the infinite value of the electromagnetic mass and the so-called vacuum polarization have been largely resolved through the work of Bethe, Schwinger, Tomonaga, Feynman and others. They have been able to interpret satisfactorily the Lamb shift in the fine structure of hydrogen and the anomaly of the intrinsic magnetic moment of the electron as due to the interaction with the radiation field.
>
> ... Less convincing are the attempts at a similar description of fields about which we have much scantier experimental knowledge.

Feynman

We have already quoted Bethe on the influence of Fermi's QED work and in the particular of the RMP exposition of the theory on an entire generation of physicists during the 1930s. Here we merely want to draw attention to the important influence that Fermi's RMP article had on Feynman. Already in his PhD thesis Feynman adopted Fermi's approach to QED in his investigation of whether one can give a description of the interaction between charged particles that eliminated explicit reference to the electromagnetic field by integrating out the oscillator degrees of freedom corresponding to it, in his Lagrangian formulation of quantum mechanics. The opening remarks of section 11 of his dissertation are the following:

> The problem which we discuss in the this section is the quantum analogue of the problem discussed in the section 4 of the first part of the paper. Given two atoms A and B, each of which interacts with an oscillator O, to what extent can the motion of the oscillator be disregarded and the atoms be considered as interacting directly? This problem has been solved in the a special case by Fermi who has shown that the oscillators of the electromagnetic field which represent longitudonal waves could be eliminated from the Hamiltonian, provided an additional term be added representing instantaneous Coulomb interactions between the particles. Our problem is analogous to his except that in the general case, as we can see from the classical analogue, we shall expect that the interaction will not be instantaneous, and hence not expressible in the Hamiltonian form.
>
> *(*Feynman *1942)*

Finally, as is apparent throughout Feynman's "Mathematical Formulation of the Quantum Theory of Electrodynamics" – the last paper which he wrote on his post-war QED work, but which in fact was the point of departure for all his QED researches – the foundation for his formulation of QED was Fermi's 1932 *Reviews of Modern Physics* article.

Epilogue

We will let Bethe's insights into Fermi's way of doing physics constitute our concluding remarks:

> My greatest impression of Fermi's method in theoretical physics was of its simplicity. He was able to analyze into its essentials every problem, however complicated it seemed to be. He stripped it of mathematical complications and of unnecessary formalism. In this way, often in half an hour or less, he could solve the essential physical problem involved. Of course there was not yet a mathematically complete solution, but when you left Fermi after one of these discussions, it was clear how the mathematical solution should proceed.
>
> This method was particularly impressive to me because I had come from the school of Sommerfeld in Munich who proceeded in all his work by complete mathematical solution. Having grown up in Sommerfeld's school, I thought that the method to follow was to set up the differential equation for the problem (usually the Schrödinger equation), to use your mathematical skill in finding a solution as accurate and elegant as possible, and then to discuss this solution. In the discussion finally, you would find out the qualitative features of the solution, and hence understand the

physics of the problem. Sommerfeld's way was good one for many problems where the fundamental physics was already understood, but it was extremely laborious. It would usually take several months before you knew the answer to the question.
It was extremely impressive to see that Fermi did not need all this labor. The physics became clear by an analysis of the essentials, and a few order of order of magnitude estimates. His approach was pragmatic...
Fermi was a good mathematician. Whenever it was required, he was able to do elaborate mathematics; however, he first wanted to make sure that this was worth doing. He was a master at achieving important results with a minimum of effort and mathematical apparatus.
By working in this manner he clarified the problems very much, especially for younger people who did not have his great knowledge. For instance, his formulation of quantum electrodynamics is so much simpler than the original Heisenberg and Pauli that it could be very easily understood. I was very much intimidated by the Heisenberg-Pauli article, and could not see the forest from the trees. Fermi's formulation saw showed the forest. The same was true in the paper we wrote together, concerning various formulations of relativistic collision theory. Fermi's formulation of neutron diffusion, the age theory, has been extremely fruitful in making quick calculations of neutron diffusion even in complicated cases. I could multiply this list easily, just from my own experience with Fermi and his work. (Bethe in Segrè 1970).

REFERENCES

1. BETHE H.A. 1930. *"Über die nichtstationäre Behandlung des Photoeffekts."* Annalen der Physik 4:443-449.
2. BETHE H.A. 1955. *"Remarks at the Memorial Symposium in the honor of Enrico Fermi."* Reviews of Modern Physics 27/3, p.253.
3. BETHE H.A. and FERMI E. 1932. "*Über die Wechselwirkung von zwei Elektronen."* Zs. Für Physik 77:296-306.
4. BREIT G. 1929. *"The effect of retardation on the interaction of two electrons."* Physical Review 34:555-573.
5. BREIT G. 1933. *"The Quantum Theory of Dispersion."* Part VI and VII.Reviews of Modern Physics 5/2:91-140.
6. DIRAC P.A.M. 1927a. *The quantum theory of the emission and absorption of radiation.* Proceedings of the Royal Society A 114 243-265.
7. DIRAC P.A.M. 1927b. *The quantum theory of dispersion.* Proceedings of the Royal Society A 114:710-728. 243, 710, 1927)
8. FERMI E. 1926. *"Argomenti pro e conto la ipotesi dei quanti di luce".* Nuovo Cimento. 3:47-54.

9. Fermi E. 1927. *"Sul meccanismo dell'emissione nella meccanica ondulatoria."* Rend. Lincei 5:795-800.
10. Fermi E. 1929a. *"Sopra l'elettrodinamica quantistica."* Rend. Lincei 9:881-887.
11. Fermi E. 1929b. *"Sulla teoria quantistica delle frange di interferenza"*. Nuovo Cimento. 7:153-158. 41
12. Fermi E. 1930. *"Sopra l'elettrodinamica quantistica."* Rend. Lincei 12:431-435.
13. Fermi E. 1931. *"La masse eettromagnetiche nella elettrodinamica quantistica"*. Nuovo Cimento. 8:121-132.
14. Fermi E. 1931. *"Sur la théorie de la radiation"*. Annales de L'Institut Henri Poincaré 1:53-74.
15. Fermi E. 1932. *"Quantum Theory of Radiation"*. Reviews of Modern Physics 4:87-132.
16. Fermi E. 1950. *Conferenze di Fisica Atomica*. Entry 240 in volume 2 of Fermi 1965. Pp. 744-755.
17. Fermi E. 1951. *Elementary Particles*. New Haven: Yale University Press.
18. Fermi E. 1965. *Collected Papers* (Note e memorie). 2 v. Chicago: University of Chicago Press.
19. Feynman R.P. 1942. *The Principle of Least Action in the Quantum Mechanics*. PhD. Dissertation. Princeton University.
20. Feynman R.P. 1950. *"The mathematical formulation of the quantum theory of electromagnetic interaction*. Phys. Rev. 80:440-457.
21. Heisenberg W. 1930. *"Die Selbstenergie des Elektrons."* Zeitschrift fur Physik 65:4-13.
22. Heisenberg W. and Pauli W. 1929. *"Zur Quantendynamik der Wellenfelder."* Zeits. für Physik 56:1-61.
23. Heisenberg W. and Pauli W. 1930. *"Zur Quantendynamik der Wellenfelder. II"* Zeits. für Physik 59:168-190.
24. Jordan P. and Klein O. 1927. *"Zum Mehrkörperproblem der Quantentheorie."* Zeits. Physik 45:751-763.
25. Jordan P. and Wigner E. 1928. *"Über das Paulische Äquivalenzverbot."* Zs. f. Physik 47: 631-651.
26. Kragh H. 1991. *"Relativistic Collisions: The Work of Christian Møller in the Early 1930s.* Archive for History of Exact Sciences 43:299-328.
27. Landau L. and Peierls R. 1930. *"Quantenelektrodynamik im Konfigurationsraum."* Zs. f. Physik 62:188-200.
28. Low F. 1952. *"Natural Line Shape."* Physical Review 88:53-57.
29. Pauli W. 1979. Wissenschaftlicher Briefwechsel mit Bohr, Einstein, Heisenberg U.A. BAND I:1919-1929. Hermann, A., von Meyenn, K, and Weisskopf, V. Eds. Berlin: Springer Verlag.
30. Roqué X. 1991. *"Møller Scattering: a Neglected Application of Early Quantum Electrodynamics."* Archive for History of Exact Sciences 43:197-264.
31. Segrè E. 1970. *Enrico Fermi Physicist*. Chicago: University of Chicago Press.
32. Sommerfeld A. and Schur G. 1930. *"Über den Photoeffekt in the der K-Schale der Atome."* Annalen der Physik 4/5:409-432.

33. WEISSKOPF V. and WIGNER E. 1930. *"Berechnung der naturlichen Linienbreite auf Grund der Diracschen Lichttheorie."* Zs f. Physik 63:54-73.

34. WEISSKOPF V. 1931. *"Zur Theorie der Rezonanzfluoreszenz."* Annalen der Physik (5) 9:23-66.

35. WEISSKOPF V. 1934. *"Über die Selbstenergie des Elektrons."* Zeitschrift fur Physik 89:27-39.

36. WEISSKOPF V. 1934. *"Berichtung zu der Arbeit: Über die Selbst-energie des Elektrons."* Zeitschrift fur Physik 90:817-818.

37. WENTZEL G. 1926. *"Zur Theorie des photoelektrischen Effekts."* Zs f. Physik 40:574-

38. WIGHTMAN A. 1996. *"How it was learned that quantized fields are operator -valued distributions."* Forschritte Phys. 44:143-178.

ENDNOTES

1. "The light-quantum has the peculiarity that it apparently ceases to exist when it is in one of its stationary states, namely, the zero state, in which its momentum, and therefore also its energy, are zero. When a light-quantum is absorbed it can be considered to jump into this zero state, and when one is emitted it can be considered to jump from the zero state to one in which it is physically in evidence, so that it appears to have been created. Since there is no limit to the number of light-quanta that may be created in this way, we must suppose that there are an infinite number of light-quanta in the zero state, so that the N_0 of the Hamiltonian (1.3.24) is infinite." Dirac 1927a.
2. Fermi repeatedly returns to these solutions. See for example, page 115 of Fermi's Notebook I . Reel 69 of Archives for the History of Quantum Physics.
3. The self-energy calculations by Heisenberg and Fermi were carried out before Dirac had formulated his hole theory wherein all the negative energy states of the Dirac equations are filled, and holes in that distribution would correspond to positrons. The calculations by Oppenheimer and by Waller had indicated that to lowest order of perturbation theory the self energy of an electron diverged linearly in a pre-hole theory.

Sam Schweber

Obtained his PhD in theoretical physics from Princeton University in 1952. He thereafter was a postdoctoral fellow with Gian Carlo Wick at Pittsburgh and with Hans Bethe at Cornell. Since 1955 he has been at Brandeis University where he is presently the Koret Professor of the History of Ideas and professor of Physics. Since the early 1980s he has focused his research interests on the history of science and the history of modern physics in particular. He is a faculty associate in the department of the history of science at Harvard University. He is the author of a textbook on relativistic quantum field theory (1961), and more recently of a history of quantum electrodynamics (1994), and of a study in the parallel lives of Hans Bethe and J. Robert Oppenheimer (2000).

Michelangelo De Maria

Fermi and Applied Nuclear Physics during the War (1939-1945)

I will discuss the fundamental role played by Enrico Fermi in the conception and realization of the first nuclear reactor. In particular, I will analyse his scientific contributions to the "pile", from his choice of carbon as "moderator" to the lattice arrangement of graphite and uranium, from his theory of reactors to the so-called "exponential experiments". I will also examine his crucial role in the "ignition" of the "Plutonium project". Such a role was played by Fermi in a manifold, rapidly changing context. Indeed during the war years, physicists and Fermi in particular became active protagonists of the abrupt shift from the "little science" pattern of research, typical of Fermi and his group at Columbia University in 1939-1941, to the "big science" pattern, which he started to practice in 1942-1944. Moreover, for the first time physicists became scientific advisors and consultants of politicians, military and industrialists, and participated in the decision-making process of the most important choices in the war.

Fermi e la fisica nucleare applicata negli anni del conflitto bellico (1939-1945)

Il mio intervento è centrato sul ruolo fondamentale avuto da Enrico Fermi nella realizzazione del primo reattore nucleare. Viene esaminato il suo contributo scientifico alla "pila", dalla scelta del carbonio come "moderatore", alla combinazione di grafite ed uranio, dalla teoria dei reattori ai cosiddetti "esperimenti esponenziali".Viene preso in esame anche il ruolo cruciale da lui avuto nell'ignizione del "Progetto Plutonio", in un contesto rapidamente mutevole. Durante la guerra i fisici, e Fermi in particolare, furono attivi protagonisti della repentina svolta dalla "little science", tipiche di Fermi e del suo gruppo alla Columbia University nel 1939-1941, alla "big science" che egli iniziò a mettere in atto nel 1942-1944. Inoltre per la prima volta i fisici divennero consiglieri, scientifici e consulenti di politici, militari ed industriali, prendendo parte attivamente ai processi decisionali sui più importanti snodi del conflitto.

Dominique Pestre

New Large Accelerators in the World in the Forties and Eearly Fifties

This paper presents half a dozen stories involving protons and physicists, electron accelerators and high politics. The aims of the study are two-fold. The first is to offer a series of historical analyses describing how decisions were arrived at concerning some of the largest accelerators constructed from the 1930s to the 1970s throughout the world. Accelerators are the basic tools of what many consider to be the most fundamental physics undertaken during the last half century, and they are expensive and technically complex machines.
My second aim is to reflect upon the decision-making processes as they occurred in XXth Century big physics. "Spontaneously", I would say, one might be tempted to imagine such processes as being of a linear, rational, "scientific" kind – the final decision resulting from an exhaustive analysis of all possible solutions and of their relevance for the unresolved problems of physics. My study belies the systematic dominance of such a process and suggests, on the contrary, the playing out of different logics of a completely different kind.

I nuovi grandi acceleratori realizzati nel mondo negli anni quaranta e nei primi anni cinquanta

Questa relazione riporta una serie di eventi riguardanti protoni e fisici, acceleratori di elettroni ed alta politica con un duplice obiettivo. Il primo è quello di offrire una serie di analisi storiche riguardanti le modalità decisionali su alcuni dei più grandi acceleratori costruiti nel mondo tra gli anni 30 e gli anni 70. Gli acceleratori, macchinari complessi ed estremamente costosi, sono gli strumenti basilari di quella che molti considerano la fisica fondamentale della seconda metà del secolo scorso. Il secondo è fare una riflessione sui processi decisionali del ventesimo secolo nella "big physics". Credo che si possa essere facilmente portati a pensare che tali processi altro non fossero che la naturale conseguenza di un processo logico lineare e razionale, di impronta scientifica, la decisione finale di una analisi esaustiva di ogni soluzione possibile e della loro attinenza con i quesiti irrisolti della fisica. Il mio studio smentisce questa tesi e suggerisce, al contrario, che su tali decisioni abbiano influito una logica di natura completamente diversa.

Giulio Maltese

Enrico Fermi and the Birth of High-Energy Physics after World War II

The motivation of the work is to outline the influence that Fermi exerted on the birth and the rapid growth of high-energy physics in the period 1946-1954, after his return to Chicago from Los Alamos till his untimely death in 1954. This influence is manifold, as it ranges from Fermi's role as a founder of the so-called "Chicago School" of physics, where many important scientists came from, to contributions to theory, like his interpretation of the Conversi, Pancini, and Piccioni experiment or his and Yang's bold hypothesis concerning the composite nature of pions. Other areas where Fermi played a major role include the policy of science in the post-war years and the path-breaking experimental work he did on pion-nucleon scattering, that eventually led to the discovery of the 3-3 resonance. Fermi's influence on the development of modern physics can be seen also in his systematic pushing towards the use of electronic computers, which he regarded as an effective mean to help research. In the concluding remarks Fermi's attitude as a theoretical physicist will be discussed, and his outlook of theoretical physics will be put in the framework of physics as it was in the fifties.

Enrico Fermi e la nascita della fisica delle alte energie dopo la seconda guerra mondiale

Questa relazione si prefigge lo scopo di delineare l'influenza esercitata da Fermi sulla nascita ed il rapido sviluppo della fisica delle alte energie negli anni 1946-1954, dopo il suo ritorno a Chicago da Los Alamos e fino alla sua morte prematura nel 1954. Tale influenza si manifestò in vari modi, e spazia dal ruolo di Fermi come fondatore della cosiddetta "Chicago School", dove si formarono molti importanti scienziati, a contributi teorici come l'interpretazione dell'esperimento di Conversi, Pancini e Piccioni, o all'audace ipotesi, formulata assieme a Yang, sulla natura composita dei pioni.

Altri settori nei quali Fermi giocò un ruolo di primo piano vanno dalla politica della scienza negli anni del dopoguerra al suo pionieristico lavoro sperimentale sulla diffusione del pione-nucleone, che condusse alla scoperta della risonanza 3-3.

L'influenza di Fermi sullo sviluppo della fisica moderna può essere riconosciuta anche nella sua spinta verso l'utilizzo dei calcolatori elettronici, che Fermi considerava uno strumento molto efficace per la ricerca. Il commento conclusivo riguarderà l'atteggiamento di Fermi come fisico teorico e la sua opinione sulla fisica teorica verrà posta nel contesto dello stato della fisica agli inizi degli anni cinquanta.

Introduction: Enrico Fermi and a new field of physics

Of the almost sixteen years that Enrico Fermi spent in the US, since he fled from Europe at the end of 1938 to his untimely death in 1954, his best known achievements are the first self-sustained chain reaction (Chicago, December 2, 1942) and the first test of a fission atomic bomb (Alamogordo, July 16, 1945). In the latter enterprise, Fermi was an influential member of the most incredible team of physicists ever assembled in the history of science. The consequences of those successes went well beyond the community of scientists and marked the history of mankind in the 20th century. This is probably the reason why Fermi's achievements in the postwar years, while at least comparable to the ones quoted above, are less widely known.

Fermi's astonishing contributions to physics continued during the years 1946-1954, a period that witnessed the rapid flourishing of "high-energy nuclear physics", as both a follow-on of wartime military projects, as far as funding and science organization were concerned, and as a consequence of the will that many physicists, including Fermi, displayed to go back to pure science after focusing on applied physics in wartime years.

Enrico Fermi was one of the key actors in this new field of physics in the US after World War II. He deeply influenced the birth and rapid growth of high-energy physics in the period 1946-1954, after his return to Chicago from Los Alamos till his death in 1954. Fermi's contributions ranged from his role as a founder of the so-called "Chicago School" of physics, where many important particle physicists came from, to theoretical achievements or ideas, like his interpretation of the Conversi, Pancini, and Piccioni experiment or the bold hypothesis concerning the composite nature of pions that he and Yang put forward in 1949. An influential member of the community of physicists, Fermi was President of the American Physical Society during 1953. He served (1947-1950) as a member of the General Advisory Committee (GAC) of the Atomic Energy Commission (AEC). In these and other capacities, Fermi strongly favored the development of high-energy physics, pushing towards particle accelerators and electronic computers, that he considered as the fundamental tools of particle physicists. As an experimental physicist, using the Chicago 450 MeV synchrocyclotron Fermi performed a path-breaking work that eventually led to the discovery of the pion-nucleon 3-3 resonance and put the isospin concept at the center of the stage in the study of strong interactions.

Fermi was deeply interested in all fields of the physics of "fundamental" particles, a term he preferred to "elementary". He taught physics to a generation of young scientists, many of whom played a major role in the revolution that marked the development of physics in the second half of the 20th century. He took lively part to the debate on the inadequacy of theory to account for the growing number of facts that experiments continuously brought to the surface at the beginning of the fifties. In those times his pragmatic outlook of theoretical physics was especially suited for the status of the discipline, driven, as it was, by experimental discoveries. Young students at Chicago in his late years mostly knew "this" Fermi, and "this" Fermi will be the subject of the present paper.

A major shift

In 1945, at the age of 44, Enrico Fermi was the greatest neutron physicist in the world. He mastered this subject so thoroughly that he could safely say that he felt neutrons were, so to speak, a sort of "relatives" to him.[1]

However, just when he was at the top of his fame as a neutron physicist and he was widely considered an "oracle" at Los Alamos laboratory, he decided a major change and set out to tackle the "high-energy nuclear physics" as it was then called that field of physics that would eventually give rise to the modern physics of elementary particles. It was a paradigm shift comparable to the one Fermi had made in 1932, when he had left atomic and molecular physics, which he considered by then well established, to devote himself and his group to the more promising field of neutron physics. In his biography of Enrico Fermi, Segrè recalls what was the context, at Los Alamos in 1945, where this decision was taken: "One could go on with nuclear physics, while waiting for the instruments to tackle elementary particles to be ready; the shift, however, was necessary, to keep staying in the forefront of physics. In the mean time, one could start preparing himself, studying everything was then known, and working to make available instruments required by future work. Fermi, Allison and I kept talking of all these things while going down the steep walls of the Cañon de Frijoles and then stepping along the river towards the Rio Grande in a strange and exotic landscape made of Indian ruins, vividly colored rocks, cactuses and *piñones*".[2]

---

1 E. Fermi, "Conferenze di Fisica Atomica", *FNM*, vol. 2, paper no. 240, p. 756. The following abbreviations are used: *EFP*, Enrico Fermi Papers, Department of Special Collections, University of Chicago Library; *EAP*, Edoardo Amaldi Papers, Department of Physics, University of Rome I; *FNM*, *Enrico Fermi, Note e Memorie (Collected Papers)*, Accademia Nazionale dei Lincei and University of Chicago Press, Rome and Chicago, 1965; *PR*, Physical Review.

2 E. Segrè, *Enrico Fermi, fisico*, Zanichelli, Bologna, 1987, p. 170, my translation.

To be sure, Fermi had started thinking to high-energy nuclear physics even before. In 1944, when working at Hanford at the start-up of the first plutonium production reactor, Fermi told Leona Marshall of his plans to build a betatron after the war. Eventually a betatron was not the kind of machine that was best suited for the research Fermi had in mind, but his goal was clear: he wanted to devote himself to particle physics, "to find out all there was to know about nuclear forces".[3]

Fermi left Los Alamos with his family on December 31, 1945. He had been appointed Charles H. Swift Distinguished Service Professor of Physics at the Institute of Nuclear Studies of the University of Chicago. However, "at the end of the war, the physicists who returned to the University of Chicago to form the Institute of Nuclear Studies found a physics department with bare shelves".[4] The building of the Institute was still to come, as Fermi wrote to Segrè in June 1946: "During the winter and spring I have done practically all my work at Argonne and I expect to continue the same way until we shall have some beginning of a building of our own probably about one year from now;"[5] thus, "it was reasonable, therefore, that we turned to the excellent heavy water reactor facility at the Argonne Laboratory with its high thermal neutron flux, to investigate aspects of neutron physics which had been bypassed in the drive to the wartime objectives".[6]

Together with Leona Marshall and others, Fermi in 1946-1947 devoted himself to neutron physics, and especially to neutron scattering, giving important contributions to the study of the diffraction of neutrons by crystalline substances and investigating the hypothesis of a neutron-electron interaction.[7] However, experimental activity at Argonne started decreasing as Fermi got more and more involved into his activity as a teacher and, as he later said, as a "student" of the new theoretical physics.

The "Chicago school"

In his letter to Segrè of June 24, 1946, Fermi also spoke of his activity as a teacher: "I have been giving one official course and some unofficial teach-

[3] L. MARSHALL LIBBY, *The Uranium People*, Charles Scribner's sons, New York, 1979, p. 108.
[4] L. MARSHALL, introduction to papers nos. 227-231, 234 and 235, *FNM*, vol. 2, p. 578.
[5] E. Fermi to E. Segrè, June 24, 1946, *EFP*, box 11.
[6] L. MARSHALL, introduction to papers nos. 227-231, 234 and 235, *FNM*, vol. 2, p. 578.
[7] See *FNM*, vol. 2, papers nos. 226-231 and 234-235.

ing to a small group of students".[8] Fermi's was an understatement. As Segrè put it, "Soon the rumour spread that Fermi was about to create a new school of physics, and a group of extraordinary students gathered at Chicago".[9] Between 1946 and 1953, Fermi gave twenty-three courses,[10] teaching to many first-class students. Among them was Chen Ning Yang who later recalled: "Fermi gave extremely lucid lectures. In a fashion that is characteristic of him, for each topic he always started from the beginning, treated simple examples and avoided as much as possible 'formalisms'. (He used to joke that complicated formalism was for the 'high priests'). The very simplicity of his reasoning conveyed the impression of effortlessness. But this impression is false: The simplicity was the result of careful preparation and of deliberate weighing of different alternatives of presentation".[11]

In addition to giving regular courses, Fermi created a sort of "late afternoon graduate school of physics", again described by Yang: "it was Fermi's habit to give, once or twice a week, informal unprepared lectures to a small group of graduate students. The group gathered in his office and someone, either Fermi himself or one of the students, would propose a specific topic for discussion. Fermi would search through his carefully indexed notebooks to find his notes on the topic and would then present it to us. I still have the notes I took of his evening lectures during October 1946 – July 1947. It covered the following topics in the original order: theory of the internal constitution and the evolution of stars, structure of the white dwarfs, Gamow-Schönberg's idea about supernovae (neutrino cooling due to electron capture by nuclei), Riemannian geometry, general relativity and cosmology, Thomas-Fermi model, the state of matter at very high temperatures and density, Thomas factor of 2, scattering of neutrons by para and ortho hydrogen, synchrotron radiation, Zeeman effect, "Johnson effect" of noise in circuits, Bose-Einstein condensation, multiple periodic system and Bohr's quantum condition, Born-Infeld theory of elementary particles, brief description of the foundation of statistical mechanics, slowing down of mesons in matter, slowing down of neutrons in matter […] The fact that Fermi had kept over the years detailed notes on diverse subjects in physics, ranging from the purely theoretical to the purely experimental, from such simple problems as the

---

8 E. Fermi to E. Segrè, June 24, 1946, *EFP*, box 11.

9 E. Segrè, *Enrico Fermi*, cit., p. 171, my translation.

10 V.L. Telegdi, "Enrico Fermi in America", in: *Symposium dedicated to Enrico Fermi on the occasion of the 50th anniversary of the first reactor*, Atti dei Convegni Lincei, vol. 104, pp. 71-90, Rome, 1993, p. 81.

11 C.N. Yang, introduction to paper no. 239, *FNM*, vol. 2, p. 673.

best coordinates to use for the three-body problem to such deep subjects as general relativity, was an important lesson to all of us. We learned that *that* was physics. We learned that physics should not be a specialist's subject; physics is to be built from the ground up, brick by brick, layer by layer. We learned that abstractions come *after* detailed foundation work, not before".[12]

Several Fermi's students were awarded the Nobel Prize in physics: C. N. Yang and T.D. Lee (1957), O. Chamberlain (1959), J. Steinberger (1988), J. I. Friedman (1990). In addition to them we have to mention J. W. Cronin (1980), a young student at the Institute of Nuclear Physics at the time of late Fermi, and M. Gell-Mann (1969), who between 1952 and 1954 was a young teacher at the Institute for Nuclear Studies.

Mention must also be made of Emilio Segrè, who shared the Nobel Prize in 1959 with Chamberlain and was the first of Fermi's students in Italy.

Finally, Maria Goeppert Mayer, whose work on the nuclear shell model was awarded the Nobel Prize in 1963, acknowledged a decisive suggestion she had from Fermi in her quest.

Most of his students were aware of the privilege they had to study under the guidance of such a great teacher. In 1954 Chamberlain wrote to him: "I am very grateful to you for the time and effort you have invested in me in the past. If I am to be regarded as a decent physicist, it is mostly because of your training".[13]

According to Steinberger "Fermi's courses [...] were models of transparent and simple organization of the most important concepts. He went to a great length to show those of us who had finished the courses and were working on our Ph.D., theses how to attack a variety of simple, general problems in different branches of physics, by gathering us together one or two evenings a week [...] proposing a problem, and then, perhaps later, going through the solution".[14]

Friedman described the atmosphere at the Institute for Nuclear Studies: "It is difficult to convey the sense of excitement that pervaded the Department at that time. Fermi's brilliance, his stimulating, crystal clear lectures that he

---

[12] C.N. YANG, Introduction to paper no. 239, *FNM*, vol. 2, pp. 673-674. This way of teaching, typical of Fermi's style, dated back to his Italian years; see, for example E. SEGRÈ, *Autobiografia di un fisico*, Il Mulino, Bologna, 1995, pp. 66-67; E. Amaldi, "Commemorazione del Socio Enrico Fermi"; E. Persico, "Commemorazione di Enrico Fermi"; F. Rasetti, "Enrico Fermi e la Fisica Italiana", in: C. BERNARDINI and L. BONOLIS (Eds.), *Conoscere Fermi*, Società Italiana di Fisica, Bologna, 2001, p. 27, 39, 50.

[13] O. Chamberlain to E. Fermi, February 2, 1954, *EFP*, box 11.

[14] J. STEINBERGER, "A particular view of particle physics in the fifties", in: L.M. BROWN, M. DRESDEN, L. HODDESON, (Eds): *Pions to Quarks: Particle Physics in the 1950s*, Cambridge University Press, Cambridge, 1989, pp. 307-308.

gave in numerous seminars and courses, the outstanding faculty in the Department, the many notable physicists who frequently came to visit Fermi, and the pioneering investigations of pion proton scattering at the newly constructed cyclotron all combined to create an especially lively atmosphere. I was indeed fortunate to have seen the practice of physics carried out at its 'very best' at such an early stage in my development. I also had the great privilege of being supervised by Fermi, and I can remember being overwhelmed with a sense of my good fortune to have been given the opportunity to work for this great man. It was a remarkably stimulating experience that shaped the way I think about physics".[15]

Emilio Segrè, his first student in Italy and later a Nobel prizewinner in 1959 recalled Fermi's style in his lectures for graduate students: "His lectures were absolutely informal and not prepared in advance. We gathered in the afternoon at the Institute and maybe some initial conversation suggested him the topic for his lecture. If, for example, we asked him to clarify the problem of capillarity, Fermi would improvise a magnificent talk on the mathematics of capillarity [...] in other cases the level was quite higher and Fermi explained to us the last paper he had read: in this way we became familiar with the famous Schrödinger's papers on wave mechanics [...] Fermi's teaching concerned almost exclusively theoretical physics and he made no distinction between students that were supposed to become theorists or experimentalists. He himself, while being first of all a theoretician, worked also as an experimenter. His knowledge and his interests concerned all fields of physics, and he duly read several papers. He preferred practical problems and distrusted theories too abstract or generic; however, whatever problem in whatever field of physics – classical mechanics, spectroscopy, thermodynamics, solid state theory, and so on – fascinated him and stimulated his ingenuity and his sense of physics [...] A curious aspect of Fermi's style was his slow pace, even when he dealt with simple problems. A simple-minded observer might have wondered why Fermi wasted so much time in elementary algebra; however, when difficulties occurred that would have stopped a less brilliant scientist, Fermi could dispose of them without changing his speed. He gave the impression of a steam-roller that proceeded slowly but that could not be stopped [...] When he had discovered a new method he stored it in his memory, and he often applied it later to problems that looked quite different from the one that had originated the method itself".[16]

[15] J.I. Friedman, autobiography, http://www.nobel.se.physics.

Even more than a teacher, Fermi was actually, to quote Gell-Mann's words, "the meson that kept the Institute together". At his funeral, his friend S.K. Allison said that "the Institute is *his* Institute, for he was its outstanding source of intellectual stimulation. It was Enrico who attended every seminar and with incredible brilliance critically assayed every new idea or discovery. It was Enrico who arrived first in the morning and left last at night, filling each day with his outpurring of mental and physical energy".

According to V.L. Telegdi, "it is imaginable […] that some other physicist (or group of physicists) might have obtained the research results that Fermi achieved while at Columbia and in Chicago (including the realisation of the first nuclear chain reaction), but it defies the bounds of human imagination to speculate that any other man or woman might have played Fermi's role as a teacher (in the broadest sense of this term). Through the influence of his students, Fermi effectively revolutionized the training of physicists in the United States and, hopefully, in the whole Western world".[17]

All of the Nobel prizes listed above were awarded for discoveries made in the field of elementary particle physics; more generally, many of Fermi's students spent their scientific careers in high-energy physics. It can safely be assumed that the first great contribution that Fermi gave to elementary particle physics was the "Chicago School" of physics that he created.

From mesotrons to pions: Fermi and the experiment of Conversi, Pancini, and Piccioni

In 1934-1935 the Japanese physicist Hideki Yukawa put forward his theory of nuclear forces, implying the existence of a new particle (variously named as "Yukon" or "mesotron") that played the role of quantum of the nuclear force field and was supposed to have a mass of $\mu \approx 200 m_e$ (m_e being the mass of the electron). In 1936-1937 Carl Anderson and Seth Neddermeyer observed in cosmic rays a sort of "heavy electron" with a mass similar to that of the particle hypothesized by Yukawa. In the spring of 1937 it was hypothesized for the first time that the particle observed by Anderson and Neddermeyer could actually be Yukawa's particle.

Then it followed a period (1938-1943) in which physicists measured the

[16] P. DE LATIL, *Fermi: la vita, le ricerche le testimonianze*, Edizioni Accademia, Milano, 1974, pp. 170-171, my translation.

[17] M. Gell-Mann's biography, Santa Fe Institute, *http://www.santafe.edu*; S.K. ALLISON, "Enrico Fermi 1901-1954", *Physics Today*, no. 8, 1955, p. 9; V.L. TELEGDI, "Enrico Fermi in America", cit., p. 71.

properties of the mesotron (mean lifetime and decay properties), trying to fit it into the framework of Yukawa's theory, albeit with increasing difficulties. It was a series of experiments, carried on in 1943-1946 by the Italian physicists Marcello Conversi, Ettore Pancini and Oreste Piccioni that eventually demonstrated that mesotrons behaved in a way that could hardly fit into Yukawa's scheme. Basically, according to theory, slow positively charged mesotrons traversing the matter should prefer to decay rather than be absorbed by a nucleus, since Coulomb repulsion should prevent them from reaching the nucleus. On the other hand, negative Yukawa particles should strongly prefer absorption to decay. While in the Conversi et al. experiment positive cosmic ray mesons behaved the way the theory said, and negative cosmic ray mesons were absorbed in iron, again as expected, negative cosmic ray mesons were not absorbed in a light element like carbon![18]

Immediately, Conversi, Pancini and Piccioni communicated this result to Edoardo Amaldi, who was then in Washington, about to complete a three-month trip to the US. Looking for major wisdom, Amaldi wrote to Fermi: "I think you'll be interested in the last data from M. Conversi, E. Pancini, O. Piccioni on the death of mesons of both signs [...] It may be inferred from these results that in iron only positive mesons decay, and that in carbon both positive and negative mesons decay, with equal probability".[19]

Fermi realised the importance of the matter and started working on it. He soon replied to Amaldi: "Thanks a lot for your letter from Washington, in which you tell me of the results of the experiments by Conversi, Pancini and Piccioni on the decay of mesotrons in carbon and in iron. Teller and I have made some calculations and discussions on the meaning of these experiments, and our conclusions are summarized in the enclosed manuscript. We would like to publish it or something very similar as a letter to the Phys. Rev. and naturally we would like to ask Conversi et al. for the permission to quote their results".[20] Two generations of Italian physicists, ranging from the young Pancini, Conversi and Piccioni, to the elder Amaldi, the former "boy of Via Panisperna", and finally to Enrico Fermi, "the Pope" and the leader of that group, were involved in executing and analyzing what is today considered the experiment that initiated the era of high-energy physics.

[18] For more details see A. Pais, *Inward Bound: Of Matter and Forces in the Physical World*, Oxford University Press, Oxford, 1986, chapter 18.

[19] E. Amaldi to E. Fermi, November 28, 1946, *EFP*, box 9, my translation.

[20] E. Fermi to E. Amaldi, January 3, 1947, *EFP*, box 9, my translation.

Furthermore, it was through Amaldi and Fermi that the news of the experiment was spread in the US.[21]

Soon, the experimental result of the Italian physicists became a stimulating research topic at the Institute for Nuclear Studies. Jack Steinberger, then a young student, recalled: "For me, particle physics began in 1947, when I was a graduate student at the University of Chicago. Enrico Fermi gave a seminar on the results of the Conversi, Pancini and Piccioni experiment [...] It was a beautiful and important experiment, and Fermi's explanation was extraordinarily lucid, as well as stimulating and exciting".[22] Fermi (who had already studied in 1939 the anomalous absorption of cosmic rays in air) devoted himself to analyze the outcome of the Conversi et al. experiment, together with Teller. It appeared that the result obtained by Conversi and his associates pointed toward a very weak interaction between cosmic ray ("μ") mesons and nucleons. Teller recalls that "Weisskopf had arrived at a similar conclusion and through correspondence we arranged a short joint note".[23] The analysis by Fermi, Teller and Weisskopf demonstrated that the time of capture from the lowest orbit of carbon was not less than the time of natural decay, i.e. 10^{-6} s. This was in disagreement with the estimate of the theory for a factor ranging from 10^{10} to 10^{12}. The interaction between μ-mesons and nucleons was therefore much weaker than demanded by Yukawa's theory. In a second and more detailed paper, Fermi and Teller looked for an alternative explanation to the effect discovered by Conversi et al. They hypothesized that the time of capture in carbon was so long that the relatively great number of meson decays might take place during the time of capture. Fermi and Teller therefore studied the capture mechanism in depth. They found that the capture time is much shorter (from 10^{-9} to 10^{-13} s) with respect to the average lifetime of μ-mesons (2×10^{-6} s), and the hypothesis of identifying μ-mesons and mesotrons was definitely proved to be fallacious.[24]

The discussion of the Italian experiment was one of the two major themes at the Shelter Island Conference (held on June 2-4, 1947), the first of a series

[21] M. Conversi, E. Pancini, O. Piccioni, "On the Disintegration of Negative Mesons", *PR*, vol. 71 (1947), pp. 209-210. The paper was published on February 1, 1947.

[22] J. Steinberger, "A particular view of particle physics in the fifties", in: L.M. Brown, M. Dresden, L. Hoddeson, (eds): *Pions to quarks,* cit., p. 307.

[23] E. Teller, introduction to paper nos. 232 and 233, *FNM*, vol. 2, p. 615. The joint paper was: E. Fermi, E. Teller, V. Weisskopf, "The decay of negative mesotrons in matter", *PR*, vol. 71 (1947), pp. 314-315, also in *FNM*, vol. 2, paper no. 232.

[24] E. Fermi and E. Teller, "The capture of negative mesotrons in matter", *PR*, vol. 72 (1947), pp. 399-408, also in *FNM*, vol. 2, paper no. 233.

of three conferences organized by J.R. Oppenheimer devoted to theoretical physics as well as to theoretical implications of available experimental results. In particular, there was substantial concern due to the apparent difficulty in reconciling the high rate of production of μ-mesons in the high atmosphere with the weak interactions that these mesons showed while traversing matter. Oppenheimer ventured to hypothesize that this situation might eventually mean a breakdown in the "customary formalism of quantum mechanics".[25]

The analysis by Fermi, Teller and Weisskopf emphasized the striking discrepancy between the value of the lifetime observed for the negative μ-meson and the value that should be expected if μ-meson were responsible for nuclear forces. It played a major role in leading to a new theory, formulated by Marshak and Bethe at the Shelter Island Conference, where the authors put forward the so-called two-meson hypothesis, according to which the μ-mesons investigated by Conversi et al. were not the Yukawa particle, but merely one of its decay products. This hypothesis was soon confirmed by the discovery of the π-meson or "pion" in the cosmic rays by Powell et al. at Bristol.[26] High-energy nuclear physics had just begun.

The coming of the *Big Science* era

It is well known that the post-war years marked the birth of the so-called *Big Science*. A lot of words have been spent to describe what *Big Science* was, and I will not add mine to this substantial *corpus* of writings. For the purpose of the present paper, it suffices to quote a letter from Segrè to Fermi of February 1946, soon after both of them had come back home from Los Alamos: "We are now settled again in our old house, without telephone. Being back to civilization after suffering for shortage of water is not that bad. At Radiation [Lab] orgies of first class engineering are going on, but for the time being practically nothing as to detecting end of machines. The Radiation Lab looks pretty much like Los Alamos and war projects and is even more industrial than Los Alamos was. It seems to me that they rely

25 R.E. Marshak, "Particle physics in rapid transition: 1947-1952", in: L.M. Brown, L. Hoddeson, (eds.): *The Birth of Particle Physics*, Cambridge University Press, Cambridge, 1983, pp. 376-401, on pp. 379-381.

26 R.E. Marshak and H.A. Bethe, "On the Two-Meson Hypothesis", *PR*, vol. 72 (1947), pp. 314-315; C.M.G. Lattes, H. Muirhead, G.P.S. Occhialini, C.F. Powell, "Process involving Charged Mesons", *Nature*, vol. 159 (1947), pp. 694-697; see also R.E. Marshak, *Meson Physics*, McGraw-Hill, New York, 1952, pp. 194-195.

mostly on money from Groves".[27] Segrè's words show the two most distinctive features of *Big Science*, namely the coming of new cooperative projects, on a scale never seen before, and the strict connection between military and peacetime organisations, later testified by the fact that the three federal funding agencies that supplied the bulk of support for American particle physics in the post-World War II decades, namely the Atomic Energy Commission (AEC), the Office for Naval Research (ONR), and the National Science Foundation (NSF) directly grew out from the Manhattan Engineering District (MED).[28]

Fermi's attitude towards changes implied by the new organization of science was many-sided, and cannot be easily subsumed under just one interpretative key. In January 1946 he wrote to his Italian friends E. Amaldi and G. C. Wick: "Also in the US physics has undergone deep changes, due to the war. Some are for the best: now that people got convinced that physics can be used to make atomic bombs, everybody keeps talking about funds of several millions of dollars. It's impressive that the biggest concern regarding money will be to figure out enough things to buy. To be sure, there are also serious drawbacks. The most serious is represented by military secret. In this respect the general hope is that a good deal of the scientific results that are still kept secret will be published in the near future; for the time being, however, things proceed quite slowly. Another drawback is that a substantial part of the public opinion is convinced that wartime scientific successes were mostly due to the super-organization of scientific enterprise. Therefore, they conclude that super-organization is the best way to promote scientific progress also in peacetime. The majority of physicists believe that this would be a mistake. However, there are always candidates to the role of super-organizers who think differently. Finally, many physicists are now much more busy with politics than with science, and spend their time at Washington in pleasant conversations with Senators and Congressmen".[29]

Thus Fermi was not altogether in favour of changes brought in by the post-war metamorphosis occurring in science. One year later, his concerns were even deeper than that, as he explicitly spoke of a "crisis": "The crisis through

[27] E. Segrè to E. Fermi, February 7, 1946, *EFP*, box 11, my translation.

[28] L.M. BROWN, M. DRESDEN, L. HODDESON, "Pions to quarks: particle physics in the 1950s", in: L.M. BROWN, M. DRESDEN, L. HODDESON, (eds): *Pions to Quarks*, cit., pp. 10-11.

[29] E. Fermi to E. Amaldi and G.C. Wick, January 24, 1946, *EAP*, box E1, my translation; also in: E. AMALDI, *Da via Panisperna all'America*, edited by G. BATTIMELLI and M. DE MARIA, Rome, Editori Riuniti, 1997, pp. 166-167.

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 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".[30] Fermi's last statements give striking evidence of the need he felt to go back to "purity" in science.

Fermi was however a strong advocate of some aspects of high-energy physics. He belonged to the generation that, quoting Schweber's words, "had come of age with quantum mechanics" and, besides him, included people like Hans A. Bethe, Julius R. Oppenheimer, Ernest O. Lawrence, Isidor I. Rabi, to name just a few. During the war they had been associated to military projects, and as Johann Von Neumann said, they had become "better scientists and impurer men", i.e. wartime projects had requested them to become more and more involved into applied science projects.[31]

When the war ended, many of them, and especially the nuclear physicists who had worked at the Metallurgical Laboratory and at Los Alamos, "sought ways to become once again purer men and purer scientists. Guaranteeing and demonstrating the peaceful uses of atomic energy was one avenue for redemption [...] A second avenue to purity was unraveling the secrets of nature at the subnuclear level. For many physicists the wartime experience had reinforced the notion that only pure physics – physics for physics' sake – was 'basic' or 'fundamental' physics and 'good' physics. High-energy physics

[30] E. Fermi, unpublished address given at the Union College on the Commencement Day of year 1947, *EFP*, box 53.

[31] See S. Schweber, "A Historical Perspective on the Rise of the Standard Model", in: L. Hoddeson, L. M. Brown, M. Riordan, M. Dresden, (eds.): *The Rise of the Standard Model. Particle Physics in the 1960s and 1970s*, Cambridge University Press, Cambridge, 1997, p. 646; see also J. Von Neumann to O. Veblen, May 21, 1943, quoted in S. Schweber, cit., p. 657.

offered fertile ground for both purification and 'good' physics. Most of the members of the GAC [the General Advisory Committee of the AEC, where Fermi served from 1947 to 1950], and […] many of those serving on the advisory committee to ONR – the bodies that decided on the support of high-energy physics after World War II – were nuclear physicists that had been associated with Los Alamos. Their support of high-energy activities was important for the growth of high-energy physics. In fact, these men – Bacher, Rabi, Oppenheimer, Lawrence, Fermi and so on – were some of the most convincing advocates of high-energy physics, and the spectacular flowering of the field owes much to their effectiveness as proponents […] They were also statesmen who could interface between the scientific and the political realms and their political and diplomatic efforts within the councils of state made possible the construction of the laboratories and the requisite subsequent funding".[32]

Fermi was aware that, for particle physics "to go back to its tradition of research for the sake of uncovering new truths" new facilities were needed, namely accelerators and computers. We have already seen that he was thinking of a betatron as early as 1944. Plans to purchase from General Electric a 100 MeV betatron for the Institute of Nuclear Studies were being discussed in the Fall 1945. It was expected to be able to produce mesons in order to investigate the nature of nuclear forces. The assembling of the machine was strewn with several difficulties, and it became evident later that a 100 MeV machine accelerating electrons could not produce mesons for the required investigations. It was therefore proposed to modify the original project, transforming the original machine into a one that could operate either as a betatron or as a synchrotron. Eventually, the betatron at Chicago started operating partially during the year 1950-1951 and was employed mostly to investigate the properties of gamma rays. In the end, it was not so important for the physics Fermi had in mind, for which a synchrocyclotron like the one operating at Berkeley was better suited. A project to build a synchrocyclotron at Chicago started in 1947, Fermi being one of the main proponents. The machine started operating in the spring of 1951, and for some time it was the most powerful particle accelerator in the world. Being able to accelerate protons at 450 MeV, this machine allowed Fermi and his group to perform fundamental experiments of scattering of pions impinging on nucleons.

