José Manuel Sánchez Ron

Maria Goeppert Mayer: From Göttingen to the Nobel Prize in Physics

1980 – 2020
40 años CSN

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To all the women who wanted to be scientists,
but couldn't.

'We pass through this world but once.
Few tragedies can be more extensive
than the stunting of life, few injustices
deeper than the denial of an opportunity
to strive or even to hope, by a limit
imposed from without, but falsely identified
as lying within.'

STEPHEN JAY GOULD,
The Mismeasure of Man (1981)

Contents

Introduction

It is 2020, the year we will all remember for the fateful Covid-19 pandemic, and the Spanish Nuclear Safety Council, which it is my honour to chair, is now forty years old. The Council's origin dates back to the Spanish Nuclear Energy Board, which was created in 1951 to supervise all nuclear energy matters, part of a wave of similar organizations set up in other European countries after World War II. It was not until 1980 when the law was enacted that created the Nuclear Safety Council, an institution independent from the national government, whose primordial aim is to oversee nuclear safety and radiological protection for human beings and the environment.

Although its primary functions include regulating nuclear and radioactive facilities and monitoring the environment, the Council has other, less well-known objectives, such as developing and promoting research programmes. The fact is, while the Spanish Nuclear Safety Council is proud to be the repository of the experience and knowledge built up over its forty-year lifetime, it is also keenly aware that it must not flag in its commitment to encourage research and continuous learning, so our organization can be sure of doing its job well in future.

On this premise, and in commemoration of our fortieth anniversary, we decided to publish this book, which will also be a means of remembering and celebrating Maria Goeppert Mayer, one of the most important scientists in the development of nuclear physics. A woman and a scientist who loved, lived and worked for science. In this we have had the inestimable cooperation of the book's author. José Manuel Sánchez Ron: physicist, emeritus professor of the history of science, member of the Spanish Royal Academy and corresponding member of the Royal Academy of Exact, Physical and Natural Sciences, to name just a few achievements of his very lengthy career. He has done an exceptional job of reconstructing Maria Goeppert Mayer's life and work on the written page.

There are many scientists with ties to nuclear science or radiology whom we could have chosen as our subject, but we decided to do homage to an extraordinary woman who lived a life of science from a very young age and possessed a phenomenal talent for physics and mathematics, yet was unable to lead a normal scientific career because she was a woman.

Although she earned her PhD in physics in 1930 when she was barely 24 years old, and in a day when women rarely went to university, she could not get decent work as a scientist. Institutions looked down on her, and she could not find a scientific job worthy of her magnificent mind. Even so, she did not give up. As she crisscrossed the United States accompanying her husband, she worked with the very finest minds in quantum physics, yet no academic position was hers. Her intellect bulldozed a path for her among her peers, and she became recognized worldwide as an authority. However, it was not until 1946 when she truly began her career with an academic position in a Chicago university physics department.

Another aspect of her biography I would like to highlight is her willingness to work with others and share knowledge in the understanding that science is something that affects all of humanity. Whenever she could, she welcomed new studies and partnerships that enabled her to probe deeper into different fields or areas she had not yet thoroughly mastered. In so doing she wrought ties with the foremost scientists of her age, and in many cases she turned those ties into personal friendships.

Maria Goeppert Mayer succeeded at becoming a prestigious physicist, but that did not mean she had all the acknowledgement she deserved. It was not until 1960, when she was 54, that she was offered a full-time job as professor of physics, at the University of California. It was her first recognized, properly paid job.

Three years later, in 1963, she, Hans Jensen (and Eugene Paul Wigner) received the Nobel Prize in Physics 'for their discoveries concerning nuclear shell structure.' She thus became the second woman to win a Nobel Prize in science after Marie Curie received hers in Physics (1903) and Chemistry (1911). Only two more women have become Nobel science laureates after her, the Canadian Donna Strickland (2018) and the United States' Andrea M. Ghez (2020), both of whom also shared their prizes with other laureates. The Nobel in Physics is therefore the Nobel Prize won by the fewest women.

At long last Maria Goeppert Mayer not only received acknowledgement as a great scientist, but had done great science. After a lengthy career spent largely in the shadows, setting an example as a scrapper and a prevailer, leaving her mark wherever she went, she finally saw bureaucratic barriers topple before her stubborn determination and sheer ability. For that reason, in this publication commemorating the fortieth anniversary of the Spanish Nuclear Safety Council, we wish to do her honour with the greatest of acknowledgements, a permanent place in our memory.

But I would also like this book to bring women's contribution to science throughout history to the forefront. Women's roles have usually been kept quiet, invisible, off to one side. Science ignores gender; it deals only in research and knowledge, which do not distinguish between men and women. For that reason we hope this publication will help to normalize and bring notice to the past, present and future work and capability of all women researchers, scholars and scientists in the complex universe of science.

Josep María Serena i Sender
Chairman, Spanish Nuclear Safety Council

Foreword

Science is one of greatest part of our human heritage. It plays a decisive role in freeing us from the vice-like grip of myth. Its sister, technology, relieves us of all kinds of physical drudgery. And what can I say about medicine, the hybrid, the good Samaritan, that wonderful combination of science, technology and art (the art of the doctor-patient relationship) to which practically all of us turn at one time or another in our lives. As science is the heritage of humanity, one might expect that men and women would appear in approximately comparable numbers in the history of the various scientific disciplines, but that has not been so. Until very recently the number of men that have left a deep mark on science has dwarfed the number of women who have done the same (it is too early to judge what is happening right now, although women's presence in science has certainly increased significantly). Precisely because of the imbalance (an unjust one, in view of the fact that intellectual capabilities appear to show no sexual difference), we should always encourage greater awareness of the work of those women scientists who stood out in a world dominated by men. Maria Goeppert Mayer (1906-1972) was one of the women who left their mark on the world of science. And she did it in physics, a discipline apparently hostile to women, at least to judge by the Nobel Prizes. Only four women have won the Nobel Prize in Physics to date: Marie Curie (1903), Maria Goeppert Mayer (1963), Donna Strickland (2018) and Andrea Ghez (2020).

In this book I have attempted to reconstruct the life and work of Maria Goeppert Mayer, placing it within the context of the scientific and national worlds she lived in (in Germany and the United States). What those worlds were like is to be seen in the following chapters, but right now I would like to stress one particular aspect of her biography: she was particularly gifted at theoretical physics, a discipline in which she received a magnificent education at the University of

Göttingen, one of the leading schools for the most advanced physics of her day, quantum physics. However, when we look at her career as a whole, we can see that she, unlike young colleagues from her Göttingen years like Weisskopf, Mulliken, Elsasser and Houtermans, was prevented from pursuing any consistent or continuous programme of research due to the 'circumstances' of her life. The main 'circumstance' of her professional life was that she married a scientist, Joseph Mayer. He always helped her, but most of the institutions where he worked had anti-nepotism laws that made it impossible –or so they claimed– to give Maria any actual position or pay. She had to sit in the back seat, conform to the scientific interests of the people who were doing research at the institutions where her husband taught. These scientists (like Karl Herzfeld, Edward Teller and Enrico Fermi) recognized her talent, as had Max Born and James Franck at Göttingen before them. And every time, at every institution graced with her presence, from Johns Hopkins University to Columbia University, from the University of Chicago to the University of California, San Diego, she shone. She left her mark. So it was until she achieved her great success, the nuclear shell model that won her the Nobel Prize.

I have enjoyed writing this book very much, and I have learned a great deal exploring Maria Goeppert Mayer's worlds, but it was not I who proposed her as the subject; it was the Spanish Nuclear Safety Council. I am extremely grateful to the Council for that decision. And also for being allowed to participate in the celebration of the fortieth anniversary of the Council's creation. It is an honour and a pleasure.

Madrid, 22 September 2020

CHAPTER I

The Scientific World of Maria Goeppert Mayer: Quantum Mechanics

This book is about Maria Goeppert Mayer, but before we look at her life and contributions to science, it would be a good idea to get an overview of the scientific world where she worked. That world was none other than the world of quantum physics, the set of theories whose first pillar is what we call quantum mechanics. Quantum mechanics is the most fundamental part of quantum physics, although it does not span all the 'scenarios' the cosmos has to offer. In this chapter I will briefly review something of the history of this science. Quantum mechanics combined with the special and general theories of relativity to revolutionize physics, sparking changes that affected humanity deeply with some of their applications.

Spectroscopy, Astrophysics and Black-body Radiation

Most people say quantum physics originated when in 1900 Max Planck introduced the 'quantum of action'. This is not entirely right, though, because to understand Planck's contribution it is vital to bear in mind the work done by physicist Gustav Robert Kirchhoff (1824-1887) and chemist Robert Wilhelm Bunsen (1811-1899). The two of them built a firm foundation for the branch of physics and chemistry called 'spectroscopy', which concerns the study of the lines appearing in the spectra of chemical elements. It was Isaac Newton (1642-1727) who discovered in his research that, when light passes through a glass prism, it breaks down into different colours (the colours of the rainbow), producing what is called a 'spectrum'. Newton's work should not be called 'spectroscopy', however. For that we have to wait until 1752, when Thomas Melvill became intrigued by the fact that, when solid bodies (and liquid bodies) are heated to a high-enough temperature, they emit radiations. Melvill passed the light emitted by a sodium flame through a prism, and he observed a continuous spectrum pierced by a series of bright lines. That was the first known observation of an *emission spectrum*.

Exactly fifty years after Melvill's observations, that is, in 1802, William Hyde Wollaston noted that the spectrum of sunlight contained a number of dark lines Newton had not detected. Wollaston took these to be the borders of the natural colours. Some years later (1814-1815), Joseph von Fraunhofer examined the spectrum of sunlight in much closer detail, discovering close to six hundred dark stripes, which were known from that time forward as *Fraunhofer lines*. But he did not stop at the discovery of new lines; he also set out to map where they lay in the spectrum. He determined the position of three hundred and twenty-four lines. That was the real birth of spectroscopy.

In autumn 1859, while Kirchhoff was doing some preliminary work for a joint project with Bunsen, he made a surprising observation. A few years earlier, Leon Foucault (1849) had found that the so-called 'D lines' (dark lines) Fraunhofer observed in the solar spectrum matched up with the bright yellow lines detected in sodium flames. This effect could be observed quite easily by passing sunlight through a sodium flame and into a spectroscope (an instrument basically made up of prisms like the ones Newton used): if the sunlight was weakened enough, Fraunhofer's dark lines would be replaced by the bright lines from the flame. But Kirchhoff's discovery was to note that, if the intensity of the solar spectrum increased above a certain limit, the dark D lines became even darker when a sodium flame was interposed. He immediately grasped that this was something fundamental, though he did not know how to explain it.

A day later Kirchhoff was struck by an explanation that was soon confirmed by fresh experiments: a substance that can emit a certain spectral line also possesses a strong capacity to absorb that same line. That was why the characteristic sodium D lines became darker when the sunlight went through a sodium flame before reaching the spectroscope. Another manifestation of this property was that merely interposing a low-temperature sodium flame was enough to produce the D lines artificially in the spectrum of an intense light source that did not originally display them. But this fact led to a fundamental conclusion: D lines (dark lines) appearing in the solar spectrum had to be due to the fact that the sun's atmosphere contained sodium, which caused the spectral lines by means of some phenomenon involving absorption.

Kirchhoff informed the scientific community of his ideas before the year was out, but he kept working on them for some time afterward, as shown by a letter he wrote on 6 August 1860 to the chemist Otto Linné Erdmann, published the following year by Englishman Henry Roscoe in an excerpt translated into English in the *Philosophical Magazine*. Kirchhoff wrote,

> Since I sent in my last report to the Berlin Academy, I have been almost uninterruptedly engaged in following out the investigation in the direction I there indicated. I will not now speak either of the theoretical proof I have given of the facts I there announced, or of the experiments by help of which Bunsen and I have shown that the bright bands in the spectrum of a flame serve as the surest indications of the metal present therein; I will take the liberty, in this communication, of informing you of the progress I have made in the chemical analysis of the solar atmosphere.
>
> The Sun possesses an incandescent, gaseous atmosphere, which surrounds a solid nucleus having a still higher temperature. If we could see the spectrum of the solar atmosphere, we should see in it the bright bands characteristic of the metals contained in the atmosphere, and from the presence of these lines should infer that of these various metals. The more intense luminosity of the sun's solid body, however, does not permit the spectrum of its atmosphere to appear; it reverses it, according to the proposition I have announced; so that instead of the bright lines which the spectrum of the atmosphere by itself would show, dark lines are produced. Thus we do not see the spectrum of the solar atmosphere, but we see a negative of it. This, however, serves equally well to determine with certainty the presence of those metals which occur in the sun's atmosphere. For this purpose we only require to possess an accurate knowledge of the solar spectrum, and of the spectrum of the various metals.

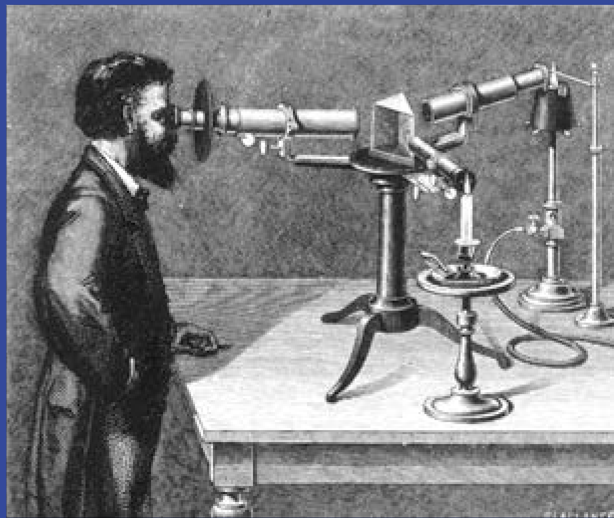
The consequences of Kirchhoff's arguments and observations were clear. For the first time the composition of heavenly bodies could be studied just by analysing the light they gave off. In other words, a new science was born, astrophysics, which could be used to address questions that astronomy, for all its several thousand years of study, could not answer. In his memoirs (1906), Roscoe, who worked with Bunsen for a time in Germany, recalled the impact these developments had on him. 'I shall never forget the impression made upon me by looking through Kirchhoff's magnificent spectroscope, arranged in one of the back rooms of the old building in the Hauptstrasse, which then served for the Physical Institute, as I saw the coincidence of the bright lines in the iron spectrum with the dark Frauenhofer's lines in the solar spectrum. The evidence that iron, such as we know it on this earth, is contained in the solar atmosphere, struck one instantly as conclusive. And yet not more than forty years had elapsed since Comte in his *Système*, arguing that investigators should not waste their time in attempting the impossible, used as an example of what he meant by the impossible that the knowledge of the composition of the sun at a distance of 91 millions of miles must for ever remain unattainable.' 'It will no longer be necessary to touch a body to determine its chemical nature: it will be enough to see it', French chemist Jean-Baptiste-André Dumas wrote in 1861. And, of course, he recognised, as everyone did, that this was the beginning: 'What today the state of the current optical instruments allows to achieve with respect to the Sun and the main fixed stars, other new advances will allow man to try with respect to the most distant and luminous stars, and thus recognize the elements by which God has formed the worlds that populate the Universe'. Thirty years later the hopes for the new method had become consolidated, as shown by the words pronounced by astronomer and spectroscopist William Huggins in his speech as president of the British Association for the Advancement of Science, at the association's annual meeting in Cardiff: 'Astronomy, the oldest of the sciences, has more than renewed her youth. At no time in the past has she been so bright with unbounded aspirations and hopes. Never were her temples so numerous, nor the crowd of her votaries so great.'

In addition to making it possible to analyse the composition of non-earthly bodies, spectrography had another basic use, which Bunsen and Kirchhoff explained in a long article they published in two parts in 1860 and 1861:

> Spectrum analysis is also important from another standpoint, as it can lead to the discovery of as-yet unknown elements. If there are indeed bodies spread throughout nature in such small quantities as to lie hidden from our common methods of analysis, they can be expected to be discovered by mere inspection of the spectrum. Experience has given us the occasion to confirm this hypothesis, because, based on positive results of spectrum observation, we believe we can accurately assert that, in addition to potassium, sodium and lithium, there is a fourth alkali metal, whose spectrum is as characteristic and as simple as that of lithium. Our apparatus indicates but two lines for this metal, a weak blue Cs^{β}, which almost matches that of strontium, $Sr\delta$, and another $Cs\alpha$, also blue.

And so they discovered a new metal, which they named 'caesium' (symbol Cs), 'from *caesius*', they wrote, 'which the ancients used to designate the blue of the upper part of the firmament. This name seems to us to be justified by the facility with which one may confirm, by the beautiful blue colour of the incandescent vapours of this new simple body, the presence of some thousandths of a milligram of this element mixed with soda, lithia and strontia.' And rubidium (Rb), 'from rubidus, which, among the ancients, served to designate the deepest red', due to the 'magnificent deep red colour' of two of the new metal's lines. During the following years other elements were identified through spectrographic analysis: thallium (William Crookes, 1861), indium (Reich and Ritcher, 1863), helium, detected by Norman Lockyer in 1869 in solar protuberances, the discovery being confirmed in the laboratory (1895) by William Ramsay and Lord Rayleigh; gallium (Paul E. Lecoq de Boisbaudran, 1875), scandium (Lars F. Nilson, 1879) and germanium (Clemens A. Winkler, 1886).

R. Bunsen, G. Kirchhoff
and H. Roscoe in 1862



Engraving showing a scientist with a spectroscope

X-ray photograph of Lord Kelvin's hand,
taken in 1896

Henri Becquerel

All this was important, in fact transcendental, but one concept remains in the explanation of the origin of Planck's work in 1900, a concept that represented a specific property of light- and heat-emitting bodies. Kirchhoff introduced it in 1859-1860 in the context of his spectrographic research: the 'perfect black body', shortened to 'black body' ('Ich will solche *Körper vollkommen schwarze*, oder kürzer *schwarze* neenen', he wrote). A black body is in reality a hypothetical object that completely absorbs all heat radiation that reaches it and is also a perfect emitter of that same radiation. The radiation a black body gives off covers all wavelengths (distances between the peaks, or troughs, of waves), but has its maximum radiation at a specific wavelength that depends on the body's temperature (when the body's temperature rises, the maximum wavelength decreases, which is tantamount to saying that the inverse of the radiation's wavelength, its frequency, grows).

The problem that immediately arose was how to find a mathematical expression for the distribution of energy in a black body according to temperature and wavelength (or frequency). Other physicists than Kirchhoff worked on the problem as well, such as Josef Stefan (1835-1893), Lord Rayleigh (1842-1919), Ludwig Boltzmann (1844-1906), Wilhelm Wien (1864-1928) and James Jeans (1877-1946). But it was Max Planck who found the definitive expression. To understand just what Planck did, however, we must review some other unexpected discoveries of the last five years of the 19th century.

The Risk of Predicting the Future

With electrodynamics well sorted out by James Clerk Maxwell in the 1860s, more and more late 19th-century physicists came to believe that, with Newtonian dynamics and Maxwell's electrodynamics, the theoretical bases for describing nature were now indeed complete. An extraordinary physicist, Albert Abraham Michelson (1852-1931), who received the Nobel Prize in Physics in 1907 (the first U.S. citizen to do so), is credited with having apparently said the following in a speech he gave on 2 July 1894 at the inauguration of the University of Chicago's Ryerson Physical Laboratory, or at least so it is reported in the article bearing his signature: 'It seems probable that most of the grand underlying principles have been firmly established and that further advances are to be sought chiefly in the rigorous application of these principles [...]. The future truths of physical science are to be looked for in the sixth place of decimals'.