[32] S. SCHWEBER, "A Historical Perspective on the Rise of the Standard Model", cit., p. 657 and 646.

Fermi had a long standing interest in numeric calculation. In 1928 he had computed the solution of the differential equation of the Thomas-Fermi atom using a small desk calculator. In 1945 he had shown a lively interest in the electro-mechanical accounting machines then used at Los Alamos for scientific calculations, and he had proposed to solve a numerical problem with them.[33] With the coming of the *Big Science* era, Fermi promptly realized that electronic computers were the natural tools to process the big amount of data coming out of accelerators and to help sorting out the intricacies of the theories of nuclear forces.

Speaking in 1947 on the future of nuclear physics he said: "Theory is usually rather helpless in attacking a thoroughly new problem unless it is supported by experimentation. For this reason, there are now great hopes of further progress. New experiments will become possible with the development of giant cyclotrons, like the one recently operated at Berkeley, and other large accelerating machines, which are now being planned and developed in several Institutions. These machines will permit to study in the laboratory particles of energies approaching those of cosmic rays. It seems justifiable to expect that their investigation in the laboratory may offer valuable leads for the exploration of nuclear properties. Many physicists hope that it may even be possible to produce artificial mesotrons and to demonstrate directly their connection with nuclear forces postulated by Yukawa. The overall problem of the nucleus, however, will not be solved by the knowledge of the forces alone. Many nuclei are of extreme complexity and contain hundreds of neutrons and protons closely packed together, so that even if the laws of their dynamics were understood, their application to such a complex system would present a formidable mathematical problem. Its complexity is such that the hopes of finding exact solutions by conventional methods of analysis are exceedingly small and it appears more probable that numerical methods will have to be used. I would like to point out the importance that electronic computing machines will have in this respect. The ENIAC, an early model of this type of machines, has been developed in Philadelphia during the war and is now operating successfully. Some promising results in its application to problems of nuclear physics already have been obtained. But the men who are responsible for the Eniac development are not resting on their present achievements and work is actively going on to build what may properly be called an electronic mathematical

[33] See N. Metropolis' introduction to paper no. 256, *FNM*, vol. 2, p. 861.

brain. I put great hopes in the help that the physicists will be able to derive by these very promising developments".[34]

In order to process the results that in 1952 started to come out from Chicago (as well as from other groups) on pion-nucleon scattering, and particularly in order to investigate on phase-shifts, Fermi decided to use the MANIAC I, the first of a series of three electronic computers that his friend Nick Metropolis had built at Los Alamos. He soon became fluent in writing the code for various problems to be processed by the MANIAC. In 1953-1954 he became interested in the project of having a big electronic computer at Chicago.

A proposal was submitted to AEC in the Spring 1954 and it was favourably considered. This led to a more detailed proposal, submitted in July 1954. "George" – that was to be the name of the Chicago computer – was supposed to start operating in the spring of 1955. However, Fermi died in the fall of 1954 after his last journey to Italy. On that occasion, he was requested by two researchers of the University of Pisa – Marcello Conversi and Giorgio Salvini – of his opinion concerning how to spend a substantial amount of money at that time available for the University of Pisa.

According to the line he was pursuing at Chicago, Fermi's prompt suggestion was to build an electronic computer, as he wrote to the rector of the University of Pisa: "On the occasion of my stay at the Varenna Summer School, Professors Conversi and Salvini mentioned that Pisa University might have at its disposal a great amount of money for the progress and the development of Italian research. On being questioned about the various possibilities of employing such funds, I thought that the idea of building an electronic computer in Pisa was by far the best. An electronic computer would constitute a research instrument from which all science and research activities would profit, in a way that is currently inestimable".[35]

According to Metropolis, who worked with him on the MANIAC, "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

[34] E. FERMI, "The future of nuclear physics", address given on the occasion of the award to Fermi of the Franklin Medal of the Franklin Institute, April 16, 1947, *EFP*, box 53.

[35] E. Fermi to E. Avanzi, August 11, 1954. Translation as in G. DE MARCO, G. MAINETTO, S. PISANI, P. SAVINO, "The Early Computers of Italy," *IEEE Annals in the History of Computing*, vol. 21 (1999), pp. 28-36, on p. 32.

of computers, and his remarks were always of a penetrating nature. [...] 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 analysing, nuclear particle tracks in emulsions of 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".[36]

Fermi served as a member of the GAC – the consulting committee of the AEC – from 1947 to 1950. In this capacity, he had the task to advise on projects and funding requests. Among those, requests for particle accelerators were the most conspicuous, as new ideas allowed to build more and more powerful machines and as it was clear that particle accelerators were to take over cosmic rays as far as particle production was concerned. In 1941 Kerst had invented the betatron, and in 1945 the principle of phase-stable acceleration, proposed by Vekser and McMillan, allowed to approach relativistic energies and was soon put to use in the 184" proton-synchrocyclotron at Berkeley and in a number of electron-synchrotrons with energies near 300 MeV.

In 1947 a debate arose concerning Lawrence's proposal of building a 6-GeV proton-synchrotron at Radiation Lab, in addition to the other machines at that time almost completed or underway (the mentioned 184" synchrocyclotron, a 330 MeV electron-synchrotron and a linear accelerator).[37] All these machines were supported by the Manhattan Engineering District (MED) and its successor, the AEC "at a level thirty times that of prewar laboratory budget".[38] In November 1947 "Fermi questioned the need for more energetic machines before those under construction at the Berkeley Radiation Laboratory were completed and their energy ranges explored and he worried that 'it would harm science to have [the GAC] endorse what appeared to be an unthoughtful project.'"[39] Eventually, the AEC decided to fund a smaller and upgradable machine at Berkeley, and another proton-synchrotron at Brookhaven, on the East Coast. The Berkeley Bevatron (which eventually achieved 6.2 GeV) and the Brookhaven Cosmotron (3 GeV) started operating in early 1954 and in Spring 1953, respectively, and were to dominate the scene of particle accelerators in the 1950s.

[36] N. METROPOLIS, introduction to paper no. 256, *FNM*, vol. 2, p. 861.

[37] Lawrence's initial proposal concerned a 10-GeV machine, later halved to 5 GeV "to avoid the appearance of greed" and finally raised to 6 GeV following the advice of E. McMillan and W. Panofsky, who argued that 6 GeV, "the energy thought necessary for nucleon creation, might be a more scientific goal," R. Seidel, "The postwar political economy of high-energy physics", in: L.M. BROWN, M. DRESDEN, L. HODDESON, (eds): *Pions to Quarks*, cit., p. 498.

[38] Ibidem.

Contributions to theory

As soon as Fermi completed his experimental activity on neutrons at the Argonne, he came back to theory. On February 2, 1948 he wrote: "I have for a few months retired from active work in experimental physics, and became a theoretical physicists, or at least, a student of theoretical physics. This is a practice that I have had for many years, to interrupt for a while the experimental work and devote myself to learning and understanding what has been done in theory in the meanwhile".[40]

New contributions to theory soon appeared. In the summer of 1949 Fermi and Yang prepared a paper where they questioned whether the pion was a fundamental entity or a composite particle formed by the association of a nucleon and an antinucleon.[41] This model accounted for pion's triplet isospin and its otherwise rather surprising negative intrinsic parity. The authors knew they were proposing just a tentative idea, as Yang recalled: "As explicitly stated in the paper, we did not really have any illusions that what we suggested may actually correspond to reality [...] Fermi [however] considered the question we raised as worthy of publication".[42] Today we know that none of the mentioned particles is elementary. The question raised by Fermi and Yang exerted a deep influence of the development of physics, giving rise to a vein of research in this field. Their model directly inspired Sakata, who in 1956 generalized the idea to include strangeness by taking the lambda hyperon to be a third fundamental constituent of a triple (formed also by proton and neutron) of particles that, together with their antiparticles, were to be the basic constituents of all the hadrons. Sakata's Nagoya associates developed this idea, pointing out that the group SU(3) was the appropriate generalization of the isospin group SU(2), which was the basis of the Fermi-Yang model. The latter exerted its influence also on the quark models of the 1960s.[43]

Next, Fermi tackled the problem of preparing a rough but reliable theoretical framework for the experimental data which were about to come out

[39] Ibidem.

[40] E. Fermi to J. Stearns, February 2, 1948. *EFP*, box 11.

[41] E. Fermi and C. N. Yang, "Are mesons elementary particles?", *PR*, vol. 76 (1949), p. 1739, also in *FNM*, vol. 2, paper no. 239.

[42] C.N. YANG, introduction to paper no. 239 in *FNM*, vol. 2, p. 674.

[43] See L.M. BROWN, M. DRESDEN, L. HODDESON, "Pions to quarks: particle physics in the 1950s", in: L. M. BROWN, M. DRESDEN, L. HODDESON, (eds): *Pions to Quarks*, cit., p. 18; L.M. BROWN, M. RIORDAN, M. DRESDEN, L. HODDESON, "The Rise of the Standard Model: 1964-1979", in: L. HODDESON, L.M. BROWN, M. RIORDAN, M. DRESDEN, (eds.): *The Rise of the Standard Model. Particle Physics in the 1960s and 1970s*, cit., p. 10; E. SEGRÈ, *Enrico Fermi*, cit., p. 174.

from accelerators. Initial experiments seemed to indicate that pi-mesons were pseudoscalar particles, coupled to the nucleon field through gradient (pseudovector) coupling. This situation had consequences which were just the reverse of the situation in QED, where the strength of elecron-photon interaction decreases with increasing energy, thereby ensuring the convergence of radiative cross-sections at high energies. The smallness of the coupling constant in the electron-photon interaction ($\alpha=2\pi e^2/hc\approx 1/137$) is another feature leading to finite cross sections in QED in the relativistic limit. These two features justify in QED the use of weak coupling theory and the application of standard perturbation methods even at relativistic electron energies. In pi-meson theory, however, the situation was just the opposite, since due to the nature of the coupling between pi-meson and nucleon, the meson-nucleon interaction increases with increasing energy. Furthermore, the coupling constant between nucleon and meson is much higher than in QED. Thus, as Marshak wrote in his textbook on mesons in 1952, "it would appear to follow that, at high energies, multiple meson processes should occur with appreciable probability and the perturbation-theoretic methods should break down".[44] For all these reasons, Fermi deemed appropriate to leave aside the perturbation theoretic approach and to build a different theoretical framework.

Anderson recalls: "During 1949-1950 Fermi began to prepare for the developments in high energy physics which were starting to come out of Berkeley [where the 170-inch cyclotron started operating in November 1946 and produced its first artificial mesons in early 1948], and would soon be coming from many other laboratories as well. In particular, he began to prepare himself and his colleagues and students at Chicago for the experiments which they would soon be able to do with the pi-mesons from the new cyclotron nearing completion in the Institute for Nuclear Studies. Fermi [...] needed a framework in which to set the information which came to him [...] for this he developed simplified methods for calculating the orders of magnitude of the pertinent quantities, the cross-sections of the processes of interests. His position was that the meson theories were not correct anyway, so why take the trouble to calculate anything with them exactly. Fermi's methods were a boon for experimentalists, who had difficulty in following the sophisticated way in which the theorists liked to put forth their theories".[45]

[44] R.E. Marshak, *Meson Physics*, cit., p. 275.

[45] H.L. Anderson, introduction to papers no. 241 and 242, in *FNM*, vol. 2, p. 789.

According to Segrè, Fermi once compared this effort of organizing available experimental data into a theoretical framework to the work that one might have done in Lorentz's times, before quantum mechanics, in order to explain atomic spectra.[46]

Fermi's method consisted in assuming that "as a result of fairly strong interactions between nucleons and mesons the probabilities of formation of the various possible numbers of particles are determined essentially by the statistical weights of the various possibilities. [...] 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. [...] This volume 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".[47]

Fermi's theory was not the first one; before him other authors, like Heisenberg and Lewis had proposed multiple meson production theories, to account for processes occurring at relativistic nucleon energies.[48] Fermi's method was taken quite seriously and was used for a long time. In order to test it at high energies, Fermi in 1953 was one of the first experimenters to use the 1.5 BeV Cosmotron, the proton-synchrotron that started operating fully at Brookhaven in the spring of 1953.[49] In 1953 Fermi worked out a tentative statistical theory of production of strange particles in pion-proton collisions.

There are several other topics in theoretical particle physics which Fermi was interested in and where he occasionally gave some remarkable suggestions. They range from *V*-particles to nucleon number conservation, from parity conservation to the nuclear shell-model. A thorough examination goes beyond the scope of the present paper. Here I will just give some examples.

---

[46] E. SEGRÈ, *Enrico Fermi*, cit., p. 174.

[47] E. FERMI, "High Energy Nuclear Events", *Progr. Theor. Phys.*, vol. 5 (1950), pp. 570-583, also in *FNM*, vol. 2, paper no. 241, on p. 790; see also E. FERMI, "Angular Distribution of the Pions produced in High Energy Nuclear Collisions", *PR*, vol. 81 (1951), pp. 683-687, also in *FNM*, vol. 2, paper no. 242.

[48] See R.E. MARSHAK, *Meson Physics*, cit., p. 282ff.

[49] See: E. FERMI, "Multiple Production of Pions in Pion-Nucleon Collisions", Academia Brasileira de Ciencias, vol. 26 (1954), pp. 61-63, also in *FNM*, vol. 2, paper no. 263; "Multiple Production of Pions in Nucleon-Nucleon Collisions at Cosmotron Energies", *PR*, vol. 92 (1953), pp. 452-453; Errata corrige in *PR*, vol. 93 (1954), pp. 1434-1435, also in *FNM*, vol. 2, paper no. 264.

Fermi was among those who received a preprint of the paper by Rochester and Butler announcing the discovery of the fist *V*-particle, which he acknowledged with interest.[50] The new particles started to raise considerable interest at the beginning of the fifties, and soon became one of the most puzzling topics in theoretical physics. While in his Silliman Lectures, given at Yale in the spring of 1950,[51] Fermi made no mention of the new particles, only a year later, in 1951, he was taking them quite seriously. During the second Rochester Conference (January 11-12, 1952) one full day was devoted to new particles, and in that year the first theoretical ideas on the new particles emerged.[52] The puzzle coming with *V*-particles was the contrast between their relatively high production rate, which showed that they were strongly interacting particles and their long lifetimes, which showed that they decayed by the weak interaction. Feynman reported in 1951 that Fermi and W. Fowler had conjectured that a steep potential barrier between nucleon and pion might suppress Λ-hyperon decay without inhibiting its production. In particular, Fermi remarked that this would be the case if the Λ-hyperon possessed a spin of, say, 13/2.[53] Also Feynman favoured the idea of states of high angular momentum for *V*-particles with respect to the idea of associated production, that had been put forward in 1952 by Abraham Pais.[54] In a visit to Caltech Fermi and Feynman discussed the hypothesis and the two of them collaborated a little bit at long distance on the idea of high angular momentum as an alternative explanation.[55]

In the same year (1951), using an approach similar to the one suggested by Fermi in his statistical production theory, Sachs suggested that neutral *V*-particles were merely an excited state of the neutron. In order to account for the very long lifetime of this state, Sachs suggested that the nucleon had a

---

[50] G.D. ROCHESTER and C.C. BUTLER, *Nature*, vol. 160 (1947), p. 855; E. Fermi to G. D. Rochester, December 3, 1947, quoted in A. PAIS, *Inward Bound*, cit., p. 512.

[51] E. FERMI, *Elementary Particles*, Yale University Press, 1951.

[52] In 1952 Marshak's textbook (R.E. MARSHAK, *Meson Physics*, cit.) was the first to contain a chapter on new particles. See also H.A. BETHE and F. DE HOFFMANN, *Mesons and Fields*, Row, Peterson and Company, New York, 1955, vol. 2, chapter 51.

[53] A. PAIS, *Inward Bound*, cit., p. 518; "From the 1940s into the 1950s", in: L.M. BROWN, M. DRESDEN, L. HODDESON, (eds): *Pions to Quarks*, cit., p. 351. Feynman's remark is dated June 7, 1951, and was a note added to his lectures on high-energy physics: R.P. FEYNMAN, "High-energy phenomena and meson theories", unpublished notes of lectures given at Caltech, January-March 1951.

[54] A. PAIS, "Some Remarks on the V-Particles", *PR*, vol. 86 (1952), pp. 663-672.

[55] M. GELL-MANN, "Strangeness", in: *Colloque International sur l'Histoire de la Physique des Particules*, Paris, July 21-23, 1982, *Journal de Physique*, vol. 12, Colloque C-8, supplément au n. 12, Decembre 1982, pp. 395-408, on p. 398.

very complex structure involving many mesons in virtual states. The excitation was therefore distributed over many degrees of freedom, so the probability for formation of that state that led to the disintegration might be possibly very small. Just before publication Sachs sent the manuscript to Fermi, asking for his opinion; Fermi, however, who was soon to leave to Los Alamos, was too busy to examine it in detail.[56]

Later, in 1953, Fermi pondered the matter more deeply. He confessed to his former student Uri Haber-Schaim: "I still am very puzzled at the properties of these various particles and I hope very much that some of them may merely turn out to be alternate modes of disintegration, which would make the interpretation a little bit easier".[57]

Fermi also sketched a theory of *V*-particles as states of high values of angular momentum (angular momentum selection rules were to slow down the disintegration rate).[58]

More or less at the time he wrote his attempt (early Fall 1953), Gell-Mann went to visit Fermi in Chicago, and explained to him the strangeness scheme, that he had worked out during the summer. "He sounded very skeptical" Gell-Mann recalled later "when I told him about explaining the strange particles by means of displaced isotopic spin multiplets. He said he was convinced more than ever that high angular momentum was the right explanation".[59] A little frustrated, Gell-Mann had however the chance, one or two days later, of seeing a letter that Fermi's secretary was writing to G. Cocconi, who was then investigating on the consequences of Fermi's and Feynman's proposal of high angular momentum. In his reply, Fermi warned Cocconi that there was in Chicago Gell-Mann "speculating about a new scheme involving displaced isotopic spin multiplets and perhaps that was the explanation of the curious particles rather than high angular momentum".[60] While Gell-Mann recovered from his depression, he became a little angry with Fermi for having been skeptical at his strangeness scheme a few days earlier.

Fermi, however, was not fully convinced. In February 1954, he wrote to Cocconi: "If Pais and Gell-Mann are right, the long lifetime is due to a selec-

[56] R.G. SACHS, "On the Nature of the *V*-Particles", *PR*, vol. 84 (1951), pp. 305-307; see also R.G. Sachs to E. Fermi, June 14, 1951, and Fermi's reply of July 5, *EFP*, box 11.

[57] E. Fermi to U. Haber-Schaim, May 12, 1953, *EFP*, box 10.

[58] *EFP*, Notebook D1, box 45. The theory is sketched in entries dated October 2 and 3, 1953.

[59] M. GELL-MANN, "Strangeness", in: *Colloque International*, cit., p. 401.

[60] M. GELL-MANN, "Strangeness", in: *Colloque International*, cit., p. 401, Gell-Mann's paraphrase of Fermi's letter.

tion rule that permits only pair interaction. I am not sure that this is finally proved and the alternate interpretation of strange particles as states of high angular momentum is still possible". Thus, apparently, still in 1954 Fermi favored the interpretation of the nature of strange particles as states of high angular momentum with respect to Pais' associate production or Gell-Mann's and Nishijima's, scheme based on the new strangeness quantum number.[61]

Fermi played an important role, in a discussion with Gell-Mann that took place in a class at the University of Chicago in 1954, in giving some hints that eventually led Gell-Mann to propose, together with Abraham Pais, the concept of "particle mixture". From Gell-Mann's account of the episode, it emerges that he feared quite a bit Fermi's objections: "Whenever Enrico came to a seminar, a lecture, a colloquium, or a course, if he didn't like something he interrupted. The interruption was not a minor matter; it continued until Enrico felt happy about what the speaker was saying, which often took essentially forever, that is to say the seminar ended, Enrico was still not happy, and the speaker never finished what he was going to say. If it was a course, as in this case, the course could be blocked for a week or two, while at each class he came in and started objecting where he had left off at the end of the previous class".[62]

Now, according to Gell-Mann's strangeness scheme, charged kaons had to have isospin 1/2. But this implied that there had to exist two distinct neutral K-mesons, K° and K'°, that were one the anti-particle of the other. However, they had to decay in exactly the same manner and differed only for strangeness quantum number (S = +1 for K° and S = −1 for K'°). According to the most plausible reconstruction, Fermi objected that, for K° and K'° to be considered distinct particles, one should be able to "see" such a difference in the laboratory, for example from decay mode or lifetime data. To be sure, the name "particle" should be reserved to objects with a unique lifetime.[63]

---

[61] E. Fermi to G. Cocconi, February 24, 1954, *EFP*, box 9. See also: M. GELL-MANN, "Isotopic spin and new unstable particles", *PR*, vol. 92 (1953), pp. 833-834; T. NAKANO and K. NISHIJINA, "Charge independence for *V*-particles", *Progr. Theor. Phys.*, vol. 10 (1953), p. 581. For an historical account see A. PAIS, *Inward Bound*, cit., chapter 20; A. PAIS, "From the 1940s into the 1950s", and L.M. BROWN, M. DRESDEN, L. HODDESON, "Pions to quarks: particle physics in the 1950s", in: L.M. BROWN, M. DRESDEN, L. HODDESON, (eds): *Pions to Quarks*, cit., p. 351 and pp. 19-21.

[62] M. GELL-MANN, "Strangeness", in: *Colloque International*, cit., p. 402.

[63] See: J.W. CRONIN, "The Discovery of CP Violation", in: L. HODDESON, L.M. BROWN, M. RIORDAN, M. DRESDEN, (eds.): *The Rise of the Standard Model*, cit., pp. 114-115; A. PAIS, *Inward Bound*, cit., p. 521; V.L. FITCH and J.L. ROSSITER, "Elementary particle physics in the second half of the twentieth

Gell-Mann recalls that Fermi "finally came up with a clinching argument. He said, 'I can write K° = A + *i*B, where A and B are both real fields with definite charge conjugation, and you have in each case a neutral particle that is its own charge conjugate'. Gell-Mann, who had already gone through an objection like this by an anonymous referee just a few months before, was ready for Fermi's objection: "Yes, that's true" he replied "but in the production of strange particles, because of strangeness conservation, it is the K° and the K°' that matter; in the decay, if it is into pions or photons or both, then it will be your A and B that matter and that have different lifetimes".[64]

That was the seed of the idea of particle mixtures. A year later, Gell-Mann and Pais were led to analyze the whole matter in the light of charge-conjugation invariance and to conjecture that, as far as decay is concerned, the true particles should be K_1 and K_2 (Fermi's original A and B, respectively), having definite and different lifetimes and definite behaviour with respect to charge conjugation. K° and K'° having definite strangeness, are to be considered in the production processes, and are linear combinations of K_1 and K_2.[65] Furthermore, only K_1 decays into $\pi^-\pi^+$, while K_2 does not have a longer lifetime, about 100 times longer, supposing the decay mode to be $K_2 \rightarrow \pi^- + \pi^+ + \gamma$. On the other hand, K° and K'° particles, with definite strangeness, are "real" objects insofar as production phenomena are concerned. The concept of particle mixture was soon confirmed by experiments carried out at Brookhaven in 1955 and was one of the pieces of new physics that nature was to teach human beings through neutral kaons.[66]

A very important question that Fermi helped to raise was the possible existence of a spin-orbit interaction in the shell model of nuclei, a hint he gave Maria Goeppert Mayer, who was working in 1948-1949 with "nuclear magic numbers", describing the existence of unusually stable configurations of neutrons or of protons whatever the associated number of the other nucleons.

Maria Mayer was awarded in 1963 the Nobel Prize for the invention of the shell model of nuclei and acknowledged Fermi's suggestion in her Nobel

century", in: L.M. BROWN, A. PAIS, B. PIPPARD (Eds.), *Twentieth Century Physics*, 1995, vol. 2, p. 658. I take pleasure in thanking Prof. J. W. Cronin, who took part to the mentioned class and gave me some additional information on the discussion between Gell-Mann and Fermi.

[64] M. GELL-MANN, "Strangeness", in: *Colloque International*, cit., p. 402.

[65] M. GELL-MANN and A. PAIS, "Behavior of Neutral Particles under Charge Conjugation", *PR*, vol. 97 (1955), pp. 1387-1389.

[66] See also A. PAIS, *Inward Bound*, pp. 521-522; L.M. BROWN, M. DRESDEN, L. HODDESON, "Pions to quarks: particle physics in the 1950s" and W. CHINOWSKY, "Strange Particles", in: L.M. BROWN, M. DRESDEN, L. HODDESON, (eds): *Pions to Quarks*, cit., pp. 22-23, p. 334 and p. 338.

Lecture: "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 immediately answer: 'Yes, of course, and that will explain everything'."[67]

Finally, it is worth mentioning the issue of the conservation of parity, that was to be unveiled by two students of Fermi, C.N. Yang and T.D. Lee. Yang reported that Fermi was "always very much interested in the question of parity conservation". Segrè ventured to say that Fermi might have had some suspicion about parity conservation since he frequently stated "criptically" that "nobody had ever changed right into left-hand in space".

While it is impossible to know what Fermi exactly meant, an indirect contribution that he gave to this discovery can be discerned in Pais' words about Yang' and Lee's analysis of parity conservation: "Lee and Yang faced the challenge […] they started a systematic investigation of the then status of experimental knowledge concerning the verification of the space reflexion invariance and charge conjugation invariance. Their conclusion was that for one group of interactions neither invariance had so far been established […] The work of T.D. and of Frank, as they are affectionately called, is characterized by taste and ingenuity, by physical insight and formal power. Their counsel is sought by theorist and experimentalist alike. In this they have more than a touch of the late Fermi".[68]

Pion scattering and nucleon structure

In 1951 Fermi came back to experimental physics, as his colleague and friend H.L. Anderson recalls: "in the spring of 1951 the big synchrocyclotron at Chicago started operating. It could accelerate protons at 450 MeV and a copious number of pions could be produced with these. The machine had been built with the idea that Fermi would be the principal user and when

[67] M. GOEPPERT MAYER, "The shell model", in: *Nobel Lectures, Physics, 1963-1970*, Elsevier, Amsterdam, 1972, pp. 20-37, on p. 29; "On Closed Shells in Nuclei. II", *PR*, vol. 75 (1949), pp. 1969-1970, on p. 1970; E. SEGRÈ, *Enrico Fermi*, cit., p. 175; C.N. YANG, introduction to paper no. 239, *FNM*, vol. 2, p. 674.

[68] A. PAIS, *Inward Bound*, cit., pp. 532-533; C.N. YANG, introduction to paper no. 238, *FNM*, vol. 2, p. 673; see also Proceedings of the International Conference on Nuclear Physics and the Physics of Fundamental Particles. University of Chicago, September 17 to 22, 1951, p. 2 and 109; E. SEGRÈ, *Autobiografia di un fisico*, cit., p. 346.

it was finally complete he spent a great deal of time familiarizing himself with its operation, laying out the pion beams and measuring their intensity and energy".[69]

An "International Conference on Nuclear Physics and the Fundamental Particles" was held at Chicago from September 17 to 22, 1951, on the occasion of the inauguration of the cyclotron. Approximately 200 scientists attended, and forty of them came from foreign countries. The conference almost coincided with Fermi's 50th birthday, which was celebrated informally by some of his old friends at the breakfast table. Fermi delivered the first paper of the conference. He listed some 21 "fundamental" particles, expressing a belief in the existence of antinucleons, still to be discovered. He also expressed his conviction that "philosophically, at least some of these 21 particles must be far from elementary. The requirement for a particle to be elementary is that it [has to] be structureless. Probably some of these 21 particles are not structureless objects. They may even have some geometrical structure, if geometry has any meaning in such a small domain".[70]

The initial experiments aimed at measuring the transmission of first negative and then positive pions through liquid hydrogen targets. The first results, obtained with negative pions, seemed to indicate that pions behaved as pseudoscalar particles, thereby allowing to rule out at once several meson theories. Further results with π^- gave evidence that the interaction between pions and protons was strong and of range of the order of the pion Compton wavelength. This result confirmed that pions must play a central role in nuclear interactions. The real surprise, however, came from a comparison of the cross sections for π^+ and π^-. Let us consider the processes:

(A) $\pi^- + p \rightarrow \pi^- + p$ (elastic scattering)
(B) $\pi^- + p \rightarrow \pi^\circ + n$ (charge exchange scattering)
(C) $\pi^- + p \rightarrow \gamma + n$ (radiative capture)
(D) $\pi^+ + p \rightarrow \pi^+ + p$ (elastic scattering)

The π^+ measurements started in mid-December 1951 and soon brought new news. Results showed that $\sigma(\pi^+)$ (where σ is the cross-section) was 2-3 times bigger than $\sigma(\pi^-)$. This was a puzzling result, due to the greater mul-

[69] H.L. ANDERSON, introduction to paper no. 246, *FNM*, vol. 2, p. 825.

[70] E. FERMI, "Fundamental Particles", Proceedings of the International Conference on Nuclear Physics and the Physics of Fundamental Particles. University of Chicago, September 17 to 22, 1951; also in: *FNM*, vol. 2, pp. 825-828, p. 826.

tiplicity of reactions involving π^- (processes A, B, C) with respect to reaction D, the only one involving π^+. According to Anderson: "This anomaly puzzled Fermi very much. I recall the day [December 21] we were measuring this cross section. Fermi was running the counters. He had a stopwatch in one hand, a slide rule in the other, the desk calculator was clattering away, and his eyes were keeping a close watch on the flashing neon indicators to detect any possible misbehavior. After he recorded each count, he would calculate the cross section. He kept shaking his head because it kept coming out so high. There was so little for me to do that I just sat back and began to go through my mail. On this day there was a preprint of a paper by Keith Brueckner on meson nuclear scattering. 'Enrico,' I said after glancing at one of the curves, 'here's a guy who seems to think the π^+ cross section should be higher than the π^-.' Fermi was disparaging in his retort. 'How should he know anything about it?'. 'But Enrico' I persisted, now taking the trouble to scale off Brueckner's curve, 'this fellow Brueckner says we ought to be getting about 120 millibarns for the cross section.' 'We're getting even more than that' admitted Fermi. 'Let me have a look at that paper.' Then, 'Will you take over for 20 minutes while I go up to my office?' I suppose he consulted his 'Artificial Memory' for he was back in 20 minutes with a broad grin. 'The cross sections will be in the ratio 9:2:1 for the π^+:π°:π^- scattering,' he announced [i.e. the following relationship holds: $\sigma(D)$:$\sigma(B)$:$\sigma(A)$=9:2:1]. This would be the case if the dominant interaction which took place was for the state of isotopic spin 3/2".[71]

Thus it was taken the path that would have shed some much needed light on the pion-nucleon scattering processes and on the behavior of nuclear forces. The concept of isotopic spin (or isospin), that had been introduced in 1932 by Heisenberg, was suddenly brought at the center of the stage. It was understood that, as far as one can neglect the contribution of electromagnetic forces, isospin is a conserved quantity in processes involving nuclear forces. This in turn provided some hint on the existence of symmetries and on the possibility to organize hadrons in isospin multiplets. The scattering results obtained by Fermi for process (D), when interpreted in the light of Brueckner's theory, showed that scattering was dominated by the state where total isospin T of the pion-nucleon system and the total angular momentum J were both 3/2. The discovery of a strong peak in the $\sigma(\pi^+)$ centered at 155

[71] H.L. Anderson, "Meson Experiments with Enrico Fermi", *Rev. Mod. Phys.*, vol. 27 (1955), pp. 269-272, on p. 270.

MeV (in the center of mass system) was the first hint of the existence of a nuclear resonance, even if it took some years before this was generally acknowledged, soon after Fermi's death. The resonance was later to be denoted by "3-3 resonance" or "Δ", a multiplet with charges Δ^{++}, Δ^{+}, Δ°, Δ^{-}. Its role in πN (pion-nucleon) scattering is that of a real particle, being formed according to the reaction $\pi+N \rightarrow \Delta$ and decaying according to the reaction $\Delta \rightarrow \pi+N$. Besides total angular momentum (3/2), isospin (3/2), and parity (even) it has a definite value of mass (1232 MeV/c^2) and lifetime (10^{-23} s), thereby showing all the attributes of an unstable particle. Δ was the first of a long series of nuclear resonances, to be discovered later, at different energies. It had a great impact on the development of theoretical physics as it provided, for example, a hint on the composite nature of nucleons.

Back to Fermi's experimental results, it has to be emphasized that they provided theoreticians with a tool – isospin – independent of perturbation theory which could hardly be applied to nuclear interactions, since the magnitude of the interaction made perturbative methods - that had proven to be so useful in QED – useless. According to Pais: "Relations like σ(D):σ(B):σ(A):=9:2:1 came as a blessing; theorists had at least something to offer their experimental colleagues. Moreover, since isospin has nothing to do with perturbation theory meson theories, it could serve as a reliable guide to what needed explanation by alternative theoretical methods. Isospin does not, of course, suffice to inform how it should be explained that, for example, a certain state dominates at a certain energy, as in the Chicago experiments. Symmetry saves, but only up to a point".[72]

Soon three letters were sent to the *Physical Review*, all of them received on January 21, 1952.[73] Fermi's experiments raised the interest of theoreticians, who hoped that these experiments may hold the key to the understanding of nuclear forces.

Richard Feynman, then in Brazil, corresponded with Fermi, sending predictions, based on different meson theories, concerning nucleon-pion cross sections. In his reply Fermi employed a method based on the analysis of the phase shifts. The method was not new, but "Fermi's adoption of the tech-

---

[72] A. PAIS, *Inward Bound*, cit., pp. 486-487.

[73] H.L. ANDERSON, E. FERMI, E.A. LONG, R. MARTIN, D.E. NAGLE, "Total cross section of negative pions in hydrogen"; E. FERMI, H.L. ANDERSON, A. LUNDBY, D.E. NAGLE, G.B. YODH, "Ordinary and exchange scattering of negative pions by hydrogen"; H.L. ANDERSON, E. FERMI, E.A. LONG, and D.E. NAGLE, "Total cross section of positive pions in hydrogen"; *PR*, vol. 85 (1952), p. 934, p. 935 and p. 936. Also in *FNM*, vol. 2, papers nos. 248, 249, 250.

nique revived the interest in it, and it came into wide usage thereafter".[74] At the second Rochester conference, held on January 11-12, 1952, Fermi "was quite excited about the new experimental results and about his letter to Richard P. Feynman on phase shifts".[75]

Reporting on Brueckner's interpretation of his own results, Fermi asserted that "One can therefore interpret the experimental results by postulating the existence of a broad resonance level T=3/2 in the band of energy 100-200 MeV, with the consequence that practically all the scattering comes through T=3/2 in this energy region".[76] However, even if he had initially worked out in detail the resonance hypothesis, Fermi was publicly much more cautious and even doubtful about the existence of a resonance level.[77]

His attitude was perfectly adequate: "soon after Rochester II, C. N. Yang pointed out an ambiguity in the phase-shift analysis of the πN scattering experiments [see below], and it took several years before the T=3/2, J=3/2 πN resonance [...] was placed on a completely sure footing. [...] But it is fair to say that after Rochester II, the concept of isospin invariance of the πN interaction, and consequently the search for other symmetry principles, moved into the forefront of theoretical thinking in particle physics. Moreover, the methods developed to confirm the Δ resonance were used to establish the existence of scores of hadronic resonances in succeeding years. The determination that the pion was a pseudoscalar particle, that the πN interaction was isospin-invariant, and that the first excited state of N possessed the quantum numbers T=3/2, J=3/2 seemed to provide a reasonable starting point for a dynamical theory of the strong πN interaction".[78]

The next task was to obtain more detailed information about the scattering process. This could be done by making angular distribution measurements. It was expected that at low energies only *s*-and *p*-waves are important.

---

[74] H.L. ANDERSON, introduction to papers no. 251 and 255, *FNM*, vol. 2, p. 844; E. Fermi, "Letter to Feynman," January 18, 1952, *FNM*, vol. 2, paper no. 251, pp. 844-846. See also: R. Feynman to E. Fermi, December 19, 1951, *EFP*, box 9.

[75] C.N. YANG, "Particle physics in the early 1950s," in: L.M. BROWN, M. DRESDEN, L. HODDESON, (eds): *Pions to Quarks*, cit., p. 41. Fermi's letter to Feynman was reproduced as Appendix 3 in the conference proceedings.

[76] E. FERMI, in "Proceedings of Second Rochester Conference, Jan. 1952", unpublished mimeograph, edited by A.M.L. MESSIAH and H.P. NOYES, p. 26.

[77] See: H. ANDERSON, "Early history of physics with accelerators", in *Colloque International sur l'Histoire de la Physique des Particules*, cit., pp. 101-161, on p. 130ff; see also "Proceedings of Second Rochester Conference, Jan. 1952", cit., p. 31ff and p. 37ff.

[78] R.E. MARSHAK, "Scientific impact of the first decade of the Rochester conferences (1950-1960)," in: L.M. BROWN, M. DRESDEN, L. HODDESON, (eds): *Pions to Quarks*, cit., pp. 651-652.

	$s_{1/2}$	$p_{3/2}$	$p_{1/2}$
T=3/2	α_3	α_{33}	α_{31}
T=1/2	α_1	α_{13}	α_{11}

Table 1. Phase shifts and states for the pion-proton scattering

Then the following equation holds, between the differential cross section and the scattering angle:

$$d\sigma / d\Omega = a + b \cos\theta + c \cos^2 \theta \qquad (1)$$

Taking spin 0 for the pion, and knowing that spin is 1/2 for nucleons, one has three possible states: $s_{1/2}$, $p_{1/2}$ and $p_{3/2}$, since total spin can be 1/2 and 3/2. These three states can exist in either of the isotopic spin states T=1/2 and T=3/2. Thus we have six different states. Therefore, six phase shifts can describe the scattering as reported in Table 1.

For each energy, measurements at three angles for each of the processes $\pi^+ \rightarrow \pi^+$, $\pi^- \rightarrow \pi^\circ$, $\pi^- \rightarrow \pi^-$, would give nine experimental values for the cross sections, which, though affected by experimental errors, would allow to determine the six phase shifts, which in turn would give detailed information on the scattering process and on the hypothesized existence of a resonance. Fermi had analysed the data in terms of phase shifts initially using desk calculators. In that period, the first of a series of three electronic computers, the MANIAC I (MAthematical Numerical Integrator and Computer) had become operational on March 15, 1952. Since the relationships between cross sections and phase shifts is not a simple one and had to be repeated many times, and in order to include experimental data obtained by the Columbia and Carnegie groups, Fermi proposed to use the new computer at Los Alamos.[79] Mathematically, it had been decided to adopt a least-squares type of fit was to make maximum use of the data. The problem was therefore to find the six phase shifts $\alpha_1 \ldots \alpha_6$ that minimize the quantity M in the following equation:

$$M(\alpha_1 \ldots \alpha_6) = \sum_{i=1}^{9} [\sigma_i\,(computed) - \sigma_i\,(measured)]^2 / \varepsilon_i^2 \qquad (2)$$

where the ε_i are the experimental errors in the measurements of cross sections σ_i.

[79] See H. L. Anderson, "Meson Experiments with Enrico Fermi", cit., p. 271; N. Metropolis, introduction to paper no. 256, *FNM*, vol. 2, p. 861.

As Anderson recalled, “The MANIAC fascinated Fermi and he had been itching to get his hands on it. Now he had a problem to put to the machine. Metropolis would teach him how, and he could feed in his questions, watch the machine digest them, and gather the answers as they rolled out”.[80] The machine actually found the set of phase shifts. The problem was that it found too many. “For some years following” Anderson recalled “the experts in pion physics talked about the different possible solutions. There was the Fermi solution and the Yang solution; then a new one called the Fermi-Metropolis solution, also the Steinberger solution and finally the Bethe-De Hoffmann solution”.[81] Fermi struggled hard in the last two years of his life to discover the solution to the phase shift problem. He spent the summer of 1952 and of 1953 working with MANIAC, at Los Alamos; moreover, in 1952-1954 he intensively corresponded with Metropolis from Chicago. In the fall of 1953 Fermi’s work was expanded and extended by Hans Bethe, Frederic De Hoffmann, Nick Metropolis and E. Alei, who used new data that began to emerge from other laboratories. A number of techniques were used to discriminate between the sets of phase shifts found by the MANIAC. After Fermi’s death it came out that “the Bethe-De Hoffmann solution, which was really Fermi’s original choice extended properly in the higher energy region, was most probably the correct one”.[82] Thus, the existence of the 3-3 pion-nucleon resonance was firmly established.

Epilogue: Fermi’s outlook of theoretical physics and physics in the fifties

To complete my overview on Fermi’s contributions to high-energy physics, I plan to examine Fermi’s attitude towards theoretical particle physics in the context of the crisis that theoretical physics went through in the early fifties.

First of all, Fermi was a very peculiar theoretician, as Anderson has empha-

[80] H.L. Anderson, “Meson Experiments with Enrico Fermi”, cit., p. 271.

[81] H.L. Anderson, introduction to papers no. 257 and 258, *FNM*, vol. 2, p. 871.

[82] H.L. Anderson, introduction to papers no. 257 and 258, *FNM*, vol. 2, p. 871. See also H.L. Anderson, “Meson Experiments with Enrico Fermi”, cit., p. 272; F. De Hoffmann, N. Metropolis, E. Alei, H.A. Bethe, *PR*, vol. 95 (1954), p. 1586; H.L. Anderson, E. Fermi, R. Martin, D.E. Nagle, “Angular distribution of pions scattered by hydrogen,” *PR*, vol. 91 (1953), pp. 155-168 (also in *FNM*, paper no. 257); E. Fermi, M. Glicksman, R. Martin, D.E. Nagle, “Scattering of negative pions by hydrogen,” *PR*, vol. 92 (1953), pp. 161-163 (also in *FNM*, paper no. 259); E. Fermi, N. Metropolis, E. Alei, “Phase shift analysis of the scattering of negative pions by hydrogen,” *PR*, vol. 95 (1954), pp. 1581-1585 (also in *FNM*, paper no. 260).

sized in a panel discussion: "We've been discussing [...] about how [...] the experimentalists are not very much inspired by the theory, and the theorists somehow manage to anticipate some of the developments in working out some of their ideas. But I want to mention another kind of theorist, because of my familiarity with Fermi's work. He was the kind of theorist who didn't work that way at all. If you examine all the theoretical works of Fermi, and there were some very distinguished ones, as you know, they were always done with the idea of explaining some experimental fact. [...] At the end of almost every paper in which he made a theoretical development, there was always a calculation to show that the theory gave some agreement with the experiment".[83]

Secondly, at the beginning of the fifties, Fermi appeared as a disenchanted theoretician. When he realised the severe drawbacks of the available meson theories, Fermi soon got convinced that there was something deeply wrong with the theory itself and that it was not worth it to use it extensively in calculations that might prove to be altogether wrong. That was the motivation for his statistical theory of multiple meson production that he used as a guideline in his investigations. This attitude began to emerge as early as 1951, when he was already involved in experimental research on pion-nucleon scattering.

In October, speaking to an audience of three thousand people assembled in the Chicago Civic Opera House for the twentieth anniversary meeting of the Institute of Physics in Chicago, he said that "when the Yukawa theory first was proposed, there was a legitimate hope that the particles involved, protons, neutrons and pi-mesons, could be legitimately considered as elementary particles. This hope loses more and more its foundation as new elementary particles are rapidly being discovered [...] Of course, it may be that someone will come up soon with a solution to the problem of the meson, and the experimental results will confirm so many detailed features of the theory that it will be clear to everybody that it is the correct one. Such things have happened in the past. They may happen again. However, I do not believe that we can count on it, and I believe that we must be prepared for a long hard pull".[84]

At the beginning of the fifties Fermi was convinced that theoretical physics was going towards a new revolution, like the one represented by the intro-

[83] H.L. ANDERSON, in: L.M. BROWN, L. HODDESON, (eds.): *The Birth of Particle Physics*, cit., p. 268, first round-table discussion.

[84] E. FERMI, "The Nucleus", *Physics Today*, vol. 5 (March 1952), pp. 6-9, also in *FNM*, vol. 2, paper no. 247, p. 834.

duction of quantum mechanics, some twenty-five years before. He was doubtful that quantum mechanics held within a region (that he called "contact") having the size of the range of nuclear forces, i.e. 10^{-13} cm, and he believed that current physics could only explain facts occurring outside nucleus.

At the Chicago Conference, in September 1951, he said: "It is desirable to arrange experimental data so as to exhibit most clearly the features which come from fundamental particle interactions taking place at 'contact', namely within about 10^{-13} cm. This may be done by assuming quantum mechanics holds in regions outside 'contact'(there is little doubt in my mind that it does), and using it to remove from consideration phenomena which do not depend on what happens in the 'contact' volume. The result is a compressed expression of experimental results, in which the nature of fundamental interactions between particles may be more easily discernible".

"Within a volume corresponding to 10^{-13} cm" he added "there are lions which will eat us if we get within".[85] As we have seen, also Oppenheimer showed a similar standpoint at the Shelter Island Conference in 1947. Yang remarked that in 1945-1955 "physicists born before 1905 seemed to have general reservations [...] whether or not quantum mechanics were applicable inside of 'the electron radius' [...] On the other hand, in that same period, younger physicists, those of my generation, seemed to have very little inclination to question the validity of quantum mechanics".[86] This would not be the first instance of a generational difference within the community of physicists.

Fermi conjectured that the revolution he foreshadowed in physics might eventually demand new mathematics and new geometry, to deal with regions inside "contact": "Perhaps the introduction of a finite size of the elementary particles or even a granular geometry such as is suggested by Heisenberg and Snyder may be clues to the solution".[87] Thus Fermi showed some influence from Heisenberg's *S*-matrix theory, i.e. a theory based only on observables quantities, which banished distances shorter than 10^{-13} cm and time intervals shorter than $3x10^{-24}$ s.[88]

Facing such a disparaging situation, Fermi thought that theoretical physics should go back to the origins of science and proceed adhering strictly to the

---

[85] E. Fermi, "Fundamental Particles", cit.; also in: *FNM*, vol. 2, pp. 825-828, p. 827.

[86] C.N. Yang, "Particle physics in the early 1950s," in: L.M. Brown, M. Dresden, L. Hoddeson, (eds): *Pions to Quarks*, cit., p. 42.

[87] E. Fermi, *Elementary Particles*, cit., p. 24.

[88] See, for example, H. Rechenberg, "The early S-matrix theory and its propagation (1942-1952)," in: L.M. Brown, M. Dresden, L. Hoddeson, (eds): *Pions to Quarks*, cit., p. 552.

evidence provided by experimental data. At the Chicago Conference he asserted that "Theoretical research may proceed on two tracks: 1. Collect experimental data, study it, hypothesize, make predictions, and then check. 2. Guess; if nature is kind and the guesser clever he may have success. The program I recommend lies nearer the first track".

One month later, at the Institute of Physics Anniversary he was more explicit, asserting that "it is difficult to say what will be the future path. One can go back to the books on method (I doubt whether many physicists actually do this) where it will be learned that one must take experimental data, collect experimental data, organize experimental data, begin to make working hypotheses, try to correlate, and so on, until eventually a pattern springs to life and one has only to pick out the results. Perhaps the traditional scientific method of the textbooks may be the best guide, in the lack of anything better".[89] Fermi's recommendations were followed in some cases in which the observations were exceedingly puzzling, like the case of *K*-mesons: "the senior physicists took the general attitude of Enrico Fermi: Collect evidence, but make no assumptions about the identity of any of these K-mesons until the evidence allows no other possibility".[90]

In analysing Fermi's role in the physics of the early fifties, one should also keep in mind the status of theoretical physics as it was in those years and, in particular, its relationship with experimental physics: "In recent years, when theory called for new particles (such as the *W* and the *Z*), experiments obligingly provided them, but in the fifties experiment outran theory and produced surprise after surprise. Neither the muon nor the strange particles were expected, nor were they welcomed, for the most part, for they destroyed what might have been a consensus for a new unification. Without the muon, physicists had anticipated a closed system in which the electron, proton, and neutron were the constituent particles of matter, while the photon and Yukawa meson were field quanta that carried the electromagnetic and strong interactions. Add the neutrino for weak interactions, and complete the picture by including the antiparticles of the fermions. The muon changed all that".[91]

In the context of the fifties, where theoretical physics had to face an increasing number of "surprises" coming from experiments it seems that a

---

89 E. FERMI, "Fundamental Particles", cit.; also in: *FNM*, vol. 2, pp. 825-828, p. 827; "The Nucleus", cit., p. 834.

90 R.H. Dalitz, "*K*-meson decays and parity violation," in: L.M. BROWN, M. DRESDEN, L. HODDESON, (eds): *Pions to Quarks*, cit., p. 435.

91 L.M. BROWN, M. DRESDEN, L. HODDESON, "Pions to quarks: particle physics in the 1950s", in: L.M. BROWN, M. DRESDEN, L. HODDESON, (eds): *Pions to Quarks*, cit., p. 4.

physicist like Fermi, who was used to work with experimentalists, who himself was an excellent experimentalist, and a pragmatic theoretician, used to focus on experimental evidence, possessed to the highest extent the abilities and the attitude so badly needed by the way physics was in the fifties.

Fermi's influence may well have gone beyond his own activity as a researcher, since he actually taught a style of making physics to a generation of physicists that with their discoveries were to revolutionize this discipline between the fifties and the sixties. It has already been noted the pragmatism of the postwar generation of U.S. theorists and the somewhat pragmatic spirit of theories like the *S*-matrix and the one grounded on the dispersion relations that played an important role in the fifties.[92] There are several reasons for this and a detailed analysis of those reasons is beyond the aim of this paper. However, it can be safely said that among those reasons, Fermi's influence played a significant role, and the fact that many protagonists of the just mentioned theoretical fields had been his students or collaborators is *per se* revealing.

Did the revolution that Fermi foreshadowed actually happened in physics? With hindsight we can say "yes and no". On the one hand, quantum mechanics still holds and no region where it is not applicable has been found. On the other hand, theoretical physics has undergone deep changes since the early fifties, and we may well venture to say that a true revolution has occurred with the introduction of quarks, QCD and electroweak theory. Quantum field theories, whose validity seemed confined to quantum electrodynamics at Fermi's age, have been brought again into the forefront of physics, after the importance of non-Abelian gauge theories was properly recognised.

As a concluding remark, I would like to show Fermi's standpoint concerning one of the issues that were to raise considerable debates in modern times, i.e. the intrinsic usefulness of particle physics, or, in other words, the rationale behind investing huge amounts of money to support more and more expensive facilities and experiments. He was a man deeply committed to science, and he believed that "the vocation of a scientist is to drive back the frontiers of our knowledge in all directions". He had a deep faith in the intrinsic usefulness of doing fundamental particle physics anyway, whatever complex or abstract this research might be.

This is testified by his words in an address he gave just before that momen-

[92] See, for example, A. Pickering: "From field theory to phenomenology: the history of dispersion relations," and S. Schweber, "Some reflections on the history of particle physics in the 1950s," in: L.M. Brown, M. Dresden, L. Hoddeson, (eds): *Pions to Quarks*, cit., p. 587 and pp. 671-674.

tous Rochester II conference, where he foreshadowed the debates later to come: "Some of you may ask. What is the good of working so hard merely to collect a few facts which will bring no pleasure except to a few long-haired professors who love to collect such things and will be of no use to anybody because only few specialists at best will be able to understand them? In answer to such question I may venture a fairly safe prediction. History of science and technology has consistently taught us that scientific advances in basic understanding have sooner or later led to technical and industrial applications that have revolutionized our way of life. It seems to me improbable that this effort to get at the structure of matter should be an exception to this rule. What is less certain, and what we all fervently hope, is that man will soon grow sufficiently adult to make good use of the powers that he acquires over nature".[93]

While the view concerning the high-energy particle physics has meanwhile perhaps changed, Fermi's last statement is more than ever modern, its scope not merely concerning physics, but the whole realm of science.

This research has been partially supported by the Italian Embassy in the US. I wish to thank the management and the staff of the Department of Special Collections of the University of Chicago Library for kind and effective help in my research on Fermi's paper.

Giulio Maltese
He is with Ibm as a Research staff member working on automatic speech recognition. Since 1987 he has been doing research in the history of science, focusing on the history of rational mechanics, on the development of general relativity, and on the history of physics in Italy in the 20th century. As a contract professor he has given courses on history of mechanics at the universities of Genoa and Rome ("La Sapienza"). He has also given courses and seminars on the foundations of mechanics and of electrodynamics. He is a member of the Group for the history of physics at the Department of physics of "La Sapienza" University of Rome; of the Italian Society of Physics, and of the Italian Societies for the History of Science and for the History of Physics and Astronomy. He authored three books on the history of mechanics and several papers on various topics concerning the history of physics. He is currently focusing on Enrico Fermi's role in the development of physics in the forties and the fifities during Fermi's stay in the United States (1939-1954).

[93] E. FERMI, "The Future of Nuclear Physics," unpublished address, Rochester, January 10, 1952, *EFP*, box 53.

Robert Seidel

Enrico Fermi, High-Energy Physics and High Speed Computing

The field of high-energy physics saw early applications of the computer as a scientific instrument, both for data analysis and as a controller of the accelerator and other detectors. The transfer of this technology from wartime military laboratories to peacetime high-energy physics laboratories shows how Enrico Fermi conceived the computer as a scientific instrument in high-energy physics.

Enrico Fermi, la fisica delle alte energie ed il calcolo ad alte prestazioni

Il campo della fisica delle alte energie utilizzò le prime applicazioni del computer come strumento scientifico, sia per quanto riguardava l'analisi dei dati sia come strumento di controllo dell'acceleratore ed altri rivelatori. Il trasferimento di tale tecnologia dai laboratori militari di guerra ai laboratori di fisica delle alte energie in tempo di pace, mostra la concezione che Enrico Fermi aveva del computer come strumento scientifico nella fisica delle alte energie.

Computers and World War II

Computers are the transcendent instruments of experimental high-energy physics at the end of the 20th century. They are involved in high-energy physics experiments from the cradle to the grave. Computers design new accelerators, detectors, and other experimental apparatus, computers operate this apparatus tirelessly during the extended periods of time required to conduct such experiments and computers sort, interpret, analyze, and present the results. Peter Galison has even suggested that in modern high-energy physics experiments, they take over many the traditional role of the physicists in high-energy physics experiments.[1]

Enrico Fermi stands at the forefront of those physicists who brought the computer into the service of high-energy physics. He saw that the techniques developed at Los Alamos for the design of nuclear weapons could be transferred to more peaceful studies of nuclear behavior. He initiated a research program before his death in 1954 that eventually transformed the study of high-energy physics and was directly responsible for the introduction of computer data analysis techniques less than a decade later. This is the story I would like to tell you today.

The early history of computers is well-known, and the contributions of physicists like Howard Aiken, John Atanasoff, John Mauchly, and other pioneers in the construction of computers has been the subject of several conferences over the past decade to celebrate the semi-centennial of the electronic computer.

The exigencies of World War II accelerated the development of these machines and, more importantly perhaps, saw enhanced use of the electromechanical computers associated with the firms of Burroughs, IBM, and NCR, including work on code-breaking machines in England and the United States that was hidden from history for a quarter of a century. Even for the better-known machines, there is a great deal that remains shrouded in secrecy about their early operations. ENIAC, for example, ran a program – which is still classified – to evaluate Edward Teller's design of the classical super (H-bomb) in late 1945. Most of the early computers were used to run similar programs for Los Alamos in the postwar decade, and those programs typically stressed the capacity of the machines.

During World War II at Los Alamos, the calculations required to predict the behavior of implosion designs of fission weapons were performed on

[1] PETER GALISON, *Image and Logic: A Material Culture of Microphysics* (Chicago, 1997), 391-2.

IBM business machines. Enrico Fermi, who came to Los Alamos in 1944 after completing his work on the design of nuclear reactors for plutonium production, took a particular interest in the newly installed IBM machines, according to Nicholas Metropolis, who, with Richard Feynman and other computationally inclined physicists, had assembled them to replace teams of human "computers" who had performed the necessary calculations previously.

Fermi's F-Division also housed Edward Teller, who had abandoned his early interest in implosion to work on the design of a fusion weapon, a design which Fermi had stimulated by suggesting the possibility of triggering a fusion bomb with a fission bomb to him in 1942.

Fermi devoted most of his efforts at wartime Los Alamos to the perfection of their reactor, and it is unclear whether he provided Teller with more than moral support for his work. We do know that Fermi attended a meeting at Los Alamos in the spring of 1946 that discussed the ENIAC's results, and although different attendees reported varying views of the potential revealed for the super by these calculations, Teller wrote the final report of the meeting in his usual optimistic fashion.

Interdisciplinary use of computers

Since the IBM electromechanical computers at Los Alamos were collocated with other tools of nuclear physics: accelerators, reactors and detectors, it was a propitious place for a synergistic interaction between the more fundamental aspects of nuclear physics and calculation. The interdisciplinary use of computers at Los Alamos to solve weapons related problems developed techniques useful in nuclear physics studies. Indeed, Fermi calculated a numerical formula for atomic masses that he had derived at Los Alamos soon after the atomic bombs were dropped on Japan, apparently recognizing the advantages of electromechanical over analogue and manual computation. Fermi sought to translate that potential to the University of Chicago after World War II by hiring Nick Metropolis to develop a computer for the Institute of Nuclear Studies – now the Enrico Fermi Institute for Nuclear Studies – which he agreed to head after leaving Los Alamos.

Nor was he alone in attempting to beat computational swords into academic plowshares. John von Neumann, who had worked on both the atomic bomb and the ENIAC, and who suggested the 1945 calculation to which I have referred, sought military funds to build a computer of his own design at the Institute for Advanced Study in Princeton in 1946. When military funding for von Neumann's IAS machine faltered, the AEC picked up the tab, in large part because von Neumann had become an important asset to the Los Alamos weapons design program.

In the meantime, Los Alamos physicists continued to elaborate machine calculation. One example may serve to illustrate this. The Monte Carlo technique was developed to study neutron diffusion in critical assemblies at Los Alamos by von Neumann, Stan Ulam, and Nicolas Metropolis. It rapidly spread to other applications in nuclear weapons design and statistical calculations. Ulam, like Metropolis, left Los Alamos after the war to pursue an academic career, but both men returned as consultants and, ultimately, as staff members. Von Neumann was at Los Alamos every summer as a consultant. The shortage of theoretical physicists in the postwar era required the laboratory to make extensive use of such agreements, and put a premium on lightening the burden of the thought process whenever possible.

The invention of Monte Carlo techniques resulted from the interactions of Metropolis, von Neumann and Ulam, who colonized fields of physics research in their crusade to popularize their techniques. While statisticians found little novel in them, and most other mathematicians did not have access to computers, Monte Carlo techniques were useful in a number of applications of interest to physicists including meteorology, turbulent behavior in fluids, and Newtonian mechanics.[2] The development of numerical hydrodynamics and weather forecasting owed not a little to their efforts.

The FERMIAC

Fermi developed the first "Monte Carlo" computer, the FERMIAC, to model the processes of fission, scattering, and absorption of neutrons in a spherical assembly composed of several materials. Since each of these processes had measurable cross sections, but competed, statistical weights had to be calculated for each interaction, and then applied to the operation of the device.

Monte Carlo techniques readily lent themselves to electro-mechanical and electronic calculation, and Los Alamos mathematicians and physicists developed a series of codes to perform them. These were sufficiently large to warrant being named after large mammals and their offspring, e.g. Hippo and Baby Hippo. Like many modern software programs, these codes were kluges and required years to write. Since the electronic computers capable of running

[2] Von Neumann (1946).

them weren't built yet, this was less of a problem than it might have been.

In the meantime, Fermi led a new initiative at the University of Chicago to develop a different kind of numerical assault on the nucleus intended to reveal not so much how the nucleus split apart or fused with other nuclei, but rather how it was held together and interacted with mesons. As the famous experiments of Marcello Conversi, Ettore Pancini and Oreste Piccioni here in Rome had shown, positive and negative mesons were absorbed in different fashion by carbon and iron, and this in turn led to the discovery of the pi-meson as well as the elaboration of a theoretical explication of it by Bethe and Marshak.[3] This was a problem that fired Fermi's interests at Chicago and led him into high-energy physics. The Office of Naval Research [ONR] provided the opportunity for Fermi to realize some, but not all, of his dreams at Chicago. As Herbert Anderson, Fermi's close associate in this period, recalled, the Director of ONR traveled to Chicago to see Fermi and asked him "Look, Fermi, isn't there something you would like to do? I'd like to get you the money for it."

After discussing the matter with Fermi and Teller, Anderson offered to build anything Fermi wanted, a computer, a cyclotron, or a betatron. Teller recommended a computer, but Fermi preferred a synchrocyclotron, larger than Berkeley's, which Anderson and other members of the Institute for Nuclear Studies began to build.[4] The high-energy proton synchrocyclotron at Berkeley inspired the Institute for Nuclear Studies, among others, to copy the new technique of frequency modulation first applied in the 184-inch cyclotron. The Chicago machine, whose energy was greater than that of the Berkeley machine, would, he hoped, be capable of extending knowledge into new energy regions and producing pions.

The University of Chicago, the Office of Naval Research and the Atomic Energy Commission paid for the construction of the accelerator under Fermi's supervision as part of a multifaceted program of nuclear physics research.[5] The decision to forego a computer, however, meant that

---

[3] DONALD H. PERKINS, "Cosmic-ray-work with emulsions, 1940s to 1950s, in HODDESON, ET AL., *Pions to Quarks* (Cambridge, 1989), 90-1. Fermi's compatriot, Bruno Pontecorvo, initiated a similar research program at Dubna. See BRUNO M. PONTECORVO, *Establishment of the weak-interaction notion*, loc. cit., p. 369-372.

[4] HERBERT L. ANDERSON, "The Cyclotrons in my Life," *Los Alamos National Laboratory Report* LALP 88-15 (August, 1989).

[5] "Research in Atomic Structure and Energy in the Institute of Nuclear Studies, the Institute of Metals and the Institute of Radiobiology and Biophysics", The University of Chicago. Fund Raising Brochure dated June 15, 1946.

Metropolis spent most of the next two years working at Princeton with John von Neumann, where he participated in the design of the computer at the Institute for Advanced Study and the modification of the ENIAC for stored-program operation.

The connection between Chicago and Los Alamos were also strengthened by Edward Teller who, like Fermi and Metropolis, spent summers at the New Mexico laboratory working on the problem of the Super bomb. Other Chicagoans with a *pied-a-terre* at Los Alamos included Harold Agnew, Herbert Anderson, and Anthony Turkevich. Besides designing the analog computer named after him, Fermi took an interest in the development of the Monte Carlo technique, a form of which he had used in the 1930s. During his summers at Los Alamos as a consultant, he was constantly in the company of Ulam, and, after his return to the laboratory to build a computer in 1948, Metropolis. Metropolis, disappointed by the slow pace of computer development at the University of Chicago, readily accepted Los Alamos's invitation to build a von-Neumann machine there.

When the team of Eugene Gardner and Cesar Lattes succeeded in manufacturing the first pions in the 184-inch in 1948, Fermi began to plan experiments using them with the Chicago synchrocyclotron.

The nature of the mesons which carried the nuclear force was an important focus of theoretical and experimental physics in the postwar era, as Fermi pointed out a significant shortcoming of theory in *Elementary Particles*, his Silliman Lectures at Yale in 1950.

A great deal of work has been devoted to the field theory of mesons first proposed by Yukawa in his attempt to explain nuclear forces. The meson of Yukawa should be identified with the π-meson of Powell (briefly called here pion). The μ-meson of Powell (called here muon) is instead a disintegration product of the pion, only weakly linked to the nucleons and therefore of little importance in the explanation of nuclear forces. The Yukawa theory has proved a very valuable guide in experimental research and probably contains many correct leads to a future theory. In particular it is partly responsible for the discovery of the production of mesons in the collision of fast nucleons. On the other hand, the attempts to put this theory in a quantitative form have had very mediocre success. Often a ponderous mathematical apparatus is used in deriving results that are no better than could be obtained by a sketchy computation of orders of magnitude. This unsatisfactory situation will perhaps improve only when more experimental information becomes available to point the way to a correct understanding. The purpose of this discussion is not to attempt a mathematical treatment of the field theories but rather to exemplify semi-quantitative procedures that are simple and may be helpful in the interpretation of experiments. There are several cases in which not much would be gained by a more elaborate mathematical treatment since a convincing treatment has not yet been discovered. In other cases the qualitative discussion presented here may serve as an introduction to more complete elaborations of the subject.[6]

The desire for such a semi-quantitative understanding of pion-proton scattering led Fermi to devise a heuristic theory based upon the available phase space for interactions between pions and the s- and p- shells of protons.

This involved several simplifying assumptions, including the one that the s and p but not d-waves, were involved in the interaction. Although this produced orders of magnitude as an heuristic guide to such reactions, according to Herbert Anderson, I.I. Rabi believed that if such statistical methods worked, there was nothing new to learn in high energy physics. Fermi's oversimplified theory served as a standard against which to measure experimental results of multiple production of mesons and reveal non-statistical processes.[7]

The MANIAC

The phase-shift analysis, however, required the solution of nine equations. Preliminary attempts to do so manually had convinced him that the computer could accelerate the solution. By the use of the least squares method, equations governing the statistical analysis of phase shift-angles of scattering of pions of s- and p- waves could be approximated. Fermi brought the problem to Los Alamos in the summer of 1952 where he ran it on Metropolis's recently completed machine, the MANIAC.

In order to run the calculation, Fermi converted it into hexagesimal computer code.

He also programmed the machine himself.

Fermi returned to the University of Chicago in the fall, and presented a series of lectures advertising numerical techniques using digital computers at Chicago and Argonne. The presentation of the results of MANIAC's calcu-

[6] ENRICO FERMI, *Elementary Particles*, Yale University Silliman Memorial Lectures, 1950 (New Haven: Yale University Press, 1951) p. 3. See ch. 4, esp. pp. 79-84, for Fermi's invocation of statistical methods.

lations at the Rochester Conference at the end of the year, however, did far more to establish the computer as a participant in the work. As Fermi put it:

> "With the use of an electronic computer the phase shifts can be computed in five minutes, since there is one code for all calculations. With each calculation only taking about five minutes, one can learn something of the mathematics of the problem by varying the conditions a little... the phase shifts are then used to calculate the cross section. The results invariably want the cross section to look as they do experimentally. In this calculation on the S and P phase shifts are used".

Dramatically, Fermi then presented the new data for which the conference had been waiting, first remarking that the courier when he got here handed him a small piece of paper on which there were written, as is proper for something that comes from Los Alamos, certain numbers which then had to be decoded. In this case, they converted from binary to decimal notation... using the program.

Fermi noted that the calculated cross section represented the observed cross section very well: "The phase shifts have no business to represent the observations so well. That is, for the nine measurements this set is inconsistent statistically with the errors given. The most striking difference from the previous results is in ...phase shifts.... of...angles [extremely sensitive] to a change in cross section [that] varied all over the map".[8]

The phase shift for the pion-nucleon interactions identified the first "resonance," presaging a flood of short-lived "particles" over "a period of many years when the newest sets of phase shifts were reported at nearly every conference on particle physics". Although other solutions fit the data as well as Fermi's, the Chicago group admitted that their data did not extend far enough in energy to support the resonance hypothesis.' By 1953, Fermi still argued that the experimental facts might be explained in some other way.[9]

Fermi's tentative interpretation of the numerical and experimental results

---

[7] FERMI, *Coll. Pap.*, II, 780. FERMI, "High Energy Nuclear Events," *Progress in Theoretical Physics 5* (1950) 570-583. Fermi compared the predictions of this technique to cosmic-ray results in order to ascertain its validity. FERMI, "Angular Distribution of the Pions Produced in High Energy Nuclear Collisions," PR 81 (1951) 683-687.

[8] FERMI, "Report on Pion Scattering," Proceedings of the Third Annual Rochester Conference (Dec. 18-20, 1952), pp.859-60.

[9] ROBERT L. WALKER, "Learning about nuclear resonances with pion photoproduction," in LAURIE M. BROWN, MAX DRESDEN, and LILLIAN HODDESON, eds., *Pions to Quarks: particle physics in the 1950s: based on a Fermilab Symposium* (Cambridge University Press, 1989) 117-118.

of his pion-proton experiments did not diminish the interest awakened in the new computational techniques he announced. In addition, Fermi posed the feasibility of automatically scanning and measuring, as well as analyzing, nuclear particle tracks in photographic emulsions in the summer of 1952.

Computational physics

When Luis Alvarez applied computers to the problem of data analysis of bubble chamber film in the late 1950s, he hired two of Fermi's students, Arthur Rosenfeld and Frank Solmitz, to write the computer programs for this work.

Another of Fermi's students, Darragh Nagel, built one of the first computer designed and controlled proton accelerators, the Los Alamos Meson Physics Facility. Nicolas Metropolis returned to Chicago after Fermi's death to found a computer research institute and build MANIAC III. Metropolis and his associates completed the work on phase shift analysis that Fermi had begun at Chicago. The Institute for Computer Studies and the Argonne National Laboratory, which built the AVIDAC, the ORACLE and GEORGE computers in the 1950s, also carried forward the work which Fermi pioneered in Chicago. Indeed, Fermi initiated the project when he applied to the AEC and ONR for funds to build a computer at the University of Chicago in 1954.[10]

Edward Teller was also very influential in promoting the use of computers in the Institute for Nuclear Studies, although, as in so many of his enthusiasms, he sought to go too far and too fast to suit Fermi's taste. He found an equally enthusiastic colleague in Ernest Lawrence after he went to the University of California Radiation Laboratory where he was responsible for the introduction of computers for weapons design and other purposes at the Radiation Laboratory's Livermore branch.[11] There, under the direction of Sydney Fernbach and Berni Alder, computational physics became, in the

[10] See Fermi's "Preliminary Proposal for High Speed Electronic Computer" to George A. Kolstad, Director of Physics Research section of the Research Division of AEC, dated April 16, 1954 and notes and correspondence with Kolstad in May 1954, including a telephone conversation from Kolstad to Fermi in which he stated the research division would recommend acceptance and recommending final proposal "stress particularly the educational value….how far we want to copy the Oracle and …major new components that we would plan to install….space that could be allotted to the machine, including air conditioning and other specifications and [what personnel and what kind of new personnel as well [sic] as a tentative time schedule". Fermi Papers XI:5, University of Chicago.

[11] Cf. Inter alia, McKenzie, Seidel (1997).

1960s, a major enterprise, although the most visible and fruitful applications in high energy physics were made at the main branch of the Laboratory in Berkeley by Alvarez, who also enjoyed Lawrence's enthusiastic support in adapting the computer. Von Neumann, whose logical design of these computers long survived both his IAS machine and him, left a comparable institutional legacy at Princeton in the Matterhorn project, which applied computational techniques developed in support of hydrogen bomb development to the problem of controlled nuclear fusion.

As in so many other areas, Enrico Fermi blazed a trail in the use of the computer as a scientific instrument. The pioneering activity in high-energy physics at Chicago has often been overshadowed by activity at the University of California Radiation Laboratory and Brookhaven National Laboratory in the years following the completion of the Cosmotron in 1952 and the Bevatron in 1954, as well as by the development of the hydrogen bomb, another application of physics born of Fermi's ideas. I have tried to show how, despite his continuing involvement in the application of nuclear physics, Fermi left another legacy in high-energy physics, and provided the ideas, inspiration, and individuals who used the computer as they settled the high-energy frontier.

Robert W. Seidel
He is Professor of the History of Science and Technology at the University of Minnesota, Minneapolis. He has served as Director of Charles Babbage Institute for the History of Information Processing and as the administrator of Bradbury Science Museum at Los Alamos National Laboratory. He is coauthor (with John Heilbron) of Lawrence and his Laboratory, volume I of *A History of the Lawrence Berkeley Laboratory* (Berkeley: University of California Press, 1990).

Nina Byers

Women in Physics in Fermi's Time

In the first half of the 20th century, Fermi's time, women breached barriers to higher education and became major players in physics. There were many in addition to Marie Curie who made original and important contributions to physics such as Emmy Noether, Marietta Blau, Irene Joliot-Curie, Lise Meitner, Maria Goeppert Mayer and others less well known. This talk will be about these women, their struggles to work in the field, and some of their important contributions.

Le donne nella fisica al tempo di Fermi

Nella prima metà del ventesimo secolo, al tempo di Fermi, le donne ruppero le barriere che circondavano l'educazione superiore e divennero figure di rilievo nel campo della fisica. Molte donne oltre a Marie Curie contribuirono in modo originale e rilevante alla fisica, come ad esempio Emmy Noether, Marietta Blau, Irene Joliot-Curie, Lise Meitner, Maria Goeppert Mayer e molte altre meno note. Qui si parlerà di queste donne, della loro lotta per occupare una posizione in questo settore e di alcuni dei loro rilevanti contributi alla fisica.

Introduction

Probably most people, particularly in English speaking countries, would think of one, two, or at most three women in physics in Fermi's time. But in fact there are many who deserve to be recognized as having made important discoveries, contributions of lasting value to physics. At UCLA we have collected information about twentieth century women who have made original and important contributions to physics before 1976, and find there are at least eighty-six examples. Brief biographies outlining the scientific lives and major contributions of these women can be found at the website *http://www.physics.ucla.edu/~cwp*. In this paper, we give some outstanding examples.

Before launching the subject, which is women in physics in Fermi's time, a few brief historical remarks are in order. Before the proliferation of printing presses in the nineteenth century books on science were relatively inaccessible to women and there were very few female physicists. Women did not have opportunities to study and work in institutions of higher learning. There were exceptions among aristocratic women; for example, Émilie du Châtelet (1706-1749) who translated Newton's Principia into French and bested Voltaire in a physics competition.

Just as the Renaissance came to Italy in advance of other countries of Europe, so Italy was more advanced in the entrance of women into institutions of higher learning. Laura Bassi (1711-1778) was a professor of physics in University of Bologna studying electrical phenomena along with her husband. She was perhaps the first woman to be professor of physics in a European university.

The landscape as regards there being female physicists in Europe and America radically changed toward the end of the nineteenth century. Marie Curie (1867-1934) published her first important paper in 1898. It was on a systematic study of the uranic rays Becquerel had discovered. She found that similar radiation emanated from atoms other than uranium. Consequently in a following paper, authored with her husband Pierre, the word radioactivity was introduced to refer generically to what had previously been referred to as the emanation of uranic rays [1]. Pierre was a professor in the Sorbonne where Marie was a doctoral student. Before joining with her to study Becquerel's uranic rays, he had been studying in his laboratory magnetic properties of substances.

In addition to Marie Curie, there were other women physicists whose work was at the cutting edge of physics at that time. There were several whose discoveries are of lasting value. One is Agnes Pockels (1862-1935), a German woman whose studies initiated the field of surface physics. Since her experimental work was highly original and in a new field of investigation, she failed to get it recognized in her own country. She wrote about it to Lord Rayleigh, who began to publish his own studies in surface physics more than ten years after she began hers. Rayleigh was so impressed with her experimental methods and results that he had her letter translated from the German and published it in *Nature* [2]. He wrote a brief introduction which reads in part:

> "I shall be obliged if you can find space for the accompanying translation of an interesting letter which I have received from a German lady, who with very homely appliances has arrived at valuable results respecting the behaviour of contaminated water surfaces. ... I hope soon to find opportunity for repeating some of Miss Pockels' experiments".

Pockels' studies of surface tension were forerunners of the Nobel Prize winning work of Irving Langmuir.

Another female physicist who worked before Fermi's time was Kristine Bjerrum Meyer (1861-1941) who won the 1899 Gold Medal of the Danish Academy of Science and Letters for her paper [3] on an equation of state for liquids similar to that of van der Waals for gases.

Finally I'd like to mention Henrietta Leavitt (1868-1921) whose discovery of the period-luminosity relation in light from Cephid variable stars in the Magellanic Clouds led to, and still enables, determination of intergalactic distances.

Women in Physics in Fermi's Time

I take Fermi's Time to be from 1922, when he took his doctorate in physics in Pisa, up to his premature death in 1954 and have chosen to discuss some women who have made important contributions to fields of physics in which he engaged himself. They are presented chronologically, following the timeline of Fermi's life in physics; viz.,

1922-1924: Fermi in Leiden and Göttingen
1. Tatiana Ehrenfest-Afanaseva {statistical mechanics}
2. Emmy Noether {general theory of relativity}

1925-1927: Fermi in Florence
3. Marietta Blau {photographic method of measuring particle tracks}

1928-1938: Fermi in Rome
4. Irène Joliot-Curie {artificial radioactivity}
5. Ida Noddack {suggested chemical analysis might reveal uranium fission}
6. Lise Meitner {nuclear fission}

1939-1954: Fermi in United States
7. Leona Woods Marshall Libby {work with Fermi in Los Alamos and Chicago}
8. Maria Goeppert Mayer {shell model of the nucleus}.

The sections in this paper on Emmy Noether and Marietta Blau are much longer than the others in consideration of the fact that they contributed so importantly to progress in physics in the twentieth century and relatively little is known about them, generally speaking. Relatively little is also known about the first woman on the list, Tatiana Ehrenfest-Afanaseva although she was co-author of a classic treatise on the foundations of statistical mechanics with her husband Paul Ehrenfest. She studied physics before her marriage and the marriage may have set an example to Fermi who later married the young physics student, Laura Capon.

Laura however discontinued her physics studies after she married [4].

1922-1924

In 1922 Fermi received his doctorate from the University of Pisa and took up a two year fellowship which he spent in Göttingen and Leiden. He was a young man and went abroad to see the world and become acquainted with foreign physicists. He did theoretical physics in this period writing papers mainly on general relativity and also on statistical mechanics and thermodynamics. Before he left Pisa in 1922, he had obtained the remarkable result in the general theory of relativity that space is Euclidean in the neighborhood (infinitesimal) of a worldline [5]. While in Göttingen he may have met Emmy Noether who was then a well known mathematician and had found important results in the general theory. In 1918 she published a very important paper for physics which solved a big problem, the problem of energy conservation in the general theory of relativity. In Leiden he most likely met Tatiana Ehrenfest-Afanaseva, the wife of Paul Ehrenfest who became a lifelong friend. Sadly Ehrenfest died in Leiden in 1933.

1. Tatiana Ehrenfest-Afanaseva

Tatiana Afanaseva (1876-1964) lived in St. Petersburg before she married Paul Ehrenfest. In Russia, at that time, women were not admitted to universities. There were, however, special university-level institutions that allowed women to take courses in engineering, medicine, and teaching. She attended a women's pedagogical school and the Women's Curriculum which shadowed the imperial university. Martin Klein, in his biography of Paul Ehrenfest [6], wrote the following about her:

> "Paul Ehrenfest was not the kind of thinker who develops his ideas slowly in the solitude of his study. He had to talk about them, to work them out by discussing and arguing them with a critical and competent colleague, and Tatyana was willing and able to play this role. Her quick and extraordinarily logical mind was a natural foil for his more inventive one, and her urge to probe to the very bottom of an idea was as deep as his own".

Together they wrote the classic treatise on the foundations of statistical mechanics and statistical thermodynamics [7] which was important in the development of those fields. Fermi wrote papers on the ergodic hypothesis while in Leiden that Segrè characterizes as subtle papers. This hypothesis was a central concern of Paul and Tatiana Ehrenfest. Perhaps Tatiana and Paul Ehrenfest themselves personally as well were instrumental in the development of Fermi's thinking which led him, after the discovery of quantum mechanics and Pauli's exclusion principle, to the discovery of quantum statistics for identical particles which obey the exclusion principle. This is known as Fermi-Dirac statistics, having been independently discovered by P.A.M. Dirac.

2. Emmy Noether (1882-1935)

During his fellowship in Göttingen, Fermi may have attended lectures of Emmy Noether, a mathematician whose work profoundly influenced twentieth century physics. She was a member of the group David Hilbert and Felix Klein had assembled at the University. Following Hilbert and Klein, she took an interest in mathematical physics, particularly in the general theory of relativity. Shortly after her arrival in Göttingen in the summer of 1915, Albert Einstein came and gave a series of lectures on the general theory which was then not yet complete. Almost simultaneously with Einstein's completion of the theory in November (he had been working on it for eight years), Hilbert discovered a Lagrangian formulation which solved the problem with the theory that Einstein was confronting earlier in the year. With this problem solved they had a consistent field theory, but there was the vexing physical problem with the theory that it didn't seem to have an energy conservation law such as is found in classical field theories. Hilbert asked Emmy Noether to look into this problem and she solved it with the discovery of theorems which, collectively, physicists call Noether's Theorem [8]. This relates symmetries and conservation laws. Their importance for our understanding of basic physics cannot be overstated. For example, Feza Gürsey wrote [9]:

> "Before Noether's Theorem the principle of conservation of energy was shrouded in mystery, leading to the obscure physical systems of Mach and Ostwald. Noether's simple and profound mathematical formulation did much to demystify physics".

The Theorem to which he refers consists of two theorems, which she called theorem I and theorem II. She proved them and their converses in a landmark paper read to the *Königl. Gesellschaft der Wissenschaften zu Göttingen* (Royal Society of Sciences of Göttingen) by Felix Klein in 1918. Presumably Klein presented it because Noether was not a member of the Society; it seems likely she wasn't even there when the paper was read. Records of the Society were lost in the Second World War and we do not know when women were first admitted; counterpart societies in London and Paris did not admit women until after World War II. The Royal Society (London) elected its first female member in 1945 and the Académie des Sciences of Paris in 1962; both were established in the seventeenth century.

Though the general theory of relativity was not in the main line of her research, she wrote several papers on the theory. In a letter to Hilbert, Einstein expressed his appreciation of her work and wrote:

> "Yesterday I received from Miss Noether a very interesting paper on invariant forms. I am impressed that one can comprehend these matters from so general a viewpoint. It would not have done the Old Guard at Göttingen any harm had they picked up a thing or two from her. She certainly knows what she is doing".

Emmy Noether's main line of research was the development of modern algebra. She had returned to writing papers and lecturing on this subject when Fermi was in Göttingen. Nathan Jacobson wrote about her achievements in this period [10]:

> "Abstract algebra can be dated from the publication of two papers by Noether, the first a joint paper with Schmeidler and a truly monumental

> work *Idealtheorie in Ringbereichen* [which] belongs to one of the mainstreams of abstract algebra, commutative ring theory, and may be regarded as the first paper in this vast subject ..."

Historians of mathematics see the creation of modern abstract algebra in the years 1921-1933 in the work of Emmy Noether, Emil Artin and their school [11]. Prominent mathematicians came from all over Germany and abroad to consult with Noether and attend her lectures. Though Fermi went to Göttingen to study physics with Max Born, and Noether was in the mathematics department, it seems reasonable to conjecture that Fermi attended some of her lectures.

It is remarkable that Noether was never appointed to a paid position in the faculty of the University of Göttingen. Her biographer Auguste Dick wrote [12]: "Was it because she was Jewish? There were several Jewish Ordinarii in Göttingen. Was it because she was a member of the Social Democratic Party? Or was it her firm stance as a pacifist that was frowned upon?". Today we might also ask what role did gender discrimination play? For more than a decade after receiving her Ph.D. in 1908 from the University of Erlangen, Emmy Noether worked unpaid in Erlangen and Göttingen teaching and doing mathematical research. During this period she published fifteen papers in important mathematical journals, became a member of the prestigious Circolo Matematico di Palermo and the Deutsche Mathematiker Vereinigung (DMV) [German Association of Mathematicians], and gave two lectures to the DMV.

At the invitation of David Hilbert and Felix Klein, in 1915 she joined their group in Göttingen but was refused appointment as lecturer *(Privatdocent)* by the University Senate. The stated reason the University refused to appoint her was because she was a woman. This so enraged Hilbert that he stormed out of a Senate meeting saying "I do not see that the sex of a candidate is an argument against her admission as *Privatdocent*. After all, we are a University not a bathing establishment!". After WWI more liberal attitudes prevailed and in 1919 the University granted Noether *Habilitation*. This enabled her to give University lectures and be paid for them. Before that her lectures had been announced as those of Professor David Hilbert with the assistance of Dr. E. Noether.

After March 1933 Emmy Noether was not allowed to lecture in the University. Jews were not allowed to teach in the University after Hitler came to power. In 1934 women, Jewish or not, were dismissed from their University posts in accordance with the Nazi policy of Kirche, Kinder, Küche for women. Later when there was a manpower shortage in Germany women were allowed back in the workforce. Hermann Weyl wrote about Noether in this period [13]:

> "A stormy time of struggle like this one we spent in Göttingen in the summer of 1933 draws people closely together; thus I have a vivid recollection of these months. Emmy Noether – her courage, her frankness, her unconcern about her own fate, her conciliatory spirit – was in the midst of all the hatred and meaness, despair and sorrow surrounding us, a moral solace".

She soon emigrated. She had only two job offers. One was from Somerville College, Oxford which was able to offer her room and board and a small stipend, and the other from a women's college in Pennsylvania, Bryn Mawr College, where she was offered a visiting professorship paid in part by the Rockefeller Foundation which was subsidizing jobs for German refugee scientists. She accepted the Bryn Mawr position and in addition weekly went by train to the Institute for Advanced Studies in Princeton to give an invited course of lectures [14].

Again gender discrimination worked against her because undoubtedly otherwise, on the basis of her eminence as a mathematician, she would have been given a paid position in the Institute in Princeton. Emmy Noether's untimely death in 1934, owing to a post-operative infection, led Einstein to write a memorial Letter to the Editor of the New York Times which reads in part:

> "In the realm of algebra, in which the most gifted mathematicians have been busy for centuries, she discovered methods which have proved of enormous importance. Pure mathematics is, in its way, the poetry of logical ideas. In this effort toward logical beauty, spiritual formulas are discovered necessary for deeper penetration into the laws of nature. ...
> There is, fortunately, a minority [of people] who recognize early in their lives that the most beautiful and satisfying experiences open to humankind are not derived from the outside, but are bound up with the development of the individual's own feeling, thinking and acting. The genuine artists, investigators and thinkers have always been persons of this kind. However inconspicuously the life of these individuals runs its course, none the less the fruits of their endeavors are the most valuable contributions which one generation can make to its successors". [15]

1925-1927

In this period, Fermi made one of his most important contributions to physics. Following the discovery of quantum statistical mechanics for photons by Bose, extended to ordinary molecules by Einstein, and the Pauli exclusion principle, he developed quantum statistical mechanics for identical particles that obey the Pauli principle [16]. In these years, in Vienna, Marietta Blau discovered that photographic emulsions could be used to study particle tracks. Years later, in the founding days of particle physics when he studied pion-nucleon interactions in Chicago, Fermi made very extensive use of this method.

3. Marietta Blau (1894-1974)

Marietta Blau, working in the Radium Institute at the University of Vienna, began the development of what later became known as nuclear emulsions, a photographic method of detecting particle tracks which Fermi employed to great advantage in the post WWII period in Chicago. She was the first physicist to show that proton tracks could be separated from alpha-particle tracks in emulsion. Indeed the final paragraph of her ground breaking 1925 paper [17] concludes: "The method of photographic detection of H-particles [protons] was developed ... to obtain pictures of H-particles, which struck the photographic plate with parallel incidence. As with the alpha particles, the results were series of points clearly defining a direction, where each series corresponds to the track of an H-particle. With the aid of absorption experiments and of comparison experiments where the H-radiation source of paraffin was replaced by a layer of soot with an equivalent content of carbon, it was shown that the photographic material blackening can only be explained by the effect of H-particles and not by possibly existing radiation".

From 1923 until 1938 she worked unpaid in the Institute in Vienna. The head of the Institute provided funds for her experimental work but no salary. When applying for a regular paid position she was told her chances were slim because she had two strikes against her – she was a Jew and a woman. Fortunately she had financial support from her family, prominent publishers of sheet music in Vienna. When the Nazi Anschluss occured she happened to be working in the Curie Institute in Paris and, being Jewish, was not able to return to work in Vienna.

While at the Institute in Vienna she worked with the British firm Ilford to develop stable, thick photographic emulsions which would provide for better measurements of particle tracks. She was remarkably successful. This work was precursor to the further developments by Nobel Laureate C.F. Powell; see below. Another woman, Hertha Wambacher, came to work with her in the Institute. They prepared and exposed photographic plates at high altitudes and observed nuclear disintegrations caused by cosmic rays. These were called Blau-Wambacher stars. In 1932, just after Chadwick discovered the neutron, she showed that neutrons can be detected by observing recoil protons in nuclear emulsions [18]. This was, and still is, a preferred way of detecting neutrons.

After Hitler took over Austria in 1938, she became a Nazi refugee and her life was difficult. She went back to Vienna to bring her mother out and, with a recommendation of Albert Einstein, became professor in the Technical University of Mexico City. But the conditions of work there for an experimental physicist were very poor. She left for the United States in 1944 with a job in the International Rare Metals Refinery in New York. After the war, she pioneered development of photomultiplier tubes for particle physics detectors and in Brookhaven National Laboratory carried out early studies of multipion productions by pions [19]. She had research associate appointments at Columbia University and then at Brookhaven National Laboratory, and in 1955 an associate professorship at the University of Miami. None of these American appointments were suitable for a physicist of her ability and accomplishment. And from a practical point of view, they did not provide retirement benefits or health insurance. In 1960, in declining health, Marietta Blau returned to Vienna.

C.F. Powell is usually credited with the development of the photographic method of studying particle tracks. Indeed his 1950 Nobel citation reads "for his development of the photographic method of studying nuclear processes and his discoveries regarding mesons made with this method" and the biographical notes read in part [20]:

> "In 1938, he undertook experiments in cosmic radiation and employed methods of directly recording the tracks of the particles in photographic emulsions and employed similar methods for determining the energy of neutrons, that is, by observing the tracks of the recoiling protons. The length of the track of a charged particle in the emulsion was found to give an accurate measure of its range and the great advantages of this method for experiments in nuclear physics were soon clearly established".

Indeed he did much to develop the method but Marietta Blau is key to the story of how he came to use it. This is the story as told by A.M. Tyndall for whom Powell was working in Bristol University at that time. In 1938 Walter Heitler, also a Nazi refugee, came to Bristol and asked Tyndall for funds to expose photographic plates to cosmic rays at high altitudes telling him "there was this method of studying particle tracks developed by two women in Vienna which was so simple even a theorist could do it". Tyndall found the funds and Heitler exposed a stack of plates on a mountain site.

Powell had come to work on a Cockcroft generator that was not yet working and joined with Heitler in the study of cosmic rays. He evidently found that this Blau-Wambacher photographic method had promise and the rest is history. There is a good problem here for historians to document and explain why after the war Marietta Blau was not appointed to a position appropriate to her talent and accomplishments. She was well known in Europe before the war, having been nominated several times for the Nobel Prize and having won (with Hertha Wambacher) the prestigous Lieben Prize of the Viennese Academy of Science.

Some Bristol physicists recollect that she was overlooked after the war because of her association with Hertha Wambacher, who had been a Nazi. The irony of this is that Blau was a Jew who had to flee the Nazis in 1938. Even before the Anschluss, Austria had a strong Nazi Party (though illegal sometimes) and there were influential Nazi Party members in scientific as well as other institutions. It seems likely that Blau found it necessary to maintain association with Wambacher in those years in order to continue her work. And then, of course, there is the question of gender discrimination. Speaking about Blau's inferior employment status in Brookhaven National Lab in the early fifties, Maurice Goldhaber remarked "Women were not treated very well in those days".

1928-1938

Fermi built up a premier school of physics in Rome. Many young men who later were very famous and accomplished physicists came to study and work with him. There he characteristically worked both in experimental and theoretical physics. He carried on experimental studies in nuclear physics and gave us the Fermi Theory of Weak Interactions. After Irène and Frédéric Joliot-Curie's discovery of artificial radioactivity and Chadwick's discovery of the neutron, he systematically studied the radioactivity produced by neutron bombardment of stable isotopes. He became expert in neutron and nuclear physics. But he had the misfortune of overlooking the phenomenon of neutron induced fission. This despite the paper of a chemist, Ida Noddack, who suggested that the radioactivity he observed resulting from neutron collisions with uranium might be evidence for such an effect. In this period, two women, Irène Joliot-Curie and Lise Meitner, who did discover nuclear fission with the help of chemists O. Hahn and F. Strassmann, made major contributions to physics. Time and space limitations require them to be discussed more briefly than Blau and Noether. There are very good references where more information about them can be found. Here I will give some discussion about Ida Noddack, though she has been discussed already by several speakers in this Conference.