A year after Michelson delivered these resounding and ultimately mistaken words, in 1895 Wilhelm Röntgen discovered X-rays, and the year after that, Henri Becquerel discovered radioactivity, which nobody knew how to fit into the seemingly firm, solid, closed edifice of known physics that we now call by the name of 'classical physics'. In other words, predicting the future is a risky undertaking. The solid foundations of classical physics began to tremble.

Cathode Rays and X-rays

In 1709 Francis Hauksbee the Elder, curator of experiments and instrument maker to the Royal Society of London, published a book entitled *Physico-Mechanical Experiments on Various Subjects*, in which he reported having observed that, if a glass container from which the air had been extracted and into which a few drops of mercury had been placed was shaken, a phosphorescent glow would appear. In fact, Hauksbee's experiments were stimulated by similar phenomena detected earlier in Evangelista Torricelli's barometric tubes (tubes containing some vacuum). For instance, in 1675 astronomer Jean Picard noted in the darkness of his Paris observatory that, when he moved barometers, a mysterious luminous halo sometimes appeared above the level of the wavering mercury column.

Hauksbee could run these experiments because he had a vacuum pump, an apparatus largely developed by Otto von Guericke in or around 1647. Little progress was made in vacuums, however, until the middle of the following century. In about 1855, Heinrich Geissler, a mechanic and expert glassblower of Bonn and a skilful scientific instrument maker, designed a new vacuum pump using mercury that could quite effectively draw the air out of a set of glass tubes, in whose ends he inserted electrodes. He then applied a high electrical voltage to the electrodes, producing gorgeous luminous effects (whose colours varied depending on the gas used).

In the century of electricity, it comes as no great surprise that an experimental device such as this was also used by physicists and chemists to study the characteristics of gases and the relationship between gas and electricity. One such scientist was Julius Plücker. While conducting spectroscopic research in 1858, Plücker found that, as he extracted gas from the tube, the luminosity that at first filled the tube (produced by the difference in potential between the electrodes) gradually faded until the cathode was surrounded by a thin luminous 'envelope' whose colour varied depending on the nature of the gas inside the tube. This envelope was separated from the cathode by a dark space, which became larger as the atmosphere inside the tube became rarer. When the gas pressure was lowered to one millionth of an atmosphere, the dark space invaded the entire tube, and the only thing that could be observed was a tiny circle of violet light at the end of the cathode, while the glass took on an intense phosphorescence at the opposite end.

Later a student of Plücker's, Wilhelm Hittorf (using a pointed cathode), and Eugen Goldstein both proved that an object placed in line with the cathode cast a well-defined shadow on the luminous envelope, which suggested that what was coming out of the cathode were rays travelling in a straight line. This *cathode emission* was eventually referred to by an expression Goldstein introduced in 1876, *Kathodenstrahlen*, or '*cathode rays*' (Hittorf used the term '*Glimmstrahlen*').

What these *Kathodenstrahlen* really were was a question that took some time to answer. The reply arrived in 1897 from the director of Cambridge's Cavendish Laboratory, Joseph John Thomson (1856-1940), who proved that cathode rays consisted in currents of charged particles, *electrons* (which he called simply 'corpuscles').

Another physicist who studied cathode rays, even before Thomson puzzled out their structure, was Wilhelm Konrad Röntgen (1845-1923). In June 1894 Röntgen, who in 1888 gained a physics chair at the University of Würzburg, began working on what for him was then a new field of research: the field of cathode rays. In the course of his investigations, on 8 November 1895, he found a radiation that could travel through opaque bodies. He baptized it 'X-rays', since its nature was unknown to him. In a newspaper interview, Röntgen gave a few particulars of his discovery that bear repeating: 'It was not long after I had begun my tests when I observed something new. I was working with a Hittorf-Crook tube thoroughly wrapped up in black paper. On the table next to it lay a piece of barium platinocyanide indicator paper. I ran a current through the tube and noted a curious line running across the paper [...]. The effect was such that, according to the ideas we held at the time, it could only have been the result of light radiation. But it was quite impossible for the light to have come from the lamp, because the paper wrapping undoubtedly let no light through, not even the light of an arc lamp'.

On 28 December Röntgen turned in the manuscript of the first of three papers he prepared for the Würzburg Physical Medical Society, 'On a New Kind of Ray'. And by 1 January 1896 he already had offprints made up, which he sent, together with copies of photographs he had made (some of which would become famous, especially the 22 December photograph of his wife's hand), to leading European scientists. The second article came out in March 1896.

Just as had happened when cathode rays were discovered, the nature of X-rays was hotly debated from the start. Most physicists thought they were some kind of electromagnetic radiation; Röntgen himself did. However, there was evidence that X-rays did not behave like ordinary light rays. It took over a decade to pin down the nature of X-rays. It was in 1912, on 21 April, at the University of Munich's Institute for Theoretical Physics, when Walter Friedrich and Paul Knipping observed the diffraction of X-rays by a crystal, following up on a proposal by a former doctoral student of Planck's, Max von Laue (1879-1960). The idea that a crystal was made up of molecules or atoms distributed lengthwise and crosswise in a spatial lattice was then widely known and accepted, so the truly original contribution was the experimental association of these crystalline structures with X-rays in an effort to clarify the nature of both. If X-rays were electromagnetic waves with a short wavelength, and if crystals were made up of atoms distributed in space at short, regular distances, then, since the sizes involved were similar, there should have been interference when X-rays were directed onto a crystal. By measuring the distances between maximum intensities on the interference diagram, one might calculate the wavelength of X-rays, just as in ordinary optics. That is what they did: a copper sulphate crystal was irradiated with X-rays, producing black spots distributed at intervals on a photographic plate behind the crystal.

Radioactivity

News of the discovery of X-rays spread rapidly throughout the world. The new radiation's obvious medical applications contributed hugely to its immediate popularity (some hospitals offered X-ray services within less than a year). In France the news soon got out too, in newspapers as well as at institutions like the prestigious Académie des Sciences, which devoted its meeting of 20 January 1896 to the subject. At this session two doctors, Oudin and Barthélemy, presented a photograph they had taken of the bones of a hand using X-rays. Mathematician Henri Poincaré, who had received copies of some of Röntgen's photographs from the man himself, was tasked with presenting the pictures at the Academy meeting, to which he also took a copy of Röntgen's article. He drew attention at the time to the connection between X-rays and fluorescence.

Among those who attended the meeting of the Academy of Sciences on 20 January was Henri Becquerel (1852-1908), who held a physics chair at the Natural History Museum of Paris. Becquerel was interested by what he heard at the meeting about Röntgen's findings, and, as he had uranium salts, which displayed phosphorescent properties, ready to hand at the museum, he set out to discover whether they produced X-rays. On 24 February, little more than a month after the January meeting and barely four months after Röntgen's own discovery, Becquerel presented a paper to the Académie, 'On the Radiation Emitted by Phosphorescence', in which he said that the rays emitted by the double sulphate of uranium and potassium, a phosphorescent substance, left an image on a photographic plate after passing through a thick paper wrapping. It seemed that phosphorescence was indeed accompanied by X-rays. However, a week later, on 2 March, the Académie received another paper from Becquerel, this time much more startling. On 26 February he had been forced to put his experiments with uranium salts on hold because it was a cloudy, sunless day. He had prepared his uranium salts and wrapped up his photographic plate safely, so he put them away in a drawer, hoping that the sun would come out the next day and he could expose the salts to sunlight. The weather remained grey for several days, though, and on 1 March Becquerel decided to develop the plate. He expected to find faint images. To his surprise the pictures were very sharp indeed. Without any exposure to sunlight, without any visible phosphorescence or fluorescence, the uranium compound had emitted a radiation capable of leaving a picture on the plate. Why that was Becquerel did not know.

At the next Academy meeting, on 9 March, Becquerel reported that, in addition to darkening photographic plates, the new radiation ionized gases, making them conductive. This finding enabled him to use an instrument measuring electrical currents to determine how 'active' a sample was. He also relayed that he had kept his crystals in the dark for 160 hours with absolutely no weakening of their radiation. He had furthermore shortened the list of substances that emitted the new radiation to uranium compounds only, with two surprising exceptions: a couple of samples of calcium sulphate, which for some reason still unknown produced images through two millimetres of aluminium.

All these results together form the real core of the discovery of radiation. Becquerel continued studying the new phenomenon's properties, and he published further notes in the *Comptes rendus* of 23 and 30 March, but the essence of his discovery was complete. Every result led him to believe the emissions came from the uranium: 'All the uranium salts I have studied', he wrote in one of his notes, 'whether phosphorescent or not under light, whether crystallized, molten or in solution, have given comparable results. This has made me think that the effect is due to the presence of the element uranium in these salts, and that the metal would provide more intense effects than compounds'. He also ascertained that his samples' activity did not weaken, even after a long time: 'From the 3rd of March to the 3rd of May these substances remained inside an opaque cardboard box. Since the 3rd of May they have lain in a double lead box that never leaves the dark room […] Under these conditions, the substances continue to emit active radiation'. And he could not explain the reason for this surprising phenomenon, which he referred to as a 'kind of invisible phosphorescence'.

Whatever we may be tempted to think over a century later, at the time Becquerel's discovery did not garner too much attention. X-rays were still the most popular thing around. The person responsible for changing all that was a Polish woman, Marie Sklodowska-Curie (1867-1934).

Marie Curie obtained her licentiateship in physics and mathematics at the Sorbonne and married Pierre Curie (1859-1906), who was then professor of physics at the city of Paris's École Municipale de Physique et de Chimie Industrielles. When she decided to seek her doctorate, she found no better topic than Henri Becquerel's recently discovered phenomenon. What Marie Curie did in this, her early research in the field of radioactivity, was, first, to study the conductivity of air under the influence of the radiation emitted by uranium and, second, to find out if there were any other substances besides uranium compounds that made air a conductor of electricity. Her experimental procedure was simple enough: she placed the material she was studying on a metal plate facing another metal plate, which acted as a condenser; she then used the piezoelectric quartz electrometer (developed by her husband and his brother) to ascertain if any electrical current were passing through the air between the plates. The greater the intensity of the current, the greater the radioactive 'activity' of the substance.

Marie ran across one especially striking substance in her experiments: 'of the minerals that have shown themselves to be active, they all contain active elements. Two uranium ores: pitchblende (uranium oxide) and chalcolite (uranyl copper phosphate) are much more active than uranium itself. This fact is most astonishing and leads one to believe these ores may contain an element much more active than uranium'. She had to try and isolate the element or elements she believed she had indirectly detected. With Pierre's assistance, after three months' work, they announced the existence of a new chemical element, polonium. It was on 18 July 1898 when they presented their article 'On a New Radioactive Substance, Contained in Pitchblende' at the Académie des Sciences. This was, by the way, the first use of the word 'radioactive', that is, active in radiation, emitting radiations. Marie and Pierre also introduced the term 'radioactivity'.

During the research that led them to the discovery of polonium, the Curies found indications that there might indeed be another element accompanying the barium separated from the pitchblende. But the Curies believed they needed more chemical skills and knowledge than they had if they were to make any more headway in the problem, so they asked Gustave Bémont for help. Bémont was a chemist who was then head of mineralogical chemistry work at the École de Physique et de Chimie Industrielles. After several months' toil, they succeeded in separating the second new element, which they called 'radium' and which later proved to be more important than polonium and much more difficult to obtain. After another four years of work, the Curies managed to separate only 100 milligrams of the new element, of a considerable purity, from several tonnes of uranium ore. No wonder, then, that the price of radium was extremely high; in 1921, for example, a gram cost 100,000 dollars. It was worth it, though. Its half life (the time it takes for a sample to lose half of its nuclei) is 1,602 years, as opposed to just 138 days for polonium and nearly 4,500 million years for uranium-238. Radium is therefore a stable source of radiation for hundreds of years. Furthermore, its radiation is 3,000 times more intense than the radiation from an equal amount of uranium. In other words, its combination of a long active life and high intensity makes it far superior to any other radioactive element or substance.

For the pioneers of radioactivity, as for everyone who took any kind of interest in the matter, this was a perplexing phenomenon, one that physics and chemistry as known at the time could not explain. Inevitably scientists wondered what its origin was, and all kinds of reactions inevitably ensued. Short of a general analysis, the history of the explanation of radioactivity can be divided into three phases. The first phase lasted until 1903, and its main question was whether radioactivity was an atomic property of matter or was produced by an outside agent instead. That period ended with general acceptance that the first possibility, that it was an atomic phenomenon, was correct, although that did not stop the second option from still cropping up occasionally. The next phase covered roughly the decade from 1903 to 1913, and its big objective was to find an atomic model that could explain radioactivity. Continuous failures in the attempt eventually produced a scientific atmosphere that no longer even asked the question of the origin of radioactivity –or at least no longer asked it as frequently, earnestly or fiercely. Most scientists –but not all, of course– were convinced that the answer would come along in the future, when more powerful conceptual frameworks and theories became available. Quantum mechanics would always be that sort of 'conceptual framework'; in 1928 George Gamow and the team of Ronald Gurney and Edward Condon demonstrated separately that quantum physics provided a satisfactory, albeit not entirely full, explanation of radioactive emissions. True, a contribution published in 1905 by a then-young and unknown employee of the Bern Patent Office by the name of Albert Einstein did help begin to understand the nature of radioactivity: this was the article containing the famous equation $E=mc^2$, where E represents energy, m represents mass and c, the speed of light.

Having briefly reviewed the unexpected discoveries of the last five years of the 19th century, we have come to the point where we can go back to Max Planck and his contributions.

Max Planck

Max Karl Ernst Ludwig Planck was born in 1858 in Kiel, home of the university where his father, Johann Julius Wilhelm von Planck, taught law. The prestige and authority that Planck eventually attained in the German and world scientific communities were due not so much to his scientific capabilities (which he certainly had) as to his diligence, rectitude and strength of character. He can be considered an able scientist, a very able scientist, but he was far outclassed in that sense by Einstein, Bohr, Rutherford and Heisenberg, to give a few examples from among his contemporaries.

When the time came for him to pick a field of university study, young Max wavered between music, ancient languages and physics. Munich physicist Philipp von Jolly advised him not to study physics, because everything had already been discovered since the principles of thermodynamics had been established, and there were only a few blanks left to fill in. Planck, however, finally chose to study physics at the University of Munich, and he began in the winter semester of 1874-1875. We find some clues explaining his decision in a letter Planck wrote many years later, on 14 December 1930, to Joseph Strasser: 'I could as easily have become a linguist or a historian. What pushed me toward the exact sciences came from rather external circumstances: a mathematics course taught by Professor Gustav Bauer, which I attended at university, gave me great inner satisfaction and opened new horizons for me. The fact that I eventually turned from pure mathematics to physics had to do with my passion for issues concerning the conception of the world; issues that mathematics could certainly not solve.'

From 1877 to 1879, he pursued his studies in Berlin, where his teachers included three giants of science, physiologist and physicist Hermann von Helmholtz, mathematician Karl Weierstrass and Gustav Kirchhoff. Their teaching, however, left something to be desired, as Planck reminisced in his scientific autobiography: 'Helmholtz never prepared his classes; he was constantly breaking off to scan a notebook for the data he needed; furthermore, he was always making mistakes in his calculations on the blackboard, and he gave the impression of being as bored with us as we were with his class.' Kirchhoff did prepare his lessons carefully: 'Each sentence was in its place. Never a word too little or too many. But he gave the impression that he had learnt it all by rote, which made him dry and monotonous. We admired the speaker, but not his speeches'. Under such circumstances, 'the only way to satisfy my thirst for knowledge was to read the works I was interested in, by which I mean, of course, works having to do with the principle of energy. That was how I discovered the treatises of Rudolf Clausius, whose clarity made a deep impression on me, and into which I dove with growing enthusiasm. I especially admired his exact formulation of the two principles of thermodynamics [the law of the conservation of energy and the law of increasing entropy] and the relationship between them.'

Clausius, together with Helmholtz and Kirchhoff (no matter how unattractive Planck found their lectures), formed the foundations on which Planck built his knowledge of physics: 'I owe all my knowledge entirely to reading the masters', he stated in his inaugural address of 28 June 1894, when he accepted his chair at the University of Berlin, 'among whom I do supreme homage to the names of Hermann von Helmholtz, Rudolf Clausius and Gustav Kirchhoff.'

After presenting his habilitation in 1880, Planck was allowed to teach as a *Privatdozent* in Munich. In 1885, with publications of some renown under his belt (especially an 1883 article on the thermodynamic equilibrium of mixtures of gases), he was named extraordinarius professor (that is, associate professor) of physics at the University of Kiel, replacing Heinrich Hertz, Helmholtz's favourite disciple (Hertz, whom the University of Kiel was preparing to promote from associate professor to full professor, instead accepted an offer from Karlsruhe, where in 1888 he made his great contribution to physics: the experimental demonstration of the existence of low-frequency electromagnetic waves, which was deduced from Maxwell's theory of electromagnetism).

At Kiel Planck ploughed ahead in his scientific career, focusing on the second law of thermodynamics, increasing entropy. After four years, with another book added to his credentials in 1887 (on the law of conservation of energy, one of his great scientific passions), a magnificent opportunity came calling from the University of Berlin, the seat of learning of the capital of Prussia, the nerve centre of the German Empire, which was on its way to becoming one of the great world capitals as well. Once more it was Hertz that Berlin really wanted, but Hertz had taken up an offer from Bonn (which, by the way, shows that at the time Berlin may have held an important position in German science, but it was not yet the indisputable leader it would soon become). They picked Planck, but for an associate professorship. Three years later, in 1892, he was given a full professorship. And two years after that, with the support of Helmholtz himself, Planck was elected an ordinary member of the Prussian Academy of Sciences. He was reaching the peak of his profession. He would spend the rest of his life in Berlin, and in Berlin in December 1900 he achieved his great scientific success: the introduction of quanta of energy.

Quantum Discontinuity: Planck and Einstein

Planck had been interested in the problem of black-body radiation for a long time, but he was trying to solve it using Maxwell's electrodynamics. This was a reasonable approach, because wasn't black-body radiation just a kind of electromagnetic wave? Success eluded him, however, until one day he received some highly valuable information from colleagues at the Imperial Physical Technical Institute (Physikalisch-Technische Reichsanstalt, or PTR), in whose founding Werner Siemens (1816-1892) had been fundamental. An industrialist, a scientist and an inventor, Siemens, who had made his fortune primarily in the electricity industry, wanted to give something back to his country. His idea was to found an institute devoted to physics-related research. He put up the money needed to build and design such an institute in Charlottenburg, then a small residential town about three kilometres from Berlin's Brandenburg Gate. The idea was for the PTR to investigate all kinds of problems in physics and technology, in addition to developing and testing instruments and creating systems of measures. In response to fears of unfair competition, however, an agreement was reached to limit the range of the

PTR's work to those areas or problems that did not clash with what was being studied at universities, polytechnic schools, private industry or other state agencies. This agreement meant the new institute would be devoted mostly to metrology (the establishment and checking of units of measurement). The prospect was an especially attractive one for industry, since having precise units of measurement was a big commercial plus: exports would benefit if manufactured products met technical specifications that could be shared by as many nations as possible, and Germany was an industrial power eager to export.

The institute went into operation in 1887, with Hermann von Helmholtz as its president (succeeded in 1895 by Friedrich Kohlrausch). It was organized into two divisions, a scientific division, which was the first to have dedicated facilities, and a technological division, which took a bit longer to procure special buildings of its own. This was the first 'national laboratory' institution, and it established the model that Great Britain and the United States later adopted.