4. Irène Joliot-Curie (1897-1956)

For a brief summary of the scientific life and accomplishment of Irène Joliot-Curie, one could do no better than to read from the obituary by James Chadwick in *Nature*, 177, 964 (1956):

> "Irène Joliot-Curie was born in the stirring days of radioactivity when her parents [Marie and Pierre Curie] were making great discoveries, she grew up with radioactivity, and all her life was devoted to its study. In 1926 she married Frédéric Joliot and there began a collaboration of husband and wife in scientific work rivaling in productive genius even that of her parents. The most outstanding of their joint papers were published in the years 1932-1934. In the first of these, on the radiation excited in beryllium by alpha-particles, they reported a very strange effect which provided the clue to the discovery of the neutron.
>
> Then, after studying the conditions of excitation of neutrons by the impact of alpha-particles on various elements, they turned for a time to the "materialization" of positive electrons through the action of gamma-rays of high energy. This was followed by a systematic study of the radiations emitted from the lighter chemical elements under the impact of alpha-particles, which through the light of intuition – and good technique – led them, in early 1934, to their beautiful discovery of artificial radioactivity. An interesting feature of this discovery is that it was so long in coming; for the phenomenon of artificial activity had been expected, and sought for, since the earliest days of radioactivity. For this discovery the Joliot-Curies were awarded the Nobel Prize for Chemistry in 1935.
>
> About two years later with P. Savic, she examined in detail the artificial radioelements produced by the irradiation of uranium by slow neutrons, analysing the products and identifying them chemically, and she came within a hair's-breadth of recognizing that the phenomenon involved in the production of these elements was that of fission".

Chadwick himself won a Nobel Prize for his discovery of the neutron. He very generously remarks here that a paper of F. and I. Joliot-Curie provided a clue to his great discovery. After the announcement of his discovery they made one of the first determinations of the neutron mass and concluded his particle would be unstable and decay to proton and electron.

As regards gender discrimination, Irène Joliot-Curie's professional life was far less burdened by it than her predecessors. This must have been due in part to the fact that her mother, as well as her father and her husband, were active feminists and committed to the cause of social justice. (During WWI, with her mother she ran mobile X-ray machines which traveled from camp to camp diagnosing soldiers' wounds). Some of the positions she held were: 1918-46 Assistant to Marie Curie, Radium Institute; 1936 Undersecretary of State for Scientific Research, Léon Blum's Popular Front Government (4 months); 1946-56 Director, Radium Institute; 1946-50 Director, French Atomic Energy Commission; 1937-56 Professor, Sorbonne.

5. Ida Tacke Noddack (1896-1979)

Ida Tacke Noddack was a distinguished chemist with many publications in scientific journals, chemical journals in particular. In 1925, she discovered the missing element 75 with W. Noddack and O. Berg. They named it rhenium after the Rhine River.

In a now famous paper, "Über das Element 93", *Zeitschrift für Angewandte Chemie* 47: 653 (1934), she suggested that the radioactivity which Fermi observed resulting from neutron bombardment of uranium, which he proposed might be evidence for production of the transuranic element 93, might instead be caused by disintegration of the uranium nucleus into several heavy fragments – a process now known as fission. She suggested this could be determined by chemical analysis, and indeed chemical analyses by radiochemist Otto Hahn with Fritz Strassmann and Lise Meitner, much to their surprise, confirmed her suggestion in 1939. E. Segrè, Fermi's coworker in Rome, wrote that "the possibility of fission, however, escaped us although it was called specifically to our attention by Ida Noddack. The reason for our blindness is not clear".

In the paper which reported the discovery of rhenium, the authors also reported evidence for another missing element, the element 43 which they named masurium. This was, and is still, a disputed discovery because all known isotopes of this element are highly radioactive with half-lives much shorter than the age of the earth. In 1937, C. Perrier and E. Segrè found and identified this element in an irradiated foil in the Berkeley cyclotron and named it technetium. It is conjectured that what is considered as Noddack's premature report of the discovery of element 43 is responsible for the neglect of her prescient 1934 paper that correctly draws attention to the fact that uranium fission may be responsible for the neutron induced radioactivity Fermi observed in uranium. She also has been criticised for not following up her own suggestion and performing chemical analyses of the reaction products of neutron irradiated uranium herself. It was such a chemical analysis in Lise Meitner's lab that produced the evidence for nuclear fission.

Perhaps a contributing factor in Ida Noddack's paper not having received the attention it deserved was political. There is evidence that some colleagues observed her husband Walter in a Nazi uniform and Professor J.P. Adloff of Strasbourg University reports [22a]:

> "Together with her husband, Ida Noddack was appointed to our University by the Nazis when Alsace was annexed by Germany during WW II. They held a position probably from 1942 to 1944 when the Nazis were thrown out by the Liberation army. They managed to cross the Rhine back to Nazi Germany. Walter Noddack was professor of chemical physics (we say chimie physique) at Strasbourg University and was helped by his wife Ida. Nothing is known about the scientific work of the couple during this period. In fact, the list of publications of the Noddacks lacks any entry from 1940 to 1951. In a 1954 paper the authors write 'in 1944 an important enrichment of masurium had been obtained, but then all preparations were lost and the work was interrupted for 5 years'. It is not known if the enrichment of masurium was carried out in Strasbourg".

6. Lise Meitner (1878-1968)

Lise Meitner was an Austrian Jewish woman who came from Vienna with a doctorate in physics to Berlin in 1907 after Ludwig Boltzmann died. Collaborating with Otto Hahn, she became renowned as an experimental nuclear physicist. Their many accomplishments before and just after WWI included the discovery of element 91, protactinium. She became head of the radiophysics department of the Kaiser Wilhelm Institute, Berlin-Dahlem from 1918 to 1938, and the head of the radiochemistry department. They then worked separately until 1934 when Meitner invited Hahn to join her in reproducing and studying Fermi's new discoveries bombarding uranium with neutrons. Before that, in the twenties, she made many important contributions including discovery of radiationless atomic transitions two years before Auger, though the effect is widely known as the Auger effect; experimental confirmation of the Klein-Nishina formula for Compton scattering; and most important for Fermi's work, confirmation of Chadwick's observation of the continuous electron energy spectrum in nuclear beta decay [21]. In this time her reputation as a very careful and reliable experimentalist was extremely high so that her confirmation of Chadwick's results convinced everyone including Wolfgang Pauli that they were observing new physics. Very soon thereafter Pauli proposed the existence of the neutrino. Implementing Pauli's proposal, Fermi constructed his Theory of Weak Interactions whose quantitative predictions led to confirmation of the existence of the neutrino.

Lise Meitner is most famous for her discovery of nuclear fission. She and her nephew O.R. Frisch named the process and explained it [22]. The experimental discovery was carried out with radiochemists O. Hahn and F. Strassmann whom she invited to join her effort when she began to study neutron nucleus interactions in uranium. She expected to confirm Fermi's experiments in which he observed radioactivity that he reported as indicating production of new transuranic elements. She must have felt that having chemists in the group would be valuable in confirming those results and studying the new elements. The three of them found something surprising. After very careful analysis, they found that the observed radioactivity was due to the disintegration of the uranium nucleus into smaller nuclear fragments. By that time Austria had been annexed by Nazi Germany and, being a Jewish woman, Meitner could no longer work in Berlin. Before the Anschluss she was able to continue her work in Germany as an Austrian national. In 1938 she escaped to Stockholm and the experimental results, which were obtained in her laboratory were published without her name as coauthor though she had initiated the investigation and participated in all its phases. This discovery led to Fermi's constructing the first self-sustaining nuclear chain reaction, nuclear reactors, and the atomic bomb.

For an in depth discussion of Lise Meitner's life and work, including her close collaboration with Otto Hahn for over half a century, see Ruth Lewin Sime's fascinating biography *Lise Meitner: a life in Physics*, University of California Press, Los Angeles 1996.

1939-1954

During the war years 1939-1945, Fermi was engaged in creating the first self-sustained nuclear chain reaction, and improving nuclear reactors for power generation and for producing plutonium for atomic bombs. He was a leading scientist in the Manhattan Project to build the atomic bombs. During most of those years, and afterward at the University of Chicago, he worked closely with a young physicist, Leona Woods Marshall. After the war he returned to Chicago where he also had a close collegial relationship with Nobel Laureate Maria Goeppert Mayer. Owing to limitations of space and time, in this paper I can only give unduly brief mention of these two women. But there are excellent references for more information. Leona Woods Marshall Libby has written an autobiographical account of her work with Fermi and others in the Los Alamos Laboratory and in Chicago (see below). There are numerous good references where one can read about Maria Goeppert Mayer; in particular, her biography in "Nobel Lectures, Physics 1963-1970", Elsevier Publishing Company, Amsterdam, 1964-1970 and "Maria Goeppert Mayer, A Biographical Memoir" by Robert G. Sachs in *Biographical Memoirs* vol. 50, National Academy of Sciences (1979).

7. Leona Woods Marshall Libby (1919-1986)

Leona Woods Marshall was an experimental physicist who worked closely with Fermi in the Manhattan Project and after the war at the University of Chicago. She wrote an autobiographical account of this work entitled The Uranium People, published by Charles Scribners & Sons (1979).

8. Maria Goeppert Mayer (1906-1972) Nobel Prize in Physics 1963

Maria Goeppert Mayer was a brilliant theoretical physicist, the only woman after Marie Curie to have been awarded a Nobel Prize in Physics. Marie and Pierre Curie, with Henri Becquerel, received a Nobel Prize in Physics in 1903. In studies of nuclei, Maria Mayer discovered the magic numbers and their explanation in terms of the nuclear shell model with strong spin-orbit coupling. For this she won the 1963 Nobel Prize in Physics, with J.H.D.

Jensen who had independently proposed the strong spin-orbit coupling. It is appropriate here, in this Conference celebrating Enrico Fermi, to bring attention to Fermi's role in Mayer's Nobel Prize winning work. In her short but very famous paper "On Closed Shells in Nuclei II" she gives a summary of the evidence for the spin-orbit explanation of magic numbers, and ends the paper with the following sentence: "Thanks are due to Enrico Fermi for the remark, 'Is there any indication of spin-orbit coupling?' which was the origin of this paper" [23].

Throughout her life Maria Mayer made important contributions to many fields of physics including chemical physics, molecular physics, atomic physics, statistical mechanics and, of course, nuclear physics. With her husband Joe, she wrote a classic textbook, Statistical Mechanics, from which many generations of students benefited.

Having obtained her doctorate from the University of Göttingen in 1930, she worked in universities unpaid until 1960! In 1930 she married Joseph Edward Mayer, an American physical chemist, and went with him to Johns Hopkins University in Baltimore, Maryland. This was when no university would think of employing the wife of a professor. She endured the indignity of working as a 'Volunteer' in universities until she was appointed to a full professorship in the University of California in 1960. She kept working, she said, "just for the fun of doing physics".

Conclusion

In conclusion I want to reiterate that, owing to limitations of ability, time, and space, I have not been able to do justice to the lives and accomplishments of the women mentioned here. Hopefully more will be written about them and others who also have made important contributions to progress in physics. Rather than to focus attention on a fewer number of women, I have chosen to bring fourteen to your attention to emphasize the fact that there are many successful women physicists who tend to go unrecognized in our profession.

I am very grateful to Professor Carlo Bernardini, Luisa Bonolis and the Organizing Committe of the International Conference "Enrico Fermi and the Universe of Physics" for inviting me to speak to the Conference about women in physics in Fermi's time.

ACKNOWLEDGMENT

Advice of my colleague at UCLA Professor S. A. Moszkowski has been very helpful in the preparation of this manuscript.

REFERENCES

1. S. CURIE, "Radiations from Compounds of Uranium and of Thorium", *Comptes Rendus* 126: 1101 (1898). M. et MME. CURIE, "New Radio-Active Element in Pitchblende", *Comptes Rendus* 127: 175 (1898).

2. Surface Tension, *Nature* 43: 437 (1891); Letter from Lord Rayleigh.
3. K. BJERRUM MEYER, “Om overensstemmende Tilstande hos Stofferne”, Royal Danish Academy of Science and Letters series SN 6.IX.3, pp. 155-225 (1899).
4. L. FERMI, “Atoms in the Family: my life with Enrico Fermi”. [Chicago] University of Chicago Press [1954]
5. E. SEGRÈ, “Enrico Fermi, Physicist”, University of Chicago Press, (1970). In this book, Fermi’s papers are referenced according to the numbering in “The Collected Papers of Enrico Fermi”, University of Chicago Press (1962, 1965): e.g., [FP 11] is paper number 11 in the Collected Papers {this is his important paper on the ergodic theorem}and [FP 3] is his 1922 paper “On the Phenomena Occurring near a World Line”.
6. M.J. KLEIN, “Paul Ehrenfest Volume I: The Making of a Theoretical Physicist”, North-Holland Publishing Co. (1970).
7. P. and T. EHRENFEST, “Begriffliche Grundlagen der statistischen Auffassung in der Mechanik”, *Encyklopädie der mathematischen Wissenschaften*, vol. 4, part 32(1911); English translation by Michael J. Moravcsik, Cornell University Press (1959).
8. E. NOETHER, “Invariante Variationsprobleme”, Nachr. d. König. Gesellsch. d. Wiss. zu Göttingen, Math-phys. Klasse (1918), 235-257. English translation: M.A. TAVEL, *Transport Theory and Statistical Physics*, 1(3), 183-207 (1971).
9. N. BYERS, “E. Noether’s Discovery of the Deep Connection Between Symmetries and Conservation Laws, Israel Mathematical Conference Proceedings Vol. 12, 1999”. See *http://www.physics.ucla.edu/~cwp/articles/noether.asg/noether.html*.
10. Emmy Noether Collected Papers, ed. Nathan Jacobson, Springer-Verlag 1983.
11. NICOLAS BOURBAKI, “Elements of the History of Mathematics”, Masson Editeur, Paris 1984; English translation by John Meldrum, Springer-Verlag, Berlin Heidelberg 1994. Henri Cartan, Andre Weil, Jean Dieudonne, Claude Chevalley, and Alexander Grothendieck wrote collectively under the name of Nicolas Bourbaki.
12. A. DICK, “Emmy Noether (1882-1935)”, Birkhauser 1981; English translation by H. I. Blocher.
13. H. WEYL, *Scripta Mathematica* III. 3 (1935) 201-220; an English translation of this memorial lecture is given in Ref. 12.
14. O. TAUSSKY TODD, in *Emmy Noether in Bryn Mawr*, B. SRINIVASAN and J. SALLY (eds.), New York-Berlin, 1983.
15. A. EINSTEIN, Letter to the Editor of the New York Times, May 5, 1935. The full text may also be found in ref. 12.
16. E. FERMI, “On the Quantization of the Perfect Monatomic Gas”, presented to the Academia Lincei, February 1926. [FP30 & 31]
17. M. BLAU, “The photographic effect of natural H-rays” (in German), SBAWW (Sitzungsberichte Akademie der Wissenschaften in Wien) IIa 134: 427 (1925) (English translation by Sven Reiche and James Rosenzweig posted on the Web at *http://www.physics.ucla.edu/~cwp/articles/blau/blau-rosenz.html)*.
18. M. BLAU and H. WAMBACHER, “Photographic detection of protons liberated by neutrons. II”, ibid., 141: 617 (1932).
19. M. BLAU, “The multiplier phototube in radioactive measurements”, *RSI* 18: 715 (1947).

M. BLAU, M. COULTON and J.E. SMITH, "Meson production by 500 MeV negative pions", *Phys. Rev.* 92: 516 (1953).

20. Nobel Lectures, *Physics 1942-1962*, Elsevier Publishing Company, Amsterdam, 1964-1970.
21. L. MEITNER, "Das beta-Strahlenspektrum von UX1 und seine Deutung", *Z. Phys.* 17: 54-66 (1923). H.H. HUPFIELD and L. MEITNER, "Uber das Absorptiongesetz fur kurzwellige gamma-Strahlung", *Z. Phys.* 67: 147 (1930). L. MEITNER and W. ORTHMANN, "Uber eine absolute Bestimmung der Energie der primaren beta -Strahlen von Radium E", *Z. Phys.* 60:143 (1930).
22. L. MEITNER and O.R. FRISCH, "Disintegration of Uranium by Neutrons: A New Type of Nuclear Reaction", *Nature* 143: 239 (1939); "Products of the Fission of the Uranium Nucleus", *Nature* 143: 471 (1939); "New Products of the Fission of the Thorium Nucleus", *Nature* 143: 637 (1939).
23. M. GOEPPERT MAYER, "On closed shells in nuclei II", *Phys. Rev.* 75: 1969 (1949). See also "On closed shells in nuclei", Phys. Rev. 74: 235 (1948).

Nina Byers

In the summer of 1948, Enrico Fermi gave a course of lectures on Quantum Mechanics in Berkeley, California. That was the beginning of Professor Byers' career in physics. The next step was graduate study with Fermi at the University of Chicago. After receiving her Ph. D. in Physics from Chicago in 1956, she has held appointments to the faculties of University of Birmingham, England; Institute of Theoretical Physics, Stanford University; Institute for Advanced Study, Princeton; University of California at Los Angeles; Oxford University; and held research appointments at the European Organization for Nuclear Research (CERN) and Fermi National Accelerator Laboratory (FNAL). She is Fellow of the American Physical Society (APS) and the American Association for the Advancement of Science (AAAS) and was Official Fellow and Janet Watson Visiting Fellow of Somerville College, Oxford.

Harold Agnew

Documents on Fermi's Life

I had the privilege of starting my relationship with Enrico Fermi at Columbia University in January 1942. I assisted him with his experiment with a small exponential pile there. We subsequently moved to Chicago and constructed CP-1 ,where I was present when he brought into being man's first nuclear chain reaction. I then left and went to Los Alamos but returned to Chicago in 1946 where I studied under Fermi and Herbert Anderson who was his closest colleague since Fermi's arrival in the United States. My wife, daughter and I had the privilege of living with the Fermi's during the summer of 1946. It was during this period that we really became close friends and we really got to know Enrico, Laura, Nella, and Julio. Our relationship continued after I left Chicago and rejoined the Los Alamos Scientific Laboratory. Fermi visited the Laboratory frequently especially during the summers. He and Laura were clearly very talented individuals. When one examines Fermi's contibutions during the 20th century I don't believe anyone has made more meaningful contributions to science and technology.

Documenti sulla vita di Fermi

Ho avuto il grande privilegio di conoscere Enrico Fermi alla Columbia University nel gennaio del 1942. Fui suo assistente nell'esperimento che effettuò con una piccola pila esponenziale. Poi ci trasferimmo a Chicago dove costruimmo il CP-1, e fui presente quando Fermi realizzò la prima reazione nucleare a catena. Mi trasferii in seguito a Los Alamos, per ritornare a Chicago nel 1946, dove studiai sotto la guida di Fermi ed Herbert Anderson, che era stato il suo collaboratore più stretto sin dal suo arrivo negli Stati Uniti. Mia moglie, mia figlia ed io avemmo il privilegio di vivere con la famiglia Fermi durante l'estate del 1946, e fu in questo periodo che diventammo buoni amici e conoscemmo più intimamente Enrico, Laura, Nella e Giulio. La nostra amicizia continuò quando lasciai Chicago per tornare nel Laboratorio Scientifico di Los Alamos, che Fermi visitò frequentemente, in special modo durante le stagioni estive. Lui e Laura erano chiaramente delle persone eccezionalmente dotate, e studiando i contributi scientifici e tecnologici dati da Fermi nel ventesimo secolo il suo ineguagliato primato in questo senso appare evidente.

The Manhattan Project

In January 1942 I went to the University of Chicago to join the Manhattan Project. I was immediately sent to Columbia University to work with Enrico Fermi. When I first met him the only unusual thing that I noticed was that all of his pants pockets had zippers. All four of them. At the time he was conducting experiments using a large pile of graphite. The structure was entirely encapsulated with a sheet metal cover and was evacuated using mechanical vacuum pumps.

The pile had a radium berrylium neutron source at its center and we measured the slowing down of the neutrons using indium foils which were activated by the source's neutrons. We would insert the foils at different levels in the pile for a specific time, then remove them and run about 100 ft to the counting room where there was a set of Geiger counters. We did this hour after hour for about 10 hours each day. Fermi not only directed the work but actually took on a shift the same as the rest of us. Inserting the foils, running to the counting room with the activated foils and then taking the data. He was one of us. This always distinguished Fermi. He clearly was a genius but acted with no pretentiousness. He was a very unassuming person. He had a wonderful sense of humor.

The array of counters in their lead shields all had names, taken from the Winnie the Pooh books. They were named Pooh, Pigglet, Heffelump, etc. For non-nuclear safety reasons he decided to move the experiments to Chicago and we started to build CP-1, the first man-made chain reaction. One day a several ton load of graphite blocks was delivered around 4 pm. We had to unload the truck so along with the rest of us Fermi took off his coat and pitched in and helped unload the truck. This was Fermi. He not only supplied the brains at Chicago but when needed also supplied the brawn.

Chicago is cold in the winter and people went ice skating there near the University. Fermi had never ice skated and decided he would. We all went to the rink, got Fermi a pair of skates and after a few falls Fermi caught on and before the end of our first session was skating as well as anyone else. He was an excellent athlete and loved to compete. He liked to play tennis especially. Later on when I returned to Chicago as a graduate student we used to play tennis during the lunch hour. This required checking out a net and setting it up on the court. The professor and student took turn with this task. He was a very regular person. Not at all impressed with his position. The only sport at which he was a failure was in fly fishing for trout. Segrè who was a very good fly fisherman never let Fermi forget that at this sport he was no good.

After the war

In 1946, after the war, housing was very scarce in Chicago. I was unable to find a place for my family to live. Fermi who had a fairly large house suggested that my wife, small daughter and I come live with them. His wife Laura wanted to visit her sisters in Italy and when she was gone my wife Beverly could run the house and do the cooking etc. for Fermi and his children Giulio and Nella. We did this for almost three months until I found a place for us to live. Being part of the family for three months was a wonderful experience. Fermi preferred non-spicy food and always diluted the red wine we had for dinner half with water. We stayed on for a month or so after his wife Laura returned.

One evening she told Fermi that she had gone to the local appliance store and put her name on a waiting list for a General Electric dishwasher. [After the war appliances were scarce and one had to sign up on waiting lists for appliances, car etc.] I was astounded. Fermi had been the major consultant for General Electric who were building reactors at Hanford for the production of plutonium. I said "Enrico you know the president of General Electric. Just tell him you want a dishwasher and he will send you one tomorrow". Fermi thought for a second and said: "No that wouldn't be fair for others, we will wait our turn in line." This was classic Fermi.

Fermi liked to swim. Sometimes after work his team of which I was a member would go to Lake Michigan. On one day he decided we would swim across a little bay. I had been a varsity swimmer in high school so thought I was pretty good. But after about 15 minutes in the choppy cold water of Lake Michigan I was falling behind. Fermi who swam with what I would call a "dog paddle" style swam back to me and asked if I was O.K. I said I thought so but clearly my Australian crawl swimming style wasn't best for choppy Lake Michigan. I barely made it to the other side of the bay and with difficulty climbed up the sea wall and sat down. Fermi said: "Meet you back where we started" and plunged back in and swam back to our starting point.

I had difficulty just walking back. Fermi was known by his colleagues as the "Pope". This made it all very clear that he was the supreme authority on all matters. He held this position in all of our minds as an accepted fact. No big deal. Just an accepted realization that he really knew more than the rest of us or anyone else involved in our scientific work. Fermi especially liked young people.

He, in his position, entertained a lot but preferred to have young people. The top floor of his Chicago house had a large room in which he would invite students to come and square dance, I usually did the calling and a good time was had by all. He and Laura had these parties about once a month. When he had dinner parties for his peers he always said " We need to dilute so and so" and "so and so" with some young people". The "so and so'" are too stuffy. Chicago had an open enrollment system for graduate studies but required a 3 day written examination to decide one's future. Choices were, flunk and out, pass with a master's degree and out, or pass with option for going on for a doctorate, if you could find a faculty sponsor. I was terrified about taking the exam because I felt my peers were much smarter than me. [Subsequently 4 of my classmates have received a Nobel Prize in physics, and they were not all the really smart ones] The tests were given so that those scoring the written results had no idea as to whose papers they were grading.

I kept putting off taking the test but Laura Fermi kept urging me to do so. I went to Fermi and asked what he suggested I read. He said he had no idea because he didn't read much. I asked how he always knew what was going on. He said people came and told him and explained things to him. Then he said, which amazed me, that there were people who said they immediately understood things but he wasn't one of those. He said it took him a long time to understand what people were explaining to him but many times he realized that they really didn't understand what they were describing to him but he did. He also volunteered that one who was very quick to say he understood even before the person finished was Oppenheimer, but a lot of the time Oppenheimer really didn't understand the technical information the way Fermi understood it. He told me that if you really understood [Fermi's way of understanding] about ten things in physics you could know almost everything. I had been getting a weeks lecture on Brillouin zones, which I never understood, and asked him about it. He went to a small blackboard and in less than 5 minutes developed the whole theory and at the time I thought I understood it. But as was with most of Fermi's lectures they were so clear and so simple that you really thought you understood all but when one tried to repeat it afterwards on ones own became lost.

Very much like eating Chinese food end up very full and satisfied but shortly very empty and hungry. Of all his colleagues of his vintage, Fermi's favorite for his intellectual ability was Edward Teller. He told me this and years later Laura Fermi and his daughter confirmed this when I raised the question. Among his young people I believe Fermi thought Dick Garwin was the brightest and I also believe this even to this day. This is just a short snapshot of my interaction with Fermi.

There are many other stories such as how he saved our nuclear weapon program when he came up with the idea that plutonium from Hanford would be different than that produced in a cyclotron and had Segrè confirm his worry, but I will stop now and show a video I made from a home movie about two years before his death. Also there is a short segment showing how good a sport he was.

Harold M. Agnew
Former Director Los Alamos Scientific Laboratory, Former President General Atomics, New Mexico State Senator 1955-61, Member US National Academy of Sciences, Member US National Academy of Engineering, Recipient E.O.Lawrence Award, and Enrico Fermi Award from the US Atomic Energy Commission, Scientific Advisor to the Supreme Allied Commander Europe (NATO) 1961-1964. Flew as Scientist on Hiroshima Mission August 6,1945 with 509th Composite Group US Army Air Corps, Member Council of Foreign Relations, Fellow American Physical Society and American Association for the Advancement of Science, Adjunct Professor University of California San Diego. I received a B.A. in chemistry from the University of Denver in 1942 and a Masters and Ph.D. from the University of Chicago in 1949 (under Fermi).

Giovanni Gallavotti

Fermi and the Ergodic Problem

This paper discusses some aspects of Fermi's early work on the quasi-ergodic hypothesis, partly in relation to his last work, which showed that the hypothesis was false in cases of interest to physics.

Fermi e il problema ergodico

Si discutono alcuni aspetti del lavoro giovanile di Fermi sull'ipotesi quasi ergodica in relazione anche al suo ultimo lavoro dal quale l'ipotesi risulta falsa in casi di interesse fisico.

Boltzmann

Boltzmann's ergodic hypothesis became controversial. In a way for the first time, perhaps, the description of the World by continuum models, like point particles sysceptible of occupying a continuum of positions in a continuum phase space, was replaced by discrete conceptions in an argument that had major physical consequences.

Boltzmann viewed the world as discrete and infinitesimal calculus as a tool to approximate sums and ratios whose evaluation was needed in the physical theories. Therefore the energy surface consisted of finitely many cells that could be labeled and counted and that time evolution simply permuted in a single cycle: this is the "ergodic hypothesis". In this context it was natural to suppose that each cell visited in due time all others: he even estimated the time for this to happen, for a cm^2 of Hydrogen in normal conditions to be about (!) $10^{10^{19}} \cdot 10^{-13}$ sec (a similar estimate had been made earlier by Thomson) [3].

Once the hypothesis is accepted together with Boltzmann's theory of the relaxation times (qualitatively solved in general by the vastity of the "Boltzmann's sea" [1,2] and quantitatively analyzed in the case of rarefied gases via the Boltzmann's equation) the time averages could be computed by averaging over cells. The latter could then be "conveniently approximated" by the integrals over phase space which have become familiar to all of us and which lead to the Boltzmann-Maxwell statistics, following ideas that also Maxwell and Thomson developed independently or largely shared.

A strict interpretation in the context of classical mechanics of point particles moving in a continuum became the hypothesis that a typical trajectory moving on the energy surface visited all its points: and it seems that most physicists and mathematicians followed such an interpretation at least until the 1930's. The manifest impossibility of this, divulged by the influential review of the Ehrenfests [4], was pointed out by mathematicians and physicists. And it led to the formulation of a physically nebulous hypothesis called the *quasi ergodic hypothesis* (see footnote 99 in [4]) which basically proposed that every point of the energy surface, but a set of zero volume, evolved covering densely the surface itself.

Poincaré

In modern Physics, starting with the 1930's, the ergodic hypothesis became quite different from the quasi ergodic hypothesis (and essentially identical to Boltzmann's hypothesis). The weaker quasi ergodic hypothesis, nevertheless, is interesting in its own. While it is easy to exhibit important physical systems which do not verify the ergodic hypothesis [15], *even in the modern sense*, it is still an open problem to prove that systems of the type considered by Fermi generically verify the quasi ergodic hypothesis.

One of the main mathematical reasons in favor of the quasi ergodic hypothesis rested, perhaps, on a theorem by Poincaré (see for instance [5]) stating that a Hamiltonian system very close to an integrable cannot admit constants of motions depending analytically on the phase coordinates and on the perturbation strength μ other than the functions of the energy.

It is, at first, surprising that in the middle of the storm in which quantum mechanics was being conceived Fermi, working in the early 1920's Göttingen, devoted himself to studying the esoteric *(quasi)* ergodic problem. Considering a ℓ -degrees of freedom system, $\ell > 2$, with Hamiltonian $H_0 + \mu H_1$ which is a perturbation of an integrable Hamiltonian H_0 he *correctly* proved [6,7] that, for generic perturbing energy functions H_1, there could not exist even a *single* surface of dimension $2\ell - 2$ embedded in the $2\ell - 1$ dimensional energy surface and analytic in the phase coordinates and in μ, for small μ.

Fermi

Fermi tried to deduce [6,9] from his theorem the generic validity of the quasi ergodic hypothesis: stating that it would become generically true under an arbitrarily small perturbing force. In fact he defined "quasi ergodic" a system such that any open set σ on the energy surface generates a set of paths which cover the whole energy surface (a definition not exactly coinciding with the one mentioned above). His line of reasoning was the following: if not there would exist two open regions whose points had trajectories which did not visit the other region and which would therefore be separated by a common boundary S. Assuming that S is a smooth surface of dimension $2\ell - 2$ (to separate into disjoint sets the $2\ell - 1$ dimensional energy surface) depending analytically on μ one derives a contradiction with his theorem, just quoted, stating the generic non existence of such surfaces. *The assumption is confined to a footnote "added in proof" at the bottom of the paper.*

The proof was criticized because the smoothness of the surface was to be proved for the argument to be convincing. Fermi's answer [12] was not satisfactory although he seemed to consider the objection not very relevant from a physical point of view.

Why did he not continue to investigate the subject until 1954 when, in his last major scientific contribution, he discovered (with Pasta and Ulam) [14] that indeed the objections raised to the work of his youth were likely to be extremely serious? [15].

In order to understand one has to take into account that very likely Fermi became interested in a proof of the ergodic hypothesis because he was in Göttingen and he was probably involved in the critiques being raised to the attempts of founding quantum mechanics on the adiabatic invariants [18].

The adiabatic invariants approach, at the time (1923) under intense analysis, was based on *Ehrenfest's principle* which considered systems admitting action-angle coordinates with cyclic actions and prescribed that the action integrals $\oint_{\gamma_i}\vec{p}\cdot d\vec{q}$ over independent cycles γ_i should be integer multiples of Planck's constant h. The principle rested heavily upon the existence of a continuum of models connecting the Hamiltonians of the systems of interest. For instance, in a typical example, the harmonic oscillator and the Keplerian two body system can be continuously transformed into each other by considering the one parameter family of Hamiltonian functions H_μ.

$$H_\mu = \frac{1}{2}\vec{p}^{\,2} + (1-\mu)\frac{1}{2}\omega^2\vec{q}^{\,2} - \mu\frac{g}{|q|} \qquad 0 \le \mu \le 1$$

as the parameter μ varies between 0 and 1. By using the adiabatic invariants one could, in this case, derive the Bohr-Sommerfeld quantization rules for the Hydrogen atom from those of the harmonic oscillator (and viceversa).

Immediately after the work on the quasi ergodic hypothesis Fermi published a series of remarkable papers showing that the approach to quantum mechanics based on the Ehrenfest principle led to incorrect results in the cases in which the family of interpolating Hamiltonians H_μ was not integrable by quadratures for some intermediate μ: note that one cannot define the adiabatic integrals $\oint\vec{p}\cdot d\vec{q}$ for the Hamiltonian systems corresponding to such μ's [10,11].

Thus it seems likely that Fermi must have considered his work important as a fundamental critique to the adiabatic invariants approach to quantum mechanics (even to an extent warranting a bilingual publication, german [6] and, split in two parts, italian [7,9]. Although he did not achieve a proof of the validity of the quasi ergodic hypothesis his result certainly cast a dark cloud over the quantization based on the adiabatic invariants and it strengthened doubts already (quietly) raised by Einstein in an earlier paper [13] that Fermi did not know. This might explain also why he did not seem to pay much attention to the critiques to his work: whether or not it proved the quasi ergodic hypothesis (without the assumption added in proof it did not!) its main part was correct and it made, together with the accompanying works, the quantization based on the adiabatic invariants untenable.

One can imagine that the very young Fermi, in the austere ambiance of the famous german university of the early twenties, left to the reader to draw his own conclusions about the knell that his work rang for the adiabatic principle: perhaps he did not yet feel strong enough to withstand the consequences of a direct critique to a theory as established as the "early quantum mechanics": in the quasi ergodicity paper the word "adiabatic invariant" is not even mentioned, neither in the german original nor in the italian versions, see [17].

The discovery of stability of non-ergodic behavior

In discussing the works of Fermi on ergodic theory one has to mention what is perhaps his most important contribution to the subject: namely the effective lack of ergodicity of classical systems with energy close to the ground state (minimum value).

The discovery was made via an experimental work that has had a very strong influence in the years that followed it: the experiment was proposed by Fermi as an application of the new powerful electronic computing machines. Not only it marked the beginning of a new subject, molecular dynamics, which is becoming more and more important but it also gave strong evidence that simple systems believed to be ergodic were not such, at least for practical purposes. A harmonic chain of several (64) oscillators and with fixed extremes was considered: it appeared quite clearly that in presence of a small nonlinear perturbation an initial datum in which all the energy was concentrated on a single normal mode of the linear system *did not* evolve in such a way that energy would become shared among all the modes of the system [14,16]. No equipartition was realized during the times of observation.

The result was clearly in contrast with the beliefs of most physicists at the time who were convinced that over a rapid time scale the energy would become shared by all the modes. The possibility that the sharing would occur over time scales of enormous size of course could not be excluded: but such an explanation would undermine the theoretical bases of statistical mechanics.

Essentially at the same time Kolmogorov [19] achieved a theoretical and rigorous proof that indeed a generic perturbation of an integrable system would be such that in phase space there would be a set of positive volume whose points would evolve on trajectories that would not fill densely phase space. This means that, in the modern sense of the word, such systems are not ergodic. However excluding the 2-degrees of freedom case it remains possible (and likely) that they verify the quasi ergodic property in the sense of Fermi, *i.e.* in the sense that the trajectories emanating from any open set in phase space fill densely the energy surface.

Strictly speaking the result of Kolmogorov did not apply to the systems considered in the experiments by Fermi and collaborators: and in fact a complete theory of the latter systems is still under investigation although remarkable cases have been understood, see [20] for a review.

Therefore the work of Fermi and collaborators remains a landmark in the Physics literature and, by posing and partially answering a major question, marks the return of the problems of nonlinear mechanics among the interests of Physicists after they had been for several decades a subject left to mathematicians.

References

1. Boltzmann L., *Theoretical Physics and philosophical writings*, ed. B. Mc Guinness, Reidel, Dordrecht, 1974. See p. 206.
2. Brush S., *Gadflies and geniuses in the history of gas theory*, Synthese, 11-43, 1999. See p. 28. See also Uhlenbeck G.E., "An outline of Statistical Mechanics", in *Fundamental problems in Statistical Mechanics*, vol. II, ed. E.G.D. Cohen, North Holland, Amsterdam, 1968. See p. 3, fig. 2.
3. Gallavotti G., *Statistical Mechanics*, Springer Verlag, Berlin, 1999. See p. 140 and Sec. 1 and Sec. 3.
4. Ehrenfest P., "Adiabatic invariants and the theory of quanta", *Philosophical Magazine*, 33, 500-513, 1917. Ristampato in18.
5. Gallavotti G., "Quasi integrable mechanical systems", ed. K. Osterwalder and R. Stora, Les Houches, XLIII, 1984, *Phenomènes Critiques, Systèmes aleatories*, *Théories de jauge*, Elsevier Science, 1986, p. 539-624.
6. Fermi E., "Beweis dass ein mechanisches normalsysteme im algemeinen quasi ergodisch ist", *Physikalische Zeitschrift*, 24, 261-265, 1923. Reprinted in[8], paper n. 11a.
7. Fermi E., "Generalizzazione del teorema di Poincaré sopra la non esistenza di integrali di un sistema di equazioni canoniche normali", *Nuovo Cimento*, 26, 101-115, 1923. Reprinted in[8], paper n. 15.
8. Fermi E., *Note e Memorie (Collected papers)*, Accademia dei Lincei and University of Chicago Press, vol. , 1961, e vol. II, 1965.
9. Fermi E., "Dimostrazione che in generale un sistema meccanico è quasi ergodico", *Nuovo Cimento*, 25, 267-269, 1923.
10. Fermi E., "Il principio delle adiabatiche ed i sistemi che non ammettono coordinate angolari", *Nuovo Cimento*, 25, 171-175, 1923. Ristampato in[8], lavoro n. 12.

11. FERMI E., “Alcuni teoremi di meccanica analitica importanti per la teoria dei quanti”, *Nuovo Cimento*, 25, 271-285, 1923. Ristampato in[8], lavoro n. 13.

12. FERMI E., “Uber die existenz quasi-ergodisher systeme”, *Physikalische Zeitschrift*, 25, 166-167, 1924. Ristampato in[8], alla fine del lavoro n. 11a.

13. EINSTEIN A., “Zum Quantensatz von Sommerfeld und Epstein”, *Verhandlungen der Deutschen physikalischen Gesellshaft*, 19, 82-92, 1917. Italian reprint in GRAFFI S., *Le radici della quantizzazione*, “Quaderni di Fisica Teorica dell’Università di Pavia”, Pavia, 1993, ISBN 88-85159-09-5.

14. FERMI E., PASTA J., ULAM S., *Studies of nonlinear problems*, “Los Alamos report LA-1940”, 1955, printed in[8], Vol. II, p. 978-988.

15. BENETTIN G., GALGANI L., GIORGILLI A., “Boltzmann’s ultraviolet cut-off and Nekhoroshev’s theorem on Arnold diffusion”, *Nature*, 311, 444-445, 1984. And BENETTIN G., GALGANI L., GIORGILLI A.: *The dynamical foundations of classical statistical mechanics and the Boltzmann-Jeans conjecture*, Edited by S. Kuksin, V.F. Lazutkin, J. Pöschel, Birkhauser, 1993.

16. FALCIONI M., VULPIANI A., “Il contributo di E. Fermi ai sistemi non lineari: l’influenza di un articolo mai pubblicato”, in *Conoscere Fermi*, edited by C. and L. Bonolis, p. 274-289, Editrice Compositori, Bologna, 2001, ISBN88-7794-284-3.

17. GALLAVOTTI G., *La meccanica classica e la rivoluzione quantica nei lavori giovanili di Fermi*, edited by C. Bernardini and L. Bonolis, p. 76-84, Editrice Compositori, Bologna, 2001, ISBN88-7794-284-3.

18. VAN DER WAERDEN B.L., *Sources of quantum mechanics*, Dover, 1968 (this is a collection of the main papers on matrix mechanics with an important critical introduction).

19. KOLMOGOROV N., “On the preservation of conditionally periodic motions”, Doklady Akademia Nauk SSSR, 96, 527- 530, 1954. See also: Benettin, G., Galgani, L., Giorgilli, A., Strelcyn, J.M.: “A proof of Kolmogorov theorem on invariant tori using canonical transormations defined by the Lie method”, *Nuovo Cimento*, 79 B, 201- 223, 1984.

20. RINK B., “Symmetry and resonance in periodic FPU chains”, *Communications in Mathematical Physics*, 218}, 665-685, 2001.

Giovanni Gallavotti

Born in 1941, Mr. Gallavotti received his physics degree from the University of Rome in 1963. At the University of Florence, taught the introductory physics course (1964-65) and neutron physics (1965-66). Researcher at IHES in Paris (1966-68) and at Rockefeller University in New York (1969-70), associate professor of institutions of higher analysis (Rome, 1970-72), full professor since 1972 (mathematical methods in physics, rational mechanics, mathematical physics, fluid mechanics and advanced mechanics), visiting professor of theoretical physics at Nijmegen (1973-74), faculty member at the Linceo Interdisciplinary Centre (1986-89). Received the Italian President's National Award in 1997. Member of the Scientific Boards of IHES (1998-2003), and, as of 2000, of ESI in Vienna and the Institut Henri Poincaré in Paris.
Author of 180 articles in international journals, numerous monographs, four books in Italian (three of them also published in English translations) and one in English, and 14 monographic entries for the Italian Encyclopaedia.

Tullio Regge

Fermi and General Relativity

The contributions of Enrico Fermi to the theory of relativity are all contained in his early papers. Paper N. 1, the first one in his collected papers, deals with the inert mass of a rigid system of electric charges and in particular for a spherically symmetrical one. It turns out that the final result does not agree with Einstein's principle of equivalence.

The riddle is solved in Paper N. 2 where Fermi evaluates the effect of a uniform gravitational field on a system of electric charges and recovers agreement with general relativity. Paper N. 3 is the most important one and the so called Fermi theorem thereby contained is still widely quoted in all treatises which deal with absolute differential calculus, for instance by J.L.Synge, (*Relativity, the General Theory*, Amsterdam 1960). The theorem, among other things, proves the existence of a locally intertial coordinate system in the neighbourhood of a timelike geodesic. The paper is related to N. 2. As Persico points out in his comment (p. 17) Fermi … felt the opportunity of a more systematic treatment of this and othe similar problems, by means of a system of spacetime coordinates particularly fitted to follow the behaviour in time of phenomena happening in a small spatial region”. Paper N. 8 returns briefly on general relativity.

It is evident from his late contributions that general relativity was not the central element of interest of Fermi but that he nevertheless felt at ease in it. Emilio Segrè aptly comments in the introduction to the collected works (p. XXV) that Fermi had no difficulty in using abstract mathematics with the most rigorous standards if he saw it fit for his vision.

There is no doubt that his primary interest was physics and that mathematics was a tool. In spite of this the maturity and power of this early contribution is prodigious.

Fermi e la relatività generale

Il contributo dato da Enrico Fermi alla teoria della relatività è tutto contenuto nelle sue prime pubblicazioni. Il Documento n. 1 "Sulla dinamica di un sistema rigido di cariche elettriche in moto traslatorio", riguarda la massa inerte in un sistema rigido di cariche elettriche ed in particolare un sistema sferico simmetrico, ed i suoi risultati finali sono in netto contrasto con il principio di equivalenza di Einstein.

Il problema viene risolto nel Documento n. 2 "Sull'elettrostatica di un campo gravitazionale uniforme e sul peso delle masse elettromagnetiche", in cui Fermi valuta gli effetti di un campo gravitazionale uniforme su di un sistema di cariche elettriche, riconciliandosi con la teoria della relatività.

*Il Documento n. 3 "Sopra i fenomeni che avvengono in vicinanza di una linea oraria" è il più importante, e la teoria cosiddetta fermiana ivi contenuta è ancora ampiamente citata in tutti i trattati sul calcolo differenziale assoluto, come ad esempio da J.L. Synge (*Relativity, the General Theory, *Amsterdam 1960). Il teorema, tra l'altro, dimostra l'esistenza di un sistema locale di coordinate inerziali in un ambito temporale geodetico.*

L'origine di tale Documento è probabilmente collegata al Documento n. 2, come afferma Persico nella sua relazione (pag. 17): "Fermi …sentì l'esigenza di una trattazione più sistematica di queste ed altre questioni attraverso un sistema di coordinate spaziali particolarmente indicate a seguire l'andamento nel tempo di quei fenomeni che avvengano in uno spazio limitato".

La pubblicazione n. 8 "Sul peso dei corpi elastici", si sofferma di nuovo brevemente sulla relatività generale. Appare evidente dai suoi ultimi scritti come la relatività generale non costituisse per Fermi l'elemento centrale di interesse, cosa che non gli impedì di occuparsene agevolmente.

Emilio Segrè afferma nell'introduzione alle sue Opere Complete *(pag. XXV) come Fermi non avesse alcuna difficoltà nell'utilizzare la matematica pura secondo standard rigorosi da lui stesso applicati. Nonostante il suo interesse principale fosse la fisica e la matematica rappresentasse per lui solo uno strumento, la maturità e la potenza di questi suoi primi contributi sono comunque prodigiosi.*

Nicola Cabibbo

Fermi's Tentativo and Weak Interactions

I will discuss Fermi's beta-decay paper, and some of the further work by Fermi's close collaborators, Majorana, Wick, Pontecorvo. I will also discuss recent advances in the understanding of weak interactions, in particular quark mixing and neutrino oscillations.

Il tentativo di Fermi e le interazioni deboli

Mi soffermerò sulle pubblicazioni Fermiane riguardanti il decadimento beta, e su alcuni lavori posteriori di suoi collaboratori come Majorana, Wick e Pontecorvo, nonché sui recenti progressi nella comprensione delle interazioni deboli, particolarmente per quel che riguarda la mescolanza dei quark e le oscillazioni neutriniche.