One of the Imperial Institute's concerns was photometric studies, that is, the determination of quantities to describe the properties of light. In March 1888, just a few months after the institute's inauguration, the Deutscher Verein für Gas- und Wasserfachmänner (German Association of Gas and Water Specialists) had the Ministry of the Interior ask the PTR to help evaluate the accepted units of light intensity in use at the time, and if possible to establish an internationally accepted unit. Helmholtz enthusiastically embraced the idea, especially since the Navy was interested in improving its photometric instruments and coping with brightness loss under adverse weather conditions. While investigating this problem, Otto Richard Lummer and Ernst Pringsheim discovered that their measurements showed that, for long wavelengths, there were systematic deviations with respect to the law of black-body radiation as couched at the time (the law traceable to Wilhelm Wien). Shortly thereafter, Heinrich Rubens and Ferdinand Kurlbaum unequivocally proved that this was so (they presented their results to the Berlin Academy of Sciences on 25 October 1900). Just as soon as he completed his measurements, before publishing them, Rubens reported his results to Planck at the neighbouring University of Berlin. That same day (7 October), Planck constructed the derivation of the law to accommodate the new measurements, arriving at a new law of radiation.

Planck presented the result at the Berlin Physics Society's meeting of 19 October. The next day Rubens informed him, as Planck himself recalled in his autobiography, that 'the night before, after the meeting was adjourned, he ran a rigorous comparison of my formula and the data from his measurements, finding a satisfactory concordance at all times. Lummer and Pringsheim, too, who at first thought they had detected discrepancies, withdrew their objections soon after, because, as Pringsheim himself confessed to me, it was proved that the deviations they found were due to a miscalculation. Subsequent measurements confirmed the formula of radiation again and again, and incidentally confirmed it more and more exactly as the measurement methods used became more precise.' The new law of radiation fit the experimental results perfectly. Almost unexpectedly, as if unintentionally, Planck found that he held an apparently correct law of distribution for black-body radiation but did not know why it worked (the heuristic modification he had made did not provide a genuine theoretical explanation).

Planck immediately plunged into the task of explaining the theory behind the law. He succeeded soon after, in December. It was then when he was forced to write the equation codifying the fact, the famous $E=h\nu$, where E represents the system's energy, h is a constant that is called 'Planck's constant' today, and v is the radiation's frequency. Over thirty years later, in a letter that he wrote on 7 October 1931 to American physicist Robert Williams Wood, Planck told how, 'In short, I can characterize the whole procedure as an act of despair, since, by nature I am peaceable and opposed to doubtful adventures. However, I had already fought for 6 years (since 1894) with the problem of equilibrium between radiation and matter without arriving at any successful result. I was aware that this problem was of fundamental importance in physics, and I knew the formula describing the energy distribution in the normal spectrum [i.e., the spectrum of a blackbody]; hence a theoretical interpretation *had* to be found at any price, however high it might be. It was clear to me that classical physics could offer no solution to this problem and would have meant that all energy would eventually transfer from matter into radiation. In order to prevent this, a new constant is required to assure that energy does not disintegrate. But the only way to recognize how this can be done is to start from a definite point of view. This approach was opened to me by maintaining the two laws of thermodynamics. The two laws, it seems to me, must be upheld under all circumstances. For the rest, I was ready to sacrifice every one of my previous convictions about physical laws. Boltzmann had explained how thermodynamic equilibrium is established by means of a statistical equilibrium, and if such an approach is applied to the equilibrium between matter and radiation, one finds that the continuous loss of energy into radiation can be prevented by assuming that radiation is forced, at the outset, to remain together in certain quanta. This was purely a formal assumption and I really did not give it much thought except that, no matter what the cost, I must bring about a positive result.'

The 'act of despair' he referred to was, as he himself said, adopting the statistical formulation of entropy proposed by Ludwig Boltzmann in 1877, the famous expression for a system's entropy, $S=k\cdot\ln W$, where k is a constant (introduced later by Planck in fact and named 'Boltzmann's constant') and lnW is the Napierian logarithm of the probability that the state in question will take place. In Boltzmann's own words on introducing this formulation in 1877, 'The initial state of

Wilhelm Roentgen, 1915

Pierre and Marie Curie in their laboratory

Marie Curie taking measurements with a piezoelectric quartz electrometer

The first Solvay Conference, 1911

a system will be, in most cases, a not so probable state and the system will tend always towards more probable states, until it will reach the most probable state, i.e., the state of thermodynamic equilibrium. If we apply this to the second law of thermodynamics, we can identify the quantity that is usually called entropy with the probability of the corresponding state. Let us, then, consider a system of bodies that is isolated [and whose state is modified no further by interaction among its component bodies]. In a transformation of this sort, the total entropy of the system cannot but increase. In our present interpretation, this has no other meaning than the fact that the probability of the global state of the bodies of the system must continuously increase: the system cannot but pass from a state to a more probable state'. And later on, 'This measure of permutability coincides with entropy, save in a factor and a constant'.

The use of 'probabilities' and notions like 'the global state of the bodies' clearly implies that there could be temporary violations of the second law of thermodynamics. To bow to such a concept, to accept that increasing entropy was associated with probabilities and that consequently it was not as universal as he thought, must have been painful for a physicist of Planck's talent, and the pain was only mitigated by his making this step a 'purely formal assumption'. In other words, from the outset Planck was hard put to support the idea that this result meant that, somehow, electromagnetic radiation (that is, light, a *continuous* wave, as was supposed until then) could also be thought of as made up of 'corpuscles' (later termed 'photons') of energy, $h{\cdot}v$.

This was where the unknown Bern Patent Office employee by the name of Albert Einstein (1879-1955) came in. Einstein, who held that the energy discontinuity Planck had found was real although certainly problematic, presented his idea in an article published in 1905 in *Annalen der Physik* entitled 'On a Heuristic Viewpoint Concerning the Production and Transformation of Light'. Using statistical analysis, Einstein concluded that 'observations regarding "black-body radiation," photoluminescence, production of cathode rays by ultraviolet light, and other groups of phenomena associated with the production or conversion of light can be understood better if one assumes that the energy of light is discontinuously distributed in space.' It was a radical thesis that defied canonical physics. True, it did explain some phenomena that were troublesome from the standpoint of classical physics (such as the photoelectric effect), but, if light were somehow made up of 'particles' of energy, why had the wave theory of light been so successful so far? And how could phenomena like interference and diffraction be incorporated into the new 'quantum' framework? Einstein's answer, or rather his comment, to these questions lies in the introduction to his paper, where he wrote:

> The wave theory of light, which operates with continuous spatial functions, has proved itself splendidly in describing purely optical phenomena and will

> probably never be replaced by another theory. One should keep in mind, however, that optical observations apply to time averages and not to momentary values, and it is conceivable that despite the complete confirmation of the theories of diffraction, reflection, refraction, dispersion, etc., by experiment, the theory of light, which operates with continuous spatial functions, may lead to contradictions with experience when it is applied to the phenomena of production and transformation of light.

So appeared the first of the counterintuitive properties of quantum physics, 'wave-corpuscle duality', which Louis de Broglie addressed in 1923-1924. For a small portion of his paper's contents (application of quanta to explain the photoelectric effect), Einstein received the Nobel Prize in Physics in 1922.

But few believed in Einstein's thesis at first. Robert Millikan's words from 1949 exemplify the general reluctance: 'I spent ten years of my life testing that 1905 equation of Einstein [the photoelectric effect], and, contrary to all my expectations I was compelled in 1915 to assert its unambiguous experimental verification in spite of its unreasonableness since it seemed to violate everything that we knew about the interference of light.'

Richard Feynman put the revolutionary nature of wave-corpuscle duality (verified experimentally in 1924 by Clinton Davisson and Lester Germer and by George Thomson and Alexander Reid) nicely in one of his books, *Six Easy Pieces* (1995):

> 'Quantum mechanics' is the description of the behavior of matter in all its details and, in particular, of the happenings on an atomic scale. Things on a very small scale behave like nothing that you have any direct experience about. They do not behave like waves, they do not behave like particles, they do not behave like clouds, or billiard balls, or weights on springs, or like anything that you have ever seen.
>
> Newton thought that light was made up of particles, but then it was discovered, as we have seen here, that it behaves like a wave. Later, however (in the beginning of the twentieth century), it was found that light did indeed sometimes behave like a particle. Historically, the electron, for example, was thought to behave like a particle, and then it was found that in many respects it behaved like a wave. So it really behaves like neither. Now we have given up. We say: 'It is like *neither*.'
>
> There is one lucky break, however– electrons behave just like light. The quantum behavior of atomic objects (electrons, protons, neutrons, photons, and so on) is the same for all; they are all 'particle waves,' or whatever you want to call them.

Rutherford's Model of the Atom

The next building block in the development of quantum physics was laid in 1911 by a physicist from New Zealand who then directed the University of Manchester's Physics Laboratory, Ernest Rutherford (1871-1937).

Rutherford was also involved in researching the phenomenon of radioactivity, especially the types of radiation radioactive substances gave off, which were dubbed 'alpha', 'beta' and 'gamma' radiation, and were proved to consist, respectively, in helium nuclei, electrons and electromagnetic radiation. In the course of his research, Rutherford became quite familiar with alpha and beta particles. Therefore, unsurprisingly, it occurred to him that he might be able to use them as a tool to analyse the atom. In 1909 two researchers from his laboratory, Hans Geiger and Ernest Marsden, shot alpha particles at thin plates made of various metals. To everyone's surprise, they found that 'a small fraction of the α-particles falling upon a metal plate have their directions changed to such an extent that they emerge again at the side of incidence'; in other words, they bounced off. Two years later Rutherford himself managed to explain it by introducing a model in which the atom is made up of a central nucleus (a sphere with a radius of less than $3x10^{-12}$ centimetres) surrounded by 'a sphere of electrification' with a radius of about 10^{-8} centimetres and an equal and opposite charge.

Rutherford's atomic model looked good, but it had some major drawbacks. If one thought of it as a sort of 'miniature planetary system' governed by electromagnetic forces, then there was one obvious problem: the electrons orbiting the nucleus would be accelerated (their movement was circular), and therefore they must emit radiation, which meant that they had to lose energy. This would happen as they neared the nucleus, into which they would eventually unavoidably fall. In other words, an atom according to this model would be unstable.

So, another model had to be found that incorporated the traits Rutherford had used to explain alpha- and beta-particle scattering. The solution was not long in coming, from a young physicist from Copenhagen, Niels Bohr (1885-1962).

Bohr's Model of the Atom

Bohr, who spent some years with Rutherford at the physics institute in Manchester, realized that, to construct a satisfactory model of the atom, he had to somehow include the Planck-Einstein quantum of energy. In the 1913 article in which he presented his ideas, he wrote, 'Whatever the alteration in the laws of motion of the electrons may be, it seems necessary to introduce in the laws in question a quantity foreign to the classical electrodynamics; *i. e*.., Planck's constant [h].'

Bohr combined classical mechanics with electrostatics, assuming that the circular orbits of Rutherford's model were stationary (that is, that they did not emit radiation), and he introduced an expression that quantized the angular momentum of the electrons' orbits (this expression allowed only certain values that were multiples of h). He then found that orbits could not gradually creep into a lower (or higher) position, but instead had to change position in a discontinuous, *quantum* jump. In this model of the atom, when an electron passes from a given orbit to a lower orbit, it emits energy in the form of a quantum of radiation; while, if the electron absorbs energy (quanta), it 'rises' to a higher level. In other words, jumps between different orbits produce different frequencies (i.e., spectral lines). In fact, one of the foremost achievements of Bohr's model of the atom was its ability to justify the mathematical relationships corresponding to different groups of spectral lines (spectra, with their sometimes thousands of lines, had posed an unsurmountable stumbling block for all previous theories of the atom). These mathematical relationships had been discovered by Johann Jacob Balmer and Johannes Robert Rydberg while 'playing with numbers', and before Bohr came along physics had been entirely unable to explain them. Spectroscopy found itself reduced to a consequence of quantum physics.

Werner Heisenberg

Although Bohr set out to craft a general theory of the constitution of all atoms and molecules, in practice his formula only explained the hydrogen atom. All his attempts to get any farther failed; he could not even extend his theory to the spectrum of helium, with its two electrons. A dozen years went by before an accommodating general theory was found (though it did not meet the requirements set by the special theory of relativity Einstein formulated in another of his ground-breaking articles, published in 1905). Of all the episodes of the history of science in which a theory undergoes a long, painful gestation, the genesis of the theory of the movement of microscopic objects, of 'quantum mechanics', as it was eventually termed, was the most laborious. During that dozen years, experimental discoveries of all kinds tumbled out, one after another, together with equally numerous and stunning theoretical developments (with a hiatus during World War I for many scientists). Experiments like those run by James Franck and Gustav Hertz, who proved the existence of the stationary statuses postulated by Bohr, and the experiments done by Otto Stern and Walter Gerlach, who proved spatial quantization (not all directions were possible in quantum processes); Arnold Sommerfeld's generalization of the Bohr model of the atom, using resources drawn from special relativity; the formulation of Bohr's correspondence principle; Alfred Landé's semiempirical formulae to explain the anomalous Zeeman effect; multiplets, discovered in London by Miguel Catalán and explained by Sommerfeld's introduction of a new quantum number; Arthur Holly Compton's experiment, which revealed the corpuscular nature of light; Louis de Broglie's wave-corpuscle duality; the statistics developed by Satyendra Nath Bose and Einstein; Hendrik A. Kramers' quantum theory of dispersion; and Wolfgang Pauli's exclusion principle. These advances culminated in 1925 in the formulation of quantum mechanics by a young 24-year-old student of Sommerfeld's, Werner Heisenberg (1901-1976).

Extraordinarily gifted in a wide range of areas, primarily intellectual activities, Heisenberg could certainly have shone in a good many fields, but it was science that finally won him over and, within science, physics. After finishing secondary school, Heisenberg underwent a long illness. As he recalled in his autobiography (*Der Teil und das Ganze: Gespräche im Umkreis der Atomphysik*, 1969, published in Spanish under the title *Diálogos sobre la física atómica*), 'I had to lie in bed for many weeks, and during my following convalescence I had much time to spend alone with my books. In those critical months a work fell into my hands whose contents enthralled me, though I only half understood. The mathematician Hermann Weyl had given a mathematical exposition of the principles of Einstein's theory of relativity under the title *Space, Time, Matter* [1918]. The discussion of the difficult mathematical methods the book dealt with and the abstract conceptual edifice of the theory of relativity looming in the background was absorbing and disturbing, reinforcing my earlier decision to study mathematics at the University of Munich.'

At the University of Munich, the city of his birth, where his father was full professor of medieval and modern language, young Werner approached the great Ferdinand Lindemann, famous for having solved the problem of squaring the circle in 1882 (which implied that π was a transcendental number). When Lindemann learned that Heisenberg had read Weyl's *Space, Time, Matter* (Weyl was himself already a celebrated mathematician), Lindemann turned the young man away, sentencing, 'Then you are already ruined for mathematics'.

Heisenberg's next option was mathematical physics, which was then (as it often is now) not always readily distinguishable from theoretical physics. Since he was in Munich, who could be a better choice than Arnold Sommerfeld? Sommerfeld was a 'short, stocky man', Heisenberg recalled, 'with a rather martial black moustache', who 'gave the first impression of being tough [but] whose natural goodness showed through as soon as he spoke his first words'.

It was a magnificent choice. To start with, Sommerfeld strove to rein in the brilliant Werner's unbridled philosophical anxieties. 'I find the underlying questions even more intriguing, perhaps, than the small individual tasks', Heisenberg told him, to which *Herr Professor* replied, 'You know, though, what Schiller said about Kant and his interpreters: "When kings build, waggoners have work to do".

We are all waggoners first! But you will see how happy it will make you if you do your work carefully and conscientiously and if moreover, as we hope, you get something out of it.'

Under Sommerfeld's direction, Heisenberg flourished scientifically and began to look into some problems of quantum physics. Still, Sommerfeld decided it would be best for Heisenberg's education not to focus exclusively on quantum physics problems, and he assigned a classic hydrodynamics problem as the subject for Heisenberg's doctoral thesis: the transition from a laminar flow to a turbulent flow. Here, too, Heisenberg displayed his prowess, developing approximation methods for handling the nonlinear equations involved. In 1923 he completed his thesis, which saw print the following year. Thus he won the title of 'doctor', though not without difficulties; a famous story tells about the trouble he had at his oral examination (July 1923), which was designed to probe a doctoral candidate's general knowledge of physics. Wien, one of the examiners, asked Werner about the resolving power of microscopes, telescopes and Fabry-Perot interferometers, and Werner did not know. Wein insisted that Heisenberg should be failed, and only Sommerfeld's help saved him, but Heisenberg received the lowest possible grade, rite (the order was: *summa cum laude, magna cum laude, cum laude* and *rite*). Heisenberg never forgot that lesson –and the humiliation. He took good care to learn about the resolving power of optical instruments, and that knowledge stood him in good stead in 1927, when he used it in the thought experiment in which he substantiated the principle of uncertainty.

Even before he defended his dissertation, Heisenberg moved to Göttingen to assist Max Born, a key character in the history of quantum mechanics and, as we shall see in another chapter, a scientist who had much to do with Maria Goeppert Mayer. So, let us look at Born.

Max Born

Max Born (1882-1970) was born in the city of Breslau (now Wrocław, in Poland), where his father was a professor of embryology at the university. In 1901, after completing secondary school, he enrolled at the University of Breslau. After three semesters there and another two summer semesters in Heidelberg and Zurich, he entered the University of Göttingen in 1904. One of the local leading lights at the time was mathematician David Hilbert, with whom Max very soon formed a relationship. Hilbert gave Max the job of preparing his class transcripts (a great honour) so other students could consult them in the reading room. In 1905 Born became Hilbert's personal assistant. In fact, one of the hallmarks of Born's scientific career was that he was so very gifted in and knowledgeable about mathematics. As we shall see later, mathematics proved decisive when the time came to put the quantum mechanics Heisenberg proposed in 1925 into a finished mathematical form.

Notwithstanding his skills, Born was not inclined to pursue mathematics. From the start he showed an interest in theoretical physics, particularly topics like electron theory (which led him to become one of the first physicists to enter the field of special relativity) and elasticity theory. In fact, the separation between mathematics and theoretical physics was still fuzzy. 'In those days, he wrote in a short autobiography, 'mathematics also encompassed mathematical physics. For instance, Hilbert and Minkowski directed a seminar on electrodynamics of bodies in motion, where they dealt with problems that would be included under the name of relativity today.'

With his doctorate in hand (he defended his thesis in late 1906), he had to perform his military service, from which he was discharged in 1907. He then decided to go to England. He reached London in April and went on to Cambridge to pursue further studies with J.J. Thomson and Joseph Larmor. 'I found that Larmor's lecture on electromagnetism taught me nothing I hadn't already learned from Minkowski, although J.J. Thomson's experimental demonstrations were splendid and exciting.'

Six months later he returned to Breslau, but he soon received an offer from Hermann Minkowski, another of the great Göttingen mathematicians, asking him to help with his (Minkowski's) work on electrodynamics and the special theory of relativity. Born said yes. He arrived in Göttingen in December 1908, but unfortunately Minkowski died very shortly afterward, in January 1909, following an appendectomy. Born's career at his alma mater was not frustrated by this turn of events, though; in fact, his status as a scientist rose fast, and he took up the problems of quantum physics. As an example of how quickly Born's career took off, in 1912 Abraham Michelson invited him to give a series of lectures on relativity in Chicago, which Born did.

In 1914, just as World War I began, Born was summoned to Berlin as an 'extraordinarius professor', the position just junior to a chaired professorship, to help Planck with his teaching obligations. Born started in the spring of 1915. There, in addition to serving his country, Born became fast friends with Einstein. Casting back again to his autobiographical notes, we read: 'During the dark days of the war (when it was hard to find sufficient food for the family) the friendship with Einstein was a great comfort. We saw each other very often, played violin sonatas together, and discussed not only scientific problems but also the political and military situation [...] We were violently opposed to the political aims of the German government and convinced that they would lead to disaster. During these years [December 1915] Einstein finished his general theory of relativity and discussed it with me. I was so impressed by the greatness of his conception that I decided never to work in this field. [...] Together we experienced the military defeat, the revolution in Berlin, and the founding of the German Republic. As it was ruled from Weimar, not from Potsdam, we hoped for a peaceful future.'

In 1919, since Max von Laue was very keen to live in Berlin, he and Born switched jobs. That meant Born moved to Frankfort and into von Laue's chaired professorship of theoretical physics. He had a little laboratory at his disposal, where Walther Gerlach and Born's assistant Otto Stern conducted experiments in 1922 proving that the spatial orientation of the angular momentum of atoms was quantized –in other words, that it did not vary in a continuous fashion (the Stern-Gerlach effect). But by the time Stern and Gerlach ran the experiment, Born was no longer at Frankfort; in 1921 the University of Göttingen asked him to replace Peter Debye as chaired professor and director of the Institute of Physics, which encompassed both theoretical and experimental physics. Born, however, was uninterested in handling the experimental side of things and convinced the ministry to make James Franck (1882-1964) a chaired professor, too, so Franck could manage the experimental division. The proposal was accepted. So, Göttingen had three chaired professors of physics: Robert Pohl, who had been promoted from associate to chaired professor in 1920, Born and Franck. As we shall see in another chapter, Franck also played a big role in Maria Goeppert Mayer's life.

In Göttingen Born focused fundamentally on the problems of quantum physics, a task in which he was helped considerably by his first two assistants, Wolfgang Pauli and Werner Heisenberg. Here is what he said about them in his fullest biography, *My Life*:

> The series of my assistants [in Göttingen] is rather remarkable, namely Pauli, Heisenberg, Jordan, Hund, Hückel, Nordheim, Heitler and Rosenfeld [...].
>
> Pauli was recommended to me by Sommerfeld [...] He was an 'infant prodigy'. [...] We met first during the summer of 1921 in Ehrwald, Tyrol, where I was spending my holiday [...] I remember that even in the most lovely or majestic mountain scenery, Pauli continued to discuss physical problems. No mental relaxation was possible in the company of this dynamic fellow. Of course he was not a real success as 'assistant'. We worked together, on refined problems of perturbation theory and its application to the quantum theory of atoms, and I learned a great deal from him, certainly more than he from me. But I had no great help from him in my routine work of teaching. I suffered at that time from asthmatic attacks and sometimes had to stay in bed for a day or two. Then Pauli was supposed to give my lecture, which was from 11 a.m. to 12 noon. But he was inclined to forget it, and if our maid was sent to remind him at half past ten, she usually found him still sound asleep.

When Pauli left, Sommerfeld recommended Heisenberg. 'He was no less an "infant prodigy". He was working at that time at his doctoral thesis [… but] Sommerfeld advised him to accept my offer in order to breathe a different scientific atmosphere. When he arrived (it must have been October 1923) he looked like a simple peasant boy, with short, fair hair, clear bright eyes and a charming expression. He took his duties as an assistant more seriously than Pauli and was a great help to me. His incredible quickness and acuteness of apprehension enabled him to do a colossal amount of work without much effort; he finished his hydrodynamic thesis, worked on atomic problems partly alone, partly in collaboration with me and helped me direct my research students.'

But let us return to Heisenberg before he moved to Göttingen.

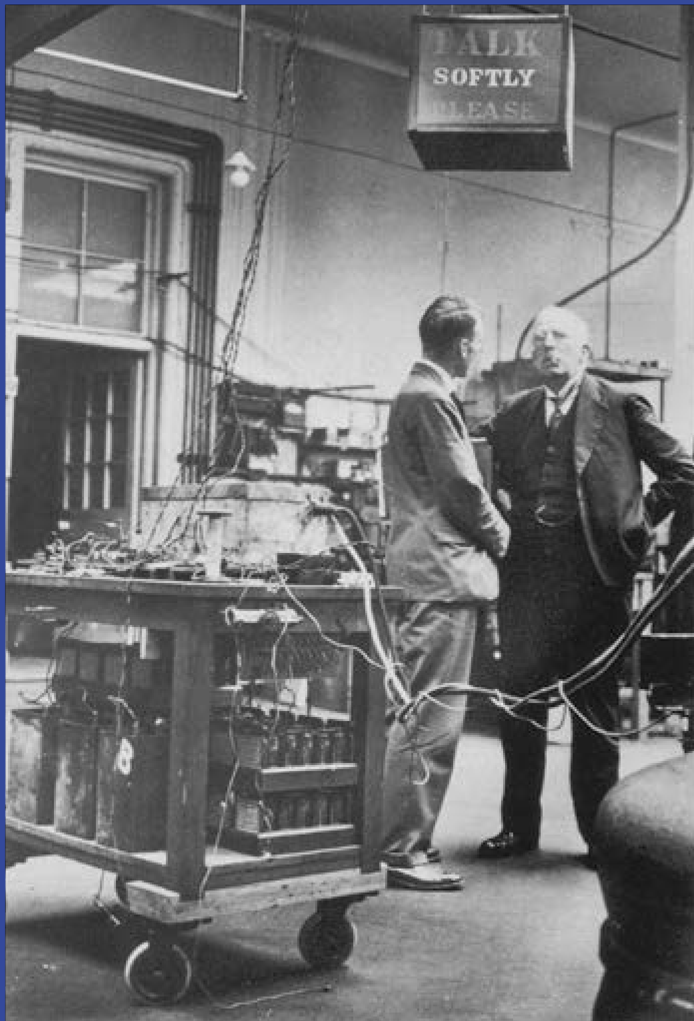
Heisenberg, Born, Bohr and Matrix Quantum Mechanics

Although, as we saw, Sommerfeld made Heisenberg do his doctoral thesis on hydrodynamics, Sommerfeld still kept Heisenberg in the loop regarding quantum physics. And one day early in the summer of 1922, as Heisenberg reminisced in his memoirs, 'Sommerfeld asked me rather unexpectedly after a long talk about atomic theory: "Would you like to meet Niels Bohr? He is about to give a series of lectures in Göttingen. I have been invited, and I should like to take you along."' Heisenberg naturally accepted (Sommerfeld covered his pupil's expenses). The lectures, the *Bohr-Festspiele* (Bohr Festival), ran from 12 to 22 June and was attended that year by Paul Ehrenfest, Alfred Landé and Wolfgang Pauli, among others, in addition to the Göttingen physicists. The festival was paid for with the interest on the fund of a hundred thousand marks that mathematician Paul Wolfskehl had bequeathed in 1906 to anyone who could prove Fermat's theorem. Heisenberg went on with his story:

> I shall never forget the first lecture. The hall was filled to capacity. The great Danish physicist, whose very stature proclaimed a Scandinavian, stood on the platform, his head slightly inclined and a friendly but somewhat embarrassed smile on his lips. Summer light flooded in through the wide-open windows. Bohr spoke fairly softly, with a slight Danish accent. When he explained the individual assumptions of his theory, he chose the words very carefully, much more carefully than Sommerfeld usually did. And each one of his carefully chosen sentences revealed a long chain of underlying thoughts, of philosophical reflections, hinted at but never fully expressed. I found this approach highly exciting…

At one point, after Bohr had referred to some work by Kramers that Heisenberg was familiar with, Heisenberg objected.

Niels Bohr and Max Planck



Ernest Rutherford at the Cavendish Laboratory

Max Planck

Niels Bohr in Göttingen (June 1922). From left to right: C. Oseen, N. Bohr, J. Franck and O. Klein (standing), M. Born, (seated)

> I then rose and advanced objections to Kramers' theory based on our Munich discussions.
>
> Bohr must have gathered that my remarks sprang from profound interest in his atomic theory. He replied hesitantly, as though he were slightly worried by my objection, and at the end of the discussion he came over to me and asked me to join him that afternoon on a walk over the Hain Mountain. There we might go more deeply into the whole problem.
>
> This walk was to have profound repercussions on my scientific career, or perhaps it is more correct to say that my real scientific career only began that afternoon.

And after relating the details of some scientific affairs –and some personal affairs– Heisenberg concluded the recollection of his first contact with Bohr in the following words:

> As we approached the edge of the town, the conversation turned to Göttingen's leading physicists and mathematicians–Max Born, James Franck, Richard Courant and David Hilbert, all of whom I had only just met. Bohr suggested that I might do part of my studies under them. Suddenly the future looked full of hope and new possibilities, which, after seeing Bohr home, I painted to myself in the most glorious colors all the way back to my lodgings.

Heisenberg and Bohr remained close until World War II drove a deep cleft between them. Denmark was one of the countries that Germany invaded, and in September 1941 Heisenberg travelled to Copenhagen as a sort of German 'cultural ambassador' to deliver a number of lectures. He seized the chance to call on Bohr, and his visit made it clear how greatly their political postures differed. But before they had really established any relationship at all, Heisenberg moved to Göttingen on Sommerfeld's advice, as we have already seen.

Born's and Franck's research in Göttingen centred on quantum physics. And Heisenberg took advantage of this. 'In the seminars run by Max Born in Göttingen during the summer of 1924', he recalled in his autobiography, 'we had begun to speak of a new quantum mechanics that would one day oust the old Newtonian mechanics, and whose vague outlines could already be discerned here and there. Even during the subsequent winter term, which I once again spent in Copenhagen, trying to develop [Henrik] Kramers' [one of Bohr's collaborators] theory of dispersion phenomena, our efforts were devoted not so much to deriving the correct mathematical relationships as to guessing them from similarities with the formulae of classical theory.'

In the quotation above, Heisenberg mentioned that he spent some time in Copenhagen with Bohr. He visited the Danish capital for the first time on 15

March 1924. That first stay in Copenhagen was not very long, only a couple of weeks, but it was intense. His host paid him every courtesy; Heisenberg's fame as a disciple of Sommerfeld and then a collaborator of Born had preceded him. Soon he would return for a longer stay; in early July Bohr wrote to inform him that the Rockefeller Foundation's International Education Board had granted him a thousand dollars to work in Copenhagen for a year. However, young Werner was a good scientist, and well appreciated, and Max Born was not prepared to give him up. So, Bohr and Born reached an agreement: since Born was going to spend the winter semester of 1924-1925 in the United States, Werner could spend that time in Denmark, but he must be back in Göttingen by 1 May 1925. Thus, Heisenberg occupied an especially choice position, since he could benefit from and combine two scientific styles, two traditions, that were different but complementary. 'Well, the emphasis in Göttingen was more on the mathematical side, on the formal side', mused Heisenberg in an interview he gave Thomas Kuhn in February 1963 as part of the Sources for History of Quantum Physics project, 'and in Copenhagen the emphasis was more on the philosophical side, I should say. This is true in the following sense: For Born, a description of physics would always be a mathematical description, so his attention was concentrated on the idea of how the mathematical scheme to describe these funny things which we see in our experiments would look. Bohr's approach in Copenhagen would be different. Bohr would ask, "Well, how can nature avoid contradictions? Now we know the wave picture, we know interference, we know the Compton effect, we know all that –how can our Lord possibly keep this world in order?" And so he wanted first to understand how contradictions are avoided, how things are connected, and he would say, "Well, only when we have sufficiently understood that, only then can we hope to put it into forms of mathematics."

It was this philosophical and mathematic 'dual focus' that helped him produce quantum mechanics as a blend of mathematical form (noncommutative matrix algebra) and philosophical approach (theory based exclusively on observable magnitudes).

Let us see how Heisenberg explained the steps that bore him to the threshold of the new quantum mechanics: 'In the winter semester of 1924-25 I had again been working in Copenhagen, developing the [wave and particle] dispersion theory along with Kramers. In this connection, certain mathematical expressions had appeared in the formulae for the Raman effect, which in classical theory figured as products of Fourier series, whereas in quantum theory they obviously had to be replaced by similarly constructed products of series having to do with the quantum-theoretical amplitudes for emission or absorption lines. [...] When, in the summer semester of 1925, I again took up the work in Göttingen, one of the earliest discussions with Born led to the conclusion that I should try to conjecture the correct amplitudes and intensities for hydrogen from the correspondence-type matching formulae of the classical theory. [...] But on going into it more deeply the problem turned out to be too complex, at least for my mathematical capacities, and I searched for simpler mechanical systems, in which the method of conjecture promised more success. In so doing I had the feeling that I should renounce any description of electron pathways, indeed that I ought deliberately to repress such ideas. I wanted, rather, to trust entirely to the half-empirical rules for the multiplication of amplitude series, which had proved themselves in dispersion theories.'

In late May, on the island of Heligoland, where he had gone for a respite from hay fever, Heisenberg was able to apply himself better to his research. Studying one of those simple systems he was looking for, the one-dimensional non-harmonic oscillator, he replaced the position coordinate with a table of amplitudes corresponding to a classic Fourier series, and he arrived at the equation describing the system's movement. In other words, he developed a calculus for the transition amplitudes of the lines of emission and absorption in atoms. He was startled to find that, in the amplitude multiplications he had to perform, A·B was not equal to B·A. Only some time later did he learn through Max Born that he had been unknowingly handling and multiplying matrices, which are just ordered sets of numbers: the different amplitudes of transition between levels. Matrix calculus is now a basic, uncomplicated division of mathematics taught at a rather early level, so knowledge of matrices and their properties (algebra) is quite widespread even among non-mathematicians. But at the time we are talking about, this was not yet the case. No wonder Heisenberg was unaware of matrices, while Born, who was well grounded in mathematics, knew all about them.

Naturally, Heisenberg had to test further requirements to see if the formulations he had developed could aspire to quantum mechanics status, but in the end all the boxes were ticked. For Heisenberg, 'one could hope to have found the basis for a quantum mechanics'.

After spending two weeks on Heligoland, Heisenberg returned to Göttingen, though he stopped off at Hamburg to see Wolfgang Pauli, whose opinion and critical powers he held in high esteem. And his friend's reaction was glowing. On 27 July, for example, Pauli wrote to Kramers:

> The 'community of true believers' would not gain honour nor much success by trying to fight against the trend in the current development of quantum theory that seeks to analyse the concepts of motion and force. I feel there is hope for making positive strides in this direction, too. I am particularly pleased to have received Heisenberg's daring suppositions (of which you have no doubt heard in Göttingen). True, he is still a good way from being able to say anything definitive, and we are just at first principles here. But what I like so much about Heisen-

> berg's considerations is the method in his procedure and the purpose that has led him to put these considerations forward. Generally I believe that, as far as my own scientific ideas are concerned, right now I am very close to Heisenberg and we feel the same about almost everything, as far as that can be in two independent people. I have also noted with pleasure that Heisenberg has learned a bit of philosophical thought with Bohr in Copenhagen and has perceptibly distanced himself from the purely formal. As a consequence now I feel less alone than I did half a year ago when (spiritually and spatially) I found myself quite isolated between the Scylla of the mystical school of the Munich rank and file and the Charybdis of Copenhagen's reactionary putsches, which you have propagandized with fanatical excess! Now my only hope is that you will no longer delay the recovery process for Copenhagen's physics, something that must eventually take place, given Bohr's strong sense of reality.

A pleasant, courteous manner was by no means one of Pauli's personal charms.

One especially interesting point of Pauli's letter is his reference to the fact that, under Bohr's influence, Heisenberg had become more 'philosophical' and was all the better for it. The surest, most immediate interpretation of this statement has to do with the fact that Heisenberg's quantum system was based on 'observable magnitudes'.

When Born read Heisenberg's manuscript back in Göttingen, he was 'fascinated'. The article was accordingly sent in for publication: 'Über quantentheoretische Umdeutung kinematischer und mechanischer Beziehungen' ('Quantum Theoretical Reinterpretation of Kinematic and Mechanical Relations'), *Zeitschrift für Physik* 33, 879-893 (1925).

The matrix quantum mechanics that first article contained had yet to be developed and formalized. That work was done by Heisenberg, together with Pascual Jordan (1902-1980) and Born, whose mathematical skills, whetted under the tutelage of David Hilbert and Felix Klein, were of great help. The main result of their collaboration was an article in which matrix mechanics assumed its most finished form. That article later came to be known as 'the Three-Man Paper' (*Drei-Männer Arbeit*): Max Born, Werner Heisenberg and Pascual Jordan, *Zur Quantenmechanik II, Zeitschrift für Physik* 35, 557-615 (1925).

The new matrix mechanics met with very different reactions. Two groups formed. On one side there were, shall we say, the all-quantum 'moderns', headed by men like Born, Bohr and Pauli, who accepted it; on the other side, the 'old-fashioned' group, or perhaps it would be better to call them 'the classicals', who rejected it.

The first group, apart from the 'Three Men' themselves, also accrued support through its core members' activities and connections. Born furnishes a fine example of how this worked. Between 14 November 1925 and 22 January 1926, Born taught a course at the Massachusetts Institute of Technology that he called 'Problems of Atomic Dynamics', in which he made sure to explain in detail the contents of Heisenberg's 1925 article, his own article with Jordan, the *Drei-Männer Arbeit* (not yet entirely finished) and Pauli's paper applying Heisenberg's mechanics to the hydrogen atom. And Born did not speak only at Massachusetts; he also visited the California Institute of Technology, the Universidad de California, Berkeley, the University of Wisconsin, the University of Chicago and Columbia University.

Meanwhile, the 'classical quantum' physicists could not help but take an interest in the new ideas, as Einstein admitted in a letter he wrote on 7 March 1926 to Hedwig Born, Max Born's wife: 'The Heisenberg-Born concepts leave us breathless, and have made a deep impression on all theoretically oriented people. Instead of dull resignation, there is now a singular tension in us sluggish people'. Even so, the highly mathematical, abstract Heisenberg-Born-Jordan formulation (remember, the theory's constructs did not include any physical picture of orbits) did not win many fans, especially among the Berlin physicists who were 'confronting quantum mechanics with wondrous mistrust' (Einstein to Ehrenfest, 28 August 1926). This posture on the part of the 'gentlemen of the continuum', as Heisenberg wryly called them in his letters to Pauli, was because the theory was too abstract for them. It had been formulated without any kind of model describing the processes taking place inside the atom. To Einstein (letter to Michele Besso, 25 December 1925), the foremost representative of the Berlin physicists, Heisenberg's matrix formalism seemed a 'magic formula […] sufficiently protected against refutation through its supreme ingenuity and complicated nature'. These physicists were repulsed even more by the way British physicist Paul Dirac (1902-1984), who developed his own version of quantum theory, usually presented his results. Einstein (letter to Paul Ehrenfest, 23 August 1926) compared it to a terrible 'balancing on a dizzying path between genius and insanity. Nothing that one can grasp firmly in hand!' Max von Laue was similarly horrified (as shown by a letter to Erwin Schrödinger, 12 October 1926) by Pauli's 'monstrous treatment' of the problem of the hydrogen atom using Heisenberg's matrix method.