Introduction

The December 1933 issue of "La Ricerca Scientifica", the journal of the Consiglio Nazionale delle Ricerche, contained an article by Enrico Fermi, "Tentativo di una teoria dell'emissione dei raggi *beta*", "a tentative theory of beta rays"[1]. The title was far too modest: the theory is still valid today, after nearly seventy year.

Fermi's "tentativo" did not have an easy life. According to Franco Rasetti, one of Fermi's closest friend and collaborator, an english version of the paper was rejected by *Nature* as too abstract. Attempts to locate copies of this manuscript or other documents relating to this episode have failed, but we hope that new evidence might emerge in the future.

Fermi attributed β decay to the action of a new type of force which acts between elementary particles which is now called the *weak force* or *weak interaction*. A number of other physical processes can today be attributed to weak interactions essentially in the form presented by Fermi's paper.

The study of weak interaction has led to important discoveries, among which, in the fifties, the violation of specular symmetry *(P)* and of charge symmetry, *(C)*. A few years later the study of K^0 mesons led to the discovery of the violation of time reversal symmetry *(T)* and of the symmetry between matter and anti-matter *(CP)*. The last few months have seen the announcement of two new example of *(CP)* violation, one again in the decays of K^0 mesons, the second in the decay of B^0 mesons, which contain a heavy quark of type *b*.

Weak interactions are of great interest because they allow the transformation of one elementary particle into another, thus revealing the intimate connection between different particle types.

The study of some problematical aspects of Fermi's theory has led in the sixties to the formulation of a unified theory of weak and electromagnetic inter-action, which contains as limiting cases Maxwell's theory of electromagnetism and Fermi's theory of weak interaction.

Beta Decay in 1933

Of the three types of radioactive emission, two, the α and γ rays, did not

---

[1] Reprinted in *Note e Memorie – Collected Papers*, Vol. I, Accademia dei Lincei and University of Chicago Press, 1962. Franco Rasetti wrote an interesting introduction to the beta decay papers. An excellent treatment of the subject is in A. Pais, *Inward Bound*, Oxford University Press, 1986.

pose substantial conceptual problems. An α disintegration, such as

$$Ra_{88}{}^{226} \rightarrow Rd_{86}{}^{222} + He_2{}^4 \qquad (1)$$

is a true *dis-integration*, a rearrangement of the protons and neutrons present in the initial Radium nucleus to form the daughter Radon and Helium nuclei. Many details remained to be clarified, but in 1933 the nature of alpha emission was well understood, and the essential fact were well interpreted by Gamow's successful 1927 theory.

In γ radioactivity photons are emitted. Although many details were missing in 1933, this phenomenon was clearly a close analogue of photon emission in atoms, *i.e.* a transition between two different quantum states of the same nucleus.

The nature of β radioactivity was only clarified with Fermi's paper. It was well known that β rays are electrons, but which was their origin? Before Chadwick's discovery of the neutron, the current hypothesis was that nuclei were composed by protons and electrons. An Helium atom, for instance, would have contained four protons, providing most of the mass, and two electrons.

In the proton-electron model of the nucleus a β decay would have been similar to an α decay, the emission of an electron already present in the nucleus, a true dis-integration.

In fact the proton-electron model had failed an important test with Rasetti's determination, in 1929, of the statistics of nitrogen's atoms, a result whose meaning was however fully appreciated later, *e.g.* in Heisenberg's 1932 paper on nuclear structure. After the discovery of the neutron the transition to the modern proton-neutron model was very rapid. But if electrons are not present in the nucleus, what happens in β decay?

There was in fact a more serious problem posed by β decay: α and γ rays are emitted with an energy equal to the difference between the energies of the initial and final nucleus, so as to guarantee the overall conservation of energy; in β decay, on the contrary, electrons are emitted, in any given transition, with a continuous energy spectrum. Niels Bohr was led to propose that energy is not exactly conserved in β decay. A very strange proposition, but it originated from the father of atomic physics, and could not be easily discarded.

The solution of this second puzzle was found by Wolfgang Pauli: in β decay a second particle was emitted together with the electron, so that the two would share in different ways the available energy. This would certainly explain why the electron appears with a range of energies. The "second particle" would have been neutral, and available data excluded that it could be a photon: a new particle, then, never observed before.

Pauli was very prudent with his idea, which he probably considered too extreme, and he did not publish it. He wrote of the idea to close friends in the famous "Dear radioactive ladies and gentlemen" letter, where he called the new particle a "neutron". He discussed it in the corridors of physics conferences, but never officially.

Pauli discussed his idea with Fermi in 1931, during the Rome conference on nuclear physics, and on this occasion Fermi proposed[2] that the correct name was not "neutron", but "neutrino", more suited to a very light particle. The two must not have discussed the matter again; while Pauli readily adopted the name proposed by Fermi, Fermi himself kept using the name proposed by Pauli.

In his talk at the International Conference on Electricity (Paris, 1932) he said:

> "... Si potrebbe pensare ad esempio, secondo un suggerimento di Pauli, che nel nucleo atomico si trovino dei *neutroni* che sarebbero emessi contemporaneamente alle particelle beta. ..." (We can think that, according to Pauli's suggestion, the atomic nucleus contains *neutrons* which are emitted together with the beta particles).

To a question from the audience he answered that these "neutrons" could not be those recently discovered by Chadwick, but much lighter particles. In 1932 Fermi was still thinking in terms of the emission of particles *already present in the atomic nucleus.* The 1933 paper would have proposed a radically different solution.

Fermi's theory of Beta Decay

Chadwick's neutron expelled the electron from the atomic nucleus, and left little space for Pauli's neutrino. The modern view of the nucleus had its official sanction in the Solvay Conference in october 1933, where Heisenberg's theory was widely discussed. It is generally assumed that Fermi started working on his beta decay theory just after this conference, completing the work in just two months. I frankly suspect that he must have started considering the problem much earlier than that. His interest in the subject is well shown by his talk in Paris the previous year (although he was not then able to pro-

[2] The Pauli hypothesis had already been discussed by Fermi with his collaborators. According to a recollection by E. Amaldi, he had proposed the name neutrino during one of these discussions with Fermi.

pose the correct theory), and the proton-neutron model of the nucleus had been essentially accepted in Rome due to Majorana's unpublished work. Majorana had turned down Fermi's proposal to present his work at the Solvay meeting.

At the core of Fermi's theory is the idea that the electron and neutrino are not preexisting in the nucleus, but are created anew at the time of their emission. To accept this idea one would have to renounce the well-established one that an electron is a particle endowed with a certain amount of material solidity and persistence.

There was of course the example of the photon, a particle which is created when light is emitted, and destroyed when light is absorbed. These processes are well understood in the quantum theory of the electromagnetic field, developed by Dirac soon after the birth of Heisenberg's quantum mechanics. That this formalism could be applied to the creation and absorption of any particle, electron included, had been shown since 1927 by Jordan and Klein.

The formalism of quantized fields, proposed by Jordan and Klein for all particles, must have struck a note very close to Fermi's scientific interests . The theory of quantized field naturally lead to the description of particles which are identical in the quantum sense, particles which must necessarily obey either the Bose-Einstein statistic, as photons do, or, as is the case with electron, the statistic he had himself discovered in 1926, the Fermi-Dirac statistics.

Fermi had not used the Jordan-Klein formalism in his famous lectures on Quantum Electrodynamics: it was not needed for his discussion of processes where electron are not created or destroyed, and Fermi was very thrifty in his choice of tools. But when the need arose, the tool was ready.

According to Fermi the beta decay of a nucleus is due to a new type of interaction which causes the transformation of a neutron inside the nucleus into a proton, with the simultaneous production of an electron-neutrino[3] pair:

$$N \rightarrow P + e + \nu \tag{2}$$

Fermi introduced the hypothesis that this process is quite analogous to that in which a proton emits a photon,

$$P \rightarrow P + \gamma. \tag{3}$$

To the photon emitted in eq. 3 corresponds the electron-neutrino pair in eq. 2.

[3] According to present conventions the neutral particle emitted in beta decay together with a negative electron is an *antineutrino*. In this paper I use the term neutrino to denote either a neutrino or an antineutrino, except in the following when I will touch on Majorana's neutrino theory.

To shed some light on the analogy Fermi had in mind we may look at the proton in eq. 3 as a radio antenna: the transition of the proton from a higher energy quantum state to a lower energy state activates an electric current which causes the emission of electromagnetic waves – the photon. In Fermi's view what happens in beta decay, eq. 2, is that the transformation of a neutron into a proton activates a new kind of current, today called a *weak* current, which causes the creation of the electron-neutrino pair.

The analogy between beta decay and photon emission is both a productive one and a very happy one. It is productive, because it allowed Fermi to propose a definite mathematical form for the interaction giving rise to beta decay[4], and a very happy one because this form proved to be essentially correct, the only needed modification having been due to the discovery of parity violation in 1956.

According to the modern view, a more precise similitude could be established between beta decay and electromagnetic induction: a variable current in a circuit generates an electromagnetic field which can than induce a current in a second circuit – the principle of operation of electric transformers.

In Fermi's theory we can see a kind of short-circuit between the weak current of the neutron-proton transition and a corresponding current whose activation lead to the production of the electron-neutrino pair.

In unified theories, which reached maturity at the end of the sixties, the relationship between weak and electromagnetic interaction is much closer than Fermi could have suspected. As in the case of electromagnetic induction, the interaction between weak currents is mediated by a field, whose quanta, the *W*, have been discovered at the beginning of the eighties. Fermi's analogy has been proven correct at a deeper level, since we now know that weak and electromagnetic interactions are different manifestation of the same force, now called the *electro-weak* force. Since the *W* has a very large mass, Fermi's original theory remains in most case an excellent approximation.

In the 1933 paper Fermi presented the mathematical structure of the theory as well as its application to the study of beta radioactivity. He was able to show that beta decays can be subdivided in two classes: the *allowed* decays, which can proceed in the limit where one neglects the motion of nucleons

[4] The analogy led Fermi to choose a "vector" form for the new interactions. Although Fermi did not discuss in his paper the general case, which was published later by Uhlenbeck and Konopinsky, many – among them Wigner – were convinced that he was aware of this possibility, but choose the most attractive form.

(proton and neutron) within the nucleus, and the *forbidden* ones, which depend on this motion, and proceed at a rate which is roughly a hundred time lower than that of allowed decays. Only with Fermi's work the well known fact that certain beta decays have a much larger transition rate than others found a quantitative explanation.

An important result of Fermi's work is the determination of the energetic distribution of the emitted electrons. Fermi was able to show that the study of the high-energy end of this distribution can be used to establish an upper limit to the mass of the neutrino, and that available data favoured a very small neutrino mass. The method proposed by Fermi has in recent years led to the current limit ($\approx 10eV$, 50000 times smaller than the mass of the electron) on the mass of the neutrino emitted in beta decay.

Fermi's theory contains a single free parameter, today called the Fermi constant, G, which determines the strenght of weak interactions, and can be determined by measuring the decay rate of one of the allowed decays. G has the dimension of the inverse of a mass squared – with good approximation $G \approx (300\ M_P)^{-2}$,where M_P is the mass of a proton. The large mass appearing in the denominator, 300 M_P, is the reason why Fermi interactions are very weak in low energy phenomena, which include all of the radioactive decays. The actual strenght of Fermi's weak interactions increases with energy, and a more complete theory, such as the modern unified theory, becomes essential for processes of very high energy.

Weak interactions after Fermi's paper

While Fermi himself was not actively devoted to the exploration of weak interactions after 1934, many of his students and collaborators, both in Italy and the United States, gave important contributions, among whom G.C. Wick, B. Pontecorvo, E. Majorana, T.D. Lee, C.N. Yang, M. Gell-Mann, R. Garwin, J. Cronin, together with many others.

Gian Carlo Wick applied the theory to beta decays with positron emission, and predicted the possibility of K capture, where a nucleus, instead of emitting a positron absorbs one of the inner electrons in the atom, so that only a neutrino is emitted.

Bruno Pontecorvo studied inverse beta decay, the process where a neutrino is absorbed by a nucleus and an electron is emitted, and proposed that this process could be used to establish the existence of the neutrino. Pontecorvo's method was in fact applied in the Cowan-Reines experiment in

1956. In later years Pontecorvo proposed to carry out experiments with beams of high energy neutrinos, an experimental program which was developped starting from the sixties and which led to important discoveries, among which the existence of different neutrino types associated with the different leptons – the electron, muon and tau.

The greatest contribution by Bruno Pontecorvo is his proposal of a new phenomenon – neutrino oscillations, where a neutrino can oscillate between different identities, an electron-neutrino transforming for example into a muon-neutrino or a tau-neutrino[5]. Recent years have seen the discovery of two different examples of neutrino oscillations, the first arising in muon-neutrinos produced in the atmosphere by cosmic rays, the second in electron-neutrinos emitted from the Sun. Neutrino oscillations are becoming one of the hottest items in High Energy Physic research.

Ettore Majorana proposed an alternative neutrino theory, where neutrino and antineutrino coincide. This idea, reformulated in the light of the discovery of parity violation (T.D. Lee and C.N. Yang, 1956), remains very much alive today, and is invoked to explain the existence of the very small neutrino masses implied by the discovery of neutrino oscillations.

The discovery of parity violation in 1956 led to the modernly accepted form of the weak interaction theory. The new form, the so called *V- A* theory (R. Feynman and M. Gell-Mann, Marshak and Sudarshan, 1958) is very close to that proposed by Fermi: beta decay and other weak interaction processes are still due to the interaction of two currents[6], but these currents do not have a definite behaviour under specular reflection. The new developments vindicated the correctness of Fermi's analogy between weak and electromagnetic processes, and at the same time concentrated the attention on the structure of the weak currents. In carrying the analogy to the extreme, Feynman and Gell-Mann proposed a strict relationship between certain terms in the weak current and corresponding terms in the electromagnetic current, leading for instance to the existence in beta decay of a phenomenon similar to magnetism, the weak magnetism, which was succesfully identified a few years later.

The impact of Fermi's 1933 paper goes well beyond the study of weak interactions. Fermi's paper was the first in which quantum field theory was used in the modern sense, and it must thus be considered the first modern

---

[5] In his original proposal Pontecorvo actually considered neutrino-antineutrino oscillations.

[6] We now know that this interaction is mediated by a field whose quanta are the *W* bosons.

paper on the physics of elementary particles. Among the many instances of the influence of this paper on subsequent work, one that stands out is certainly Yukawa's paper on the meson theory of nuclear forces.

Quark mixing, the unified theory, the violation of CP

The discovery of *strange particles* in 1953 greatly enlarged the range of phenomena arising from weak interactions. The main decay mode of particles such as the *K* mesons or the hyperons (Λ,Σ,Ξ) do not involve the emission of electron neutrino pairs, *e.g.*

$$K^+ \to \pi + + \pi^0,\ \Lambda \to P + \pi^- \tag{4}$$

a new kind of interaction between weak currents. The new particles also had decay modes similar to the ordinary beta decay (eq. 2), *e.g.*

$$\Lambda \to P + e^- + \nu, \tag{5}$$

The beta decays of strange particles posed a very annoying problem, in that they seemed to be well described by Fermi's theory, but with a constant G' substantially smaller than the one determined from ordinary beta decay, $G' \approx 0.2\ G$. This result ran against the generally accepted belief, inspired by Fermi's analogy, that the strenght of weak interactions should be, as is the case for electromagnetic forces, universal in nature. I solved this problem in 1963 by proposing that in fact weak interactions are characterized by a single universal Fermi constant, but that their strenght is in effect shared in unequal terms between processes involving the usual particles – ordinary beta decay – and those involving strange particles. This sharing is determined by a new constant, θ and is essentially a quantum phenomenon, a mixing of normal and strange particles as acted upon by weak interactions.

The mixing phenomenon has a simple expression in the language of quarks. Beta decay (eq. 2) is due to a transformation of a *d* quark into an *u* quark, while the beta decay of strange particles (eq. 5) is due to the corresponding transformation of an *s* quark, the elementary processes being in the two cases:

$$d \to u + e^- + \nu,\ s \to u + e^- + \nu \tag{6}$$

Since the two transitions lead to the same final state, there must be a linear superposition of the two initial state which maximizes the transition amplitude, corresponding to a transition

$$(\cos(\theta)d + \sin(\theta)s) \rightarrow u + e^- + \nu, \qquad (7)$$

so that the Fermi constants for the two decays would be in the ratio $\cos(\theta)/\sin(\theta)$. My proposal of the weak mixing phenomenon led to detailed predictions on different decays of strange particles, which have been checked in a succession of experiments, lastly in 1998 when a team led by R. Winston at the Fermi Laboratory near Chicago was able to study in detail the beta decay of the Ξ^0 hyperon.

An essential ingredient in the transition to the modern unified theory appeared in 1969 when S. Glashow, J. Iliopoulos and L. Maiani showed that certain difficulties in the theory could be eliminated by the existence of a fourth quark, the *charm* quark, *c*, with clearly specified weak interactions.

The early seventies saw impressive experimental results: the discovery of the first particles containing the *charm* quark, and that of an entirely new form of weak interactions, the neutral current interaction, the hallmark of the unified model. The triumph of the unified model was sanctioned with the discovery, in 1982, of the *W* and *Z* bosons. In the course of the last decade experiments at the LEP $e^+ e^-$ collider at CERN have collected an impressive set of high precision checks of the unified electro-weak theory.

The mixing story has an interesting sequel. In 1973 Kobayashi and Maskawa noted that if six different types of quarks exist (only four where known at the time) the mixing phenomenon I had proposed would offer an explanation for the violation of the *CP* symmetry observed in 1964 by J. Cronin and V. Fitch in the decay of K^0 mesons. The mixing among six quark types is described by a matrix, the CKM matrix, whose elements determine the amplitude and phase of the possible quark transitions caused by weak interactions. A non-vanishing phase in one or more of the matrix element leads necessarily to the violation of the *CP* symmetry. It turns out that *CP* violating phases in the *CKM* matrix are closely interconnected and determined by a single parameter, so that the observation of *CP* violation in the *K*-meson decays leads to definite predictions for the *CP* violation which can be observed in other transitions, such as those which involve the heavier quarks.

The credibility of this proposal was boosted by the discovery, in 1976, of a fifth quark, the *b*, known as *beauty* quark. The sixth, the *top* quark, was discovered in 1996. This summer two different groups working at Stanford in the USA and at KEK in Japan have verified a crucial prediction of the theory, the existence of well defined violation of the *CP* symmetry in the decay of B^0 mesons. The exploration of *CP* violation phenomena represent today the top priority in the study of weak interactions phenomena.

The discovery of two different instances of neutrino oscillation opens up a large new chapter in the physics of weak interactions. Neutrino oscillations are in fact described by a mixing matrix which is the analogue of the *CKM* matrix for quark mixing. In 1978 I noted that neutrino oscillation can display *CP* violation phenomena, and that in fact *CP* violation represents a rule rather than the exception. Unravelling the subtleties of neutrino oscillation will require a protracted effort which is just now beginning.

Nicola Cabibbo
Cabibbo is Professor of Elementary Particle Physics at the Roma University "La Sapienza". He is President of the Pontificia Accademia delle Scienze. He has been President of the Istituto Nazionale di Fisica Nucleare (INFN) and of the Ente per le Nuove tecnologie, l'Energia e l'Ambiente (ENEA). He discovered the phenomenon of "quark mixing" in which a new fundamental constant appears: the "Cabibbo angle". Besides the field of the elementary particles he has obtained important results on the interaction of high energy electromagnetic radiation with crystals and on the trapping of magnetic flux quanta in superconductors. He has given important contributions in the realization of the parallel supercomputers now called APE. He is member of many academies in Italy and abroad.

Jay Orear

Enrico Fermi, the Man
Excerpts from some documents

One of the purposes of this talk is to give the audience a feeling for Enrico Fermi, his personality, sense of humor, etc. by making use of some of my memories plus the few recordings that are available of him via audio tape and film. Those who have never seen Fermi alive can get an idea of what he was like from the following sources: (1) the film *The World of Enrico Fermi*, (2) the video of the 10th anniversary of the first nuclear chain reaction produced by See It Now of CBS TV news, (3) the audio tape of Fermi's lecture entitled *Physics at Columbia University, The Genesis of The Nuclear Energy Project,* and (4) Fermi's personal notes and slides on his talk as retiring president of the American Physical Society in Jan. 1954. Items (1) and (2) contain live speeches by Fermi. Also I will show quotes and TV clips from a day-long symposium on Fermi held at Cornell University on Oct.14, 1991. These speakers knew him first hand and came to similar conclusions as what Fermi was like as a person. I will give some examples of Fermi's famous "intuition" and his remarkable sense of humor.

L'uomo Enrico Fermi

Estratti da alcuni documenti

Uno degli scopi di questa relazione è dare agli ascoltatori uno spaccato della sua personalità e del suo senso dell'umorismo attraverso alcuni dei miei ricordi e delle registrazioni su nastro e video. Coloro che non hanno mai visto Fermi da vivo possono farsene un'idea dal film "Il mondo di Enrico Fermi", dal video del decimo anniversario della realizzazione della prima reazione a catena, prodotto dalla See It Now della CBS Tv News, dalla registrazione su cassetta della relazione di Fermi "La Fisica alla Columbia University, Genesi del Progetto Energia Nucleare", dalle note di Fermi e dalle slide del suo discorso di addio dall'incarico di presidente della Società Americana di Fisica nel gennaio del 1954. Il film ed il video contengono discorsi dal vivo di Fermi. Mostrerò inoltre clip ed interventi tratti da un simposio su Fermi tenutosi alla Cornell University nell'ottobre del 1991. Gli oratori lo avevano conosciuto di persona e sono arrivati alle medesime conclusioni nel formulare un giudizio su Fermi come persona. Fornirò anche alcuni esempi della famosa "intuizione" e del notevole senso dell'umorismo di Fermi.

Introduction

One of the purposes of this talk is to give a feeling for Enrico Fermi the man, his personality, creativity, intuition, sense of humor, how he related to students as well as being a great scientist and teacher. The approach is to make use of my first hand memories and experiences as well as those of others. Some of his close friends have exchanged anecdotes and have spoken at meetings devoted to Fermi. There have been symposia, dedications, birthday celebrations and 3 reunions of his former grad students. Much of such material has gone unpublished. I have had the privilege to be (a) one of his last two grad students and postdocs, (b) the primary organizer of a Fermi symposium at Cornell University on Oct. 14, 1991, (c) the organizer of two of the three Fermi student reunions and (d) a speaker at 4 of the 100th birthday celebrations in 2001. So I am in a special position to put together new materials on Fermi. If these various first hand contacts come to common conclusions about Fermi's characteristics, then it is likely that those conclusions are correct. After the Cornell symposium several attendees suggested that I edit these new materials into a book before they were forgotten. Fortunately it was all video taped by the Cornell physics department and in my retirement I have finally found time to get it transcribed and and see how it fits in with other sources of knowledge about Fermi. The two main sources of similar material are the biographies by Laura Fermi and Emilio Segrè. My approach is mainly that as seen by a grad student and postdoc, whereas Laura Fermi's approach is as a wife and Segrè's relates more to his earlier career.

The Cornell Fermi Symposium, Oct. 14, 1991

In the early 1990's those most close to Enrico Fermi were rapidly dying off; *e.g.*, Laura Fermi, Herb Anderson, and Leona Marshall. A few others were alive, but some of those were no longer capable of giving a public lecture. By 1991 many of us felt that Fermi's contribution to the world was so exceptional that it should be well documented by first hand observers. Our goal was to invite all those close acquaintances before it was too late. The occasion of the 1991 Bethe lectureship at Cornell University provided a unique gathering of close first hand observers. Dick and Lois Garwin, Hans and Rose Bethe, Bob and Jane Wilson, Val and Lia Telegdi, Boyce and Jane McDaniel and Jay Orear would all be at the same place at the same time and that would be October 1991 in Ithaca, NY at the time of Dick Garwin's Bethe lectureship. Garwin agreed that it would be a good idea to invite the surviving Fermi acquaintances and spend one day of Garwin's Lectureship sharing our memories of Fermi. Since I was the chairman of the Bethe Lecture Committee, I was able to make the arrangements. In our early planning we invited Carl Sagan to be the master of ceremonies. Orear, Garwin and Sagan did most of the planning and organizing. We tried to invite all who had known Fermi personally and most of them were able to come. The program was as follows:

Welcome	Dale Corson, Chancellor	9 am
Introduction	Carl Sagan	9:10
Pilgrimages to Rome	Hans Bethe	9:30
Film and audio clips	Jay Orear	11
The Fountain in Rome	Joe McEvoy	11:20
Experiments In the '40's	Al Wattenberg	11:35
Lunch		noon-2 pm
Columbia, Los Alamos	Harold Agnew	2 pm
Pre-Chicago years	Bob Wilson	2:15
The Fermi family	Jane Wilson	2:35
Fermi at Chicago	Val Telegdi	2:50
Chicago – Los Alamos	Dick Garwin	4:10
Fermi & technology	John Peoples	4:30
Los Alamos inventions	Perce King	4:45
Reception and dinner break		5:45
A different perspective	Nella Fermi	8:00
Panel discussion	Rosenfeld, et al.	8:30

I think all the speakers came to the same assessment of Fermi as expressed by Val Telegdi: "None of the great scientists who worked at Chicago ever had a greater impact on his immediate and world-wide surroundings than did Enrico Fermi. Nobody in the history of modern physics possessed greater versatility than he. He had just as great achievements in pure theory as in concrete experimental work. He could with equal ease solve abstract problems or design and build with his own hands astonishingly useful experimental 'tools'.... To these qualities he added those of an exceptionally lucid lecturer and expositor. As well as an active and patient thesis supervisor. … But it defies the bounds of human inspiration to speculate that any other man or woman might have played Fermi's role as a teacher in the broader sense of this term. Through the influence of his students, Fermi effectively revolutionized the training of students in the United States and one hopes in the whole world".

This summary of Telegdi's must be correct if so many independently-minded first-hand observers would come to the same conclusions as they did at this symposium. I also feel, as does Telegdi, that scientists all over the world are being exposed to Fermi's way of looking at science and doing science. A similar and even stronger appraisal of Fermi was expressed by our first speaker, Hans Bethe.

He said:

> "My conversations with Fermi showed me a completely new approach to physics. I had studied with Sommerfeld, and Sommerfeld's style was to solve problems exactly. You would sit down and write down the differential equation. And then you would solve it, and that would take quite a long time; and then you got an exact solution. And that was very appropriate for electrodynamics, which Sommerfeld was very good at, but it was not appropriate at all for nuclear physics, which very soon entered all of our lives. Fermi did it very differently, and Dale Corson already described it very well, namely he would sit down and say, "Now, well, let us think about that question". And then he would take the problem apart, and then he would use first principles of physics, and very soon by having analyzed the problems and understood the main features, very soon he would get the answer. It changed my scientific life. It would not have been the same without having been with Fermi; *in fact I don't know whether I would have learned this easy approach to physics which Fermi practiced if I hadn't been there*".

Another sign of Fermi's strong positive influence on his students and others is the large number who became Nobel Prize winners. There were Lee and Yang for the correct theory of non-conservation of parity, Owen Chamberlain for the discovery of the antiproton, Jack Steinberger for the muon flavored neutrino, and Jerry Friedman for measurements of the quarks in electroproduction. Dick Garwin was also a student of Fermi and he led the experimental discovery of parity violation in pion-muon and muon-electron decay. The Nobel Prize has not been awarded for this, but it certainly should have. Jim Cronin was formally a grad student of Sam Allison. However his office was next door to Fermi's office and he frequently visited with us and attended Fermi's courses. Cronin received the Nobel Prize for the discovery of CP violation. Maria Mayer was not a Fermi student, however she was a young faculty member who consulted and worked with Fermi. She gives credit in her paper on the shell model to Enrico for supplying key ideas. I think it is fair to say that the shell model is a joint product of the two of them. Back in Fermi's Italian days Emilio Segre was a student and Hans Bethe was a postdoc. Both have received the Nobel Prize. This is a total of 10 followers of Fermi receiving Nobel Prizes in a short period of time. I don't know of any other physicist who has left such a strong mark on his followers. A possible 11^{th} is Murray Gell-Mann who joined Fermi on the Chicago faculty as a young instructor; Millie Dresselhaus has told me the story of how Fermi at a party had patted Murray on the back and predicted that he will become a Nobel prizewinner.

I think the probability that an existing Nobel prizewinner give birth to another winner is less than 1/10. So if this is a purely random process, the probability of one winner giving birth to 10 other winners would be one tenth to the 10^{th} power or one in ten billion which is essentially impossible. The explanation lies in the fact that Fermi was the best trainer or teacher of them all.

I was asked at the time of the Cornell symposium to edit a book presenting the dozen or so invited talks. The book that has evolved is one that attempts to reveal the real person, his personal traits, his sense of humor, his famous intuition and creativity. I have chosen those talks which help reveal Fermi's personality and I have devoted a chapter to each such speaker. Each talk is verbatim from the Cornell video tapes. I have at times added some comments of my own. This first part has evolved from my Cornell talk with considerable additional material and analysis consistent with the above goals. I am very grateful to the other speakers who so kindly let me use their papers. Also I wish to thank Cornell University and its Physics Department for their splendid cooperation in making the 1991 Symposium such a great success.

I have been invited at least 7 times to give talks about Enrico Fermi. In these talks I have drawn upon some of the following audio and video sources: (1) the 50 minute film *The World of Enrico Fermi* produced by Gerry Holton of Harvard, (2) the video of the 10^{th} anniversary of the first nuclear chain reaction produced by See It Now of CBS TV news, (3) the audio tape of Fermi's 1954 lecture entitled *Physics at Columbia University, The Genesis of The Nuclear Energy Project,* (4) Fermi's personal notes and slides on his talk as retiring president of the American Physical Society in Jan. 1954, and (5) *To Fermi – With Love*, an audio recording produced by Argonne National Lab making use of 16 friends of Fermi plus a commentator. The video See It Now contains live speeches by Fermi, Arthur Compton, Leo Szilard, and

Leona Marshall. Both videos show a re-enactment of the famous phone call of Arthur Compton to Vanover Bush which gave the good news to Washington using the code: “The Italian Navigator has safely arrived in the New World”.

My first meetings with Fermi

My first course with Fermi was Quantum Mechanics taken in the Fall Quarter of 1947. I was just one face out of many. But I really met him in a more unconventional way. That same Quarter I also had registered for a physical education course called Social Dancing. Early in the course one of the coeds in the class invited me to a dance party at a girlfriend’s house. As we were walking to the house that night she happened to mention the name of her girlfriend as Nella Fermi, an art major. I asked whether her friend was the daughter of the Fermi. Being an art major, my date had never heard of Enrico Fermi. But once I entered the door, I was greeted by the warm face of my quantum mechanics instructor. Fermi did recognize my face and he asked me what I thought of his quantum mechanics course. The party was a square dance with Harold Agnew as the caller. Many were Nella’s friends and Enrico’s co-workers. I was an indirect guest of Nella and not Enrico. I was invited as a friend of a friend of Fermi’s daughter. These Fermi square dances were held once a month. I was better than the average square dancer. From then on I was on the guest list of the Fermi family. The guest list was worked out by Nella, Laura, and Enrico. Harold Agnew did the calling and supplied the dance records. Both he and I have the impression that Nella and her father enjoyed working together in organizing those parties.

I can give an idea of what a good sport Enrico was by relating one experience at those monthly parties. Sometimes between the sets of dancing, there were party games. I proposed a group version of Twenty Questions. I suggested that the guesser be one of the best logical thinkers. So Enrico was chosen and he gladly agreed to step out of the room. Then I proposed to the rest of the crowd that we not choose any object for him to guess, but instead we answer “yes” if his guess ends in a vowel, “no” if his sentence ends in a consonant and “sometimes yes and sometimes no” if the sentence ends in a “y”. So we called Enrico back into the room and stood in a circle around him. He could choose anyone in the circle to answer his first yes or no question – and so on. He rather quickly realized that he should ask some redundant questions and then he remarked: “I think you have made up a story with some built-in contradictions". I replied to him: "How could we all come up with the same crazy story and be in complete agreement with each other?" He never did discover the vowel-consonant code and finally had to give up.

Not much later by coincidence I encountered Enrico skating by himself at a University ice skating rink. He greeted me and it seemed only natural to join him. It was clear that he enjoyed young people and we got to know each other fairly well in this and subsequent tête-à-têtes on ice. It was not beneath him to associate freely with students and to treat them as equals. In fact I think he enjoyed young physics students more than some of his older colleagues.

Another example of his enjoyment of young people was that he ate lunch in the large student cafeteria (the Hutchinson's Commons) rather than the Men's Faculty Club where most of his fellow faculty members ate. The center long table at the student cafeteria became known informally as the Fermi table; however anyone was welcome. Several of those who frequented that table later became Nobel Prize winners. In the Chicago physics department the younger grad students felt that some of the older grad students (like Lee, Yang, Chew, Goldberger, Garwin, Wolfenstein, Steinberger, Rosenbluth) were better teachers on the whole than the faculty at that time (except, of course, for Fermi who was clearly the best). Fermi was a modest person and liked to be treated as one of the crowd. Just to give one example of his modesty, even though one of his many great achievements was the discovery of Fermi statistics, he always referred to it as "Pauli statistics".

My coursework with Fermi

My next Fermi course a year or so later was when he first taught Nuclear Physics at Chicago. I had been studying and working problems with classmates such as Art Rosenfeld and Bob Schluter. We had a system of refining our classroom notes together and we realized that with a little extra effort, we could type them on mimeograph stencil sheet masters and make our class notes on nuclear physics available to the entire department. All three of us had training in touch-typing. The department chairman liked our proposal and offered to pay for the materials and we would provide free labor. During the first few days of preparing the wax stencil sheets my father suggested that we switch from mimeograph to photo-offset. Then it would be especially easier to make the many drawings and equations. My father recommended a firm in Michigan that charged almost the same as the mimeograph process. Whenever we got stuck we usually consulted T.D. Lee or Frank Yang. Only when their response was not satisfactory did we consult Fermi. As one might expect, in those rare cases when Lee and Yang could not understand a part of the lecture, then neither did Fermi. Fermi's office door was always wide open and any stranger or friend was always welcome to enter (as long as he or she observed the *no smoking* sign on his desk).

Many have remarked on how simple Fermi made things seem in his lectures. But then after the lecture it was not so simple to reconstruct his reasoning. I do not blame this on any over-simplifying on the part of Fermi. It is because understanding of physics requires many successive steps of not too obvious reasoning. For this reason Art, Bob and I would occupy a nearby empty classroom immediately following each Fermi lecture and try to make sure that we each really understood the lecture we had just heard. It usually took us more than an hour to convince ourselves that we understood the one-hour lecture.

When we made the choice to switch over to the easier and superior system of photo-offset we were not aware of another advantage: now the number of copies could be unlimited rather than restricted to about 500. It quickly became clear that the "whole world" wanted copies of these Fermi lecture notes. No nuclear physics book of this breadth or talent had yet appeared on the market. Fermi's contract with the University of Chicago required that any outside money he might earn must be given to the University. So the distribution and sales now were delegated to the University of Chicago Press. They paid me $333.34 and Rosenfeld and Schulter $333.33 each for our services. As Telegdi has pointed out, this way of teaching the whole world is just one of the ways Fermi has left his mark on almost all physicists.

Teaching of the Fermi approach was not restricted to the West. The U. of Chicago Press edition was copyrighted 1950, but a Russian language edition appeared in 1951 that was in violation of international copyright agreements. It was in widespread circulation in Eastern block nations. In fact a radical Chicago student who went to a Moscow peace conference brought back a copy for me.

A second-order effect that reached an even larger audience was a college textbook entitled *Fundamental Physics* by Jay Orear, published in 1961. I wrote in the Preface: "My greatest debt is to Enrico Fermi, who not only taught me much of the physics I know, but also how to approach it. As a teacher, Fermi was well known for his great ability to make the most difficult topics seem beautifully simple in a clear, direct way with little mathematics, but much physical insight. The goal I have been aiming at is to try to present the spirit and excitement of physics in the way that Fermi might have done". This first college physics textbook used no calculus. But 8 years later I wrote a second more advanced version which did teach calculus along with the physics.

I have one more personal example of how Fermi left his mark on the entire international physics community. Fifty years ago (or 2 years before Fermi's death) most physicists were not very knowledgeable about statistical inference. In my thesis I had to find the best 3-parameter fit to my data and also the errors of those parameters in order to get the pion-proton phase shifts and their errors. Fermi showed me a simple analytical method. I spent the summer of 1958 working with L. Alvarez and one of my assignments was to write what I had learned from Fermi about statistics as a UCRL report. These statistics notes were revised in 1982 as a Cornell preprint. Counting both editions, thousands of copies were distributed all over the world at no cost to scientists living in both sides of the iron curtain.

Here is one last anecdote of the grad student days where Fermi was treated as one of the gang. In those days the University of Chicago neighborhood was not as safe as one would like. Even Fermi's son Guilio had been attacked. And so had my brother. Art Rosenfeld and I had done some "research" on tear-gas guns which were disguised as fountain pens. We discussed things like that with Fermi and he agreed that a surprise object which could incapacitate the attacker would be of some advantage. So we ordered 3 such kits by mail for us. As soon as they arrived, Fermi took his into his machine shop and he modified the trigger mechanism so it would have a quicker response. Art and I were satisfied with the original design and we had some worries that Fermi's modification might some day backfire in his pocket.

In that same period I had found a mail order company that sold inexpensive dosimeters complete with battery chargers. I told Fermi that I felt a personal dosimeter was more important than a personal fallout shelter. The shelter could do harm if it contained some undiscovered radiation after an attack. And I had lived through the Bikini atom bomb tests where I returned to a ship with some remnant radiation zones that could be found with the help of my personal survey meter. I offered to include an extra kit for Fermi in my order and he agreed with my reasoning and gladly joined in on the order. At times like that we thought of him as a fellow grad student. Not only was he fascinated with new "gadgets" just as we were, but he really treated us as equals.

Fermi intuition

To me, intuition is a kind of mental telepathy and mental telepathy is supernatural; *i.e.*, by definition it is "outside of nature" - it does not exist. So now let me give you some examples of Fermi's famous "intuition". About one or so months after the Berkeley Bevatron had been running on both electronic and nuclear emulsion antiproton searches there were still no positive results. Murray Gell-Mann had just returned from Berkeley with these negative results, which he was relating to Fermi and me in the hall just outside our office doors. Murray said now we know there is no anti-proton. But Fermi said in a very definitive and loud manner: "There *IS* an antiproton". We grad students used to say that "Fermi had an inside track to God". Within a month of that definitive pronouncement Fermi was proven correct.

Another example was his explanation of the cosmic ray vs. nuclear emulsion data of neutral and charged V-particles plus the tau particle (a charged particle coming to rest and decaying into 3 charged pions). The measured masses of these particles differed by a few standard deviations. Both Fermi and I independently felt that God would not have created so many new bosons of almost the same mass. The simpler explanation was that these observations were different decay modes of the same particle and that some of the mass measurements must have had larger errors than claimed. Fermi supervised the Chicago nuclear emulsion group and we knew that nuclear emulsions could determine masses more accurately than the cloud chambers.

In 1953 Fermi taught a Particle Physics course. I sat in on the course and took detailed notes. My notes reveal two more examples of what might be called intuition. On my pages dated April 11, 1953 Fermi explains the intrinsic parity of the pion as two spin 1/2 sub-particles in an L=1 orbit around each other and with the intrinsic and orbital angular momenta opposed to give a total spin zero of the composite particle. A spatial inversion would give a change in sign or odd parity for the composite. This is the present quark model of the pion years ahead of its time. On pages of the same date Fermi gets even intrinsic parity for the neutrino in one reaction and odd intrinsic parity in another reaction. So two different parities for the same neutrino. (This is now known to be true). I asked Fermi in class: "Suppose there is an antineutrino in the other reaction?" He said, "Let me think about that". Later that day he called me into his office and said that he still has the problem that he gets both parities for the neutrino. He admitted that he still did not understand the neutrino. I like to speculate that if he had known about the two-component neutrino in the Pauli Notes, he might have beaten Lee and Yang by 3 years.

The most famous example of Fermi's so-called intuition has to do with his Nobel-prize winning discovery of how slow neutrons can produce larger amounts of artificial radioactivity. It is true that he was the first to slow down a beam of neutrons with a slab of paraffin. But there is at this time a dispute whether he first tried a lead filter with no result and then followed it with paraffin resulting in a hundred-fold increase in the induced radioactivity. On one side of the dispute is a famous quotation by Chandrasekhar. Chandrasekhar had told Segrè that Fermi told him in a conversation about the scientific method: "When finally, with some reluctance, I was going to put it [the lead filter] 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 advance warning*, no conscious prior reasoning". This is one of the reasons why we students would joke about Fermi having an inside track to God. Segrè who was in another room at that time doesn't seem to remember that detail, but he cannot trust his memory. Chandrasekhar admits he did not write down verbatim what Fermi said to him but he feels he can trust his memory.

On the other hand Laura Fermi in her *Atoms in the family* tells a different story. On page 98 she says: "They placed the neutron source outside the cylinder and interposed objects between them. A plate of lead made the activity increase slightly. Lead is a heavy substance. Fermi said, 'let's try a light one *next*, for instance, paraffin' ". Laura Fermi's book was proofread by her husband. Too bad that Enrico Fermi, Laura Fermi, Segrè, Pontecorvo, or Chandrasekhar are no longer available to settle this dispute. I can think of 5 reasons that support Laura Fermi's version (1) The Chandrasekhar version is admittedly not verbatim. (2) The Laura Fermi version is verbatim (she was writing a book while interviewing her husband and her husband did proofread her entire book). (3) It was a *lead* box which was giving Fermi and his group inconsistent results and which they supposedly decided at that time to study in a more systematic way. (4) The heavier elements gave more complications like artificial radioactivity (and even fission which they did not understand at the time) whereas the lighter elements did not. So it made more sense to start with that which is expected to give complications. (5) If Fermi at the last minute had changed their agreed upon logical plan without any warning to Segrè, Segrè would have been annoyed and have a reason for remembering something so out of Fermi's character. At the Rome Congress honoring Fermi's 100th birthday it seemed most of the audience was on the side of Chandrasekhar. However a compromise theory was proposed that the lead experiment was done the day before the paraffin was "impulsively" selected. Whether or not the lead was used, the fact that paraffin was selected early on is a good example of uncanny intuition.

One last example of good intuition is whether Fermi believed in the Fermi-Metropolis phase shifts as defined in paper 260 of the Collected Papers of E. Fermi, Vol. II, U. of Chicago Press, 1965. In the paper delivered by Val Telegdi at the Cornell Symposium, Telegdi says the Fermi-Metropolis fit "*favored* by Fermi did not correspond to the proposed resonance". What Telegdi should have said is that "the world data at that time favored the Fermi-Metropolis phase shifts but Fermi favored the resonance fit". It is true that the full set of world data at that time taken together gave a better goodness-of-fit to the Fermi-Metropolis solution than to the solution where the p-wave phase shift went through a resonance. And it was this resonance fit that Fermi personally *always* favored. In an earlier talk I remember Herb Anderson making a statement similar to Telegdi's. These statements might cause readers to rule out Fermi as the discoverer of the first excited state of the nucleon. What Telegdi and Anderson should have said is that in their paper the Fermi-Metropolis solution gives a better goodness-of-fit value than the resonance solution. One must keep in mind that Fermi and Metropolis were doing a fit to the *combined* world data. At that time the resonance solution fit every combination of world data until the first "measurement" of the pi plus- proton total cross section was reported from Columbia University. They reported a total cross section considerably smaller than required by a p-wave resonance. They had exposed nuclear emulsion to positive pions at the resonance energy at a position near the center of the Nevis cyclotron. It was a difficult experiment because of the heavy background and the scanning efficiency for finding all the elastic scatterings is expected to be low. Fermi and I felt all along that the scanning efficiency must have been lower than what the Columbia scanners had estimated. If the Columbia data could have been corrected for this then the Fermi-Metropolis fit would be ruled out. (Later experiments at the Cosmotron using external pi plus beams at and beyond the resonance energy proved that the Columbia cross section was way too low). Fermi was so confident that there was a resonance that he tried to repeat the Columbia experiment using the Chicago cyclotron with Horace Taft as the grad student in charge. This involved mounting some nuclear emulsions and shielding near the center of the vacuum tank where residual radiation levels were significant. Members of our nuclear emulsion group took turns working short shifts inside the tank. Of course we wore film badges and dosimeters and made sure no one was exposed to more than 300 mr per week. Fermi as a member of the group insisted on taking the same dosage as Taft, Orear, Rosenfeld, and others. (The others may have been Bob Swanson and Jerry Friedman). We pointed out to Fermi that he already had accumulated more lifetime dosage than we, and that we preferred that he not crawl inside the cyclotron as we were doing. But he was an egalitarian and he felt strongly about this and he was our boss. (Nobody had any hint that he would die from cancer in the following year). We did find some elastic scatterings in our exposures but we also found heavy background that would swamp out the signal at the needed exposure levels. So we were unable to disprove the Columbia experiment as the Cosmotron did shortly after Fermi died.

My last example is a case where his remarkable intuition failed him. He himself later referred to it as his great mistake. When his Rome group irradiated uranium with neutrons in 1934 they observed more than the usual amount of radioactivity. They had expected to get radioactivity from transuranic elements. But their chemistry and halflives didn't fit in a way so that they could prove that what they saw was due to transuranic elements. A German chemist, Ida Noddack, actually published that what they saw was fission, which seemed a wild idea at that time. Apparently her idea was too wild to be taken seriously. According to Segrè, Fermi's knowledge of nuclear energy states was such as to make him think fission was not possible. Fermi never wanted to publish an experimental result unless he was sure of it. If Fermi had published that he had seen fission, the half-sized pieces would have an excess of neutrons and these neutrons would give rise to more fissions most likely in a chain reaction. Then both Germany and the U.S. might have had atom bombs in time for World War II. The world should be grateful for this one mistake of Fermi! I like to make the following analogy between the two great Italian Navigators. The first in 1492 found a whole new world, but thought it was China; the second in 1934 found fission, but thought perhaps it was just transuranic isotopes.