Heisenberg had the chance to witness Einstein's reaction to his basic approach in the flesh. In spring 1926 Heisenberg was invited to speak at a colloquium organized by the Berlin physicists to present his quantum theory. When Heisenberg finished Einstein asked him to come home with him so they could continue discussing the new quantum mechanics. 'But on arrival', Heisenberg recalled many years later, 'he at once began with a central question about the philosophical foundation of the new quantum mechanics. He pointed out to me that in my mathematical description the notion of "electron path" did not occur at all, but that in a cloud-chamber the track of the electron can of course be observed directly. It seemed to him absurd to claim that there was indeed an electron path in the cloud-chamber, but none in the interior of the atom. The notion of a path could not be dependent, after all, on the size of the space in which the electron's movements were occurring. I defended myself to begin with by justifying in detail the necessity for abandoning the path concept within the interior of the atom. I pointed out that we cannot, in fact, observe such a path; what we actually record are frequencies of the light radiated by the atom, intensities and transition-probabilities, but no actual path. And since it is but rational to introduce into a theory only such quantities as can be directly observed, the concept of electron paths ought not, in fact, to figure in the theory.'

'To my astonishment, Einstein was not at all satisfied with this argument. He thought that every theory in fact contains unobservable quantities. This principle of employing only observable quantities simply cannot be consistently carried out. And when I objected that in this I had merely been applying the type of philosophy that he, too, had made the basis of his special theory of relativity, he answered simply: "Perhaps I did use such philosophy earlier, and also wrote it, but it is nonsense all the same."'

The famous Berlin physicists were so revolted and frustrated by the work of the 'matrix' physicists that they and others experienced immense relief when, less than half a year after the discovery of the matrix formalism, Schrödinger presented his idea of wave mechanics. Wave mechanics promised a return to the field's more familiar physics, understood as 'the essence of those theories that describe physical phenomena causally through partial differential equations in space and in time'.

Schrödinger and Wave Mechanics

On 29 November 1924, French physicist Louis de Broglie (1892-1987) presented his doctoral thesis, 'Recherches sur la théorie des quanta' ('Research into the Theory of Quanta') at the University of Paris School of Sciences. There he introduced the famous wave-corpuscle duality that Einstein foreshadowed in 1909. In de Broglie's own words from an introduction he wrote for the 1963 reprint of his thesis, 'Guided by the idea of a general relationship between the notions of frequency and of energy, we acknowledge in the present study the existence of a periodical phenomenon of as-yet-undetermined nature that is linked to all isolated groupings of energy'. This assumption meant that each particle (such as the electron) could be associated with a physical wave, ψ, that is propagated in space. The Bohr-Sommerfeld quantization conditions now appeared as assertions about the number of wavelengths that exactly covered an electron's orbit around the nucleus.

One way to take de Broglie's result was to consider that he had found the wave mechanics ('wave' because it associated waves with the movement of particles) of free (non-interacting) electrons. De Broglie had blazed a trail that could be anticipated to lead eventually to a general quantum mechanics, albeit once again a quantum mechanics based on waves. Among those who believed in the French physicist's focus was Erwin Schrödinger (1887-1961), an Austrian who had held a chaired professorship in Zurich since 1921.

In a memorable series of articles published in 1926, Schrödinger developed such a wavelength quantum mechanics: 'Quantisierung als Eigenwertproblem' ('Quantization as an Eigenvalue Problem'). One trait that distinguished his mechanics from Heisenberg's from the outset was its physical significance: unlike matrix mechanics, wavelength mechanics could be visualized. And the mathematical apparatus involved was much more comfortable than that little-known newcomer, matrix calculus. Schrödinger's equations were the familiar equations in partial derivatives so well covered in the recently published (1924) book by Richard Courant and David Hilbert, *Methoden der mathematischen Physik* (*Methods of Mathematic Physics*). In fact, in the paper where Schrödinger traced how his theory related to Heisenberg's, he rejoiced in the mathematical advantages of his method:

> My theory was inspired by L. de Broglie, Ann. De Physique (10), 3, p. 22 1925 (*Theses*, Paris, 1924), and by brief, yet infinitely far-seeing remarks of A. Einstein, Berl. Ber., 1925, p. 9 et *seq*. I did not at all suspect any relation to Heisenberg's theory. I naturally knew about his theory, but was discouraged, if not repelled, by what appeared to me as very difficult methods of transcendental algebra, and by the want of perspicuity [*Anschaulichkeit*].

The physical idea initially underpinning Schrödinger's work was ably summed up by the respected Dutch physicist Hendrik A. Lorentz in a letter he wrote to his Austrian colleague on 27 May 1926:

> Your conjecture that the transformation which our dynamics will have to undergo will be similar to the transition from ray optics to wave optics sounds very tempting, but I have some doubts about it.
>
> If I have understood you correctly, then a 'particle', an electron for example, would be comparable to a wave packet which moves with the group velocity.

Those who abhorred the idea of giving up the classic maxim 'natura non facit saltus', the 'gentlemen of the continuum', welcomed Schrödinger's contributions and ideas warmly. Einstein (letter to Schrödinger, 26 April 1926) was convinced that Schrödinger had 'made a decisive advance with your formulation of the quantum condition, just I am equally convinced that the Heisenberg-Born route is off the track'; Planck (letter to Schrödinger, 2 April 1926) read his 1926 papers 'the way an inquisitive child listens in suspense to the solution of a puzzle that he has been bothered about for a long time, and I delighted with the beauties that are evident to the eye'; and Lorentz (letter to Schrödinger, 27 May 1926) said, 'If I had to choose now between your wave mechanics and the matrix mechanics, I would give the preference to the former, because of its greater intuitive clarity'. Soon, however, it was discovered that Schrödinger's initial interpretation could not stand. One of the problems, which Lorentz pointed out, was the dispersion of the wave packets, which made it almost impossible to sustain the interpretation of particles (electrons) as waves in a system of more than one particle. The trouble with the physical interpretation of Schrödinger's wavelength mechanics did not mean the theory's formulation was incorrect, though; it only meant that particular interpretation had to be discarded. This was confirmed by Schrödinger's own discovery of the 'mathematical, formal identity' of wavelength mechanics, which stressed the continuous, and matrix mechanics, which stressed the discontinuous.

At first, matrix mechanics' supporters frowned at the idea that wavelength mechanics deep down represented the same physical reality as matrix mechanics. Heisenberg was particularly loath to accept the new formulation. However, Schrödinger's theory very soon prevailed, because it was so much easier to work with. And the former defenders of the alternative framework eventually not only passed to the opposite camp, but helped configure its physical interpretation, an interpretation that turned out to be very different from what Schrödinger and the gentlemen of the continuum would have wished. In 1926 Max Born gave his probabilistic interpretation of the wave function, ψ (the object describing quantum systems), defined in the field of complex numbers, which considered $|\psi|^2$ a measurement of the density of the probability that the system was in the state represented by ψ.

The Uncertainty Principle

One paramount element of quantum mechanics that eventually came to light was the famous 'uncertainty principles' that Heisenberg formulated in 1927. Let us see how he did it.

In early May 1926, Heisenberg settled down in Copenhagen as a university lecturer and Bohr's assistant. He was only on temporary leave from Göttingen, since he did not want to break off his relations with Germany; in fact, he was expecting a juicy offer of a full professorship there (he remained in Copenhagen until June 1927).

In Copenhagen Bohr and Heisenberg continued poring over how to interpret basic aspects of the new quantum physics, especially after the talks they had had with Schrödinger when he visited. Heisenberg reported his conversations with Bohr in his autobiography:

> During the next few months the physical interpretation of quantum mechanics was the central theme of all conversations between Bohr and myself. I was then living on the top floor of the Institute, in a cozy little attic flat with slanting walls and windows overlooking the trees at the entrance to Faelled Park. Bohr would often come into my attic late at night, and we constructed all sorts of imaginary experiments to see whether we had really grasped the theory. In so doing, we discovered that the two of us were trying to resolve the difficulties in rather different ways. Bohr was trying to allow for the simultaneous existence of both particle and wave concepts, holding that, though the two were mutually exclusive, both together were needed for a complete description of atomic processes. I disliked this approach. I wanted to start from the fact that quantum mechanics as we then knew it already imposed a unique physical interpretation of some magnitudes occurring in it –for instance, the time averages of energy, momentum, fluctuations, etc.– so that it looked very much as if we no longer had any freedom with respect to that interpretation. Instead, we would have to try to derive the correct general interpretation by strict logic from the ready-to-hand, more special interpretation.
>
> For that reason I was –certainly quite wrongly– rather unhappy about a brilliant piece of work Max Born had done in Göttingen. In it, he had treated collisions by Schrödinger's method and assumed that the square of the Schrödinger wave function measures, in each point of space and at every instant, the probability of finding an electron in this point at that instant. I fully agreed with Born's thesis as such, but disliked the fact that it looked as if we still had some freedom of interpretation [...].

Of all the possible experiments Bohr and Heisenberg considered, one posed particularly serious problems for them: the experiment that, as I said earlier, Einstein had laid before Heisenberg concerning an electron's trajectory through a cloud chamber (a cloud chamber uses the fact that water droplets condense and form around the ions of a gas to enable the trajectories of the ionizing particles to be observed and photographed). Again, in his memoirs, Heisenberg said, 'On the other hand, neither of us could tell how so simple a phenomenon as the trajectory of an electron in a cloud chamber could be reconciled with the mathematical formulations of quantum or wave mechanics. Such concepts as trajectories or orbits did not figure in quantum mechanics, and wave mechanics could only be reconciled with the existence of a densely packed beam of matter if the beam spread over areas much larger than the diameter of an electron.'

Their exchanges became so heated that Bohr, exhausted, ran off to Norway for a restful skiing trip in February 1927. This turned out to be a lucky break for Heisenberg, who could now think by himself. And, in his own words,

> I now concentrated all my efforts on the mathematical representation of the electron path in the cloud chamber, and when I realized fairly soon that the obstacles before me were quite insurmountable, I began to wonder whether we might not have been asking the wrong sort of question all along. But where had we gone wrong? The path of the electron through the cloud chamber obviously existed; one could easily observe it. The mathematical framework of quantum mechanics existed as well, and was much too convincing to allow for any changes. Hence it ought to be possible to establish a connection between the two, hard though it appeared to be.
>
> It must have been one evening after midnight when I suddenly remembered my conversation with Einstein and particularly his statement, 'It is the theory which decides what we can observe.' I was immediately convinced that the key to the gate that had been closed for so long must be sought right here. I decided to go on a nocturnal walk through Faelled Park and to think further about the matter. We had always said so glibly that the path of the electron in the cloud chamber could be observed. But perhaps what we really observed was something much less. Perhaps we merely saw a series of discrete and ill-defined spots through which the electron had passed. In fact, all we do see in the cloud chamber are individual water droplets which must certainly be much larger than the electron. The right question should therefore be: Can quantum mechanics represent the fact that an electron finds itself approximately in a given place and that it moves approximately with a given velocity, and can we make these approximations so close that they do not cause experimental difficulties?
>
> A brief calculation after my return to the Institute showed that one could indeed represent such situations mathematically, and that the approximations are governed by what would later be called the uncertainty principle of quantum mechanics […].

The mathematical expression to which he referred was none other than this:

$$\Delta x \cdot \Delta p \geq h$$

where x represents position, p represents linear momentum (the product of mass times velocity), Δ represents uncertainty and h is Planck's constant. 'This formulation, I felt,' Heisenberg continued, 'established the much-needed bridge between the cloud-chamber observations and the mathematics of quantum mechanics. True, it had still to be proved that any experiment whatsoever was bound to set up situations satisfying the uncertainty principle, but this struck me as plausible a priori, since the processes involved in the experiment or the observation had necessarily to satisfy the laws of quantum mechanics.'

The end result was an article, 'Über den anschaulichen Inhalt der quantentheoretischen Kinematik und Mechanik' ('On the Evident Content of Quantum Theoretical Kinematics and Mechanics'), which was published in volume 43 (1927) of *Zeitschrift für Physik*. The abstract opening the article gives a magnificent idea of its contents:

> First we define the terms *velocity*, *energy*, etc. (for example, for an electron) which remain valid in quantum mechanics. It is shown that canonically conjugate quantities can be determined simultaneously only with a characteristic indeterminacy. This indeterminacy is the real basis for the occurrence of statistical relations in quantum mechanics. Its mathematical formulation is given by the Dirac-Jordan theory. Starting from the basic principles thus obtained, we show how microscopic processes can be understood by way of quantum mechanics. To illustrate the theory, a few special *gedankenexperiments* [thought experiments] are discussed.

At the very end of the article, Heisenberg drew a conclusion with long-range philosophical implications:

> If one assumes that the interpretation of quantum mechanics is already correct in its essential points, it may be permissible to outline briefly its consequences of principle. We have not assumed that quantum theory –in opposition to classical theory– is an essentially statistical theory in the sense that only statistical conclusions can be drawn from precise initial data [...]. But what is wrong in the sharp formulation of the law of causality, 'When we know the present precisely, we can predict the future', is not the conclusion but the assumption. Even in principle we cannot know the present in all detail [...]. As the statistical character of quantum theory is so closely linked to the inexactness of all perceptions, one might be led to the presumption that behind the perceived statistical world there still hides a 'real' world in which causality holds. But such speculations seem to us, to say it explicitly, fruitless and senseless. Physics ought to describe only the correlations of observations. One can express the true state of affairs better in this way: Because all experiments are subject to the laws of quantum mechanics, and therefore to [the uncertainty principle], it follows that quantum mechanics establishes the final failure of causality.

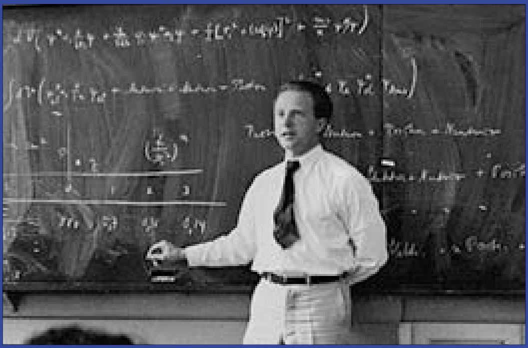
The Copenhagen Interpretation

With elements like these –firmly rejected by physicists like Einstein and Planck– the 'Copenhagen interpretation' of quantum mechanics was created, so called due

Max Born

Niels Bohr and Albert Einstein

Erwin Schrödinger



Werner Heisenberg

to Niels Bohr's huge role in formulating the interpretation and his perhaps even larger role in spreading it. The Copenhagen interpretation is usually summarized as follows: the wave function under Schrödinger's equation is made up of the 'superposition' sum of a series of wave functions (eigerfunctions) associated with the different possible physical situations, each multiplied by a certain value (the problem being a mathematical problem of the eigenvalues of operators, as stated in the title of Schrödinger's articles). In theory, if there is no 'interference' with the outside, the system yielded by the principal wavefunction evolves continuously, governed by Schrödinger's wave equation. But this situation –dominated by continuity, and in this sense similar to the situation seen in classical physics– vanishes when measurements are made. And here is where 'the observer' (or 'the measuring instrument') comes in. In the Copenhagen interpretation, the observer is especially important in quantum physics. Bohr assumed that there is a distinct separation between the observed object and the observing instrument. The object obeys 'quantum' laws, while the instrument obeys 'classical' laws. There is a kind of conceptual 'cut-off point' between the two of them. This is a tricky point, because where, exactly, is the cut-off point? The Copenhagen answer was to assume, without conclusive proof, that the net results of the theory do not critically depend on where the cut-off point is located. When a measurement (or an observation) is made, the wave function 'collapses'; that is, the system –by some unknown means– chooses a given situation expressed by one of the eigenfunctions. The only thing quantum mechanics tells us is the probability that one or another of those situations will happen, a probability that is associated with the coefficients in each of the summands making up the complete wave function. Before the measurement is taken, the system lies in a state in which different quantum states exist simultaneously in 'superposition'.

Erwin Schrödinger imagined a situation that illustrated the underlying problem of the Copenhagen interpretation. This was the famous 'Schrödinger's cat', which he presented in a three-part article published in *Die Naturwissenschaften*, entitled 'Die gegenwärtige Situation in der Quantenmechanik' ('The Current Situation in Quantum Mechanics'). He introduced the matter as follows:

> A cat is placed in a steel chamber, together with the following devilish device (which must be made safe from any direct interference by the cat): in a Geiger counter there is a small piece of radioactive substance, so small that maybe in the course of an hour one of the atoms might disintegrate, but it is also equally likely that none of them will; if this happens [i.e., the atom does break down], the counter's tube generates a discharge and through a relay a hammer is triggered that breaks a small flask of hydrocyanic acid. If we have left the full system for an hour, without tampering with it in any way, we could say the cat still lives if in that interval no atoms have disintegrated. The first atom to disintegrate would have poisoned it. The function ψ of the full system would express this, as

> it is made up of the dead cat and the living cat (excuse the expression) mixed or scattered in equal parts.

What Schrödinger was pointing out is the role of the *superposition of states* in quantum mechanics. Depending on how the wave function collapses, says the interpretation, when the observer makes a measurement, that measurement nails all the possible states in the original complete wave function down into one state, with a certain probability.But what is going on before the measurement is made? The immediate answer seems straightforward enough in the case of atoms, but, as Schrödinger pointed out, what about in the case of macroscopic objects?

Heisenberg's and Schrödinger's quantum mechanics –and Dirac's formulation, too– opened up a new world, equally scientific and technological, but that was really just the first step. Many challenges still lay ahead, such as making quantum mechanics compatible with the requirements of the special theory of relativity and constructing a theory of electromagnetism, an electrodynamics, that incorporated quantum requisites. If Einstein showed, and quantum physics later embraced, that light, an electromagnetic wave, was quantized (that is, that light had the properties of a wave and at the same time behaved like a 'current' of independent photons), and if electrodynamics as constructed by Maxwell in the 19^{th} century described light only as a wave devoid of any relationship with Planck's constant, then something was obviously wrong, and the electromagnetic field had to be quantized as well. And that was not all. Around 1927 quantum mechanics was basically (with the exception of its applications to solid-state physics) 'atomic physics'; science had only a spotty knowledge of the nucleus (i.e., nuclear physics, which will appear in another chapter of this book). But apart from that, the basic edifice of quantum mechanics was considered complete. As an example of that optimistic view, Max Born and Werner Heisenberg said in the 'Conclusions' of their presentation at the fifth Solvay Conference, which ran from 24 to 27 October 1927 (it was the first Solvay Conference German physicists attended after World War I), 'By way of summary, we wish to emphasise that while we consider the last mentioned enquiries, which relate to a quantum mechanical treatment of the electromagnetic field, as not yet completed, we consider *quantum mechanics* to be a closed theory, whose fundamental physical and mathematical assumptions are no longer susceptible of any modification'.

CHAPTER 2

Göttingen and Maria Goeppert's Early Years

María Goeppert

Maria Goeppert (spelled 'Göppert' in German) was born on 28 June 1906 in Katowice, a city in Upper Silesia, which was then part of Germany, although it later became part of Poland under the Treaty of Versailles (signed on 28 June 1919 but effective as of 10 January 1920). Maria was the only child of Doctor Friedrich Goeppert (1870-1927) and Maria Wolff, who had taught French and given piano lessons on the side before her marriage.

In 1910 the family moved to Göttingen, a small city in Lower Saxony. Göttingen was founded in the Middle Ages. Its university, Georg-August-Universität, was established in 1737 under the auspices of George II, King of Great Britain and Ireland, the last British king born abroad (he was born in Hannover and became a prince-elector of the Holy Roman Empire in 1727). Göttingen was very much a university town. The reason the family moved there was that Friedrich Goeppert was given a professorship in paediatrics at the university. He thus carried on a family tradition covering six generations of university professors (several of his siblings also held chairs). Friedrich did not confine himself to fulfilling his university duties, though; he also founded a children's clinic. Many years later, in a series of conversations with Joan Dash included in Dash's book *A Life of One's Own*, Maria said that her father 'was the kind of person whom everybody loved… I always had the feeling when he came into my room when I was sick, I felt better. And so many other children said that too. When he walked to the hospital in the morning sometimes, especially after Christmas, there would be just a whole gang of children, and saying "*O, Herr* Goeppert, I want to tell you what I got for Christmas" and I asked him, "Who are they?" And he said, "I don't know". But they all, at some time, had been sick, and seen him, and they loved him. Everybody did'. She also acknowledged that she was closer to her father than to her mother: 'Well, my father was more interesting, you see. He was after all a scientist.'

Indeed, it was her father who sparked Maria's interest in science and kept it alive. She remembered, for instance that she must have been around three years old when she looked up at the half moon and asked him what it was, and her father explained it to her. Another time, when she was seven, he prepared a special pair of glasses for her so she could watch a solar eclipse, and when it occurred he carefully explained why it was happening. 'Any sensible question he would answer.' They also tramped around the Göttingen countryside together, discovering nature.

That was not the kind of education little girls used to get, but *Herr Professor* Goeppert had his own opinions. He told her she should grow up to be more than just a *Hausfrau*, with bigger goals. And the environment Maria grew up in, there in Göttingen, helped her do it. German *Professoren* were, as she called them, powerful 'mandarins' who enjoyed an excellent standard of living. In addition, Göttingen was so small that all the teachers mingled, with the occasional addition of one of their star students to the mix. Mathematician David Hilbert, whom Maria looked on as 'the King of Göttingen', lived next door to the Goeppert family.

World War I

Just four years after the Goepperts arrived in Göttingen, on the very day when Maria turned eight, 28 June 1914, Archduke Franz Ferdinand, heir to the throne of Austria-Hungary, was shot and killed in Sarajevo with his wife, Sophia, Duchess of Hohenberg, by a nineteen-year-old Serbian terrorist, Gavrilo Princip. In early July, with the support of Kaiser Wilhelm II, Vienna decided to hand Serbia a very objectionably worded ultimatum. On 23 July the note reached the Serbian government with the instructions that they had 48 hours to answer, with no room for arguments or negotiations. On the 25th Serbia replied that it accepted all the conditions except one, which called for Austro-Hungarian agents to participate in investigations on Serbian soil. Immediately the Austro-Hungarian Empire broke off diplomatic relations, as it had intended to do from the start, and on the 28th it declared war on Serbia. Then, on the 30th, Russia mobilized all its troops. The day after that, Germany demanded that it demobilize. Russia gave no response. On 1 August Germany and France decreed a general mobilization. That very day Germany declared war on Russia. It declared war on France on the 3rd. The British, meanwhile, who would

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have preferred mediation, declared war on Germany on the 4th, the same day that the German army invaded Belgium (clearing a path to the English Channel for the Germans). So began the Great War, as it was called before other wars came along to make numbering necessary. Soon the tides of war swept up Montenegro (7 August), Japan (23 August) and Turkey (29 October). Much later, on 6 April 1917, the United States joined the Allied band.

In other words, the world was darkening, and soon it would be dyed blood red. And Göttingen did not just stand by. Here is what Max Born, probably one of the most level-headed of the German nationalists, wrote in his memoirs:

> In 1914 there was a patriotic outburst of foolish enthusiasm in all countries. We had it in Göttingen in full measure: flags and marching and singing. Troops marched through streets lined by people throwing flowers. Flags everywhere –in the streets and on the trains carrying soldiers to the front […]. The patriotic frenzy was coupled with wild rumours and a spy hunt: wells were said to be poisoned, the horses of the regiment paralysed, bridges blown up. All foreigners were rounded up and put into custody […]. The newspapers were full of pep articles. I hated the war, but I could not escape the influence of the propaganda. I believed, like all the others, that Germany was being attacked, that it was fighting for a worthy cause and that her existence was at stake […].
>
> I cannot deny that during that time I felt very much against the English, the French and above all the Russians. We were told every day about the abominable atrocities committed by the Cossacks in East Prussia. The idea of these 'Asiatic hordes' destroying the nice tidy German villages, torturing women and children and so on, infuriated me.

In that restless atmosphere, on 4 October 1914, spurred in part by worldwide fallout from the invasion of Belgium, 93 German intellectuals released what they called 'Aufruf an die Kulturwelt' ('Appeal to the Civilized World'). Author Ludwig Fulda wrote the first draft, his colleague Hermann Sudermann edited it, and Berlin novelist Georg Reicke composed the final version, which was translated immediately into ten languages and mailed to neutral nations in thousands of letters. This document did much more than anything else to cloud relations between scientists of the Central Powers (particularly German scientists) and the Allied Powers. Given its importance, I shall quote the appeal:

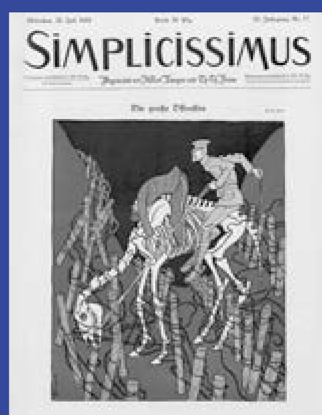
> As representatives of German Science and Art, we hereby protest to the civilized world against the lies and calumnies with which our enemies are endeavouring to stain the honour of Germany in her hard struggle for existence –in a struggle that has been forced on her. [...] As heralds of truth we raise our voices against these.
>
> *It is not true* that Germany is guilty of having caused this war. Neither the people, the Government, nor the Kaiser wanted war. Germany did her utmost to prevent it; for this assertion the world has documental proof. Often enough during the twenty-six years of his reign has Wilhelm II shown himself to be the upholder of peace, and often enough has this fact been acknowledged by our opponents. Nay, even the Kaiser, whom they now dare to call an Attila, has been ridiculed by them for years, because of his steadfast endeavours to maintain universal peace. Not till a numerical superiority which has been lying in wait on the frontiers assailed us did the whole nation rise to a man.
>
> *It is not true* that we trespassed in neutral Belgium. It has been proved that France and England had resolved on such a trespass, and it has likewise been proved that Belgium had agreed to their doing so. It would have been suicide on our part not to have preempted this.
>
> *It is not true* that the life and property of a single Belgian citizen was injured by our soldiers without the bitterest self-defence having made it necessary; for again and again, notwithstanding repeated threats, the citizens lay in ambush, shooting at the troops out of the houses, mutilating the wounded, and murdering in cold blood the medical men while they were doing their Samaritan work.
>
> *It is not true* that our troops treated Louvain brutally. Furious inhabitants having treacherously fallen upon them in their quarters, our troops with aching hearts were obliged to fire a part of the town as punishment. The greatest part of Louvain has been preserved. The famous Town Hall stands quite intact; for at great self-sacrifice our soldiers saved it from destruction by the flames. Every German would of course greatly regret if in the course of this terrible war any works of art should already have been destroyed or be destroyed at some future time, but inasmuch as in our great love for art we cannot be surpassed by any other nation, in the same degree we must decidedly refuse to buy a German defeat at the cost of saving a work of art.
>
> *It is not true* that our warfare pays no respect to international laws. It knows no indisciplined cruelty.

Maria's father, Friedrich Goeppert

Katowice, the city where Maria Goeppert Mayer was born

Monument to Gauss and Weber in Göttingen

The Göttingen Mathematical Society (1902). In the centre, with his arm on the table, is Felix Klein; seated next to him, to the left, David Hilbert; to the right, Karl Schwarzschild.



Cover of Simplicissimus, July 1916

> *It is not true* that the combat against our so-called militarism is not a combat against our civilization, as our enemies hypocritically pretend it is. Were it not for German militarism, German civilization would long since have been extirpated. For its protection it arose in a land which for centuries had been plagued by bands of robbers as no other land had been. The German Army and the German people are one and today this consciousness fraternizes 70,000,000 Germans, all ranks, positions, and parties being one.
>
> We cannot wrest the poisonous weapon –the lie– out of the hands of our enemies. All we can do is to proclaim to all the world that our enemies are giving false witness against us. You, who know us, who with us have protected the most holy possessions of man, we call to you:
>
> Have faith in us! Believe, that we shall carry on this war to the end as a civilized nation, to whom the legacy of a Goethe, a Beethoven, and a Kant is just as sacred as its own hearths and homes. For this we pledge you our names and our honour.

Among the signatories of this appeal were 15 illustrious scientists, all full professors: Adolf von Baeyer (chemistry, Munich), Karl Engler (chemistry, Karlsruhe), Emil Fischer (chemistry, Berlin), Wilhelm Förster (astronomy, Berlin), Fritz Haber (chemistry, Berlin), Ernst Haeckel (zoology, Jena), Gustav Hellmann (meteorology, Berlin), Felix Klein (mathematics, Göttingen), Philipp Lenard (physics, Heidelberg), Walther Nernst (chemistry and physics, Berlin), Wilhelm Ostwald (chemistry, Leipzig), Max Planck (physics, Berlin), Wilhelm Röntgen (physics, Munich), Wilhelm Wien (physics, Wurzburg) and Richard Willstätter (chemistry, Berlin). Göttingen mathematician David Hilbert was the only big-name German scientist who refused to sign.

The other signatories could be broken down as follows: 17 artists, 12 theologists, nine poets, seven historians, seven legal scholars, seven physicians (including the well-known Paul Ehrlich, Nobel Laureate in Medicine in 1908 and professor of bacteriology at the University of Berlin), five writers on art, four philosophers, four language scholars, three musicians, two political scientists and the director of the Deutsches Theater in Berlin.

In the social atmosphere then reigning in Germany, it was a ticklish matter to oppose the declaration publicly (nor was it easy to defend non-belligerent positions in other countries, as witness the case of Bertrand Russell in England). However, just a few days after the manifesto was published, a leading German pacifist, Georg Friedrich Nicolai, professor of physiology at the University of Berlin, prepared a response that he circulated among his university colleagues. Only two people signed it, Albert Einstein, who had by then moved to Berlin from Zurich, and Wilhelm Foerster, former director of the Berlin Observatory, one of the principal inspirations of the German Society for Ethical Culture. He, as we have seen, had also signed the Manifesto of the Ninety-Three! The document in question, entitled 'Aufruf an die Europäer' ('Appeal to the Europeans'), was distributed in mid-October, and only one philosophy student from Marburg, Otto Buek, signed it, but no independent publications printed it. It is an excellent example of the pacifism that was struggling to raise its head.

> While technology and traffic clearly drive us toward a factual recognition of international relations, and thus toward a common world civilization, it is also true that no war has ever so intensively interrupted the cultural communalism of cooperative work as this present war does. Perhaps we have come to such a salient awareness only on account of the numerous erstwhile common bonds, whose interruption we now sense so painfully.
>
> Even if this state of affairs should not surprise us, those whose heart is in the least concerned about common world civilization, would have a doubled obligation to fight for the upholding of those principles. Those, however, of whom one should expect such convictions –that is, principally scientists and artists– have thus far almost exclusively uttered statements which would suggest that their desire for the maintenance of these relations has evaporated concurrently with the interruption of the relations. They have spoken with explainable martial spirit –but spoken least of all of peace.
>
> Such a mood cannot be excused by any national passion; it is unworthy of all that which the world has to date understood by the name of culture. Should this mood achieve a certain universality among the educated, this would be a disaster.
>
> It would not only be a disaster for civilization, but –and we are firmly convinced of this– a disaster for the national survival of individual states –the very cause for which, ultimately, all this barbarity has been unleashed.
>
> Through technology the world has become *smaller*; the *states* of the large peninsula of Europe appear today as close to each other as the cities of each small Mediterranean peninsula appeared in ancient times. In the needs and experiences of every individual, based on his awareness of the manifold of relations, Europe –one could almost say the world– already outlines itself as an element of unity.
>
> It would consequently be a duty of the educated and well-meaning Europeans to at least make the attempt to prevent Europe –on account of its deficient organization as a whole– from suffering the same tragic fate as ancient Greece once did. Should Europe too gradually exhaust itself and thus perish from fratricidal war?

> The struggle raging today will likely produce no victor; it will leave probably only the vanquished. Therefore, it seems not only *good*, but rather bitterly *necessary, that educated men of all nations* marshall their influence such that –whatever the still uncertain end of the war may be– the *terms of peace shall not become the wellspring of future wars*. The evident fact that through this war all European relational conditions slipped into an *unstable and plasticized* state should rather be used to create an organic European whole. The technological and intellectual conditions for this are extant.
>
> It need not be deliberated herein by which manner this (new) ordering in Europe is possible. We want merely to emphasize very fundamentally that we are firmly convinced that the time has come where *Europe must act as one in order to protect her soil, her inhabitants, and her culture*.
>
> To this end, it seems first to be a necessity that all those who have a place in their hearts for European culture and civilization, in other words, those who can be called in Goethe's prescient words '*good Europeans*', come together. For we must not, after all, give up the hope that their raised and collective voices –even beneath the din of arms– will not resound unheard, especially, if among these 'good Europeans of tomorrow', we find all those who enjoy esteem and authority among their educated peers.
>
> But it is necessary that the Europeans first come together, and if –as we hope– enough *Europeans in Europe* can be found, that it is to say, people to whom Europe is not merely a geographical concept, but rather, a dear affair of the heart, then we shall try to call together such a union of Europeans. Thereupon, such a union shall speak and decide.
>
> To this end we only want to urge and appeal; and if you feel as we do, if you are likemindedly determined to *provide the European will the farthest-reaching possible resonance*, then we ask you to please send your (supporting) signature to us.

As I said before, the manifesto garnered almost no supporters. In his book *Die Biologie des Krieges* (*The Biology of War*), published in Switzerland in 1917, Nicolai said, 'Although we met with great approval in the private sending of the manifesto, not even those who approved it wanted to sign it: one felt the passage about Greece was not historically entirely accurate, another believed it came too late, another that it was too premature; another thought it a bad idea for science to be mixed with worldly commerce, etc. But the majority were too cowardly, or thought otherwise. Even the best Germans did not want to become good Europeans in those days, or did not dare to. But since the manifesto could gain value only if supported by the authority of well-recognized men, we scrapped our plan'.

The latter days of the war were for Germany a gradual build-up of revolutionary situations that would give birth to the Weimar Republic a few months later. Leaders like Ludendorff and Hindenburg fell from power, and the masses looked on the Kaiser as the person responsible for their past and present misfortunes and as the biggest obstacle to an armistice with the Allied Powers. As a result, the established order virtually collapsed. On 28 October 1918, the German fleet refused to sail from Kiel; the sailors raised red flags. The confusion increased as councils of workers and soldiers modelled on those in the recent Russian Revolution spread like wildfire across Germany.

Faced with this situation, the Kaiser abdicated, fleeing to the Netherlands with his family. On 9 November the social democrat ministers who had entered the Reich government a month before proclaimed the nation a republic. On the 10th the new government was installed. On the 11th the armistice ending the war was signed.

In January 1919 a Spartacist rebellion broke out in Berlin and was harshly crushed by the provisional social democrat government. Rosa Luxemburg and Karl Liebknecht, the leaders of the Spartacus League, were murdered by a group of right-wing officers. As one response to the situation, National Assembly elections were held. The Social Democratic Party did not secure the majority it needed, and it was forced to form a coalition with the democrats and centrists. Friedrich Ebert was named first president of the republic. Since Berlin was considered too dangerous a city, the National Assembly relocated to Weimar, although the government remained in the capital. Thus was born the Weimar Republic, whose constitution was passed on 31 July 1919.

The new republic immediate found itself mired in a set of circumstances that exacerbated the wound dealt by Germany's defeat. The conditions set in the Treaty of Versailles were considered an affront by most of the German people. In addition to practically liquidating the German colonial empire, the treaty amputated one seventh of the territory Germany held before the war, plus one tenth of its population. Alsace-Lorraine had to be returned to France. France also occupied the German territory west of the Rhine, from which it was to pull out gradually in a three-stage, fifteen-year withdrawal. The Saar, rich in coal, was also administered by non-Germans for fifteen years, at the end of which a referendum was to decide its fate. Furthermore, in the eastern territory, another referendum was to establish the fate of Upper Silesia. And on top of everything else, the Germans were forced to treat Poland, which it had dominated for years, as if it were a great power. Direct payments were to be made: 132 billion gold marks in compensation, plus a 26% export tax, to be collected over a 42-year period.

In addition there were other events. For instance, several times the French and the Belgians expanded their occupied territory in revenge for violations of the Treaty of Versailles (the most serious such action was the occupation of the Ruhr region from 11 January 1923 to late 1924).

While the political and social situation was bad, the economic situation for citizens, including *Herren Professoren*, was simply catastrophic. War and the treaty conditions caused inflation of a magnitude never before seen in history. Immediately before World War I, in 1913, the German mark, the English shilling, the Italian lira and the French franc were worth approximately the same, and a dollar was equivalent to about four marks, more or less. The year after the armistice, the mark fell to less than 10 percent of its previous value and stayed there until 1921, when it entered a sharp decline. In late 1923, shortly before stabilization was achieved with the introduction of the *Rentenmark*, a shilling, a lira or a franc could be exchanged for over a trillion marks! But nobody would take marks, of course. Naturally, inflation was not limited to Germany. In Austria prices eventually rose to 14,000 times their pre-war value; in Poland, they multiplied by 2,500,000; in Russia, by four billion.

A few paragraphs from the autobiography of Richard Willstätter, who won the Nobel Prize in Chemistry in 1915 and in 1916 was professor at the University of Munich and director of the Chemistry Laboratory there, afford an understanding of the situation German professors eventually found themselves in.

> Even in December 1922 I already did not have enough money for Christmas presents. The big banks had not understood the inflation. From the beginning of December until around the middle of January, I received no notice of any deposits from the large bank where the University used to deposit our various earnings. Needing money, I complained, and I was informed that 'in order to save our clients the high cost of postage we only send out monthly statements now'. Chief Secretary Krebs of the University chancellery possessed a more subtle understanding of inflation than the banks; he delayed payments a good deal longer than only one month or even several. Course fees and even more examination fees were then paid so late that during the time of severe inflation they were worthless.

The Goeppert family did not escape the hardships of the war's aftermath, though it suffered less than others. Aided by the fact that no few of her husband's patients were from the country and could only pay him with food, *Frau* Goeppert found clever ways to alleviate the strict food rationing imposed after the armistice. She even bought three piglets, two for her husband's clinic and one for the household.

At the end of the war, Maria began attending the Höhere Töchterschule, a superior secondary school for girls who wanted to be something more than house-

At the end of the war, Maria began attending the Höhere Töchterschule, a superior secondary school for girls who wanted to be something more than housewives. Her best subjects were languages and mathematics. In 1921 she left to enrol in the *Frauenstudium*, a private school run by suffragists, where students spent three years preparing to earn their *Abitur*, which gave them admission to the university system.

wives. Her best subjects were languages and mathematics. In 1921 she left to enrol in the Frauenstudium, a private school run by suffragists, where students spent three years preparing to earn their Abitur, which gave them admission to the university system. However, at the end of one year, the suffragists lost all their money and the little house where the school was located as a consequence of the dreadful inflation. And there were no other institutions like it in Göttingen. Her teachers recommended that Maria, who was then 16, attend a boys' school the following year, but she did not think much of the idea and said that she would study on her own and take the examination the next year. Her teachers warned her she would never pass, and then she wouldn't be able to go to university. She was not discouraged, though, and the next year she did indeed get herself admitted to the examination in Hannover. 'There were four or five of us [girls] from our little school', she told Dash, 'and there were about thirty boys… And of course we were much impressed, because the boys were so much older than we were, and they seemed so mature, and they were very worried.' The *Abitur*, a week of written tests and a day of oral examinations, asked questions about mathematics, French, English, German, physics, history and chemistry. Only one of the boys passed it, while all the young women passed. Now Maria could go to university.