Fermi humor

Hans Bethe in his talk at the Cornell Symposium gave an example of Fermi's humor when Fermi was at the age of 29. Not only was he a full professor, but he was a member of the Royal Academy with the title of His Excellency Fermi. The driveway to the Physics Institute also led to an important governmental department that sometimes had "classified" meetings and on such occasions the driveway was closed to the physics people. On one of those days Fermi came driving and when the guards stopped him he said "I am the driver to His Excellency Fermi. And His Excellency would be very annoyed if you didn't let me in". And as he told the story later, Fermi emphasized that he had told the whole truth: he was the driver to the Excellency Fermi, and indeed His Excellency *would* have been very annoyed.

Fermi chose to inject quite a bit of humor into his retirement lecture as the President of the APS (American Physical Society) on Jan. 29, 1954. On the next day Fermi gave a second lecture in honor of the 200th anniversary of Columbia University that he also sprinkled with humor. Both of these lectures give a good idea of his personality and style of humor. Unfortunately no audio or visual recording exists for the first, but the entire second lecture exists on audio tape and is transcribed in *Physics Today* and Segrè's book.

***(a)* The Ultimate Accelerator**

This is the unofficial title we physicists gave to the retiring president lecture. Fortunately Fermi typed out one page of notes for it with his own hands (he did know how to type). Parts of this one page are discussed below. We shall see that he does plan jokes days in advance and from the taped lecture where we can hear both Fermi laughter and audience laughter we note that he laughs heartily at his own jokes. As far as I can tell, the style of humor and delivery shown in these documents are just as I remember and to me they give some feeling of his humble, friendly and cheerful personality. My comments are in italics.

The first sentence of his page of notes says: "Congratulate Society on Loosing mediocre President and getting eccellent one". *(Spelling has not been corrected). This first joke is one of self-deprecation.*

Next sentence: "Counting number of papers... most active branches... solid state physics in which, perhaps mistakenly, we believe... nuclear Physics in which we cannot make that mistake. Since Yukawa…first suspected and then known…". *As a father of solid state physics and meson physics he can get away with criticizing them.*

Now he explains his criticism of nuclear physics: "But, to our dismay we got a lot more… many so called elementary particles… and because in addition… each… many names…number of names… stupendously great… even more than the number… which large enough". *He finds it humorous that there are even more names than there are particles.* "But to solve the mysteries higher energy data are needed. But cosmic rays above 25 BeV only one per cm^2 at an inconvenient location.

For these reasons… clamoring for higher and higher…”.

Fig. 4: Semi-log plot of beam energy vs. the year of accelerator completion. Courtesy Cornell accelerator group. Line is drawn through the highest proton energies obtained up to 1954. The accelerators after 1954 are also shown and they still tend to lie on the line determined by the pre-1954 accelerators.

In his talk his Fig. 4 was a similar plot of MeV versus time and also cost vs. time (of the existing accelerators showing extrapolation to 1994). Fermi's predicted value at 1994 is an energy of 5×10^9 MeV at a price of 170 B$. (Remarkably this energy could have been built in 1994, and at a lower price of about 11 B$ by using colliding beams. The highest energy colliding beams were achieved at Fermi National Accelerator Laboratory in 1988 at an equivalent beam energy of 2×10^9 MeV).

Fig. 5: Central Laboratory Building at Fermi National Laboratory where the world's highest equivalent beam energy of 2×10^9 MeV was achieved in 1988.

Next Fermi makes a preliminary design for a single ring proton accelerator of energy 5×10^9 MeV: “Preliminary design…8000 km, 20,000 gauss” *Such a single ring would give the desired energy, but the radius of 8000 km or 5000 mi would put the orbit 1000 mi above the surface of the earth! This is shown in Fermi's Slide 3. By now the audience must have been in hysterics. They were still talking about it when they came back to Chicago. Fermi's Slide 3 shows a single beam accelerator in orbit 1000 miles above the surface of the earth.*

“What we can learn impossible to guess… main element surprise… some things look for but see others… Look for multiple production… antinucleons…. strange particles…puzzle of long life times… large angular moment?… double formation? At present more probable…”.

Fermi's intuition was working well: this energy was achievable in 1994. A colliding beam version could have been built well under his estimated cost, but Congress ruled that the cost of ~10 B$ was too much. He was correct in predicting that the main element would be surprises (like strangeness, charm, and bottom and top quantum numbers, heavy leptons, electro-weak unification, the 6 quarks and 3 different kind of leptons, the fantastic success of the standard model, non-conservation of parity, etc). His preference for

"double formation" which is now called conservation of strangeness was also correct. I feel that Fermi was close to solving the puzzle of long lifetimes for strongly produced particles.
"…tried to photograph what I saw in the ball… and made slide.
Slide 5 – Strange particles in pion nucleon collisions.
…should realize this picture retouched…"

His Slide 5 must be Fermi's last joke in this talk. Unfortunately I was not able to find it among his papers.

***(b)* Physics at Columbia in the 1940's**

The following contains eight of the many jokes in this talk that are on tape. One can hear Fermi as well as the audience laughing while giving the joke. Sometimes he starts laughing before reaching the end of the joke.

1. "I don't know how many of you know Szilard; no doubt many of you do. He is certainly a very peculiar man, extremely intelligent *(laughter)*. I see that this is an understatement *(laughter)*. He is extremely brilliant and he seems somewhat to enjoy, at least that is the impression he gives to me, he seems to enjoy startling people".
2. "And in fact help came along to the tune of $6000 a few months after and the $6000 were used in order to buy huge amounts – or what seemed at that time when the eye of physicists had not yet been distorted – *(laughter)* what seemed at that time a huge amount of graphite. So physicists on the 7th 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)*. *(The film shown near the start of my talk showed a machinist stripped to the waist machining a block of graphite. It was producing a black cloud of graphite and the machinist was Fermi himself)*.
3. 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)*.
4. 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 – an in joke: Columbia's football team lost almost all its games)* 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?

Politics

After the first H-bomb test the possibility of a Cobalt bomb producing widespread radioactive contamination was rather obvious. Art Rosenfeld and I asked Fermi for his opinion on this and he spoke freely to us. He gave a response I did not expect. He said our military leaders would not rely on a weapon whose effects had never been tested and that the long range air patterns are too unpredictable. Now that I am older and perhaps wiser, I agree with Fermi on this.

Bob Wilson in his Cornell talk criticized the common opinion that Oppenheimer was more liberal than Fermi. (This is in agreement with a statement in Segrè's book that Fermi was more liberal than Oppenheimer, Condon, and Compton). Wilson gave the pending May-Johnson Bill on government control of atomic energy and research as an example. He, Fermi, and others felt that the May-Johnson would permit too much government secrecy in fundamental research. Wilson said that Oppenheimer was for it but ultimately Fermi strongly opposed it and supported an alternate civilian control bill. Here are the words used by Wilson in his Cornell Symposium talk:

> "Still, to my surprise, [Opppenheimer] was backing that bill [May-Johnson], and giving advice to Senator Fulbright. It mystified me. At that time, he also read a letter that he had from Fermi, who wasn't in Washington, then. Fermi, on the other hand, did criticize the bill, and did not approve it. He criticized it because of the secrecy measures, which he thought were too much, and he also criticized it because he thought it was overly organized, and that that would be something that keep the young scientists, particularly, from making suggestions and making inventions, and might dampen their creative abilities. I think that was absolutely correct. Well, okay for that. After the lunch, five of us young physicists were so impressed by Fulbright that we went around to see him. There was Curtis, from Oakridge, and Borst, and Rabivinich, from Chicago… I've forgotten all of them … a small tier of half a dozen or so who went to see Fulbright, and spent the whole afternoon with him. We found out that he was very astute in the way in which he was able to ask us questions about that which we had already made up our minds, and could advise him about. It was a nice afternoon. I got to know the other sci-

entists from the other laboratories – the Radiation Laboratory in Boston, and well as the people from Chicago. That evening (and I'm finally coming to my story!) a dinner had been arranged by Watson Davis, who you are probably not familiar with, but he was the person in Science Service, and he played a very valuable role in science at that time, and he was very much of a liberal person, and he helped in organizing the young scientists tremendously. In any case, he had the idea of having a dinner, at which there were scientists sitting around a rather large table, and between each scientist, was a politician. So, a senator, a scientist, a senator, a scientist, and all the way around (all senators, I believe, except for Wallace, who came, I believe, with a person named Neumann, a mathematician from Yale, who was to become very important). The kind of people who were there … there was Oppenheimer, and Fermi was now in town, and Szilard, Shapely… there were half a dozen young scallywags, of which I was one, and that made up the dinner. Well, it started off … of course the senators had heard about Oppenheimer, but not about Fermi .. so they asked a few questions of Oppy, about various aspects of nuclear bombs. One of them asked the question of Fermi, then. These were all social questions, about what we should do about nuclear energy. Could it be kept secret? Could other nations do that? These were bright people asking these questions. I'm afraid that Oppy, because of having to back the May-Johnson Bill before Fulbright, was a little confused, and so he tended to be a little bit wooly in responding, and he could be that without much trouble at all *(Audience laughter)*. Fermi was about the clearest, and could speak very simply and with great understanding, had been listening to us when we had been like gnats, arguing around him… my apprehensions had not been justified. The senators, when they heard the first answer from Fermi, with his clarity, directed all their questions to Fermi. There were a lot of questions and, each time they asked questions of Fermi, I had not much confidence, because I thought he'd say, 'Well, I don't really know about that. What do we know about sociology, what do we know about politics?' I could just see him doing that. (Audience laughter). He was not like that at all. He went right down our party line, without deviating in one way. Now, with Wallace present, with Fulbright present, with Toby, who was the senior Republican, there, I think it was one time that it made a big difference that somebody spoke out clearly and forcefully, and that man was Enrico Fermi".

Fortunately there were enough liberals in Congress to defeat the May-Johnson Bill. And when Oppenheimer's security clearance was revoked, Fermi testified on his behalf before Congress. Behind the scenes, Fermi privately tried without success to persuade Edward Teller not to testify against Oppenheimer.

Carl Sagan in his Cornell talk quoted a strong warning by Fermi not to make an H-bomb. Carl said: “In the October 1949 report of the General Advisory Committee to the U.S. Atomic Energy Commission, there was an addendum by Enrico Fermi and I. I. Rabi. This was a report on whether it was a good idea to build the first thermonuclear weapon, and the main report, signed by Robert Oppenheimer and others said, ‘The extreme danger to mankind, inherent in the proposal by Edward Teller and others, to develop a thermonuclear weapon, wholly outweighs any military advantage’ and the addendum, by Fermi and Rabi, made that point even more strongly. It said, “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. It is an evil thing.’ Which is, to my mind, a very strong statement” (end of Sagan’s quote). Again Fermi took a stronger position than Oppenheimer.

After World War II Fermi was in my opinion unjustly criticized by Communists and some liberals in Italy for his work on the A and H bombs. But during the war Fermi knew that we were in what was thought to be a close race with Germany in producing an A-bomb. It was believed that Germany had a head start. If Hitler had beaten us to the A-bomb, he could have forced a US surrender.

And after the war Fermi’s anti-H-bomb statements indicate that he advocated a joint US-Soviet agreement not to work on thermonuclear weapons. But again we were in an even closer race – this time with the Soviet Union. Stalin’s regime was the first to acquire and test a solid and compact H-bomb. It is not Fermi’s fault that the political leaders of both sides would not listen to scientists such as Fermi, Rabi, Bethe, Wilson and Szilard. It is a shame that some citizens of Italy were rejecting their modern day equivalent of Galileo. Fermi had brought Italian physics up from the bottom to the top in a very short time.

After the defeat of Hitler, Fermi and Oppenheimer were consulted by President Truman to choose between military use or a demonstration explosion of the first A-bomb. Fermi felt that the Japanese military leaders were in a kamakazi state of mind. They were too fanatical to be influenced by a test explosion. But surprise use on a city of military value might result in a surrender. And if not, it should be followed by a second city of military value plus an offer to let the people keep their emperor. This was an offer the emperor and the people could not refuse. At least in hindsight we see that Fermi gave advice that resulted in a prompt Japanese surrender and a savings of hundreds of thousands of lives.

Fermi and creativity

Perhaps the most famous example of Fermi's extraodinary creativity is his beta decay paper in 1933. Fermi first submitted it to *Nature* for publication, but it was rejected. The referees thought it was too far fetched and impossible. They didn't like the four-particle interaction which created an electron and neutrino out of nothing and they didn't like taking the neutrino so seriously. After Pauli had proposed the neutrino in 1930, most physicists thought of it as some kind of bookkeeping procedure. They didn't think of it as a "real" particle that had an interaction cross section. And Fermi's theory did predict a well defined energy-dependent interaction collision cross section with protons. Fermi liked to reason by analogy and he felt that if there could be electron-positron pair production in nature, there could also be electron-neutrino pair production.

He then submitted the paper to a less prestigious Italian journal where it was first published. Segrè on pages 73 and 74 comments: "Fermi's paper, written at the end of 1933 has stood the test of time with singular success; in fact, except for the nonconservtion of parity, even today very few changes would have to be made to it. …and his uncanny choice of the vector interaction was correct". (The most famous example of his extraordinary intuition).

In 1951 Fermi said in an unpublished speech: "Theoretical research may proceed on two tracks: 1. Collect experimental data, study it, hypothesize, make predictions, and then check. 2. Guess; if nature is kind and the guesser clever he may have success. The program I recommend lies nearer to the first track". He referred to track 2 as a big leap where great progress can be made all at once. He must have had his beta decay paper in mind as an example of "track 2". To me, it is an example of high creativity in science. I don't think any other physicist in 1933 was close to producing this theory of the weak interaction. But, like any other discovery, it would have come probably a few years later. Except in this remarkable case it took 25 years before others made the final improvement.

To me Fermi's weak interaction was a greater intellectual leap than Newton's checking the ratio of the acceleration of a falling apple to the falling Moon. Fermi was truly a great theoretical physicist, a great experimental physicist, a great teacher at all levels, and a great engineer. Newton was also a great theorist, experimentalist, mathematician, and engineer (I love his reflecting telescope), but perhaps not one of the best teachers. Maxwell and perhaps Galileo were in the same league as Fermi and Newton. They also were excellent in both theory and experiment.

Segrè has made a collection of the 270 most important papers of Fermi and published them in two volumes by the University of Chicago Press. Some of these papers have given birth to entire new fields of physics. Segrè also lists 13 books. I have read only a few of these papers and books. My short list of noteable discoveries is (1) the first understandable paper in quantum electrodynamics, (2) Fermi statistics and theory of solids, (3) the Thomas-Fermi model of the atom, (4) the weak interaction and beta decay theory, (5) neutron induced radioactivity which includes transuranic isotopes and not fully understood fission products, (6) first self-sustained nuclear reactors, (7) nuclear reactor patents and design, (8) neutron diffraction applications to solid state physics, (9) the A-bomb, (10) thermonuclear weapons, (11) pion beam designs, (12) his role in creating the nuclear shell model, (13) pion-proton elastic scattering, (14) discovery of the L=1 excited state of the proton, (15) acceleration of cosmic rays, (16) the approach to equilibrium.

In this paragraph I shall attempt to deal with the question of who is the best physicist in history. This is really a meaningless question unless the criteria for judging are made clear. Should it be the best theoretician of all time, the best experimentalist, or the best combined theoretician and experimentalist? Should technological contributions that are beneficial to the human race be counted? How about weapons technology that are helpful to one's country? But in spite of these difficulties, *Physics World*, the house organ of the British Institute of Physics did take a poll of its readers in December 1999 asking who is the best physicist in history without specifying any criteria. According to their results the top 10 physicists in history are: Einstein, Newton, Maxwell, Bohr, Heisenberg, Galileo, Feynman, Dirac, Schrodinger, Rutherford. I was disappointed that Fermi was nowhere on the list. At that same time *Time* magazine named Einstein as its person of the century and put him on the front cover of their centennial issue. I am very happy with Einstein being chosen by non-scientists, most of whom never heard of Fermi. If the criteria were that the physicist must be tops in theory, experiment, engineering, teaching that is felt over the entire planet, no mixing of science with the supernatural, and beneficial contributions to mankind then I think I might choose Fermi. But I am not enough of a historian of science to make expert comparisons with Maxwell and Galileo. I have read a book of Einstein quotations and I do not agree with all of them. Also I feel that quantum mechanics would not work if "God did not play dice with the universe". Einstein made some great discoveries in theory, but he was not an experimentalist or engineer. It has been said that he was not even aware of the relevance of the Michelson-

Morley experiments to special relativity. Most physicists feel that quantum mechanics is much more important to physics than gravitation.

I know that highly creative people in physics tend not to learn by studying textbooks in the conventional manner. Instead they try to work all the interesting problems. If such a person has trouble with a problem, he then goes to that part of the text. We know that Fermi used books in such a manner. I also know that Lee and Yang studied together in such a manner. I recently learned that Fermi when he was 16 and 17 learned much of physics from a 5000 page set of volumes by the Russian Chwolson. He first did a quick run-through the French edition to eliminate the 1000 pages he already knew. Then he spent several months on the remaining 4000 pages until he had mastered them. I do not know whether he used the method of working back from the problem sections. I do know that through most of his life if he was told of a new discovery, he would work it out for himself first in order to achieve a true understanding.

Educators are interested in how to train for creativity. Perhaps they could get some clues by studying the methods used by people like Fermi, Garwin, Lee and Yang when they were young.

Jay Orear
Professor Emeritus of Physics, Cornell University Graduate student U. of Chicago Sept. 1946 to June 1953 receiving Ph.D. in particle physics under supervision of Enrico Fermi. Post-Ph.D. Research Associate of Enrico Fermi June 1953 to Aug. 1954.
Instructor and then Assistant Professor, Columbia University Sept. 1954 to June 1958.
Associate to full Professor 1958 to 1995, Cornell University. Then Emeritus Professor.
Associations with Fermi: My Ph.D. was to confirm the pion-proton phase shifts discovered by Fermi. He was also the first to measure the signs of those phase shifts. Authored "Notes on Statistics for Physicists" 1958, revised 1982 (based on Fermi conversations).
Co authored "Nuclear Physics" 1949 U. of Chicago Press (based on notes of Enrico Fermi's course with A. Rosenfeld and R. Schluter). Fundamental Physics, John Wiley & Sons 1963 and Physics, Macmillan, 1979 (college introductory textbooks with a Fermi approach). With help from R. Garwin and C. Sagan organized a symposium "Memories of E. Fermi", Oct. 1991 at Cornell University. Organized reunions of Fermi thesis students Oct.13, 1991 and Dec.3, 1992.

John L. Heilbron

Experimental Nuclear Physics in the Thirties and Forties

Elementary particle physics, long the exemplar of big science, has also cultivated a reputation for purity. It traces these character traits to the nuclear physics of the 1930s and 1940s, when, however, purity and bigness did not frequently coincide. Big nuclear science, as represented most conspicuously by E.O. Lawrence's cyclotron laboratory at Berkeley, pursued philanthropic and industrial support and gave promises and sometimes realizations of useful applications in return. None of the fundamental experimental discoveries in particle physics during the 1930s – the neutron, the deuteron, artificial radioactivity, neutron activation, fission – was made in a cyclotron laboratory; and only one – the transmutation of light nuclei by proton bombardment – was made with any sort of accelerator. Those who made the major discoveries, among whom Fermi's group in Rome stood out, used small-scale, desk top apparatus. The two directions in experimental nuclear physics came together during the Manhattan project like the separated parts of a critical mass. In the late 1940s, the cyclotron became the tool of choice for many particle physicists. This development was abetted by a new design created during the last war years, by the replacement of cyclotrons by piles in the quantity production of isotopes, and by plenty of government money.

La fisica nucleare sperimentale negli anni Trenta e Quaranta

La fisica delle particelle elementari, per lungo tempo modello della "big science", ha coltivato anche una sua reputazione di purezza. Tali tratti caratteristici risalgono alla fisica nucleare degli anni 30 e 40, un'epoca in cui peraltro purezza e grandezza non coincidevano frequentemente.
La "big science" nucleare, come rappresentata efficacemente dal Laboratorio del Ciclotrone di E.O. Lawrence a Berkeley, era in cerca di sostegni filantropici e industriali, in cambio dei quali prometteva e talvolta realizzava utili applicazioni.
Nessuna delle fondamentali scoperte sperimentali della fisica delle particelle avvenute negli anni 30 – il neutrone, il deuterio, la radioattività artificiale, l'attivazione neutronica, la fissione – fu realizzata in un laboratorio del ciclotrone, ed una sola (la transmutazione di nuclei leggeri tramite bombardamento protonico) fu fatta con qualche tipo di acceleratore. Coloro che realizzarono tali scoperte, tra i quali spiccava il gruppo di Fermi a Roma, utilizzarono macchinari a piccola scala, da tavolo.
Le due direzioni della fisica nucleare sperimentale conversero nel Progetto Manhattan, come le due parti separate di una massa critica. Alla fine degli anni 40 il ciclotrone divenne lo strumento principe per molti fisici delle particelle. Tale sviluppo fu favorito da un nuovo disegno tecnico sorto negli ultimi anni del conflitto, dalla sostituzione dei ciclotroni con le pile nella produzione degli isotopi e dai consistenti finanziamenti governativi.

Cyclotron laboratories did not make the big discoveries in the 1930s

It is a great honor and greater challenge to speak to you about a subject in which Enrico Fermi played so prominent a part, and about which so much has been written and said. I have no credentials for the task except a long-standing interest, awakened in a course on quantum mechanics I took with Emilio Segrè. He taught from the English translation of the textbook used in Rome in the 1930s. My class came to know this book, by Fermi's boyhood friend Enrico Persico, by heart. Our unusual devotion was inspired by Segrè's periodic absences from Berkeley to visit Fermi, then terminally ill in Chicago. Each time he left, Segrè threatened to give us an examination on his return. Instead he would tell us about Fermi.

Segrè and Fermi quickly accommodated to what generous people call American culture. They fit in perfectly with the gigantic physics of World War II and made good use of large accelerators and reactors. But they retained their preference and ability to work in small groups. It is only slightly over-schematic to say that they fused the small-group, low-cost, physics research of prewar Rome with the interdisciplinary high-cost cyclotron development pioneered by Ernest Lawrence in Berkeley.

Particle accelerators came to dominate experimental "high-energy" physics in the late 1940s. By then they could achieve energies and intensities that made possible the creation of particles previously observable only, and infrequently, in cosmic rays; and the physicists who operated them had become more interested in exploiting the power of the machine than increasing it. The war freed cyclotroneers to do physics. During the 1930s, Lawrence had to raise funds from philanthropic organizations or government agencies concerned with medical problems. Most of the operating time of the Berkeley cyclotrons went to manufacturing isotopes of biological, chemical, or pharmaceutical interest. Reactors developed during the war took over these manufacturing jobs after it.

Cyclotron laboratories did not make the big discoveries – the Nobel-prize winning discoveries – in nuclear physics in the 1930s. The prize-winning discoverers used apparatus of traditional desk-top size and modest cost. Without the obligation to improve their instrumentation beyond what was necessary to the object in hand, or to develop large means of production to meet the expectations of funders, European nuclear physicists had the leisure to discover the neutron, artificial radioactivity, neutron excitation, and fission. The only major discovery made at a cyclotron laboratory was the transmutation of light nuclei by protons accelerated in the machine built by John

Cockcroft and E.T.S. Walton at Cambridge. This apparent exception in fact reinforces the rule. Cockcroft and Walton continued to tinker with their accelerator after it had become capable, both in fact and theory, of disintegrating lithium; and they would have kept at their improvements indefinitely if their boss, Ernest Rutherford, had not ordered them to try the experiment for which they had built the machine and he had raised the money.

Enrico Fermi's group did not need orders to do physics. Their single-minded pursuit of neutron activation was the exemplar of small-scale nuclear physics during the 1930s. It came to an end owing to the dispersal of the group on, and even before, Fermi's emigration to the United States in 1938. But by then the vein that they had mined had played out. Fermi's next moves – across the Atlantic and to the problem of realizing a controlled self-sustaining chain reaction – placed him within or in charge of a large crew and apparatus. That marked a turning point not only in exploitating atomic energy but also in bringing physicists broadened in small-scale work into collaboration with machine builders narrowed by large-scale constructions. The strength of the combination revealed itself in the atomic bomb and, less dramatically, in the rapid postwar progress of accelerator physics in the United States.

My purpose today is to illustrate this general development by comparing Fermi's group with Lawrence's. The leaders, Fermi and Lawrence, were the same age – Lawrence too is celebrating his centennial this year. Both were charismatic and convivial and commanded the loyalty of their coworkers. Each received the Nobel prize in physics and other high honors. National laboratories in the United States have been named after them. They reached the highest levels of governmental advising and generally agreed about nuclear policy. As scientists, however, they had little in common. Fermi took up nuclear physics deliberately, Lawrence by chance. Fermi stayed a theorist, Lawrence became a fund raiser. Fermi had a traditional liberal education and spoke several languages. Lawrence knew neither languages nor cultures. I can also report that Lawrence was tall and Fermi short – a metrological difference that would not be worth mentioning had it not extended to most of their close associates. The Berkeley group – Lawrence, Edwin McMillan, Luis Alvarez, and Glenn Seaborg – all were over ten percent taller than Fermi, Segrè, Edoardo Amaldi, and Oscar d'Agostino. The men matched their machines. Franco Rassetti escapes the generalization, but then he marched to his own tune.

Both groups took an interest in themselves. We have autobiographies of Alvarez, Amaldi, Seaborg, and Segrè, biographies of Fermi by his wife and by Segrè, and much about Lawrence from Alvarez and Seaborg. Biographers and historians from outside the groups have also contributed their bits. We do not lack information for a comparative history broader and deeper than I have time to suggest.

The "Golden Thirties"

In Rome

The Physics Institute on the Via Panisperna boasted an establishment to rival the Vatican. It had an Eternal Father in the person of its director, Orso Mario Corbino, who protected Fermi's group from academic and worse politics, and procured funds and assistance as necessary; a Divine Providence, that is, G.C. Trabacchi, who furnished critical radium emanation for the group's radon-beryllium neutron source from the School of Public Heath; a Pope, Fermi himself, infallible in physics; a Cardinal Vicar, Rassetti, who sometimes substituted for the pope; a Grand Inquisitor, Ettore Majorana, never satisfied with an answer; a Cardinal De Propaganda Fidei, Persico, so-called for his textbook and other advertisements of quantum theory; and a Prefect of the Libraries, Segrè, a connoisseur of the physics literature, also known for his touchiness as the basilisk, a fabulous beast that can freeze other animals by the chill of its glance.[1]

Fermi's little church had switched its attention from the periphery of the atom to its nucleus during the early 1930s in the hope of establishing itself, and thereby Italy, as a leader in some branch of physics. In a famous speech of 1929 no doubt formulated with Fermi's help, Corbino had pointed to nuclear physics as the obvious new frontier; and his insistence that the necessary experiments would have to be led by theory implied an advantage to the Rome group under the infallible Fermi. Still, Corbino's speech, which also hinted at the possible exploitation of atomic energy, scarcely made a program, and Fermi's group prepared for its transition by visiting other laboratories in Europe and the United States to learn experimental techniques and foreign languages that might come in handy. Then, in 1931, they held an international conference on nuclear physics, the first ever, which brought the leading brains engaged with the nucleus to Rome for easier picking. The discovery of the neutron in 1932 at last gave them a secure direction.[2]

---

1 HOLTON, *Minerva*, 12 (1970), 194-5; L. FERMI, *Atoms* (1954), 46-8; SEGRÈ, *Mind* (1993), 51.

2 HOLTON, *Minerva*, 12 (1974), 181, 183, 186; SEGRÈ, *Fermi* (1970), 65-8.

They had already prepared a radon-beryllium source and various detectors when they heard about the discovery of excited radioactivity by Frédéric Joliot and Irène Curie early in 1934. Joliot and Curie had used alpha particles from natural sources to create an activity lasting a few minutes. Fermi saw that neutrons would be a more effective agent than alpha particles, since they would not be repelled by the nucleus's positive charge; an inference sufficiently obvious but not, therefore, an obvious reason to replace alpha particles by neutrons in the experiment. The neutron yield from a radon-beryllium source is very low; and it took some imagination to suppose that the greater efficiency of neutrons more than compensated for their lesser number. The Rome group made a concerted effort to irradiate samples of every element they could procure. In their rush they fell off the periodic table. They identified some activities they had provoked in uranium with elements 93 and 94, to which they gave the ancient names ausonium and hesperium. They were in fact fission fragments.[3] The true ausonium and hesperium are neptunium and plutonium, first made on the earth by a cyclotron in Berkeley.

The yield of new isotopes accelerated in October 1934 when Fermi made a discovery that he later rated as his most important.[4] For reasons he could never specify adequately, he placed a piece of paraffin in the path of the neutrons from his Rn-Be source. The emerging or moderated neutrons were far more effective than those that fell directly on the target. Fermi reasoned that the neutrons lost energy in the collision with hydrogen atoms in the wax and that slow neutrons made better *agents provacateurs* than fast ones. The inference went against experience with charged particles, whose capacity to provoke nuclear reactions increased rapidly with their energy. The discovery of the different efficiencies of fast and slow neutrons had literally earth-shaking consequences. The theory of atomic piles and nuclear bombs depends critically on an understanding of the relative probabilities of fission and capture as a function of the velocity of the impacting neutrons.[5]

On Corbino's urging, the Rome group took steps to share in the profit of any future industrial exploitation of neutron activation. They applied almost immediately after their discovery of the peculiar efficiency of lethargic neutrons for an Italian patent on the process and also on some of the products.

---

[3] Segrè, *Fermi* (1970), 72-8; Amaldi, in Weiner, *History* (1977), 298-316; Amaldi, *Riv. stor. sci.*, 1 (1984), 3-23.

[4] S. Chandrasekhar, quoting a conversation with Fermi, in Fermi, *Coll. papers*, 2 (1965), 296-7, and Segrè, *Fermi* (1970), 80. Cf. Oscar d'Agostino, in Cardone and Mignani, *Fermi* (200), 69-73.

[5] Holton, *Minerva*, 12 (1970), 160.

They received the patent in 1935 and soon secured it world-wide. After the war they pressed their claims against the U.S. government, which had used their patented process in the piles that made plutonium. Eventually the U.S. Atomic Energy Commission (AEC) paid $400,000 for the patent, not much, perhaps, for an invention of world-historical importance, but over ten times the cost of all the experiments in nuclear physics done by the Rome group during the 1930s.[6]

The group began to disintegrate in 1935. That year Segrè won the chair in physics at the University of Palermo. Secondary members of the group drifted away. In 1938 Fermi went to Stockholm to receive his Nobel prize – essentially for the achievements recorded in the patent application of 1935 plus the discoveries of ausonium and hesperium – and continued from the ceremony to a professorship in Columbia University in New York City. His wife was Jewish. Segrè, also Jewish, was visiting Berkeley when Mussolini proclaimed the racial laws; he decided to remain in Lawrence's Lab and sent for his family. Rassetti, though without Jewish connections, also emigrated, to Canada, and later to the United States. He did not adjust to the American postwar regime in nuclear physics and, although hired as a professor of physics, preferred to indulge an interest in natural history that he had cultivated since childhood.[7] Big science is not for everyone.

Fermi had spent the summer of 1935 in Ann Arbor, Michigan, where European physicists gathered annually for a summer school. There he received a letter from Lawrence containing a milligram of radiosodium made at the cyclotron. Fermi could scarcely believe his counters. He had expected a microgram. In 1936 Segrè went to Berkeley to see the machine that could make so magnificent an activity. He found nothing there to resemble the great laboratories and leading physicists of Europe. Lawrence was cordial, open, boyish, enthusiastic, and generous; he invited Segrè to dinner and gave him some old cyclotron parts to take back to Italy.[8]

Neither Lawrence nor his collaborators thought that this scrap might contain anything useful that they could not recreate, much enlarged, in the upgraded cyclotron they then were building. Back in Palermo Segrè separated some radioactive phosphorus from the scrap and gave it to his colleagues in biology

[6] SEGRÈ, *Mind* (1993), 244-7.

[7] AMALDI, *Da via Panisperna* (1997), 69-80; BATTIMELLI and DE MARIA, in ibid., 17-37; AMALDI, *20th century physics* (1998), 147-52, 173-80.

[8] SEGRÈ, *Mind* (1993), 112-16.

to feed to rats. Early in 1937 he received another nice piece of cyclotron junk. He digested it with the help of Palermo's professor of metallurgy, Carlo Perrier. Among its many radioactive products they identified iosotopes of the missing element of atomic number 43, which they later named technetium. "The cyclotron evidently proves to be a sort of hen laying golden eggs", the basilisk wrote Lawrence announcing the Nobel-prizeworthy discovery. He and Perrier made the same point more soberly in the paper describing their work. "[T]his research, carried on months after the end of the irradiation and thousands of miles from the cyclotron, may help to show the tremendous possibilities of the instrument".[9] Lawrence responded with characteristic enthusiasm to Segrè's creative use of cyclotron scrap. What impressed him most, however, was not the identification of technetium – experience had taught him to be wary about claims based on complex radiochemistry – but the resourceful use of what he regarded as "trivial amounts of radiophosphorus".[10]

In Berkeley

In 1935, just before Lawrence sent Fermi the amazing millicurie of radiosodium, Lawrence had set his sights on a minimum production of 10 mCi a day. In the late summer of 1937, after many changes in design, the Berkeley cyclotron, now with pole pieces 37 inches in diameter, could deliver more than a curie of radiosodium, over a million times what Fermi had considered a useful amount two years earlier. And, in marked contrast to earlier models, the 37-inch ran reliably, much to the irritation of Berkeley's chemists, who could detect its neutrons in their laboratories 100 meters away.[11]

We know the main reason for the scramble to improve the machine and, after its attainment of reliability as well as power, to regiment its builders in shifts to oversee its performance. Lawrence had promised something to the foundations that supported his work: radioactive tracers for biological research, large quantities of radioisotopes for pharmaceuticals, and strong particle beams for the direct radiation of tumors.[12] Similar indications and expectations paid for the building of cyclotrons by Lawrence's students outside of Berkeley.[13] But even the big beam took a day and a half to make the

[9] Heilbron and Seidel, Lawrence (1989), 365-7; hereafter cited as *HS*.

[10] *HS*, 367

[11] *HS,* 271, 277.

[12] *HS,* 214-18.

[13] *HS,* 266-8.

amount of radiophosphorus needed for a single therapeutic dose. That, together with the constant work to keep the machine going and improve its productivity, occupied most of the time of Lawrence's group. As the cyclotron gained strength, however, so did the desire of a few of its attendants to do some physics with it. But how to pay the bills and do science simultaneously? Robert Wilson, then beginning the career that would make him the builder of Fermi Lab, found a way. He put small probes of material to be made hot for the doctors inside the vacuum chamber, where the beam current was strongest, and left the diminished emergent beam for physics and chemistry. This win-win situation did not produce as much science as it promised, however, because by the time Wilson had made his case, the laboratory had become preoccupied with building a super-cyclotron with 60-inch pole pieces. Amaldi passed through Berkeley in 1939 as the behemoth neared completion. He was impressed. He hoped, in vain as it turned out, to build a similar colossus as part of Mussolini's grand universal exposition of 1942.[14]

When news of the discovery of fission reached Berkeley toward the end of January 1939, the 60-inch serfs could not be kept at their tasks. J. Robert Oppenheimer, who, with his students, did the theoretical work, such as it was, of the laboratory, immediately proved that fission was impossible. That did not stop Luis Alvarez, whose acute mind seldom missed an opportunity, from looking for fission fragments created by neutrons in the 37-inch cyclotron. He found them using an ionization counter. Others obtained nice tracks of receding fragments in a cloud chamber. Oppenheimer changed his diagnosis from "impossible" to "unbelievable". By this time the call of the 60-inch could not be ignored. The laboratory did not follow up the intriguing question, raised by physicists around the world, whether fission released more neutrons than required to induce it. Strong indications that fission gave around two electrons on average for every one absorbed were obtained by, among others, Joliot in Paris and Fermi in Columbia using Rn-Be sources.[15]

Berkeley's response to fission – immediately confirming and enlarging a discovery made in Europe – had an air of déjà-vu. The same thing had happened in the case of artificial radioactivity. As soon as news of the results of Joliot and Curie arrived in Berkeley, Lawrence checked them with deuterons accelerated in the cyclotron. "To our surprise [he wrote] we found that everything we bombarded... is radioactive". The cyclotron had been making

[14] *HS*, 279-80; AMALDI, *Da via Panisperna* (1997), 75-6, 121.

[15] *HS*, 441-7.

artificial radioisotopes for months; Lawrence could have made the discovery of Joliot and Curie by examining the fillings in his teeth. "We looked pretty silly [one of Lawrence's junior collaborators wrote]. We could have made the discovery at any time".[16]

The discovery of artificial radioactivity created a program at Berkeley similar to that of Fermi's group in Rome – with notable differences. The cyclotroneers obtained their neutrons from a process identified by Rutherford and Cockcroft while trying to understand some odd results obtained in Berkeley. This process fused deuterons to make helium-3 and a neutron (or hydrogen-3 and a proton). Merely by letting ionized heavy hydrogen loose in the cyclotron, the Berkeley group made fast neutrons in plenty and closed off all probable paths to the discovery of the efficacy of slow ones.

Lawrence sprayed his fast neutrons on elements that might be transformed into what he called "synthetic radium".[17] In September 1934 he found what he wanted. He bombarded table salt with deuterons and obtained, by (d,p), radioactive Na-24, which Fermi's group had already made in two different ways, by (n,α) on aluminum and (n,p) on magnesium. But whereas Fermi merely pointed to the reactions and moved on, Lawrence seized upon radiosodium as a place to linger. He also tried to patent his (d,p) process to elude Fermi's patent on Na-24 made via neutron excitation. The patent examiner would not allow it for technical reasons having nothing to do with the Rome work. It did not seem an important setback to Lawrence. He had already patented the cyclotron. Since he believed that the cyclotron would remain the only neans of producing radioisotopes in abundance, he thought himself protected sufficiently. He worried rather about how to increase productivity.[18]

With men like McMillan and Alvarez, who joined Lawrence in the mid 1930s, the Berkeley laboratory could not limit itself to the improvement of apparatus, the manufacture of radioelements, and the hunt for new reactions. The detection of the mass-three isobars produced by deuteron fusion indicates the sort of physics the lab managed to sandwich between its bread-and-butter activities. Energy measurements indicated that He-3 slightly exeeded H-3 in weight. Majority opinion therefore held the helium isotope to be

[16] *HS*, 178-9.
[17] *HS*, 186.
[18] *HS*, 196-7.

radioactive and the hydrogen isotope stable. The authoritative Hans Bethe assigned He-3 a half-life of around 5000 years. Alvarez decided to employ the new 60-inch cyclotron, idle for want of adequate shielding, in a search for stable H-3. While tuning the machine, he noticed a burst of mass-three particles when the source was not hydrogen but helium derived from a well in Texas where it had lain for geologic ages. He-3 turned out to be the stable isobar. Alvarez then sought evidence of a radioactive H-3 in the problematic activities discovered in Berkeley. He found it in a product earlier obtained by McMillan, which had been misidentified as an isotope of beryllium. Lawrence applied this discovery immediately. He wrote to a major supporter, the Rockefeller Foundation: "Radioactively labelled hydrogen opens up a tremendously wide and fruitful field of investigation in all biology and chemistry".[19]

The Big-Time Forties

The bombs of Berkeley and Chicago

The possibility of fission bombs did not excite the leaders of nuclear physics in the United States in 1940. In so far as they thought the release of atomic energy likely, they favored engineering (and controlling!) a chain reaction in natural uranium. They changed their minds in the fall of 1941 on learning two results obtained by refugee physicists living in England: the calculations of Otto Frisch and Rudolf Peierls, which promised a big bang with only ten kilograms of U-235, and Franz Simon's estimate that a gaseous diffusion plant covering only forty acres could produce a kilogram of pure U-235 a day. Lawrence immediately proposed to study the electromagnetic separation of the uranium isotopes on a grand scale, using cyclotroneering techniques and a huge new magnet – with pole pieces 184 inches in diameter – that he was building with a grant from the Rockefeller Foundation. Indeed, only by mobilizing the magnet for war could he obtain the steel and other strategic war materials needed to complete it. He asked for and received $400,000 from the government, the easiest money he had ever raised.[20]

Meanwhile, Fermi had been investigating the possibility of a self-sustaining chain reaction in natural uranium interspersed with a graphite moderator. It

[19] *HS*, 368-73; ALVAREZ, *Adventures* (1987), 68-70.

[20] HEWLETT and ANDERSON, *New world* (1962), 33-44; RHODES, *Making (*1986), 320-5, 339-40; *HS*, 504-9.

was understood that a product of the pile would be plutonium and that plutonium might be as explosive as U-235. Fermi, Segrè, and Lawrence met to plan the use of the 60-inch cyclotron to make enough plutonium to investigate its fission properties. The work, done by Segrè and Seaborg, made clear that plutonium could serve in a bomb. Then came Pearl Harbor, a flood of money, transfer of the Columbia work and personnel to Chicago, and the assembly, under Fermi's direction, of the first self-sustaining pile. It went critical almost exactly a year after the United States declared war on Japan and Germany.[21]

The two projects then went forward in parallel as part of the Manhattan Engineer District, the code name given to the bomb business when the U.S. Army Engineers took it over in 1942. Fermi's project industrialized under Dupont and moved to Hanford, Washington; although he lost control of development and criticized some decisions of Dupont's engineers, he remained close to the project and continued to make contributions to it. Lawrence and his engineer-physicists guided the electromagnetic separation work during its development phase and at Oak Ridge, Tenessee; but eventually it too was industrialized. Gigantic production plants arose in those remote places to house and service the uranium piles, plutonium separation tanks, and "calutrons", or banks of mass spectrographs, for the electromagnetic separation of U-235. The Hanford industry based on Fermi's pile produced all the material for the bomb that destroyed Nagasaki. Lawrence's calutrons supplied some of the enriched uranium for the Hiroshima bomb.[22]

Fermi and Lawrence remained high in the councils of the Manhattan District because of their essential contributions to it and because General Groves, the project's supreme commander, liked and trusted them. The general found in each, and in abundance, the qualities needed to push the work to success. Lawrence provided enthusiasm, unquestioning patriotism, and an undefeatable conviction that all technical obstacles could be overcome. Fermi provided scrupulously honest, even downplayed, technical assessments, authoritative opinions, and uncannily clever ideas. In return for the general's trust and in keeping with their middle-class backgrounds and high responsibilities, Fermi and Lawrence identified more strongly with the military than most of their colleagues in the Manhattan District. Perhaps for this

---

21 Hewlett and Anderson, *New world* (1962), 54-6, 68, 88-9, 108-13; Rhodes, *Making* (1986), 395-401, 428-42; Holl, *Argonne* (1997), 15-20.

22 Hewlett and Anderson, *New world* (1962), 105-7, 112-14, 129, 141-59, 184-94, 207-24; Rhodes, *Making* (1986), 407-15, 431-2, 487-500.

reason as well as for his power of reasoning Fermi ranked with the directors of the District's major laboratories – Oppenheimer at Los Alamos, Lawrence at Berkeley, and Arthur Compton at Chicago – and, like them, had the constant company of a bodyguard.[23]

In the spring of 1945, the Secretary of War asked the three directors and Fermi to serve as a panel of experts to an Interim Committee he had set up to advise him about the use of the bomb and the future of atomic energy. Thus did Lawrence and Fermi, friendly rivals in small matters for a decade, find themselves collaborators on questions of immense moral and technical significance. They turned out to have the same opinions. They concurred in the unanimous recommendation that the bomb should be used without warning on a target of military significance near a populated area. In their opinion, no other demonstration, for example, a blast in an unpopulated area, would have a good chance of bringing Japan's military government to surrender. They also advised that in the immediate postwar period the government continue to support their activities at one billion dollars a year.[24]

Fermi, Lawrence, and Oppenheimer displayed their identification with the Army and the cause of atomic energy immediately after the war in their support of a proposal pushed by the military to establish an Atomic Energy Commission (AEC) as successor to the Manhattan District. This proposal vested extraordinary powers in the army and the commission over employees, raw materials, facilities, and finances. Rank-and-file scientists organized to defeat it and formed an unlikely alliance with the Bureau of the Budget, which opposed an organization able to run roughshod over civil rights and do its business insulated from the executive authority of the president. Lawrence and Fermi were among the five or ten percent of Manhattan scientists who did not join one of the federations that lobbied for a less restrictive, less secret, atomic science. Lawrence declared himself to be opposed in principle to scientists' organizations not directed to the immediate pursuit of science and Fermi, though willing to talk politics privately, declined to be drawn publicly.[25] Fermi expressed reservations through channels about the more permissive proposals that superseded the bill he had favored, and the resultant legislation, which established the AEC, bore traces of his interven-

---

[23] SEGRÈ, *Fermi* (1970), 102-3, 125-6, 136; GROVES, *Now it can be told* (1962), 61-2, 296-7, 377; CHILDS, *Genius* (1968), 355-6.

[24] HEWLETT and ANDERSON, *New world* (1962), 356-9, 367-8; RHODES, *Making* (1986), 649-51, 696-7, 750-9.

[25] SMITH, *Peril* (1965), 49-50, 148, 166-7, 249, 383, 395.

tion. The result satisfied him sufficiently that he accepted appointment to the new commission's General Advisory Committee (GAC), chaired by Oppenheimer, for a three-year term beginning with the commission's birth on January 1, 1947.

Partial demobilization

Fermi had been able to continue his studies of reactors and neutron physics in 1943 and 1944 at the pile built to his specifications at the Manhattan District's laboratory at Argonne near Chicago. He began to distance hmself from the subject, however, as it became industrialized; and, as a member of the GAC, he gave priority to weapons over reactors. With the majority of the GAC, he doubted that economic nuclear power could be available within twenty years. The only reactor project to which he assigned a high priority was a power plant for the propulsion of ships and submarines. The Manhattan District had started a propulsion project, which the GAC considered weak and desultory. What to do about it? Fermi proposed the obvious solution: call in Lawrence to chair a committee to oversee the project and inject the enthusiasm and vitality it lacked. The GAC endorsed the proposal but the commission rejected it. On the other hand, Fermi opposed Lawrence's proposal to build a high-flux reactor mear Berkeley on the ground that Lawrence's staff did not have the necessary skill or experience.[26]

While serving on the GAC, Fermi was also building up an important school in theoretical physics in Chicago and championing high-energy physics elsewhere.[27] Although he continued to range over most of physics, he decided to shift his main concern from the nucleus to its constituents, thus duplicating the move from the atom to the nucleus that had proved so productive in the 1930s. One of his earliest contributions, which had important implications for Berkeley, developed from his analysis of an experiment done in Italy in hiding during the war by Marcello Conversi, Ettore Pancini, and Oreste Piccioni.[28] Working in a cellar where only they and cosmic rays penetrated, they managed to capture electronically the decays of a few mesotrons. (These particles had been detected in 1937 at Caltech in a cloud chamber adapted to the study of cosmic rays.) Fermi's analysis of the life times of the particles observed by Conversi, Pancini, and Piccioni indicated that they could not be

[26] Hewlett and Duncan, *Atomic Shield* (1962), 31-2, 117-18, 209, 217, 383; Segrè, *Fermi* (1970), 131-2.