Mathematics and Physics at the University of Göttingen

The University of Göttingen, where Maria Goeppert studied, as we shall see, is considered the prototype of the modern university, because it made no distinction in status between its School of Philosophy and its older Schools of Theology, Law, and Medicine (Germany, like other countries –including Spain– felt the School of Philosophy was the proper home for scientific studies). Berlin, whose university competed with Göttingen in mathematics and physics, adopted a similar stance, although much later. In 1801, after Napoleon had defeated Prussia and occupied part of its territory, Wilhelm II of Prussia charged educator and linguist Wilhelm von Humboldt (brother of the famous explorer and scientist Alexander von Humboldt) to found a modern university in Berlin (in 1949, when the university reopened its doors after the war and the establishment of the German Democratic Republic, it took the name of 'Humboldt-Universität' in its founder's honour).

Göttingen's great mathematics tradition began with Carl Friedrich Gauss (1777-1855), the 'prince of Mathematics', as he was called, author of memorable works, the foremost being *Disquisitiones Artithmeticae* (1801). Gauss studied, taught and worked in Göttingen for close to half a century. He was director of the Astronomical Observatory and professor from 1807 to his death. His immense body of work was not confined to pure mathematics; he also cultivated applied mathematics (astronomy, physics, geodesy). Like Goethe and Alexander von Humboldt, Gauss aspired to the ideal of universal knowledge, knowledge that knows no borders between disciplines like mathematics, astronomy and mechanics. One example of Gauss's versatility, his many-faceted knowledge, was his work with physicist Wilhelm Weber. Gauss and Weber laid a kilometre-and-a-half-long telegraph line in 1833, one of the first telegraph lines ever built in Germany. A monument depicting them and memorializing their partnership stands close to the Astronomical Observatory. It was sculpted by Carl Ferdinand Hartzer and was unveiled on 17 June 1899.

One of Gauss's successors at Göttingen, Felix Klein (who will soon pop up again in our story), wrote about Gauss in *Vorlesungen über die Entwicklung der Mathematik in 19. Jahrhundert* (*Lessons on the Development of Mathematics in the Nineteenth Century*, 1926), when dissecting the different types of researchers:

> There is the bold conqueror who works with lively intuition but a mishmash of concepts, who finds and unearths fresh treasures through instinct and sensitivity; and there is the careful administrator who organizes his gains, weighs each thing precisely, and is able to slot it into its place with the critical clarity and sureness of acute understanding. Only in a handful of minds do these opposing gifts come together; history then rightly assigns them to a singular position as lords and masters of their respective domains. Outside all times and above all opinions. Gauss must unreservedly be placed among these chosen few [...]. If I may draw the comparison, Gauss seems to me like the high summits in the panorama of our Bavarian mountains as seen from the north. The peaks that gradually rise from the east culminate in a gigantic colossus.

Gauss was succeeded by another mathematical luminary, Peter Gustav Lejeune-Dirichlet (1805-1859), who chose to leave Berlin for Göttingen. There he met two young men who left their own mark on mathematics, Bernhard Riemann (1826-1866) and Richard Dedekind (1831-1916), although Dedekind soon (in

1858) left to teach at the Federal Polytechnic School (ETH) in Zurich, a job he got on Dirichlet's recommendation. Riemann, on the other hand, went to Göttingen in 1846 and stayed there. In 1857 he was named extraordinarius (assistant) professor, and in 1859, ordinarius (full) professor. Two years after Riemann's early death, Alfred Clebsch (1833-1872) joined the corps of mathematicians at Göttingen. That same year he and Carl Neumann founded a new journal, *Die Mathematische Annalen*, with the intention of competing with the *Journal für die reine und angewandte Mathematik*, which largely showcased mathematicians from Berlin; the Journal, created in 1826, was better known as *Crelle's Journal* in honour of its founder, August Leopold Crelle, who was its editor until his death in 1855.

Unfortunately, Clebsch soon died, and his chair was briefly occupied by Lazarus Fuchs (1833-1902), who arrived in 1874 and left for Heidelberg the following year. Fuchs was replaced by Hermann Schwarz (1843-1921). Although Schwarz stayed longer, he did not make Göttingen his permanent academic home, either; he moved to the University of Berlin in 1892 as the successor of the great Karl Weierstrass.

Göttingen's great mathematical tradition was so drained by the many deaths, departures and arrivals that its glory days seemed to be waning. That is, until the arrival of Felix Klein (1849-1925) in 1886.

Klein's career in the professor's chair began in 1872 at the University of Erlangen. It was there, at his inaugural lecture, where he presented what is known as the 'Erlangen Programme', the thesis that there are as many geometries as there are transformation groups, that a geometry is characterized by its invariants. He later used this perspective to argue that the theory of special relativity was simply a kind of geometry (a geometry in accordance with Hermann Minkowski's presentation of the theory), and that the general theory of relativity was the geometry of the group of general transformations. In 1875 Klein left Erlangen for a chair at the Technical School of Munich, where he spent five years. In 1880 he moved on to a new chair, this time in Leipzig, which he left in 1886 for Göttingen.

His Munich years and his early Leipzig years were the finest of his mathematical career. In addition to pursuing his work in geometry, he devoted himself to an intense study of complex variable functions, developing the theory of a special kind of functions called 'automorphic functions'. In this field he encountered a young French mathematician, Henri Poincaré, who had first dipped into such problems while studying some of Lazarus Fuchs' work in differential equations. Poincaré had looked into some particular cases of automorphic functions, but the generalizations he introduced revealed the existence of hitherto-unknown functions, like zeta Fuchsian functions, which furthermore could be used, as he himself proved, to solve second-order linear differential equations with algebraic coefficients.

Klein entered a fierce mathematical competition with Poincaré, but his desire to reach results before his opponent eventually proved fatal. Poincaré was too powerful a rival as a mathematician; it has been said that the 19th century began in the shadow of a giant, Carl Friedrich Gauss, and ended with the rule of a genius of similar magnitude, Poincaré. In the opinion of Jean Dieudonné, himself a notable mathematician, 'Both were universal mathematicians in the supreme sense, and both made important contributions to astronomy and mathematical physics. If Poincaré's discoveries in number theory do not equal those of Gauss, his achievements in the theory of functions are at least on the same level even when one takes into account the theory of elliptic and modular functions, which must be credited to Gauss and which represents in that field his most important discovery, although it was not published during his lifetime. If Gauss was the initiator in the theory of differentiable manifolds, Poincaré played the same role in algebraic topology. Finally, Poincaré remains the most important figure in the theory of differential equations and the mathematician who after Newton did the most remarkable work in celestial mechanics.'

In his *Vorlesungen über die Entwicklung der Mathematik im 19. Jahrhundert, Klein* acknowledged the consequences of his rivalry with Poincaré:

> The price I had to pay for my work was otherwise extraordinarily high, namely, my health broke down completely. The next year I had to take holiday after holiday and give up all productive work. I was unable to go on until the autumn of 1884, but never again have I reached the same degree of productivity. Instead I devoted myself to elaborating on my previous ideas and later, in Göttingen, I expanded my field of work and went about the general business of organizing our science. So it is understandable that from then on I have only touched on automorphic functions sporadically. My actual productive activity in the realm of theoretical mathematics went to pieces in 1882.

Klein's ability as an organizer came into full bloom in Göttingen, especially after the departure of Hermann Schwartz and Ernst Schering, the other two full professors of mathematics. 'When Schwartz left for Berlin in 1892', said Richard Courant (another distinguished member of the Göttingen mathematics community) in the obituary he wrote on Klein's death, 'giving Klein a free hand in Göttingen, a new period of activity began for Klein, in which he became more and more prominently involved in organizational work'.

He also shone as a teacher. Max Born recalled in his memoirs that while 'Hilbert was like a mountain guide who leads the straightest and best way to the summit, Felix Klein was more like a prince who wants to show his admirers the greatness of his territory'. However, what Born wanted was 'to reach the summit quickly'. One particularly fine outcome of Klein's organizational activities was that he managed to get a full professorship at Göttingen offered to David Hilbert (1862-1943) in 1895. In fact, Klein had been trying to snag Hilbert (whom Klein looked on as the great promise of German mathematics) for Göttingen ever since Schwartz gave up his chair in 1892, but to no success, and in the end the position went to Heinrich Weber. By the time Weber decided to take up an offer from Strasbourg in 1895, Klein had gained much more clout at the university and got his way relatively easily. Hilbert moved to Göttingen, where he remained the rest of his life, bringing a mathematical prestige and strength to the university that probably none of his contemporaries could have commanded.

In 1902 Ferdinand Frobenius and Schwarz offered Hilbert the chair left empty in Berlin by Fuchs's death. No mathematician had ever rejected an offer from the Prussian capital before, but Hilbert did, though he did get something out of the deal: the director of university affairs at the German Ministry of Education, Friedrich Althoff, who controlled practically all business involving German university chairs, agreed to the creation of a new chair of mathematics at Göttingen for a close friend of Hilbert's, Hermann Minkowski (1864-1909). Minkowski was an extraordinary mathematician then at the Federal Polytechnic School of Zurich, where not long before he had taught a young student by the name of Albert Einstein; in time Minkowski would give Einstein's theory of special relativity its canonical four-dimensional presentation (when Minkowski died, Edmund Landau took his place). From then on, exact sciences at Göttingen went from strength to strength, always under the helmsmanship of Klein. In 1904 Carl Runge (1856-1927) was appointed to the new chair in applied mathematics. Ludwig Prandtl (1875-1953), the great rising star of aerodynamics, arrived at the same time as Runge. Together Runge and Prandtl directed the new Institute of Applied Mathematics and Mechanics. The Institute of Geophysics, directed by Emil Wieckert (1861-1928), and the Institute of Electrical Engineering, headed by Hermann Theodor Simon (1870-1918), were also founded at Göttingen under Klein's influence. Another distinguished scientist at Göttingen was the astronomer Karl Schwarzschild (1873-1916), director of the astronomical observatory that Gauss had directed.

In chapter 1 I alluded to Hilbert's interest in theoretical physics, as attested by the fact that he chose physicists as his assistants. One of his assistants was Max Born, who, as we also saw in chapter 1, later had a great influence on Maria Goeppert. Others of Hilbert's assistants included Lothar Nordheim, a former pupil of Born's, and the great mathematician John von Neumann, the author of important contributions to mathematical physics, who had just arrived in Göttingen. During the winter semester of the 1926-1927 school year, Hilbert gave a course consisting in two two-hour classes a week entitled 'Mathematische Methoden der Quantentheorie'. One outcome of the course was an article on quantum mechanics signed by Hilbert, von Neumann and Nordheim, in which they tried to solve a problem related with transformation theory as introduced by Paul Dirac (the problem basically concerned the equivalence between the formulations –'representations'– of quantum mechanics produced by Heisenberg and Schrödinger).

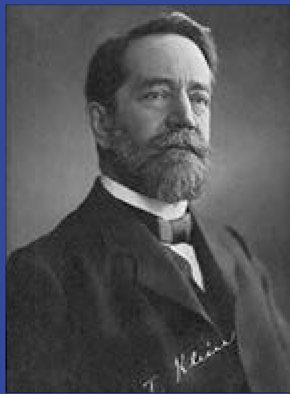
Klein himself was basically a 'pure' mathematician, and it was no mere coincidence that he purposefully favoured applied mathematics. Quite the contrary. He believed the association between basic and applied mathematics would work to the benefit of both. And he made sound moves putting his ideas into practice. The Göttinger Vereinigung zur Forderung der angewandten Physik und Mathematik (Göttingen Association for the Development of Applied Mathematics and Physics) was established in 1898 mainly due to him. Rather than promote specific research projects supporting individual scientists, the association strove to create and maintain institutes to research and teach certain subjects. Although the association came to have almost 50 industrialists as members, the soul of the organization (apart from Klein) was a chemist, Henry Bottinger, general manager of the Bayer company; when he died in June 1920, the Göttinger Vereinigung eventually disbanded.

We can get a good idea of the kind of development mathematics underwent in Göttingen from the fact that between 1890 and 1914 Göttingen certified 18 pure or applied mathematicians (some physicists and engineers with mathematical skills can be included in this category in this case) as *Privatdozenten* (doctors entitled to teach enrolled students university courses for a fee). These included names such as Hermann Weyl (Hilbert's disciple, who took Hilbert's chair when Hilbert retired in 1930), Arnold Sommerfeld, Constantin Carathéodory, Gustav Herglotz, Max Born, Richard Courant (who in 1922 founded Göttingen's Institute of Mathematics, which later became famous; he directed the Institute until Hitler's rise to power forced him to emigrate to New York, where he created an institute of applied mathematics), Theodore von Kármán, Otto Blumenthal and Ernst Zermelo. In contrast, from 1897 to 1901 no *Privatdozenten* were certified in Berlin.

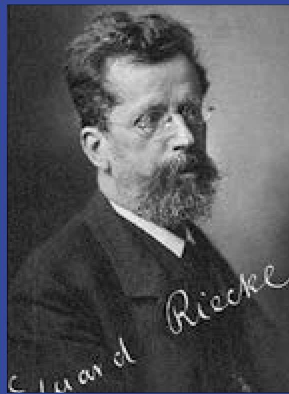
Most of Göttingen's *Privatdozenten* had more to do with Hilbert than with Klein, but even though Klein was devoted mostly to educational and organizational duties, he also spent time with the *Privatdozenten*. For example, Klein and Arnold Sommerfeld, whom we met in the previous chapter, future leader of a magnificent school of physics in Munich, prepared a singular, extensive body of work on the theory of the top, *Über die Theorie des Kreisels* (four volumes, 1897-1912).

The changes were more sudden in physics than in mathematics, among other reasons because physics experienced much more of a spurt of radical changes than mathematics. Things especially happened between 1914 and 1921. At the end of the 19th century, the Göttingen physics world was ruled by Eduard Riecke

Hermann Minkowski



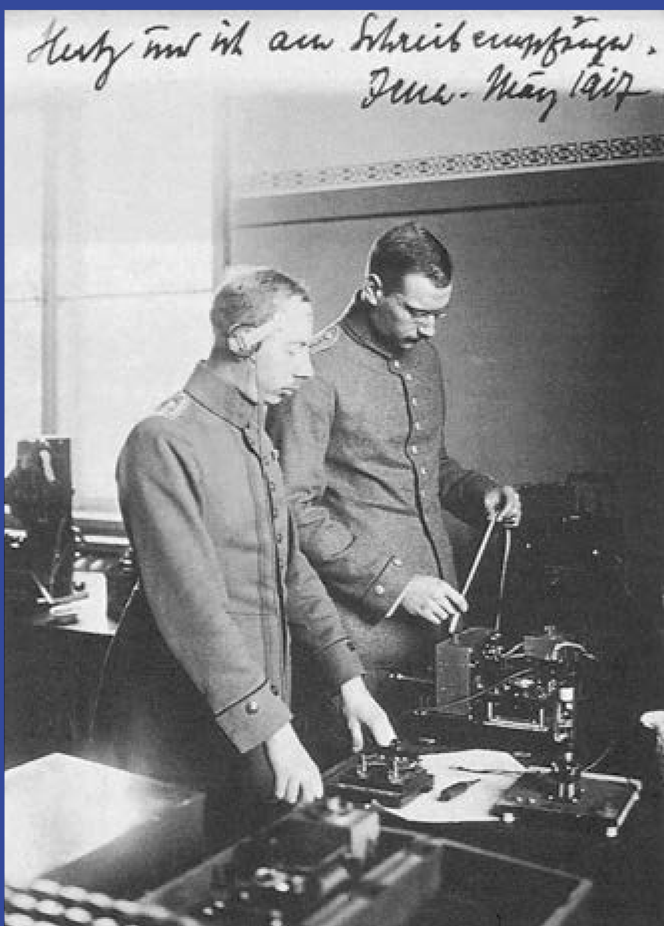
Felix Klein



Eduard Riecke

David Hilbert

Woldemart Voigt



Gustav Hertz and Walther Gerlach serving in the army during World War I (Jena, 1917)

(1845-1915) and Woldemar Voigt (1850-1919). Both received professorships: in 1882 Riecke took a chair in experimental physics, and in 1883 Voigt did likewise in theoretical physics (or rather mathematical physics, since the term 'theoretical physics' had not yet been established). Although they shared some interests, Riecke focused mostly on the corpuscular structure of electricity, Geissler tubes and conductivity theory, while Voigt (probably the most productive German theoretical physicist of the 1880s) worked with the properties of crystals and the interaction between crystals and light. In 1905 the facilities at their disposal were greatly improved when a new building was opened at the Institute of Physics, which they shared. Riecke was supposed to retire in 1915, and, although Voigt was five years his junior, he hoped to retire at the same time. The war forced them to change their plans and remain at their jobs at first, but in 1915 Riecke died. Voigt stayed on in the chair, but by then another, younger, very eminent physicist had come onto the scene, Peter Debye (1884-1966), the Dutchman who in 1936 would win the Nobel Prize in Chemistry for his work on molecular structures. Debye was one of the few scientists grounded in both theoretical and experimental physics (and physical chemistry), so he was an ideal candidate to direct the Göttingen Institute of Physics by himself. Riecke and Voigt agreed. Furthermore, Debye also had the support of the influential Hilbert, who was always interested in physics. In September 1914 Debye, who had been an extraordinarius professor at the University of Zurich since 1911, accepted an appointment as a 'personal' ordinarius professor, with the 'personal' tag scheduled to be eliminated when the two Institute of Physics directorships were melded into one in 1916. Then Debye had the backing of Robert Pohl (1884-1976), who held the post of extraordinarius professor (he was promoted to ordinarius professor in 1920).

Debye did not linger long at Göttingen, though. In 1919 the Federal Institute of Technology in Zurich offered him a chair. Göttingen tried to keep him. They called on the ministry for help, arguing that losing Debye would make Prussia look inferior to Switzerland (after all, in 1914 it was Göttingen that had persuaded Debye to leave the University of Zurich). But the chances of holding onto him were slim, given Germany's rocky post-war socio-political and economic situation. And Debye did take up Zurich's offer. But what looked like a tragedy for Göttingen physics turned out to be just the opposite, because Debye's replacement was, as we saw in chapter 1, Max Born; and he was joined by James Franck, as likewise explained in chapter 1. One took charge of theoretical physics and mathematics, and the other teamed up with Pohl to run the experimental physics side. Three institutes (Experimental Physics I, Experimental Physics II and Mathematical Physics), all sharing the same building. Quantum physics found a powerful home in Göttingen under the management of Born and Franck, who were stimulating leaders and attracted young scientists (Pohl, who dealt especially with teaching the younger undergraduates and wrote some introductory physics texts, had his own circle of followers, although they were a different set).

James Franck

Alongside Max Born, James Franck was another important person in Maria Goeppert's scientific career, so his story must be told.