[27] Bonolis, in Bernardini and Bonolis, *Conoscere Fermi* (2001), 364-77.

[28] Rossi, *Cosmic rays* (1964), 127-8; Piccioni, in Brown and Hoddeson, *Birth* (1983), 222-9, 239-40; Conversi, in ibid., 243-8; Fermi, *Coll. papers, 2* (1965), 615-17.

the carriers of nuclear force proposed by Hideki Yukawa. The upshot was that the mesotron found in cosmic rays could not be Yukawa's particle – a distinction confirmed by evidence of the transformation of a charged Yukon (a π meson) into a mesotron (a μ meson). This golden event took place in a photographic emulsion exposed to cosmic rays by Giuseppe Occhialini, who had studied under Persico and no doubt learned his quantum mechanics from the famous text. At the time, 1947, Occhialini was working in Bristol with Cecil Powell and the Brazilian Cesar Lattes.[29]

Meanwhile Berkeley had commissioned the 184-inch cyclotron, or, to be technical, synchro-cyclotron, which worked on a principle invented by McMillan. Theorists had warned in 1939 that the conventional cyclotron would not produce the 100 MeV deuterons that Lawrence promised the Rockefeller Foundation because their relativistic increase in mass would throw them out of phase with the accelerating radio-frequency field. McMillan suggested that if the particles were injected into the machine in discrete bunches and the frequency of the field altered appropriately as they gained energy, they could stay in phase with the acceleration. With other modifications of the original design, McMillan's principle of phase stability enabled Berkeley's physicists and engineers to double the energy originally promised, to 100 MeV per nucleon. Although that was just enough to make mesons, the Berkeley physicists did not look for them when the 184-inch started running in November 1946. Only late in 1947 did they begin the search. Eugene Gardiner, who had received his Ph.D. in 1943 under Lawrence with a thesis on calutron sources, oversaw a group that exposed emulsions in the paths of mesons created in a target struck by the synchro-cyclotron beam. They found nothing using the emulsions made for them by Eastman Kodak. Success came with Lattes, who brought the emulsions used by the Bristol group and the experience to recognize meson tracks. In February 1948 Berkeley registered the first machine-made mesons.[30]

These mesons were charged. Theories of cosmic rays by Oppenheimer and others had predicted an uncharged pion, which could not easily be spotted in the sea of particles in the 184-inch machine. No matter. McMillan had built an electron synchrotron with the special support and blessing of General Groves. Pions generated via the synchrotron decayed into photons

---

29 ROSSI, *Cosmic rays* (1964), 131-41; LATTES, in BROWN and HODDESON, *Birth* (1983), 307-10; MARSHAK, in ibid., 376-86.

30 MARSHAK, in BROWN and HODDESON, *Birth* (1983), 386-7; HEILBRON, SEIDEL, and WHEATON, *Lawrence* (1981), 51-60.

detected by coincidence counters. By then, 1950, Lawrence had already started on a new machine. The synchro-cyclotron produced particles of several hundred million electron volts; its successor, the Bevatron, paid for by the AEC, would scale up by an order of magnitude and operate on a new principle. It would confine particles to orbits of fixed radius within a narrow circular evacuated ring that ran through a series of small bending magnets whose fields would be adjusted to correct for the relativistic increase in mass. When the laboratory received permission to proceed with the Bevatron in 1948, it hoped to accelerate protons to around six billion electron volts, which, according to the calculations of Fermi and others, might be sufficient for the creation of antiprotons. In fact, the design of 1948 would not have reached 6 GeV because it provided for a large gap to accommodate the circulating particles within the magnets that held them in their orbits. Designers worried that they could not control the beam sufficiently to permit a smaller gap, which would have allowed a stronger field and the confinement of more energetic particles.[31]

As the frame of the bevatron rose above San Francisco Bay, the Soviet Union detonated its first atomic bomb and the GAC debated the merits of a crash program for a fusion bomb. Fermi had favored research into thermonuclear explosives, but he joined the rest of the GAC in recommending to the commissiom that it not rush for the so called "super". They invoked both moral and technical reasons for their advice: since a thermonuclear bomb could be made as large as its maker pleased, there was no natural limit to the devastation it could wreak, and, moreover, no one knew how to make one. The commissioners accepted this advice and referred the decision to President Truman. The decision distressed Lawrence and Edward Teller, who pushed their views as high as they could reach. They were helped by the revelation of atomic espionage and the outbreak of the Korean War.[32] Truman authorized a crash program. Lawrence rushed to do his part, which, he decided, was to make nuclear and thermonuclear explosives in large quantities by both particle accelerators and a high-flux reactor. As we know, the commission, acting on Fermi's advice, turned down the reactor. But it sunk a large amount of money into Lawrence's factories for making explosive iostopes in his favorite manner – by bombardment.

---

[31] Heilbron, Seidel, and Wheaton, *Lawrence* (1981), 76-8; Heilbron, in De Maria et al., *Restructuring* (1989), 172-4.

[32] Hewlett and Duncan, *Atomic Shield* (1962), 530-7, 581-4.

The first stage of a suitable production accelerator was built in the early 1950s at a disused airbase not far from Berkeley in the Livermore Valley. The machine did not run in circles like the cyclotron, but in a straight line, using huge oscillators and long drift tubes to hurry and control the beam within the greatest nothingness – that is, the largest vacuum – ever created by man. The design scaled up the proton linac that Alvarez had built from radar oscillators and other parting gifts to Lawrence from General Groves. This large and expensive business came to an early end when the commissioners realized that they could obtain the raw materials for fission and fusion bombs more cheaply in other ways.[33]

Lawrence's initiative had two grand consequences, however. For one, the site of the production accelerator became the nucleus of the Lawrence Livermore Laboratory. For another, when the builders of the prototype production accelerator returned to Berkeley to finish the Bevatron, they brought with them experience with beam control and high vacuum systems that made them confident they could reduce the gaps between the pole pieces enough to reach 6 GeV. And so the great machine came into full-scale operation in November 1954 with the capacity to make antiprotons. Once again, the need to do practical work made a Berkeley accelerator capable of prodigious physics.[34]

In keeping with tradition, however, most of the early experiments at the Bevatron concerned particles discovered elesewhere – pions and also kaons, the strange particles first detected by cosmic-ray physicists in photographic emulsions in 1947. The exotic life-styles of kaons occupied the attention of several groups at Berkeley, including Segrè's. The information they collected helped prompt the invention of the concept of hypercharge and the discovery of the non-conservation of parity. Early in 1955, Segrè's group and others, each in its own way, began to look for antiprotons. Segrè's won in October 1955, using advanced counters and up-to-date magnets designed with the help of Piccioni. The machine men then turned the tables on the observers of nature's direct bounty. Segrè sent film exposed not to the heavens but to the Berkeley Bevatron to Amaldi's cosmic-ray scanners in Rome, where they soon found a minuscule cataclysmic explosion in which an antiproton ended its career.[35] At last, an accelerator had made and studied particles not previously found in cosmic rays and had opened a field of

---

33 HEILBRON, SEIDEL, and WHEATON, *Lawrence* (1981), 62-75.

34 HEILBRON, in DE MARIA ET AL., *Restructuring* (1989), 176-84.

35 Ibid., 185-91.

research not adumbrated by more modest instruments. The manufacture and detection of the antiproton marked the full realization of the potential of Lawrence's machine, and vindicated his twenty years of devoted effort to make his cyclotron the ultimate tool in fundamental physics.

Lawrence had further satisfaction in the success of Seaborg, whose group, persisting in the old game, made one transuranic element after another in a high-intensity, moderate-energy, cyclotron erected below the Bevatron on a hill overlooking the Berkeley campus. Two of these superheavy elements, which rejoice in the unlikely names berkelium and californium, celebrate Lawrence and his laboratory. Another, number 100, bears a name that comes more readily to the mind and tongue: fermium.

References

1. Alvarez L. W. *Adventures of a physicist*. New York: Basic Books, 1987.

2. Amaldi E. "Il caso della fisica". In *Le consequenze culturali delle leggi razziali*. Accademia dei lincei. *Atti dei convegni*, vol. 84. Rome: the Academy, 1990. Pp. 107-133. In Amaldi, *20th century physics* (1988), 141-67; 168-90 (English tr.).

3. Amaldi E. "Personal notes on neutron work in Rome in the 30s and post-war European collaboration in high-energy physics". In Charles Weiner, ed. *History of twentieth century physics*. New York: Academic Press, 1977. Pp. 294-351. (International School of Physics "Enrico Fermi", course LVII, July-August 1972.)

4. Amaldi E. *Da via Panisperna all'America*. Ed. Giovanni Battimelli and Michelangelo De Maria. Rome: Riuniti, 1997.

5. Amaldi E. "Neutron work in Rome in 1934-36 and the discovery of uranium fission". *Rivista di storia della scienza, 1* (1984), 1-24. In Amaldi, *20th century physics* (1998), 5-28.

6. Amaldi E. *20th century physics: Essays and recollections. A selection of historical writings*. Ed. Giovanni Battimelli and Giovanni Paoloni. Singapore: World Scientific, 1998.

7. Battimelli G. and De Maria M. "Prefazione". In Amaldi, *Da via Panisperna* (1997), 15-55.

8. Bonolis L. "Cronologia dell'opera scientifica di Enrico Fermi". In Carlo Bernardini and Luisa Bonolis, eds. *Conoscere Fermi nel centenario della nascita*. Bologna: Editrice Compositori, 2001. Pp. 319-77.

9. Brown L.M. and Hoddeson L. eds. *The birth of particle physics*. Cambridge: Cambridge University Press, 1983.

10. Cardone F. and Mignani R. *Enrico Fermi e i secchi della sora Cesarina. Metodo, prejudizio e caso in fisica*. Rome: Di Renzo, 2000.

11. Childs H. *An American genius. The life of Ernest Orlando Lawrence, father of the cyclotron*. New York: Dutton, 1968.

12. CONVERSI M. "The period that led to the 1946 discovery of the leptonic nature of the 'mesotron.'" In BROWN and HODDESON, *Birth* (1983), 242-50.

13. FERMI E. *Collected papers*. Ed. EDOARDO AMALDI ET AL. 2 vols. Chicago: University of Chicago Press, 1962-65.

14. FERMI L. *Atoms in the family*. Chicago: University of Chicago Press, 1954.

15. GROVES L.R. *Now it can be told. The story of the Manhattan project*. New York: Harpers, 1962.

16. HEILBRON J.L. "The detection of the anti proton". In MICHELANGELO DE MARIA ET AL., eds. *The restructuring of physical sciences in Europe and the United States 1945-1960*. Singapore: World Scientific, 1989. Pp. 161-217.

17. HEILBRON J.L. and SEIDEL R.W. *Lawrence and his laboratory. A history of the Lawrence Berkeley Laboratory*. Vol. I. Berkeley: University of California Press, 1989.

18. HEILBRON J.L., SEIDEL R.W. and WHEATON B. *Lawrence and his laboratory. Nuclear science at Berkeley* 1931-1961. Berkeley: University of California, Office for History of Science and Technology, 1981.

19. HEWLETT R.G. and ANDERSON O.E. Jr. *The new world, 1939-1946*. University Park: Pennsylvania State University Press, 1962. *(A history of the United States Atomic Energy Commission,* vol. 1.)

20. HEWLETT R.G. and DUNCAN F. *Atomic shield, 1947-1952*. University Park: Pennsylvania State University Press, 1962. (*A history of the United States Atomic Energy Commission,* vol. 2.)

21. HOLL J.M. with HEWLETT R.G. and HARRIS R.R. *Argonne National Laboratory, 1946-96*. Urbana: University of Illinois Press, 1997.

22. HOLTON G. "Striking gold in science: Fermi's group and the recapture of Italy's place in physics". *Minerva,* 12 (1974), 159-98.

23. LATTES C.M.G. "My work in meson physics with nuclear emulsions". In BROWN and HODDESON, *Birth* (1983), 307-10.

24. MARSHAK R.E. "Particle physics in rapid transition: 1947-1952". In BROWN and HODDESON, *Birth* (1983), 376-401.

25. PICCIONI O. "The observation of the leptonic nature of the 'mesotron' by Conversi, Pancini, and Piccioni". In BROWN and HODDESON, *Birth* (1983), 222-41.

26. RHODES R. *The making of the atomic bomb*. New York: Simon and Schuster, 1986.

27. ROSSI B. *Cosmic rays. A dramatic and authoritative account*. London: George Allen and Unwin, 1964.

28. SEGRÈ E. *Enrico Fermi, physicist*. Chicago: University of Chicago Press, 1970.

29. SEGRÈ E. *A mind always in motion. The autobiography of Emilio Segrè*. Berkeley: University of California Press, 1993.

30. SMITH A.K. *A peril and a hope. The scientists' movement in America 1945-47*. Chicago: University of Chicago Press, 1965.

John L. Heilbron

John.L. Heilbron, formerly professor of history and the vice chancellor at the University of California, Berkeley, is now senior research fellow at Worcester College and at the Museum for the History of Science, Oxford. His general subject of research is the history of the physical sciences and their institutions from the Renaissance into the 20th century. His work most relevant to the Fermi Conference,Lawrence and his Laboratory, a history of the Lawrence Berkeley Laboratory, vol. 1(Berkeley, 1989), was written in collaboration with Robert Seidel. It includes much information about the development of nuclear physics in Europe and the US in the 1930s as well as a local history of the Berkeley accelerators. His recent book, *The sun in the church: Cathedrals as solar observatories* (Harvard, 1999), describes the contributions to astronomy and calendrics made by observers at meridian lines installed in a few major churches, all but one of them in Italy, during the 17th and 18th centuries, and the operation of the censorship of books on cosmology after the condemnation of Galileo. His latest work is the *Oxford companion to the history of modern science* (Oxford, 2003), of which he was the general editor.

Leon Lederman

The Beginnings of Pion and Muon Physics

My talk will review the birth and early evolution of High Energy Physics (Particle Physics) as it emerged from the series of post world war II accelerators. This will touch, in a detailed way, the work of Fermi at Chicago in the decade of the 1950's. Fermi, in Chicago with a spectacular group of students, was a prime mover in opening this field. The major physics concerns of the day were the properties of pions and muons. The scattering of pions by protons gave physics a glimpse of the strong force whereas muons and their related neutrinos were the entry to high energy weak forces. My own work at Columbia and research at Berkeley, Rochester, Liverpool, etc. are relevant to that seminal epoch at the beginning of a new field.

La nascita della fisica dei pioni e dei muoni

La mia relazione verterà sulla nascita e l'evoluzione iniziale della fisica delle alte energie (fisica delle particelle) derivante dalla serie di acceleratori costruiti dopo la seconda guerra mondiale, analizzando dettagliatamente il lavoro svolto da Fermi a Chicago all'inizio degli anni 50. A Chicago Fermi, con un gruppo spettacolare di studenti, fu un pioniere in questo campo. I principali temi di studio dell'epoca erano le proprietà dei pioni e dei muoni. Lo scattering dei pioni per mezzo dei protoni aprì alla fisica uno squarcio sulla forza nucleare forte, dove i muoni ed i relativi neutrini rappresentavano la strada verso le forze nucleari deboli.

Il mio lavoro alla Columbia University e la mia ricerca alla Berkeley University, a Rochester, a Liverpool ecc. riguardano quell'epoca determinante che segna l'inizio di un nuovo settore di ricerca.

I send this note instead of attending this meeting because of the disruptions to all of our lives by the tragic events of September 11th. I had been looking forward to coming to Rome, celebrating Enrico Fermi's centennial and seeing many old friends. The scientific community, which so reveres Fermi's contributions, both in science but also in style, must now maintain our faith in rationality, which is threatened on all sides. It is my personal belief that we must understand and act wisely on the root causes of terrorism. But now let me make a few remarks relevant to the Centennial.

My assignment, for the Fermi Centennial, was to discuss the early period of pion and muon physics. I was among the first post-WWII graduate students to get a Ph.D. at Columbia University's NEVIS Cyclotron Laboratory. The date was 1951. My thesis was on the lifetime of the pion and the mass of the muon. My advisor was visiting Professor Gilberto Bernardini. Through Gilberto, I met Fermi several times. Our involvement with pions was essentially simultaneous; Fermi's Chicago Period included a new collection of awesome students that Fermi seemed to attract. It was quintessential Fermi, with an almost seamless mix of theory and experiment. I recall being delighted that the great Fermi was working on the same things as I. NEVIS came on line a few years before Chicago. John Tinlot and I had discovered how to get beams of pions out of the accelerator, focused by the fringing field of the cyclotron magnet. We had "hot and cold" pion beams! Our Berkeley competitors were not so lucky. Our negative beams went out to ~150 MeV, but positive pions (obtained from backwards emission in proton-target collisions) died at about 60 MeV. We worked on lifetimes of pions, on scattering of pions from a carbon plate in a Wilson Chamber, on the mass of the muon and the properties of the neutrino. Fermi's group concentrated on pion-proton scattering. I still recall the excitement of Fermi's "Rochester Conference" presentation of his negative pion scattering. The cross section was large, definitively establishing the strong interaction of pions after some disturbing cosmic ray results.

When Fermi's group turned to positive pions, the results were even more spectacular. The cross section rose dramatically. When it was last seen, it was at about 135 MeV, heading steeply upward. The suspicion was a resonance but it took several years to establish the "3-3" resonance, although Fermi, on the basis of a glance at a paper written by Keith Bruekner, predicted the famous ratio of the three pion-proton cross sections (pi plus to pi plus; pi minus to pi zero and pi minus to pi minus) as 9:2:1.

Fermi led in the reduction of the data via a phase shift analysis. Again, it was only after Fermi's death that the correct phase shifts were established and the isospin 3/2, angular momentum 3/2 resonance firmly established.

I will never forget the first Rochester Conference in 1950. I was the only graduate student present and found myself standing next to Enrico on the lunch line. Desperate to show my deep knowledge, I asked him:

"Professor, what do you think of the evidence for the V-zero-two which we just heard?"

He looked at me and gave a response that became famous:

"Young man, if I could remember the names of these particles, I would have been a botanist".

We were in a new field which emerged from the fields of cosmic rays and nuclear physics. The beginnings in the accelerators of Chicago, Berkeley and Columbia are the clear progenitors of a field that has led to our current understanding of the Standard Model of Fundamental Particles and its essential coupling to the astrophysics of the origins and evolution of the universe.

It is clear that Enrico Fermi's personal leadership, his scientific style and his influence on students was a major force in the establishment of physics in the United States.

My personal contact with Fermi in visits to Chicago, in Rochester Conferences, in his early summer visit to Brookhaven just months before his illness, was a seminal experience. I was later honored to become Director of the Fermi National Accelerator Lab (Fermilab) and to receive the Enrico Fermi Medal at the hands of President Bill Clinton in 1993. I now send my warmest greetings to the Centennial assembly convinced that the pursuit of our efforts to understand the world, and to insist that this knowledge be applied compassionately, is the highest form of tribute to the memory of Enrico Fermi.

Leon Lederman
Internationally renowned high-energy physicist, is Director Emeritus of Fermi National Accelerator Laboratory in Batavia, Illinois and holds an appointment as Pritzker Professor of Science at Illinois Institute of Technology, Chicago. Dr. Lederman served as Chairman of the State of Illinois Governor's Science Advisory Committee. He is a founder of and Resident Scholar at the Illinois Mathematics and Science Academy, a 3-year residential public high school for the gifted. Dr. Lederman was the Director of Fermi National Accelerator Laboratory from June 1, 1979 to June 30, 1989. He is a founder and Chairman of the Teachers Academy for Mathematics and Science, active in

the professional development of primary school teachers in Chicago. In 1990 he was elected President of the American Association for the Advancement of Science, the largest scientific organization in the US. He is a member of the National Academy of Science and has received numerous awards, including the National Medal of Science (1965), the Elliot Cresson Medal of the Franklin Institute (1976), the Wolf Prize in Physics (1982), the Nobel Prize in Physics (1988) and the Enrico Fermi Prize given by President Clinton in 1993. He served as a founding member of the High Energy Physics Advisory Panel of the United States Department of Energy and the International Committee for Future Accelerators. Lederman chairs the Committee on Capacity Building in Science of the Paris-based International Council of Scientific

Luciano Maiani

Perspectives in High Energy Particle Physics

Fermi was much interested in high energy physics at the end of his life: we recall here the main lines of his thought in order to show the continuity of the development of this extremely important frontier of knowledge since his time. The importance of high energies is stressed as far as some unpredictable improvements in the last 50 years both in the instruments (colliders and computers) and in the theory (symmetries and new particles). LHC at CERN is therefore a natural achievement in Fermi's legacy.

Prospettive della fisica delle alte energie

Fermi fu estremamente interessato alla fisica delle alte energie nell'ultima parte della sua vita. Vogliamo qui ricordare e tracciare le linee principali del suo pensiero, tanto da evidenziare la continuità di sviluppo di questa frontiera così rilevante della conoscenza.

L'importanza della fisica delle alte energie è evidenziata dagli imprevedibili miglioramenti sia nella strumentazione (computer e Collider) sia nella teoria (simmetria e nuove particelle) degli ultimi cinquant'anni.

L'LHC del CERN rappresenta dunque una conseguenza logica dell'eredità fermiana.

Introduction: a Fermi's legacy

In January 1954, E. Fermi gave a talk entitled "What Can We Learn With High Energy Accelerators?" at the American Physical Society. He was then leaving the APS Chair, which he had taken during 1953. The University of Chicago Library has short personal notes that Fermi wrote for the talk as well as the slides of the figures.

It was indeed a crucial moment in particle physics.[1] The discovery of many new particles in cosmic rays had opened a new world and stimulated the development of particle accelerators. Big projects were starting in the US, the URSS and Europe, where CERN was being created just for this purpose.

At that time, Fermi was fully engaged in particle physics. On the experimental side, he was studying the π-N cross sections at the Chicago Synchrocyclotron, finding the first indications of the 3/2-3/2 resonance and the confirmation of the isotopic spin symmetry in π-N interactions. On the theoretical side, Fermi was impressed by the wealth of new particles that were being discovered in the high-energy cosmic ray interactions. Not all these particles could really be elementary! Together with C. N. Yang, he had developed, in 1949, a model of the π mesons as bound states of a nucleon-antinucleon pair, the precursor of the quark model of mesons and baryons, which was going to be discovered by Gell-Mann and Zweig some twelve years later.

What to do with high-energy accelerators? Fermi underlines the difficulty of looking into a "very, very cloudy crystal ball". He mentions the observation of antinucleons, the puzzle of the long lifetime of strange particles (high angular momentum barrier, or associated production, which he qualifies as "at present more probable"), the need for precision measurements. But also the possibility of "a lucky break, or theoretical leap, or more probably a combination of hard work, ingenuity and a little bit of good luck". All that and much more did in fact happen from the 1950s until now in High-Energy Particle Physics. Progress is exemplified in Figure 1, by the chart of what are now considered to be the elementary constituents of matter, the three generations of quarks and leptons.

The forces acting on quarks and leptons are described by a coherent theoretical framework, usually referred to as the Standard Theory (see box). They encompass the familiar electromagnetic forces acting between charged particles, the weak forces responsible, among other processes, of the beta decay of nuclei, and the strong forces that bind quarks into nucleons (proton and

[1] See M. Jacob and L. Maiani "L'eredità di Enrico Fermi nella fisica delle particelle", in *Conoscere Fermi*, edited by C. Bernardini and L. Bonolis, SIF, ed. Compositori, Bologna 2001.

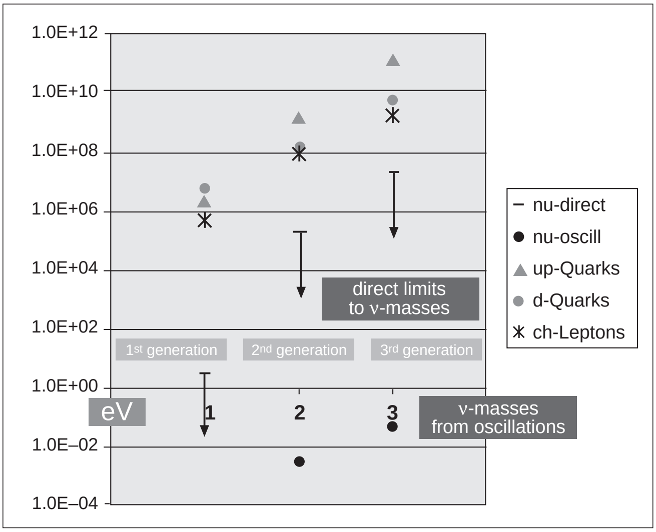

Figure 1
The mass spectrum of quarks and leptons (ascending powers of eV). Upper bounds to neutrino masses are taken from beta decay spectra; estimates of ν_μ and ν_τ masses are from solar and atmospheric neutrino oscillations

neutron) and nucleons into nuclei. To those forces, one has to add those associated with the, still hypothetical, Higgs field, which are responsible for the arising of particle masses, as discussed below.

In the Standard Theory, the Electromagnetic and weak forces are unified in a simple scheme and all the three forces are determined by the same principle: the invariance under transformations which may vary arbitrarily from point to point (in jargon, a gauge symmetry). This similarity is a strong hint that it may be possible to discover a more unified scheme which encompasses all forces, including the Higgs and, most important, the gravitational forces. New dynamical concepts and new symmetries will be certainly required to accomplish this very ambitious further step in our knowledge of Nature.

Colliders

To illustrate the potential of particle accelerators, Fermi considered in his seminar a proton accelerator running on a maximum circle around the Earth. With a magnetic field of 2 Tesla, this gives an energy E_{Max} = 5 10^{15} eV. It is the energy of the cosmic rays around the 'knee', the most energetic cosmic rays that can be accelerated by the galactic magnetic clouds, according to Fermi's ideas developed in the very same years.

By extrapolating from the plots of energy or cost vs. time of the nuclear

1. Normal Matter (Galaxies, stars, us,…)

QUARKS (M. Gell-Mann, G. Zweig, 1962) | LEPTONS

$$\begin{pmatrix} u \\ d \end{pmatrix} \qquad \begin{pmatrix} \nu_e \\ e \end{pmatrix}$$

2. Analogous structures at higher energy

$$\begin{pmatrix} c(1974) \\ s \end{pmatrix} \qquad \begin{pmatrix} \nu_\mu(1962) \\ \mu \end{pmatrix}$$

$$\begin{pmatrix} t(1994) \\ b(1977) \end{pmatrix} \qquad \begin{pmatrix} \nu_\tau(2000) \\ \tau(1976) \end{pmatrix}$$

3. Forces

Electromagnetic	⟶	PHOTON (A. Einstein, 1905)
Weak	⟶	W, Z (CERN, 1982)
Nuclear	⟶	GLUONS (not seen in isolation)
Gravitational	⟶	GRAVITON (not yet observed)
Mass Generation	⟶	HIGGS BOSON (not yet observed)

Particles of the Standard Theory. The year of the first experimental observation is indicated next to each particle, but for the "classical" ones (the electron, the muon, the nuclear beta decay neutrino and the three quarks of Gell-Mann and Zweig).

installations of the time, Fermi concluded that this energy could be reached in the year 1994, at a cost of about 170 billion US dollars.

The key to high energy and relatively low cost (very low indeed, compared to Fermi's extrapolation) has been, of course, technological innovation, above all the invention of "colliders", structures which are capable to accelerate and store two beams of particles, then made to collide head-on at a few, fixed points. The discovery has made possible a gigantic leap forward in the energy available for the collision, the energy in the center of mass, which in turns determines the discovery potential of the machine.[2]

The first electron–positron collider was realized in Italy, at the Frascati National Laboratories (AdA, 1962) by Bruno Touschek and collaborators.

---

2 For relativistic particles, the c.o.m. energy in the fixed target mode is $\sqrt{2E_{beam}M_{target}}$, while for two symmetrically colliding beams is $\sqrt{4E_{beam}^{(1)}E_{beam}^{(2)}} \approx 2E_{beam}$. Thus the available energy increases much faster with E_{beam} in the second case. Colliders, on the other hand, pose enormous technological challenges to achieve sufficiently high density of beam packets and to store them for enough time, so as to have an appreciable number of collisions.

Proton–proton (ISR, CERN, 1971), proton–antiproton ($Sp\bar{p}S$, CERN, 1981) and electron-proton (HERA, DESY, 1992) colliders followed.

Transforming back to the fixed-target energy, the Tevatron (proton-antiproton collider, 2 TeV in the c.o.m.) reached 2 10^{15} eV in 1987. LEP (electron-positron, 200 GeV in the c.o.m.) and HERA (electron-proton, 300 GeV in the c.o.m.) have explored about the same energy range with probes unthinkable at Fermi's time. The Large Hadron Collider (LHC, proton-proton collider with 14 TeV in the c.o.m.) will reach 1 10^{17} eV in 2006, 20 times E_{Max}, at an all-out cost of about $5 billion.

If the VLHC which is being considered today at the Fermi National Laboratory or the Eloisatron proposed by INFN will be realized, with a center-of-mass energy of 200 TeV and corresponding to fixed-target energy around $2 \cdot 10^{19}$ eV, mankind will have been able to produce collisions at an energy equivalent to that of the highest-energy cosmic rays that can originate from nearby galaxies.[3]

Symmetry in Particle Physics

On the theoretical side, *symmetry* has been a crucial concept to investigate the role of the new particles.

In plain language, symmetry implies well-balanced proportions (from the Greek words *σψμ*, 'with', and *μετροσ,* 'measure'). Symmetric objects have grace and beauty. The most beautiful vistas, whether faces or buildings, are the most symmetric, the most perfect. What is more important for us, the natural balance of symmetry leads to *predictability*. We can guess a hidden part of a figure, if we know the symmetry, which supervises its design.

Symmetry is demanding. The slightest fault, and the symmetry is no longer faithful. The picture in Figure 2 (Pala della Misericordia) looks left right symmetric at first sight, but this is not exact. The Madonna has an asymmetric belt's knot; the praying figures are not symmetrical. Most important, the figure is illuminated from one side. The fully symmetric picture looks more flat, static and hieratic, the real Madonna is human and closer to us.

In the Madonna del Parto (Figure 3), the angels are almost left right symmetric, but the different colours of their dresses give movement to the whole picture. And, of course, the Madonna in the centre is now "breaking the symmetry", with the wonderful curve associated to her maternity. We still do not

---

3 i.e. those below the GZK cut-off due to the onset of π-meson production in the scattering of cosmic ray protons off the microwave cosmic background photons.

Figure 2
Pala della Misericordia. The real picture is shown *(left)* together with the artificially reconstructed picture "predicted" by left-right symmetry, from the right half of the figure

know why symmetry is relevant to physics, but predictability is the key of its success. To describe fully the complexity of the world, however, some of the most beautiful symmetries have to be broken. Figure 4 shows with a few examples the power of symmetry and the need for symmetry to be broken.

Can we break a local symmetry?

Piero della Francesca could introduce variations at will in his symmetry pattern. But, is this possible in Nature? Is it possible at all to violate the symmetry? And if so, are there limitations?

There is only a numerable infinity of discrete symmetries (the "crystallographic groups"). Similarly, we can classify by numerable series the continuous groups. It would be relatively easy for God to assign a symmetry to the world! Symmetry breaking, instead, belongs to the realm of unpredictability, fantasy and chaos (is this why the Madonnas and angels of Piero are so fas-

Figure 3
La Madonna del Parto. The angels are left-right symmetric to great extent, except for the colours of their dresses, but the Madonna breaks the symmetry quite dramatically

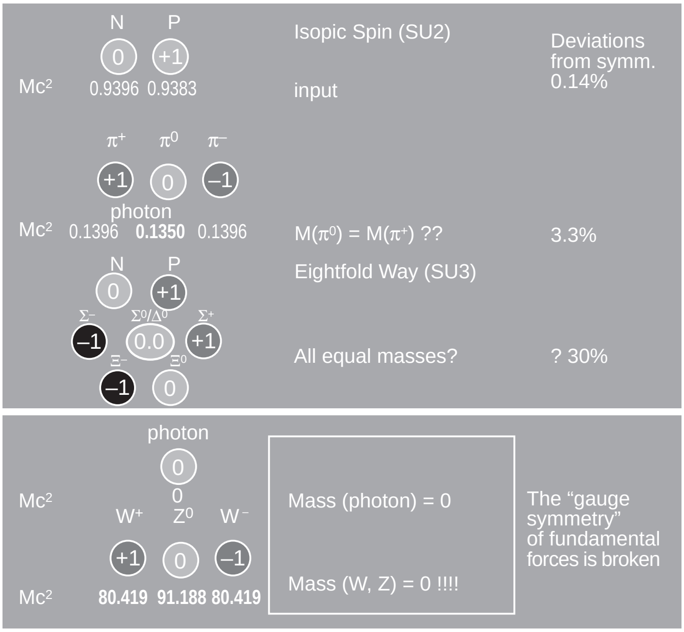

Figure 4
Symmetry in particle physics: predictions vs. reality. *Top:* predictions of the approximate global symmetries in Particle Physics. *Bottom:* local symmetry predicts equal masses for the photon and for the intermediate bosons, in flagrant contradiction with reality

cinating?). In mathematical terms, symmetry can be broken in infinitely many continuous ways.

Seen in this context, the issue belongs to the wider philosophical question of the uniqueness of the fundamental laws of physics. Is there only one consistent set of laws and therefore only one consistent Universe? Or can we put arbitrary parameters in the basic laws such that there may exist, or at least we can "imagine", different, equally consistent Universes, distinguished by the actual values of these parameters (masses, electric charges, Newton constant)? We have made limited but interesting progress on these fascinating questions.

First, we have to distinguish between symmetries where the same transformation (say, a rotation) is applied at all points of space and time, the so-called "global symmetries", and symmetries where laws are invariant under transformations, which can be chosen differently from point to point in space and time. The latter are called "local", or "gauge", symmetries.

There is essentially no restriction to violate any global symmetry. However, more important is the second case, which includes the Einstein Theory of gravity and the theories introduced by Yang and Mills in 1954 (the so-called "gauge theories"), known today to accurately describe the interactions of fundamental particles.

In these cases, introducing symmetry violations in the basic laws (technically, adding non-symmetric terms in the Action) leads to *mathematical inconsistency.*

There are quite a number of qualifications to append to this very blunt statement, but I think my theoretical colleagues would agree that it describes correctly the situation: no Piero della Francesca has the freedom to "deform" even slightly the Yang Mills or Einstein basic laws.

But then, how are we going to account for the asymmetries observed in Nature, namely the unequal masses of photons, W and Z particles, or the masses of quarks and leptons, which also should vanish in the symmetric world? The solution is simple and fascinating.

A field pervades all space and affects the way particles move. Whilst the basic laws are exactly symmetric, the very presence of this field violates the symmetry, in that the field itself "distinguishes" different particles related by symmetry. By their interaction with this background field, W and Z acquire a mass but the photon remains mass-less, leptons and quarks acquire different masses.

In this picture, the "vacuum", the state where "there is nothing", is not empty at all. Rather, it is like the surface of a perfectly calm lake: there seems to be nothing because it is everywhere equal to itself. In collisions, waves can

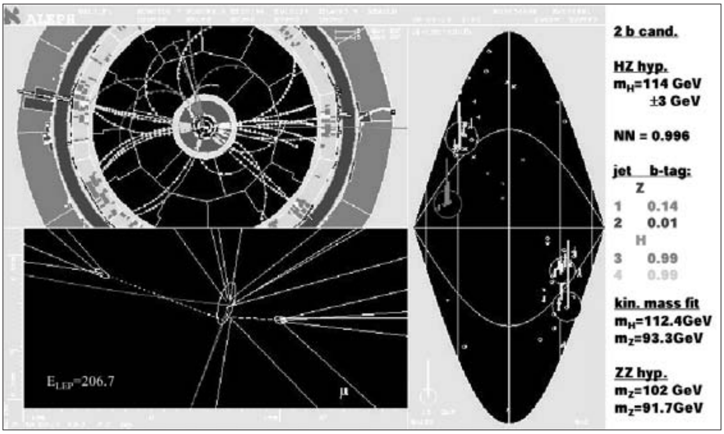

Figure 5
ALEPH: candidate event for $e^{+}+e^{-} \to Z+H$, followed by $Z \to hadrons$ (jets 1 and 2) and $H \to b+\bar{b}$ (the dotted lines indicate the path of neutral unstable particles which decay in jets 3 and 4 and are identified as beauty particles); the decay into a beauty particle pair is the theoretically preferred decay mode of the Higgs boson and is used to select events which should contain this particle with higher probability

be produced on the surface of the lake, some of which correspond to a new particle: the Higgs boson. The Higgs boson is needed for the mathematical structure of the theory to agree with what we see in Nature, but the whole picture gives a description of vacuum which may lead us to a new vision of the Universe, in particular of the primordial Universe (inflation, chaotic Universe).

Higgs Hunting

I would not go so far as to call the Higgs Boson the "God particle"[4], but it is clear that the observation of this particle is crucial, to give solid foundation to our theory of Elementary Particles and to validate the more advanced views on the primordial Universe. This justifies the excitement that has pervaded the world of physics (not only!) when some tantalizing evidence of a Higgs boson was seen, last summer, in the ALEPH experiment at CERN.

The definitive analysis of the LEP data still shows some evidence of a Higgs boson, but the degree of confidence that the events seen are not due to a sta-

[4] L. LEDERMAN with D. TERESI, *The God particle*, Dell Publishing, New York, 1993.

tistical fluctuation is smaller than what was indicated by the preliminary analyses made at the end of the year 2000.

This dry scientific statement has recently given rise to a curious debate on a scientific journal, following a rather unrefined interpretation of the LEP data analysis given by its scientific editor, which reads[5]: "No sign of the Higgs boson. The legendary particle that physicists thought explained why matter has mass probably does not exist. So say researchers who have spent a year analysing data from LEP accelerator at the CERN nuclear physics lab near Geneva".

The ensuing discussion has made clear[6] the importance of continuing the search with the TeVatron, at the Fermi National Laboratory in the US, and later with the LHC.[7] In one year running, the LHC will be able to clarify definitely the issue of the Higgs boson, not only in the energy range indicated by the LEP events but also in all the wider energy range compatible with the present theory of particle interactions.

More symmetry at High Energy

The fundamental particles that we see in Nature feature different values of their intrinsic angular momentum, spin. Quarks and leptons, the constituents of matter, carry 1/2 unit of spin, the Higgs boson and the particles that mediate the different forces carry integer values of the spin. Spin equal to zero for the Higgs boson, spin equal to one for the intermediaries of the strong, electromagnetic and weak forces, and spin two for the elementary quantum of gravity, the graviton.

In a truly unified scheme, all these particles should be related to each other by some symmetry, which then has necessarily to transform into one another particles with different spin, unlikely any other of the known symmetry transformations.

---

5 *New Scientist*, 5 Dec. 2001.

6 Edward Witten, Princeton New Jersey: "One question is whether the Higgs boson exists; the answer is almost certainly yes…". MICHAEL CHANOWITZ, Lawrence Berkeley National Laboratory: "I would argue even more strongly that the precision data does not support the standard model prediction of the mass of the Higgs boson, based on my recent analysis of the data *(Physics Review Letters, vol 87,* p 23802). The standard model may well be "dead" but the Higgs boson can survive, accompanied by other – as yet unknown – new physics. Until the nature of this new physics is known, we cannot predict the mass of the Higgs boson". JOHN ELLIS, CERN: "Those measurements suggest strongly that the particle weighs less than about 200 gigaelectronvolts (GeV). Direct searches for the Higgs boson at LEP tell us that it must weigh more than about 114 GeV, leaving plenty of space for it to exist… You quote John Swain as being prepared to bet large amounts of money that the Higgs boson will not be found: many of us particle physicists are each prepared to bet £100 against him. Let us see how much money he is prepared to put where his mouth is!".

7 See finally the article by G. KANE and E. WITTEN, *New Scientist,* 30 March 2002.

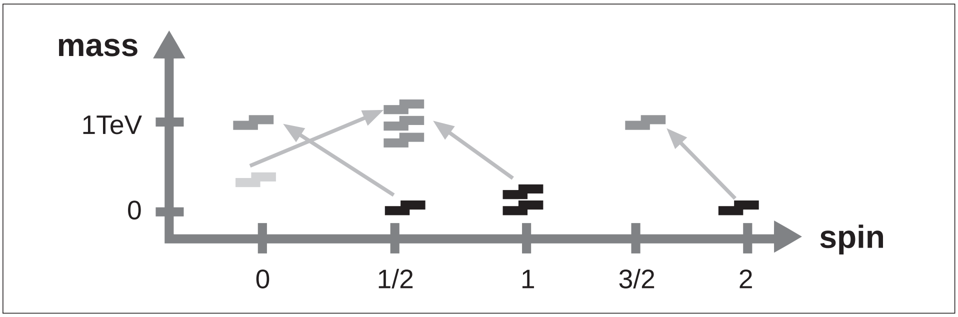

Figure 6
The spectrum of particles of different spin in SUSY theories. The lowest level is filled by the particles of the Standard Theory (Higgs boson, quarks and leptons, vector bosons and the gravitons, with spin 0, 1/2, 1 and 2, respectively). Each of these particles has a SUSY partner with a spin differing by 1/2 unit and a mass of the order of 1 TeV. At present, we have only experimental lower limits to the masses of SUSY partners, of the order of some 100 GeV, obtained from the non-observation of any such particle at LEP, the Tevatron and Hera

For some time it was believed that such a symmetry would be so restrictive that it would not be compatible with any possible interaction among particles, a clear absurdity. In the 70s, in Russia and at CERN[8] a completely new kind of symmetry, able to transform particles with spin differing by 1/2 unit[9] was discovered and shown to be compatible with the usual laws of Quantum Theory and Relativity. The new concept was so remarkable that it was dubbed Supersymmetry (later SUSY for brevity), to distinguish it from normal symmetries, and the properties of quantum field theories enjoying such a symmetry have been systematically studied since then. It was also found that theories with local Supersymmetry must necessarily encompass gravity, which shows that this concept provides the natural bridge between particle forces and gravity.

Two new aspects have been brought into this matter during the 80's. The first one is a stability condition on the Higgs boson mass that requires that the supersymmetry partners of the known particles have to appear in a mass range of the order of 1 TeV (1 TeV = 1000 GeV). This is very attractive indeed. While there is little doubt that Supersymmetry must apply in the real

8 By D.V. Volkov and V.P. Akulov and by J. Wess and B. Zumino, respectively.

9 As a consequence, the generators of Supersymmetry obey anti-commutation relations, unlike the generators of a usual symmetry; it is precisely this anti-commuting property that allows super symmetry to escape the no-go theorem alluded to before (due to S. Coleman and J. Mandula theorem) and permits relativistic, supersymmetric field theories with non vanishing interaction.

world, because of unification with gravity, the particles characteristic of this symmetry could be so heavy as to escape being produced at the energies reachable with particle accelerators.

The second element has been the observation of large quantities of non-radiating (dark) matter in the Universe. The dark matter makes large massive halos around Galaxies and it accounts for the largest fraction of the matter in the Universe. The dark matter is "seen" by its gravitational effects, but it seems very unlikely that it is made by usual atoms, nuclei etc. Rather, it could be made by heavy, electrically neutral particles, which have only a very weak interaction with the normal matter or with the electromagnetic field and that are remnants of the Big Bang. The SUSY partners of the Higgs boson or of the vector bosons could be ideal candidates as constituents of the dark matter, and again a mass scale of 1 TeV would be consistent with the dark matter cosmological properties and distribution.

The arguments just mentioned indicate that SUSY particles may form most of the Universe's mass and appear in a range accessible to the accelerators of the next generation. In particular, the LHC should cover most of the energy range where such particles are predicted to appear. The search for signals associated with the SUSY partners of quarks, leptons and gluons is an essential part of today's high-energy frontier.

How many dimensions?

In the '30s, P. Kaluza and O. Klein, in an attempt to write a unified theory of electromagnetism and gravity, made the hypothesis that our physical space has one more additional dimension. If the subspace corresponding to the additional dimension is "curved" upon itself, with radius R, waves of wavelength larger than $2\pi R$ could not be fitted into it, therefore ordinary light would not propagate in the additional dimension and we would not perceive it. Similarly, if R were much smaller than the typical wavelengths of our electrons and nuclei, according to the wave mechanics of De Broglie and Schrödinger, normal matter would be prevented to move into the new dimension. Only very energetic particles, with momentum $p >> h/(2\pi R)$, with h the Planck constant, would be able to "feel" a space with more than 3 dimensions.

For a long time, the Kaluza Klein (KK) idea has remained an intriguing but unwarranted hypothesis. The situation changed when it was found that the "string theories", the best available candidate theory for unifying gravity with quantum mechanics, do require a high dimensional space to be mathematicaly consistent. All of a sudden, we learn that the KK idea is not only possible, but it is in fact required.

Only waves of wavelength such that $\lambda = (2\pi R)/n$, with integer n, can propagate in the additional dimension, corresponding to a momentum[10] $p_5 = n\, h/(2\pi R)$. A mass-less particle in the full 5-dimensional space-time would have a momentum, which satisfies the Einstein null condition ($\vec{p}$ represents the usual three-dimensional momentum):

$$E^2 - (\vec{p})^2 - (p_5)^2 = E^2 - (\vec{p})^2 - (n\frac{h}{2\pi R})^2 = 0$$

that is:

$$E^2 - (\vec{p})^2 = (n\frac{h}{2\pi R})^2 \equiv (M_n)^2$$

This result means that each mass less particle in normal space, like the photon, the graviton, etc, is accompanied by a "KK tower" of massive companions, with mass M_n ($n = 1, 2$ etc.) which are called its KK excitations. KK particles are stable if the extra component of momentum is conserved (as it happens for the usual momentum). These particles must be rather heavy on particle mass scale (say more than a few hundred GeV) and therefore R must be rather small, since otherwise we would have seen their effects, in particular there would be a lot of such particles as remnants of the Big Bang.

In high-energy collisions, if energy were enough, we would start producing the low-lying excitations. As energy, and n, increases we would be sending wave packets of smaller and smaller wavelength in the new dimension, and we would explore it with finer and finer resolution. While particles are turning around the curved dimension, our macroscopic detectors would see energy and electric charge disappearing into nothing and coming back (periodically) from nothing. Together with the observation of the typical spectrum of KK excitations of mass M_n, the lack of energy and charge conservation in our three-dimensional space would be a most unique sign of the existence of new dimensions.

Recently, the issue of additional dimensions has taken a dramatic turn with the realisation that in most string theories, particles associated with normal matter (electrons, quarks, photons, gluons, etc.) are confined to a three-dimensional surface in multidimensional space, called a p-brane. In the simplest version of such theories[11] gravity only can extend to the full space. In this case, there is no need for a microscopic radius of curvature to avoid us

[10] We consider a space-time with one time-like and four space-like dimensions; the additional curved dimension is labelled as the fifth dimension.

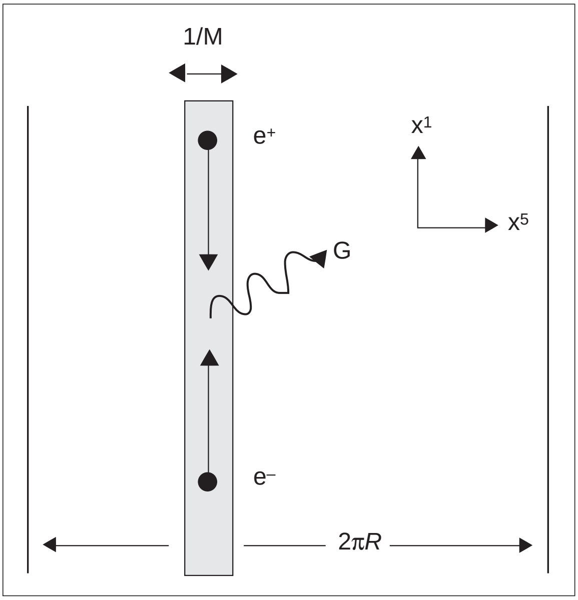

Figure 7
In certain string theories, normal particles are dynamically confined to a p-brane (represented as a grey slab in the figure[13]) while gravity can propagate in the full space. The extra dimension could even be macroscopically large. Deviations from the Newton's law, the gravitational pull between two bodies at distance r decreases proportionally to $1/r^2$, are expected for values of *r* of the order of or smaller than the radius of the extra dimension, *R*. The present experimental tests of the Newton's law do not exclude this possibility for *R* of the order of a micron, or smaller

going in the new dimensions, confinement to the p-brane assures it. The only limit to the radius arises from the fact that the Newton law we observe with macroscopic bodies (force inversely proportional to the square of the distance) is itself indicative of a three-dimensional space. But we have checked Newton's law only down to distances of millimetres or, more recently, microns.[12] This leaves open the issue of a macroscopic KK radius R! Gravity in the full multidimensional space would be still characterized by a constant of the dimension of a mass, but if the radius is large, this constant could be of the order of 1 TeV, thus eliminating the disparity of scale between the W mass (about 0.1 TeV) and the mass which characterizes gravity in three dimensions, the so-called Planck mass of order 10^{16} TeV.

If this picture were true, the mass of KK excitations of the graviton would be way smaller than in the other case. Also they would not be stable, since the p-brane can absorb any momentum in the additional dimensions. In fact, one could ask if reactions like:

$$e^+ + e^- \to \gamma + (KKtower - of - gravitons)$$

[11] N. ARKANI-HAMED, S. DIMOPOULOS and G. R. DVALI, *Phys. Lett.* B 429, 263 (1998).

[12] C. D. HOYLE et al., Feb. 2001.

[13] L. HALL, ICHEP2000, Osaka.

are already occurring at LEP. If one can produce excitations of the graviton up to very high order, a large cross-section would result. This process would then produce a typical distortion at low energy of the photon spectrum in the reaction:

$$e^+ + e^- \rightarrow \gamma + (unobserved - particles)$$

with respect to what predicted by the Standard Theory. No distortions have been observed so far at LEP.

The LHC would push further the limit on the additional dimension (or observe it!) with the study of reactions like:

$$P+P \rightarrow gluon + (unobserved - particles)$$

The Large Hadron Collider

Started in 1996, the construction of the Large Hadron Collider proceeds at full speed at CERN. The LHC is a proton-proton or ion-ion collider to be housed in the 27 km underground circular tunnel nearby Geneva, where the LEP collider has been operating until the end of the year 2000. The design energy of each proton beam is 7 TeV, corresponding to the design magnetic field of 8.1 Tesla in the super-conducting dipoles that keep the protons in circular orbits inside the tunnel. Oppositely circulating beams cross in eight fixed points around the circumference, four of which are reserved for the experiments. Protons are packed in very dense bunches which are stacked in the orbit at intervals of about 7.5 m in space (25 ns in time) from each other. Very high design luminosity is foreseen, of 10^{34}cm^{-2}sec^{-1}. At this luminosity, there are about 30 elementary proton-proton collisions at each bunch crossing and correspondingly a very high flux of secondary particles bunched with a 40 MHz frequency.

The limited transverse size of the tunnel and the need of very high magnetic field have required an innovative and compact design for the accelerator.

Each dipole has two parallel apertures, which house the vacuum pipes. Coils are designed so that the magnetic field is oriented in opposite directions in the two apertures. Therefore, two independent proton beams can run in opposite directions inside the same dipoles.

A current of 12 kA is circulating in super-conducting cables, kept at 1.8 °K by super-fluid Helium. Cables are made of Ti-Nb filaments, imbedded into a copper matrix.

In turn, the super-fluid Helium is distributed by a cryogenic line, which runs in parallel to the dipoles.

One problem of high-energy, high-field cryogenic machines is synchrotron radiation. Increasing with the fourth power of energy, synchrotron radiation in the LHC deposits about 0.2 W/m. In cryogenic machines, this power is expensive to carry away, due to the low temperature of the walls where it is to be dissipated (thermal capacity goes like the fourth power of the absolute temperature). For this reason, in the LHC, there is an inner beam screen kept at about 19 K (gaseous helium cooling), considerably higher than the 1.8 K temperature of the cold mass of the dipoles.

There are in all 1236 dipoles. Alternating with the dipoles, super-conducting quadrupoles complete the main magnetic structure of the LHC. The basic cell of the structure is 120 m long (with 6 dipoles and 2 quadrupoles). Figure 9 shows an artist's view of the LHC.

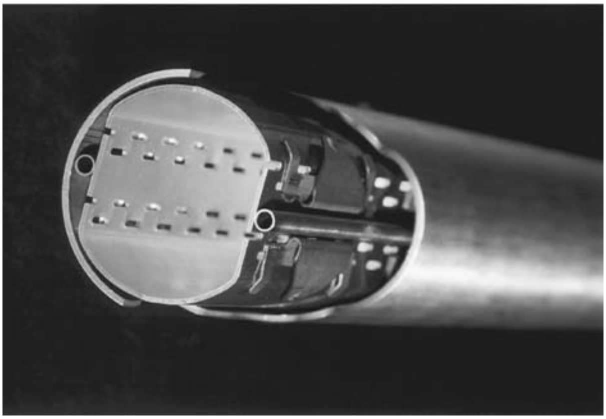

Figure 8
In high-energy cryogenic proton machines, the power deposited by synchrotron radiation is difficult to remove because of the low temperature of the dipoles, 1.8 °K. In the LHC the heat is deposited on the an inner tube, which is kept at 19 °K by gaseous helium coolant circulating in the small tubes visible on both sides of the inner tube

Figure 9
Artist's view of the LHC. Dipoles in blue, quadrupoles in white. The cryoline is not visible, except for the grey tube shaped at right angles, which feeds the super fluid He in the cryostats of the magnets

Figure 10
A view of String2 in the SM18 Hall of CERN (September 2001)

A half-cell of the LHC (called String2) is at present operating at CERN, with dipoles and quadrupoles of the final design, to test the properties of the very complex magnetic system. Figure 10 shows a picture of String2 in the SM18 Hall of CERN. String2 has been powered for the first time on Sept. 27, 2001, when it has reached successfully the 12 kA current, corresponding to the nominal magnetic field (and energy) of the LHC.

At the moment of writing (April 2002) the R&D and prototyping phase is completed. The main industrial contracts (cables, dipole assembly, cryogenic line) have been adjudicated and signed; industrial production and installation have started. It will be quite a remarkable enterprise. For one the production of super conducting cable for the LHC amounts to little less than 30% of the world production.

The excavation of the two big halls for the general-purpose experiments, ATLAS and CMS, is well advanced, after several problems, in particular for the CMS cavern.

The caverns will be handed over to the experimental collaborations in April 2003 (ATLAS) and July 2004 (CMS).

A new schedule for the commissioning of the LHC has been recently defined, which foresees the super-conducting dipoles completed in mid 2006

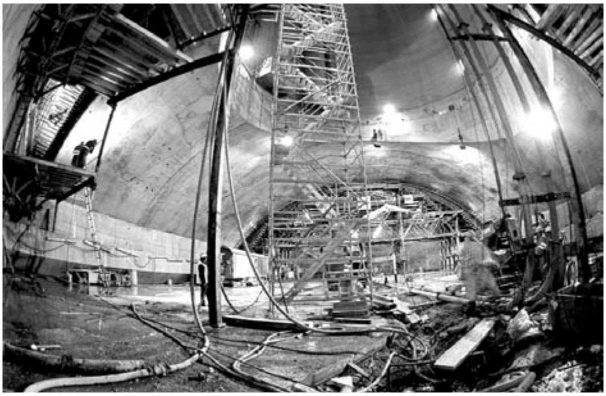

Figure 11
The vault of the ATLAS cavern (September 2001). The concrete vault is suspended with cables, to allow for the excavation of the lower part of the cavern, down to some 30 m below the ground level shown in the picture. The ATLAS cavern is the biggest in the world to be excavated in the type of rock (molasse) present below CERN

and the first physics in 2007. The schedule is based on the contractual dates for the main items.

In 2001, the LHC has gone through a mid-project review of the cost to completion. The review indicates some 20% global extra cost for the machine hardware and installation and for the preparation of the experimental halls. A discussion is going on between the Council and CERN Management to compensate for the extra costs, which envisages a plan for savings, the reduction of non-LHC activities and consequent budget reallocation to the LHC and the prolongation of the period of payments up to the year 2010.

Experiments at the LHC

The effective energy in proton-proton collisions is directly related to the energy carried by the proton constituents, quarks and gluons. In turn, the constituent density decreases as its energy approaches the proton full energy. The collision probability itself, in addition, decreases with constituent energy. Thus the energy range that can be explored with a proton-proton collider is considerably limited with respect to the nominal beam energy. However, with a given beam energy and a given running time it is still possible to observe the interactions of the harder constituents, *provided* we have enough proton collisions per unit time, i.e. a sufficiently high luminosity. Luminosity can be traded for energy. At a given beam energy, as luminosity goes up the interactions of the harder constituents become more and more visible and the machine potential for discovery goes up.

This concept is particularly important for the LHC, which had to fit in the existing LEP circular tunnel, of a radius of about 4 km. With the magnetic field also limited by the available technology to 8-9 Tesla, the luminosity handle has been vital to extend the discovery potential of the machine well inside the TeV region, where signals of new physics are expected.

From the start, the LHC has aimed at values of luminosity one order of magnitude larger than what could be considered as "normal" for proton machines, and what detectors of the time could stand. To design detectors capable to face the luminosity challenge, a large R&D program and important conceptual developments have been required. Key issues have been radiation hardness and capability to handle the enormous flux of information, which goes through the detectors (the products of 30 high-energy collisions repeating at 40 MHz frequency). The programme has produced very innovative detectors, now in the phase of industrial production, and is essentially over.

Four experiments are foreseen at the LHC. Two general-purpose detectors, ATLAS and CMS, to search for the Higgs boson, signals of super-symmetric particles and what ever else may be found at high energy.

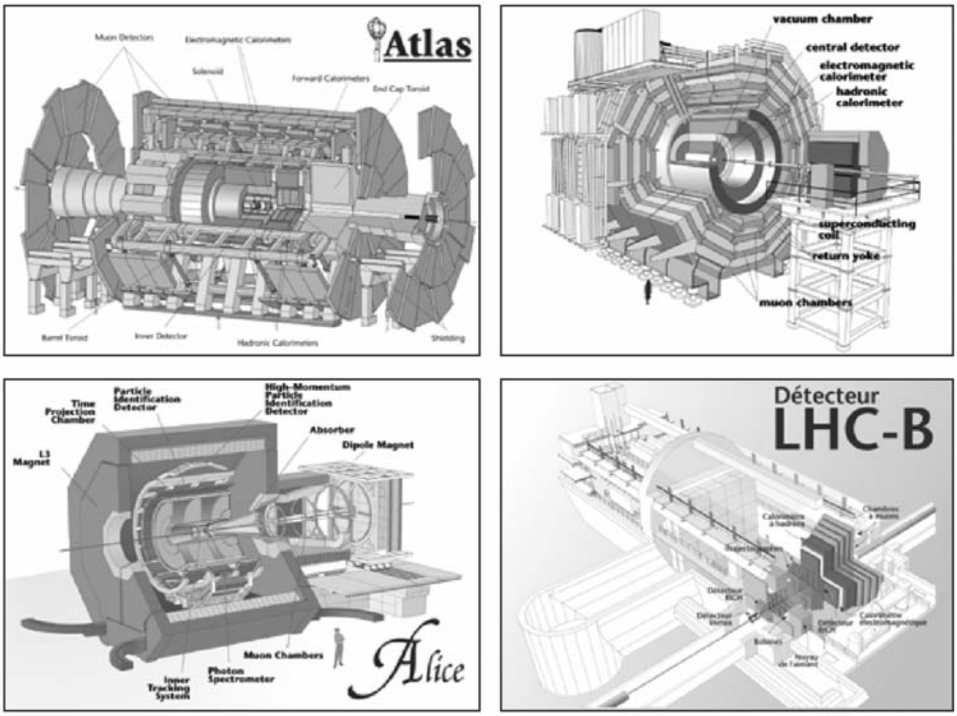

Figure 12
Artist's view of the four LHC detectors

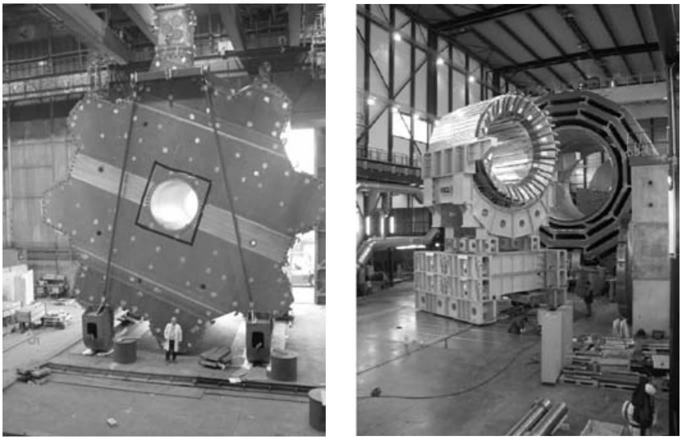

Figure 13
Left: the ATLAS End-Cap cryostat during construction. *Right:* parts of the CMS detector being assembled at CERN

A smaller size detector, ALICE, is designed to study the high-energy heavy ion collisions. At these energies, the collision is supposed to produce a new state of nuclear matter, the quark-gluon plasma, where quarks and gluons are not confined inside hadrons as it happens at low temperature. Hints of the new phase have been observed at CERN, with the SPS, and similar collisions, at higher energy with respect to the SPS but still lower than those of the LHC, are being studied at the Relativistic Heavy Ion Collider, RHIC, in Brookhaven. Finally, a fourth detector, LHC-b, is optimised for the study of CP violation in decays of particles containing the b quark, extending and completing the studies which are being done at present with the so-called B-factories, in the US and in Japan.

The LHC experiments will have to deal with enormous quantity of data. The amount of information that will come out of the collisions is the order of the information carried by 300 million simultaneous phone calls, the information accumulated in one year will be in the order of some 10^{15} bytes. Analysing and storing this information will require technologies which are still in development.

Another problem is the distribution of the information to the global collaboration, which build and will operate the detectors. A new infrastructure is needed, to make available the data and the applications to analyse them to the single users diffused around the globe. Because of the similarity with the power grid, which distributes the electrical power to a diffused population of users, this new infrastructure has been named the "Computing and Data Grid" (see Figure 14). The Grid concept will find application in many fields other

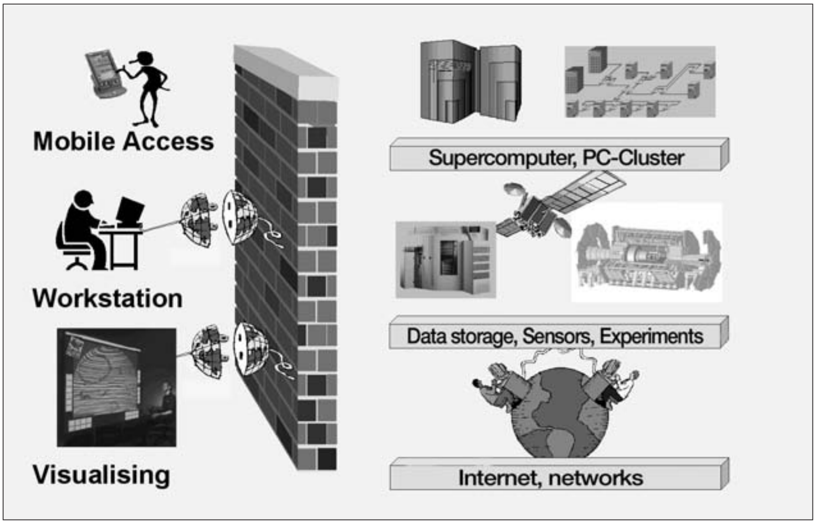

Figure 14
Schematic representation of the Grid. The analogy with the distribution of energy in the power grid is emphasized

than particle physics.[14] The development of the LHC Data Grid is raising a considerable interest in other scientific disciplines and in industry.

Conclusions

Many of the problems that Fermi could enumerate in the fifties have been solved by the Standard Theory, notably the composite nature of the hadronic particles (proton, mesons and so on) and the common origin of the weak and electromagnetic forces. The next generation of accelerators should shed light on the new problems that the Standard Theory leaves unsolved.

There are many fascinating discoveries waiting for us in the High Energy Frontier. They range from what we could define as *"normal business"* – finding the Higgs boson or discovering low-energy SUSY – to *"new world"*, like finding that there are extra dimensions in our space-time.

The High Energy frontier does not exhaust particle physics. We certainly need to understand better the physics of flavour, that is neutrinos and the ori-

[14] *The Grid: blueprint for a new computing infrastructure,* I. FOSTER and C. KESSELMAN editors, Morgan Kaufmann Publishers Inc., San Francisco, California, 1999.

gin of matter antimatter symmetry violation.

Developing new tools for particle acceleration is still the key to affordable high-energy. More than ever, we need to support research in the field of particle accelerators, in big laboratories like CERN, but also, and most importantly, in our Universities.

As for strategy, a consensus is emerging on the roadmap to High-Energy Physics. First, and most important, the LHC has to be completed as soon as possible and exploited. The LHC is supposed to give us the much-needed indication of what is the solution to the problem of particle masses (the Higgs boson?) and of the hierarchy of mass scales (SUSY?). The complete exploration of the sub-TeV region, particularly in the lepton sector, requires in addition a high luminosity, e^+e^- Linear Collider in the class which is now arriving to technological maturity (either based on super-conducting cavities as in the TESLA project at DESY, Germany, or on warm cavity technology of the NLC at SLAC, US, and of the JLC at KEK, Japan).

The next step would be a Multi-TeV accelerator, something for which we do not have the appropriate technologies, yet. The most advanced study today refers to the two-beam accelerating principle for electrons developed at CERN (the Compact LInear Collider project, CLIC), capable to produce field gradients in excess of 150 MeV/m (i.e. 3 TeV over 20 km!). A Very Large Hadron Collider, a proton-proton collider with 200 TeV c.o.m. energy has been considered in Europe (the Eloisatron project) and is studied at the Fermi National Laboratory, in the US. Proposals and studies of a $\mu^+ \mu^-$ collider are being entertained in US and, to a minor extent, in Europe but are still in their infancy.

On the flavour physics side, the violation of matter-antimatter symmetry, now studied with the so-called B factories at SLAC and KEK, will be continued with the LHC.

A long baseline neutrino beam exists in Japan (K2K, from KEK to the Kamioka underground laboratory) others are being built in CERN (from CERN to Gran Sasso, in Italy) and the Fermi National Laboratory (from FNAL to the Soudan mine). Studies for the production of very intense neutrino beams (Neutrino-factory) are being carried out in the US and in Europe, to produce a new generation of long-baseline neutrino beams, such that could be detected in underground laboratories placed at distances of some thousand kilometres. Similar developments are being considered in Japan, in connection with the construction of a very intense proton source, the Japan Hadron Facility.

Can we realise the ambitious plans I have just described, in a reasonable time, say 15 to 20 years? Can we afford them? It is becoming more and more clear that new mechanisms for international collaboration are needed, carrying further what has been done for the LHC. Better efficiency is needed in decision-making, now left to separate negotiations inside each region (Europe, US, Japan) and to difficult approaches between different regions. Also, mechanisms must be found to follow the user distribution, in order to keep in the picture the young generations, which are formed in the Universities.

To realize the full programme, a transition to a new global organisation may be necessary, similar to the transition that Europe underwent fifty years ago, from National Laboratories to CERN. The wide discussion, which has started in the last years, gives reasons to believe that a solution may be not too far.

Luciano Maiani
Luciano Maiani was born in Rome in 1941. He obtained his degree in physics in 1964 and started working as a researcher at the Istituto Superiore della Sanità in 1964. He collaborated with Prof. R. Gatto's group in the field of Teorethical Physics at the University of Florence. He became post-doctoral fellow at the Lyman Laboratory of Physics, Harvard, in 1969, visiting Professor at the Ecole Normale Supérieur in Paris in 1977 and at CERN in Geneva in 1979.
He has been Professor of Theoretical Physics at the University of Rome "La Sapienza" since 1976, was President of the INFN from 1993 to 1998, and is presently Director General of CERN, the European Organization for Nuclear Research located in Geneve.
Luciano Maiani is author of over 100 scientific publications on the theory of elementary particles. His most important achievement, reached with the collaboration of Gashow and Iliopolous, is the theoretical prediction of "charmed particles", that has been crucial to the formulation of the unified theory of Electroweak Interactions.
He is member of the Accademia dei Lincei and the Accademia Nazionale delle Scienze, also called XL.
He was awarded the Honorary Degree by the University of Aix-Marseille, St. Petersburg's and the Slovak Academy of Science. He was given the Matteucci medal by the Accademia Nazionale delle Scienze, also called XL, and the J.J Sakurai prize by the American Physical Society.

Chen Ning Yang

Enrico Fermi

They that have power to hurt and will do none,
That do not do the thing they most do show,
Who, moving others, are themselves as stone,
Unmoved, cold, and to temptation slow;
They rightly do inherit heaven's graces,
And husband nature's riches from expense;
They are the lords and owners of their faces,
Others but stewards of their excellence.[1]

Enrico Fermi was, of all the great physicists of the 20th century, among the most respected and admired. He was respected and admired because of his contributions to both theoretical and experimental physics, because of his leadership in discovering for mankind a powerful new source of energy, and above all, because of his personal character. He was always reliable and trustworthy. He had both of his feet on the ground all the time. He had great strength, but never threw his weight around. He did not play to the gallery. He did not practise one-up-manship. He exemplified, I always believe, the perfect Confucian gentleman.

Fermi's earliest interests in physics seem to be in general relativity. Starting from around 1923 he began to think deeply about the "Gibbs paradox" and the "absolute entropy constant" in statistical mechanics. Then, as Segrè later wrote,[2]

> As soon as he read Pauli's article on the exclusion principle, he realized that he now possessed all the elements for a theory of the ideal gas which would satisfy the Nernst principle at the absolute zero, give the correct Sackur-Tetrode formula for the absolute entropy in the limit for low density and high temperature, and be free of the various arbitrary assumptions that it had been necessary to introduce in statistical mechanics in order to derive a correct entropy value.

This research led to his first monumental work, and to the "Fermi distribution", "Fermi sphere", "Fermi liquid", "Fermions", etc.

It was characteristic of Fermi's style in research that he should follow this abstract contribution with an application to the heavy atom, leading to what is now known as the Thomas-Fermi method. The differential equation involved in this method was solved by Fermi:

---

1 *Shakespeare's Sonnets,* No. 94.
2 E. Segrè, in *Collected Papers of Enrico Fermi (*1962, Univ. of Chicago Press), p. 178.

> Numerically with a small and primitive hand calculator. This numerical work took him probably a week. E. Majorana who was a lightning-fast calculator and a very skeptical man decided to check the numerical work. He did this by transforming the equation into a Riccati equation and solving the latter numerically. The result agreed exactly with the one obtained by Fermi.[3]

Fermi's love of the use of computers, small and large, which we graduate students at Chicago observed and admired, began evidently early in his career and lasted throughout his later life.

Fermi's next major contribution was in quantum electrodynamics, where he succeeded in eliminating the longitudinal field to arrive at the Coulomb interaction. Fermi was very proud of this work, as his students at the University of Chicago in the years 1946-1954 knew. (But it seems today that few theorists under the age of 65 know about this contribution of Fermi's). It again was characteristic of Fermi's style that in this work he saw through complicated formalisms to arrive at the basics, in this case a collection of harmonic oscillators, and to proceed to solve a simple Schrödinger-like equation. The work was first presented in April 1929 in Paris and later at the famous Summer School at Ann Arbor in the summer of 1930. G. Uhlenbeck told me in the late 1950's that before this work of Fermi, nobody *really* understood the quantum theory of radiation, and that this work had established Fermi as among the few top field theorists in the world.

I shall skip describing his beautiful contribution in 1930 to the theory of hyperfine structure, and come to the theory of β-decay. According to Segrè, Fermi had considered, throughout his life, that this theory was his most important contribution to theoretical physics. I had read Segrè's remarks in this regard, but was puzzled. One day in the 1970's, I had the following conversation with Eugene Wigner in the cafeteria of Rockefeller University:

Y: What do you think was Fermi's most important contribution to theoretical physics?

W: β-decay theory.

Y: How could that be? It is being replaced by more fundamental ideas.
Of course it was a very important contribution which had sustained the whole field for some forty years: Fermi had characteristically swept what was unknowable at that time under the rug, and focused on what can be

---

[3] F. RASETTI, in *Collected Papers of Enrico Fermi* (1962, Univ. of Chicago Press), p. 277.

calculated. It was beautiful and agreed with experiment. But it was not permanent. In contrast the Fermi distribution *is* permanent.

W: No, no, you do not understand the impact it produced at the time. Von Neumann and I had been thinking about β-decay for a long time, as did everybody else. We simply did not know how to create an electron in a nucleus.

Y: Fermi knew how to do that by using a second quantized ψ?

W: Yes.

Y: But it was you and Jordan who had first invented the second quantized ψ.

W: Yes, yes. But we never *dreamed* that it could be used in *real physics*.

I shall not go into Fermi's later contributions. Nor into his relations with students which I had written about before.[4] I shall only add a couple of stories about Fermi.

One of Fermi's assistants at Los Alamos during the war was Joan Hinton, who became a graduate student at the University of Chicago after the war. When I began working in late 1946 for Sam Allison, she was a fellow graduate student in the same laboratory. In the spring of 1948 she went to China and married her boy friend Sid Engst and settled down in China permanently to do agricultural work. (Hers was a very interesting story that should be written down. I hope she will do it soon). In the summer of 1971 during my first visit to the New China, half a year before Nixon, I accidentally met her in a hostel in Da-zhai, then a model agricultural commune in the County of Xi-Yang. Surprised and delighted, we reminisced about the Chicago days: how I was awkward in the laboratory, how I almost accidentally electrocuted her, how I had taught her a few sentences of Chinese, how I had borrowed a car and had driven her to the La Salle station to embark on her long trip to China, etc., etc. She asked me whether I remember the farewell party that the Fermis had given her before she left. I did. Did I remember the camera that they had given her that evening? No, I did not. After a pause, she said she had felt, a few days before that farewell party, that she should tell Fermi about her plan to go to the communist controlled area of China. So she did. And what did Fermi say? "He did not object. For that I am eternally grateful". I considered this such an important statement[5] that after coming back to Stony Brook, I called Mrs. Fermi in Chicago and reported to her my whole encounter with

---

4 C.N. Yang, in *Collected Papers of Enrico Fermi* (1962, Univ. of Chicago Press), p. 673.

5 Joan Hinton went to China in the spring of 1948, before the Chinese communists' victory over Chiang Kai-Shek, and two years before the Korean War. If she had planned to go to China after the beginning of the Korean War, I am sure the US Government would not have allowed her to go.

Joan in Da-zhai. A few years later, Joan visited Chicago herself and had the opportunity to visit with Mrs. Fermi and her daughter, Nella Fermi.

I shall end this article by quoting from page 48 of my *Selected Papers* (1983):

> Fermi was deeply respected by all, as a physicist and as a person. The quality about him that commands respect is, I believe, solidity. There was nothing about him that did not radiate this fundamental strength of character. One day in the early 1950's, J. R. Oppenheimer, who was the Chairman of the important General Advisory Committee (GAC) of the Atomic Energy Commission (AEC), told me that he had tried to persuade Fermi to stay on the GAC when Fermi's term was up. Fermi was reluctant. He pressed, and finally Fermi said, "You know, I don't always trust my opinions about these political matters".

Chen Ning Yang

Chen Ning Yang, son of a mathematician, was born in 1922 in Hofei (Anhwei). Upon graduating from college, in 1946, he was awarded a scholarship to the University of Chicago, where he worked initially with Fermi and Teller. In 1949 he and Fermi co-authored an article in which they discussed the possibility that pions were particles composed of one nucleon and one antinucleon. When physicists began to discover strange particles, Fermi's and Yang's idea was taken up by Shoichi Sakata, whose work contributed to the development of the idea of quarks by M. Gell-Mann and G. Zweig.

In 1949 Mr. Yang joined the Institute for Advanced Studies at Princeton, and in 1955 became a professor. In 1966 he was appointed head of the department of theoretical physics at the State University of New York at Stony Brook.

In 1954, in collaboration with R. L. Mills, Mr. Yang tried to explain interaction among nucleons by the request for local gauge invariance under the SU(2) isospin group. The attempt failed in this specific case but opened the way to the formulation of the theory of electroweak interactions by S. Glashow, A. Salam and S. Weinberg.

Given the difficulties encountered in interpreting certain mesonic decay phenomena, in 1956 Mr. Yang, in collaboration with Tsung Dao Lee, advanced the theory that symmetry through spatial reflections might be violated in weak interactions. The two scientists suggested possible experimental controls independent of this hypothesis, which was demonstrated in 1957 by the Chinese physicist Chien Shung Wu.

That same year, Lee and Yang were awarded the Nobel Prize.

Giorgio Salvini

Concluding Remarks

We are at the end of these days dedicated to Enrico Fermi. It has been an immersion in that first half of our recently past century, and a further invitation to mediate those origins which determined a new progress of our knowledge, and new questions on the future of men.

Looking from the high levels that we reached due to the contributes of Fermi and his great coevals, Heisenberg, Pauli, Jordan, Dirac, Wigner, we have seen the consequences to day of their work. The new vision of physics apparead so dazzling to some scientists that they thought that the extreme possible limits of our scientific knowledge had been reached. We know to day that those genii rather opened the door of a new world which we still have to explore, in its many dimensions of space and time, in the relation between gravity and other forces, in the properties of order, chaos and complex systems. A new world which we only scratched until now.

It is a merit of these great scientists not only to have given the leading ideas, but also having shown the precise methods to resolve an immense number of specific problems, and to get preliminary but clear conclusion. Are they superb mountain guides of existing mountains, or they also invented and opened new peaks and valleys? I am not able to reply to this question.

Historians shall have still to work on the documents left to us from Enrico Fermi. We know that his print on the politics of this time is not the main mark of his intense life, but still it is very important for uman history. In fact we can see in Enrico Fermi also the greatest direct link between sciences and other human activities. T. Regge wrote, in 1992 (Fermi symposium, p. 216):

> "Our responsability in healing the rift between Science and Society is immense, our failure to do so will place a heavy burden on future generations, and threaten life on that starship called Earth which looks smaller by the day".

The life of Fermi was too short. But every day of him was important and significant, and controlled by a calm intense curiosity for the facts of nature, and the results from the laboratory.

He was constently a great subtle teacher, in Rome, in Los Alamos, in Chicago. Let me recall what Valentino Telegdi said of Fermi as a teacher in Chicago, where he was one of his best students and collaborators.

> "No single individual in this century has contributed so much to physics, through theory as well as axperiments, as did Enrico Fermi. Still, in this speaker opinion, his greatest contribution in the Chicago period lays in his teaching. Through his students and their teachings, the Fermi spirit is still alive to day" (Quoted Fermi Symposium, p. 71).

Many of those who remembered him in this room during these days did join to the admiration for the capacity of Fermi to illuminate the mind of this colleagues and listeners. We had the privilege of listen the comments of persons who spent days with him as their students and coworkers:

Leon Lederman, whom I knew since when he was a student of Gilberto Bernardini in Illinois, wrote:

> "It is clear that Enrico Fermi's personal leadership, his scientific style and his influences on students was a major force in the establishment of physics in the United States".

Chen Ning Yang wrote:

> "Enrico Fermi was, of all the great physicist of the 20th century, among the most respected and admired. He was always reliable and trustworthy. He exemplified, I always believe, the perfect Confucian gentleman".

Let me make a final comment on Enrico Fermi, and our destiny. The character of Enrico Fermi may also be an example to us. His life developed through political difficulties and dangers and he had take in short time decisions of fundamental importance for him, his country, his family. He succeded to make his choices with calm and a strong determination, and always worrying for the destiny of the others, students, coworkers, colleagues.

We cannot foresee the future of human kind, and we know that also Fermi was thoughtful and uncertain about it. Our future shall depend on us, ideas, schools, capacity to understand and help each other. It will depart from our work to day, and develop on unpredictable ways. But we have the responsability to prepare for it the best starting line.

This conferences dedicated to Enrico Fermi, came at the right moment in these difficult times, as an invitation to ponder our future.

Report on the Celebration for the Centenary of Enrico Fermi's Birth

Carlo Bernardini, Rocco Capasso

By decree of 11 June 1999, the Minister for Cultural Properties and Activities, responding to a request dated 5 March 1998 from the Italian Society for the Progress of Science, gave the National Committee "the task of promoting, preparing and implementing events suitable to celebrate the centenary of the birth of Enrico Fermi".

The National Committee for the Celebrations of the Birth of Enrico Fermi (1901-2001) was made up of the Minister for Cultural Properties and Activities; the Foreign Minister; the Education Minister; the Minister of Universities and Scientific and Technological Research; the Industry Minister; the President of the Latium Region; the President of the Tuscany Region; the President of the Province of Rome; the President of the Province of Pisa; the Mayor of Rome; the Mayor of Pisa; the Rector of "La Sapienza" University of Rome; the Rector of the University of Pisa; the Director of the Pisa Superior Normal School; the Director-General of the Central Office for Bibliographic Properties, Cultural Institutions and Publishing; the Director General of the Central Office for Archaeological, Architectural, Artistic and Historical Properties; the Director General of the Central Office for Archival Properties; the Foreign Ministry's Director General for Cultural Relations; the Education Ministry's Director General for Cultural Exchange; the President of the Italian Society for the Progress of Science "called" of the XL; the President of the National Lincei Academy; the President of the National Academy of Science; the President of the Pontifical Academy of Science; the President of the National Research Council; the President of ENEA; the President of the National Institute for Nuclear Physics; the President of RAI (the Italian state broadcaster); the President of the National Electrotechnical Industry Association; the President of the Italian Encyclopedia Institute; the President of the Italian Physics Institute; the President of the Italian Nuclear Association; Professors Luigi Amerio, Franco Bassani, Enrico Bellone, Carlo Bernardini, Fabio Bevilacqua, Gianni Bonera, Rocco Capasso, Gianfranco Chiarotti, Renato Cialdea, Umberto Colombo, Maurizio Cumo, Luigi Dadda, Michelangelo De Maria, Alberto Gigli Berzolari, Giulio Giorello, Giuseppe Giuliani, Giorgio Israel, Salvatore Lorusso, Luciano Maiani, Carmine Marinucci, Franco Prattico, Alessandro Pascolini, Renato Angelo Ricci, Carlo Rubbia, Giorgio Rumi, Giorgio Salvini, Fabio Sebastiani and Roberto Vergara Caffarelli.

At its first meeting, on October 19, 1999, the National Committee elected Carlo Bernardini as its chairman and Rocco Capasso as secretary-treasurer.

As proposed by the chairman, Mr. Bernardini, the committee voted to set up an executive board composed of Franco Bassani, Carlo Bernardini, Rocco Capasso, Renato Cialdea, Maurizio Cumo, Michelangelo De Maria, Giorgio Israel, Salvatore Lorusso, Carmine Marinucci, Alessandro Pascolini, Renato Angelo Ricci, Giorgio Salvini, Fabio Sebastiani and Roberto Vergara Caffarelli.

The National Committee for the Celebrations of the Birth of Enrico Fermi organized a series of projects with high cultural content, including a book titled *Conoscere Fermi* (All About Fermi), an international conference, an exhibition, a CD-ROM, a Web site, a documentary film, an historical marker, a postage stamp and the volumes on the subject of "Enrico Fermi".

Book "Conoscere Fermi"

The book (17.5x24.5 cm, 385 pp., 48 ill., 24 b&w photos) was produced by the editorial staff of the Italian Physics Society with funding from the Education Ministry.

The texts, intended to be considered for use in teaching physics to high school seniors, were written by twenty eminent physicists.

News conference (28 Sept. 2001)

The National Committee held a news conference on September 28, 2001, to present the programme of events. On this occasion the Italian media were able to talk with the illustrious guests, in particular with some of Enrico Fermi's direct descendants. The guests included A. Agnew, G. Holton, J. Orear, Giulio Fermi's wife and children, Nella Fermi's daughter, and the National Committee's president and secretary-treasurer.

Exhibition "Enrico Fermi and Universe of Physics" (29 Sept. - 5 Nov. 2001)

The exhibition and the international conference were both held in Rome, at the Dioscuri Theatre exhibition and conference complex operated by the Cultural Properties Ministry's Directorate-General for Bibliographic Properties and Cultural Institutions.

Visitors at the Exhibition "Enrico Fermi and the Universe of Physics"

The exhibition was opened by the President of the Republic on September 29, 2001, the centenary of Fermi's birth, and ran through November 5. The opening ceremony was attended by leading representatives of Italian science and culture (including Giorgio Salvini; Edoardo Visentini, president of the National Lincei Academy; Carlo Rubbia, president of ENEA; Enzo Iarocci, president of the Italian Institute for Nuclear Physics; and Francesco Sicilia, Director-General for Bibliographic Properties and Cultural Institutions) and many of Fermi's relatives, some of whom live in the United States.

The exhibition was designed for the general public, in particular high school students. It described Fermi's life and work in Italy and America, a human experience that paralleled and helped determine the history of the 20th century. Enrico Fermi made an enormous contribution to the rebirth of science in Italy, and left a cultural heritage that is still bearing fruit through the work of his students.

To help young students understand the various apects of Fermi's contributions to physics and modern technology, the exhibition was organised along an historical path divided into seven phases (reflecting the architectural structure of the Dioscuri complex) centring on the principal events in the great scientist’s life: the days of a young genius, on the brink of fame, an Italian school of science, refuge in America, a new era begins, Fermi’s legacy in Italy and the world, his last message.

On display were original scientific instruments from Rome University’s museum, Fermi’s notebooks from the Domus Galileana in Pisa, letters from the University of Rome’s Amaldi Archive, Fermi’s essay for admission to the Pisa Normal School, books used by Fermi as a student and books he wrote, a fraction of the moderator from the first atomic pile, and scientific instruments used today by researchers in fields opened by Fermi. Besides photographic materials, the exhibition contained six areas for viewing documents on film from the historical archives of Italy’s Istituto Luce, RAI-TV, and the Argonne, Los Alamos and Oak Ridge laboratories in the United States, plus a multimedia station, to give visitors a better grasp of the most important moments in Fermi’s life and work.

The exhibition’s original design enabled the materials to be shown to their best advantage, combining objects, texts, images and video inserts in a unitary presentation. An important contribution to its effectiveness was provided by the constant presence of university physics students, who gave visitors the benefit of personal contact and adapted the viewing paths to their specific interests.

The opening of the International Conference “Enrico Fermi and the Universe of Physics”

International Conference "Enrico Fermi and the Universe of Physics"
(29 Sept. - 2 Oct. 2001)

Documentary film on the Fermi exhibition

This film (37′), coordinated by Alessandro Pascolini, organised as a visit to the exhibition, is of extraordinary topical interest. Perfectly up-to-date and interwoven with interviews and brilliantly structured inserts, it is a model of effective and innovative scientific communication and popularisation.

CD-ROM

The CD-ROM was planned by the National Committee, co-ordinated by Michelangelo De Maria, created by UNICITY and produced by CASPUR at the end of the Fermi celebrations. Its main purpose was to "reconstruct Enrico Fermi's life and work in the larger institutional, political, scientific and technological context of an era of enormous historical and social upheavals".

The CD-ROM is a multimedia "gateway" by which to enter Fermi's life as a man and a scientist against the background of his historical period. The film segments, animations, photographs and original documents that interpret and recount Fermi's life up to the time of his move to the United States occupy more than an hour's time and constitute a significant account of the scientist and his times. It contains over 500 photos and documents, many of them not easy to come by, that describe various aspects of Fermi's scientific training, development and research.

The CD-ROM also shows Fermi receiving the Nobel Prize, his Chicago period, the construction of the first atomic pile, and the historic event on December 2, 1942. It includes accounts by A. H. Compton, L. Marshall and L. Szilard (in English, with Italian translations available), and illustrates Fermi's 1954 lectures at Varenna and his last speech. On the CD, Fermi lives on in the memory of Laura Fermi, Edoardo Amaldi, Emilio Segrè and other friends and colleagues who describe the man they knew.

This wealth of original documents, animations and graphics is compounded by over 400 pages of historical texts written for the CD-ROM.

The Physics Glossary, with around a hundred entries, enables non-specialist users to approach the world of modern nuclear and particle physics.

The Biographies section presents, with texts and photos, around a hundred scientists to whom it seemed appropriate to call the attention of contemporary Italian culture.

In sum, the CD offers a new way of understanding 20th century history, a new way to study its facts, events, politics, culture, science and society.

Under an agreement between CASPUR and the Italian magazine *Le Scienze,* the CD-ROM was distributed with the magazine's March 2002 issue and kept available at newsstands through September 2002, and can be ordered by mail for a period of three years. This means that as of October 2002, CASPUR, in agreement with the E. Fermi National Committee, can distribute the CD to schools, universities, libraries, etc.

Web site

The Web site went on line at *www.enricofermi.it* even before the exhibition opened at the Dioscuri complex. Besides containing many of this exhibition's materials (including the entire text of the book *Conoscere Fermi,* which can thus be acquired by downloading), it includes those presented at the one organised by the Ministry of Universities and Research in the spring of 2001 on "One Thousand Years of Science in Italy".

Historical marker

In agreement with Rome city councillor Giovanni Borgna's press office, an historical marker was unveiled at Via Gaeta 19 on September 28, 2001. In addition to local authorities and representatives of the National Committee, the ceremony was attended by Sarah Fermi, Daniel and Rachel Fermi, Jay Orear and Harold Agnew. The event was reported in various newspapers, including Corriere della Sera and La Nazione.

Postage stamp

The Italian Post Office issued a commemorative stamp in 3.5 million copies. Enrico Fermi is shown in the left foreground, the atomic pile on the right, and in the background a backboard on which appear Fermi's handwriting and signature. The name ENRICO FERMI, the dates "1901-1954", the word "ITALY" and the denomination "800 – € 0,41" complete the stamp. On the issue date, the Rome Philatelic Office placed on sale a postcard reproducing the commemorative stamp, priced at 1000 lire plus the stamp's face value.

Volumes

Besides this volume, the National Committee has also sponsored the Fermi centenary celebrations organized by SIF (Italian Society of Physics) and ICRA (International Center for Relativistic Astrophysics), with the engagement of partly financing both a special edition of the "Nuovo Cimento B" by SIF – containing most of the papers on Fermi – and the volume "Fermi and Astrophysics" by ICRA, to be published and diffused worldwide by the World Scientifics of Singapore.

In the opinion of the Committee, the organisers and a large part of the media (press, radio, TV and the Internet), the events to date have indeed helped bring back to Italy the extraordinary figure of one of the greatest physicists of all time. Public interest seems clearly destined to increase, in part thanks to the publication of this volume of *Proceedings,* edited by ENEA and the Italian Society for the Progress of Science, which constitutes an extraordinary reference source for scholars.

The community of physicists, research institutes and scholars now has the important task of keeping alive the memory and the example of this great scientist, at once a theoretical and an experimental physicist, an engineer and a mathematician. In a way, this many-sided nature of Fermi's was absolutely unique on the world scene, and one may well wonder how the precocious student came to attain such heights. Much is known of his studies, but it is not yet clear what price the young man of 18 paid, in terms of commitment and sacrifice, to be able to outstrip all his teachers and win the university chair of theoretical physics at barely 25. Those of us who organised the celebrations can only be thankful for the opportunity provided by the Ministry of Cultural Properties and Activities to render this service to our country's culture.

On behalf of the executive board and the National Committee, in concluding this note we wish to thank the Ministry for its funding and support of the program carried out to honour Enrico Fermi.

Edito dall'ENEA
Relazioni Esterne – Unità Comunicazione
Lungotevere Thaon di Revel, 76 – 00196 Roma
www.enea.it

Edizione del volume a cura di *Laura Falera, Giuliano Ghisu* e *Diana Savelli*

Progetto grafico e stampa: *Litografia Fabiano snc – Canelli (Asti)*

Finito di stampare nel mese di maggio 2003