Franck belonged to a family of Jewish bankers, and at his family's urging he started studying law and economics in Heidelberg. Soon, however, his interests strayed toward natural science, and he started sprinkling geology and chemistry classes in among his law classes. In 1902 he met Max Born, who was then a young student like him. Born, who became a lifelong friend, convinced Franck to study physics. Franck left Heidelberg for the University of Berlin, where he earned his doctorate in 1906 and completed his habilitation (enabling him to teach as *Privatdozent*) in 1911. In Berlin in 1914 Franck and Gustav Hertz conducted one of the crucial experiments of what can be called the 'first stage' of quantum physics. Their research looked at the interaction among electrons and atoms of a mercury gas inside a tube. The electrons in question came from a hot metal wire and were accelerated by an electrical field, so their energy could be measured. Franck and Hertz thus successfully proved that mercury atoms could only absorb certain quantities of energy that were very consistent with the Planck-Einstein light quanta, confirming the structure of the model of the atom according to Bohr. For this work Franck and Hertz received the 1925 Nobel Prize in Physics.

The war interrupted Franck's research, as it did that of other scientists, including Born. Franck volunteered for military service, but he did not remain in the army long; he soon returned from the Russian front with a severe case of dysentery. He resumed his scientific career in February 1916 with a professorship at the University of Berlin. But when Fritz Haber started preparing toxic gases at the Kaiser Wilhelm Society (Kaiser-Wilhelm-Gesellschaft) Institute of Chemistry and Physics for use in the war, Franck, together with other leading scientists like Otto Hahn, Gustav Hertz and Hans Geiger, was commissioned to test gas masks and filter types. Three days after Kaiser Wilhelm's abdication, on 28 November 1918, Franck received his discharge from the army.

Since the Treaty of Versailles forbade Germany from developing new weapons, Haber decided to overhaul the Institute of Chemistry, Physics and Electrochemistry from top to bottom, creating some new divisions. One of them was tasked with doing research into ionizing and exciting atoms and molecules, and in January 1919 Franck was named division director under a five-year contract. But, as we said, he did not stay long, because in 1921 he joined Born in Göttingen.

Of all Franck's personal traits, two really stood out. First, there was his extraordinary focus, his personal method as an experimental physicist. Peter Pringsheim, an expert in luminescence (fluorescence and phosphorescence) who knew Franck from his Berlin days, described this feature as follows: 'Once he has a problem firmly in his sights, then he mulls it over incessantly. You get the impression that he even dreams about it. He likes talking it over with his students, with his colleagues, with everyone, but not so much to convince them as to perceive the problem more clearly himself. And then he finally figures out a solution that appears so simple once he has formulated it that one wonders why it hasn't always been clear to everyone.' His second trait was his accessibility. In the words of one of his doctoral students, Heinz Maier-Leibnitz, 'James Franck scheduled his day well: he always found time for conversations; his interest in fellow humans was absolutely stunning [...]. Franck was the source of the institute's good spirit.'

Victor Weisskopf (1908-2002) described Franck similarly, but with more details. Weisskopf was an Austrian physicist who led a brilliant career, first in Germany and later, over a much longer period, in the United States. He also rose to the position of Director-General of CERN, the European high-energy physics laboratory in Geneva. In his memoirs Weisskopf recalled the atmosphere that reigned in Göttingen in those days and the personality of some of its leading figures, particularly Born and Franck.

> When I arrived in Göttingen in 1928, I found that while the regime was much less strict than in Munich [where he had studied with Sommerfeld], it had other advantages. The ranking professor of theoretical physics was Max Born, one of the leading contributors to the development of quantum mechanics. He kept somewhat aloof from his students and had a rather formal approach to physics. When lecturing, he tended to express everything in complex mathematical terms. While I preferred a less formalistic approach, I was nonetheless awed by his presence. I knew he was a great man, and despite all our differences, I wanted to emulate him. In fact, he was very friendly and encouraging to me. But when I came in contact with the new quantum mechanics, I felt that it was an esoteric theory far removed from ordinary human experience. I was still very young and idealistic, and my involvement with human affairs and social issues had become very important to me [...].
>
> In spite of several positive encounters with Born, my scientific collaboration with him was not as close as I had expected. He was, by nature, a man who kept to himself under ordinary circumstances. Shortly before my arrival in Göttingen, he had suffered a stroke and was even less approachable. Still, we had some contact. One day Born called me to his office and told me he was preparing a textbook on optics. He knew that I had developed an interest in the behavior of

light in crystals. He said he hadn't yet thought about the chapter on that subject and needed my help. I was to read the literature and to sketch out a synopsis of what I thought should be in the chapter. I wrote it up, but the result was so unlike Born's style that he couldn't include it in the book without making a great many changes. When the book was finally published [Optik: Ein Lehrbuch der elektromagnetische Lichttheorie, 1933], I was unable to recognize the chapter as something I had written.

The two senior professors in experimental physics were James Franck and Robert Pohl. I was especially attracted to Franck, who had won the Nobel Prize in 1925 for demonstrating that atoms in collision gain or lose energy in quantum steps. In contrast to the aloof Born, Franck was warm and very personable and loved interacting with his students, who were all impressed by him. He also had a group of lively young assistants who were very accessible to us and deeply devoted to their boss. It was a great pleasure to discuss physics with Franck. I was impressed by his intuitive understanding of science, his uncanny feeling for physics, and his marvelous way of working. He seemed able to predict with great accuracy the outcome of an experiment or the result of a theoretical calculation even when he was not well acquainted with the mathematical methods involved. Because of this we said he had a direct wire to God [...].

In addition to the 'great men' there were many younger teachers at Göttingen who were at the level of assistant professors in the United States. We learned a great deal from the junior faculty because they were much more accessible and we also saw them socially. I remember in particular Walter Heitler, Lothar Nordheim and Gerhard Herzberg, who later settled in Canada and won the Nobel Prize for his work in molecular physics. Herzberg taught a course called 'Introduction to Quantum Mechanics'. This field was then so new that it was not yet part of most physics curricula, but Herzberg was young and eager to teach the latest developments. For me, Herzberg's course proved a great boon, because it helped me greatly to get acquainted with the new physics.

With Born, Franck and Pohl, plus the youngsters who clustered around them, physics in Göttingen flourished. In the summer 1932 semester, for example, 206 of the 3,662 students at the University of Göttingen, that is, 5.6%, were studying physics, while in all Germany there were 2,299 physics students out of a total of 126,381 university students, making up 1.8% of the general student population.

It was at this prestigious, revitalized university that Maria Goeppert began to study physics in 1923. But before discussing her university years, I would like to take a brief look at the situation for women in general, and particularly women in Germany, who wanted a higher education.

The recurring topics of women's emancipation and full equality of rights for both sexes began moving into the foreground in the 19th century, and they remain in the public eye now, with good reason.

Women's Emancipation

The recurring topics of women's emancipation and full equality of rights for both sexes began moving into the foreground in the 19th century, and they remain in the public eye now, with good reason. The situation dates back to long ago. Some Enlightenment thinkers looked hard at women's role in society. Such was the case of Theodor Gottlieb von Hippel (1741-1796), a highly influential member of well-to-do society in Königsberg, whose mayor he became (Königsberg has been Kaliningrad since 1945). In 1793 von Hippel published a book, *Über die bürgerliche Verbesserung der Weiber* (*On the Civil Betterment of Women*), in which he maintained that women's talent was the equal of men's and that the huge difference between the two sexes' contributions was due to the fact that women's intelligence and culture had been neglected, if not deliberately repressed. At the dawn of the French Revolution, Jean Antoine Marie Nicolas Caritat (1743-1794), Marquis of Condorcet, argued vigorously in defence of women's rights. If rights are natural, he held, they cannot be denied to people who share the same nature as others, nothing more or less than half the human race. 'Either no individual of the human race has any true rights, or they all have the same', he wrote in an essay entitled 'Sur l'admission des femmes au droit de cité' ('On Women's Admission to the Right of Citizenship', 1790). And he added, 'Rejecting women's political rights would lead us to the absurd situation of allowing women heads of state, but not women voters or elected women public officials (because women's political rights must, of course, include the right to run for office). Otherwise, to oppose political rights for women out of fear that public duties will pull women away from their family and their home would lead us to rule out everyone who has a useful occupation for the same reason: workers, artisans, etc., whereupon the National Assembly would represent only a moneyed, leisured aristocracy. And to say that women cannot hold office because they are exposed to pregnancies and passing indispositions would mean barring from public office all men who catch cold easily and become gouty in the winter'.

In the end, however, the French Revolution, that beautiful, decisive movement meant to shape the history of humanity, the revolution whose motto was

'Liberty, Equality and Fraternity', failed to uphold its principles as far as women were concerned. Von Hippel, for instance, decried that the revolution had done nothing for women's equality. 'How can a people that exists by and for the fair sex leave an entire gender out of its globally famous Equality? … The new constitution deserves a repeat of my reproaches, because it allows an entire half of the nation to be ignored […]. All human beings have the same rights. All the French men and women must be free and must be citizens'.

Nineteenth-century liberalism was certainly less flamboyant than the brilliant, dramatic French Revolution, but it ultimately did more to sway the direction of social life in some of the more-advanced societies, including a strong influence on the feminist question. The classic declaration of the liberal credo's applicability to women was put into words by perhaps the foremost of the liberalist theorists, John Stuart Mill (1806-1873). Feminists took his essay 'The Subjection of Women', written in 1861 and published in 1869, as their bible. The opening paragraphs of Mill's book sounded in their ears for many a long year:

> The object of this Essay is to explain as clearly as I am able, the grounds of an opinion which I have held from the very earliest period when I had formed any opinions at all on social or political matters, and which, instead of being weakened or modified, has been constantly growing stronger by the progress of reflection and the experience of life: That the principle which regulates the existing social relations between the two sexes –the legal subordination of one sex to the other– is wrong in itself, and now one of the chief hindrances to human improvement; and that it ought to be replaced by a principle of perfect equality, admitting no power or privilege on the one side, nor disability on the other.

Naturally, progress in securing greater rights for women varied from one nation to another. Let us see what happened in Germany, Maria Goeppert's homeland.

Women's Access to Education in Germany

While the German middle class was large, it appeared to be satisfied with the state of things in the decade of 1871, the year of Germany's unification under the leadership of Otto von Bismarck. The German Empire had grown more solid. Power lay in the hands of the military aristocracy, a small elite of big industrialists and the emperor. The progressive demands of the revolution of 1848 had slowly given ground until they had ebbed almost to nothing. How did this happen? The case of the feminist movement is a good example. In 1848 Louise Otto-Peters (1819-1895), who eventually headed the Allgemeiner Deutscher Frauenverein (General Association of German Women), publicly called for the equality of the sexes, including votes for women. In 1865 the association demanded full equality in education, including the admission of girls to boys' primary schools (which the authorities considered immoral). Ten years later, all the association asked for was a proper women's education for motherhood. Not until the early 20th century did a more demanding German feminism emerge, and one of its main objectives was women's suffrage.

It was only shortly before the outbreak of World War I that a handful of women students began appearing regularly in German university classrooms. For years women could only attend university if they had special permission, which depended mostly on *Herr Professor*'s personal views. Prussia, the Empire's foremost state, only decided to allow women at its universities in 1908. The year before that, there were just over 300 women at non-Prussian universities, but once Prussia lifted its veto, the numbers grew rapidly. By 1909 German universities had 1,400 women students; in 1911, 2,500; and in 1921, 8,300.

Nor did it help that there were very few schools where girls could prepare for university entrance examinations (as boys did at the *Gymnasium*). On 18 August 1908, a Prussian law for 'the reform of women's secondary schools' was passed, establishing a ten-year programme of studies, albeit still a programme separate from men's studies. As of their seventh or eighth year of secondary school, young women had the option to transfer to a *Studiennanstalt* (institute of higher studies), from which, after a grand total of thirteen years of schooling, they could take the *Abitur* for admission to the university system. In 1909, of the 309 girls' schools in Prussia, only 22 offered this option; in 1910 there were 27; and in 1911, 33. Furthermore, women were not at first granted access to all university schools. In 1911 they could only obtain degrees under the same conditions as men at schools of law, medicine and philosophy. Schools of theology, for instance, remained off limits. All in all that made 8,600 university-level institutions in 1926 and 21,200 in 1931, which means that the nineteen twenties were a decisive decade for women students joining the German university world.

In Prussia women could only gain access to the habilitation (the paper and examination that had to be passed before one could teach at university and qualify as a Dozent) as of 1920. In other *Lander* women received habilitation rights in 1918 and 1919. Among the first women to benefit from these rights were physicist Lise Meitner and mathematician Emmy Noether.

When the Nazis came to power, they made it one of their objectives to organize schools and universities. For the first and only time in its history, Germany had a centralized administration for its education system. One of the points on the Nazi programme was to try and limit the percentage of women to 10% of the total student body. In 1926 9% of German university students were women; in 1931 the percentage rose to 16%. In 1938 it declined to 11%, rising again to 16% in 1951 and 22% in 1960.

Women Scientists at Göttingen

Despite the obstacles in women's path to a higher education, some women nonetheless left their mark on physics and mathematics. Lise Meitner will appear later on, in another chapter. Meitner, an Austrian, worked in Berlin until Germany annexed Austria and she had to leave due to her Jewish roots. Marietta Blau was Austrian, too; Blau was a notable specialist in developing photographic techniques (emulsions) for studying radioactive phenomena and cosmic rays (Schrödinger and Hans Thirring later proposed her for the Nobel Prize in Physics). Blau's life story –like that of Hertha Sponer, Charlotte Riefenstahl-Houtermans and Emmy Noether– was terribly complicated. Except for Blau, who spent only a year in Göttingen, they were all part of the 'Göttingen circle'. For that reason and because they proved their mettle as scientists, their stories should be told.

Marietta Blau

Marietta Blau (1894-1970) studied physics in the city of her birth, Vienna. She enrolled in university there in 1914. Her doctoral thesis, which concerned a point of radiology, gamma ray absorption, was judged in March 1919 by two of the foremost Vienna physicists of that day, Franz Exner and Stefan Meyer. In the latter half of 1921, after a serious illness, Blau started work as a physicist at Fürstenau, Eppens & Co., a Berlin firm that made X-ray tubes. She left the company in January 1922 to work at the Frankfurt Institute for the Physical Bases of Medicine. Later she joined the famous Radium Institute of the Austrian Academy of Sciences in Vienna, directed by Stefan Meyer. She was not the only woman doing research there; other women scientists included Elisabeth Rona, Hertha Wambacher and Berta Karlik (some, Blau herself among them, belonged to the Verband der Akademikerinnen Österreichs, the Austrian Association of University Women). She remained at the Radium Institute until 1938, although she spent nearly the entire 1932-1933 academic year in Göttingen, funded by the Verband, working with Robert Pohl on the physics of crystals. In a letter she wrote to Berta Karlik on 22 October 1932, she referred to Pohl and the Göttingen bureaucracy in these terms: 'This is a dreadfully bureaucratic system here; a different person is in charge of each little trifle. Tomorrow I will –with trembling heart– have my first meeting with Pohl. If you are used to Meyer's friendly greetings in the morning, you cannot help considering Pohl's monarchical nodding rather strange. I would like to get in touch with Franck and Born, but I don't know how I am going to manage.'

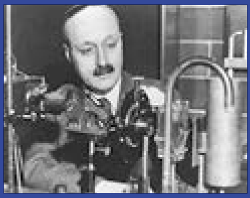
After Göttingen, in April 1933 Blau went to Paris to work for a few months at the Radium Institute directed by Marie Curie, continuing the research she had begun in Vienna, looking into neutron emissions caused by alpha particles in beryllium. She intended to return to Göttingen, but the political situation in Germany (Hitler's rise to power on 30 January 1993 and the almost immediate onslaught of repression against Jews) made her change her mind and go back to Vienna. It was in that period, in 1937, when Blau and Hertha Wambacher made their most important discovery, the 'disintegration stars' (*Zerntrümmerungssterne*) that they detected on photographic plates exposed to cosmic radiation at Hafelekar Observatory, a facility located at an altitude of 2,300 metres. The 'stars' in question were the tracks made by the nuclear reactions of cosmic rays with the nuclei of the photographic emulsion, and they could be used to identify the elementary particles involved. But on 13 March 1938 the anti-Jew plague reached Austria with the 'Annexation' (*Anschluss*), which was, let us not forget, approved by 99.73% of Austrian voters in a referendum on 10 April. And the Annexation added around 200,000 more Jews to the Nazi empire.

Marietta Blau left Vienna a day before the *Anschluss*. From Oslo, where she settled, she wrote on 21 March to her fellow Viennese physicist Friedrich Paneth, who was giving a series of lectures in England when the Annexation took place and decided then and there not to return to Austria, settling at Imperial College, London, instead: 'I left Vienna on March 12 [1938] at seven o'clock in the evening, and I was not really clear about the political situation. I should have left at the beginning of March but postponed my departure several times and perhaps was the last Austrian to pass the German border. In Vienna, we did not know what lay ahead until the last moment, and it was only on my trip that I met the German troops and realized that all hope was gone.'

Her position in Oslo was precarious, so, on Einstein's recommendation, she obtained a post at the National Polytechnic Institute in Mexico City, at its School of Mechanical and Electrical Engineering. She arrived in November 1938. Research conditions there were sketchy to non-existent, but even so she published seven articles during her five years in Mexico. She finally got permission to emigrate to the United States in 1944. In May of that year she arrived in New York and got a job in the research department of International Rare Metals Refinery Inc. She spent two years there, but after 1947 she had to leave New York because the company merged with the Gibbs Manufacturing and Research Corporation, whose headquarters were not in Manhattan, but in Janesville, Wisconsin, a city of 25,000 inhabitants. She did not like moving there at all. Luckily for Blau, on 1 January 1948 she joined Columbia University in New York on a two-year contract. She was the only woman scientist there, and her work consisted in researching and improving photographic emulsions for the study of elementary particles. When her contract at Columbia expired, she moved on to the Atomic Energy Commission's Brookhaven National Laboratory, where she finally had cutting-edge materials at her disposal. She was unsatisfied at Brookhaven as well, however, and in 1956 she joined the University of Miami. Eventually, in spring 1960 she returned

Louise Otto Peters

Emmy Noether



Fritz Houtermans and Charlotte Riefenstahl

Hertha Sponer in her laboratory.
Above, James Franck

Marietta Blau

to Vienna and the Radium Institute she missed so much, thus putting an end to her time as a wandering scientist. She died on 27 January 1970. In 1962 the Austrian Academy of Sciences conferred their Schrödinger Prize on Blau.

Hertha Sponer

Hertha Sponer (1895-1968), whose parents were Protestants, really was a proper member of the 'Göttingen circle' and, like most of the leading protagonists of this chapter, she too ended up settling in the United States. In addition, after James Franck was widowed (he lived in America by then), Sponer, who had been his assistant and a close family friend, married him.

In a resumé she prepared in 1920 for the formalities associated with the defence of her doctoral thesis, Sponer summed up her education so far: 'From Easter 1901 until Pentecost 1907, I attended schools in Neisse and Zittau. I was then given private instruction until Easter 1910 at the boarding school in Zittau. From autumn 1910 until Easter 1912, I took preparatory courses for women from Miss Strinz in Berlin in order to enter the eleventh grade in Zittau's Secondary School of Practical Sciences (*Realschule*), which I left after three months. I then attended governess's school in Hanover and Heidelberg, took the examination in September of 1913 and was employed as a governess for the next two years. During the war, from November 1915 until July 1916, I accepted a substitute teaching position at an elementary school'.

It was then, in 1916, in all the chaos of war, when Hertha left her job as a substitute teacher and enrolled in a secondary school in Breslau to prepare for the *Abitur*. This was not a girls' school; in fact, she had only one female classmate. And she passed the test, one of the 570 girls who passed the *Abitur* in Prussia in March 1917.

She knew exactly what area of studies she wanted to pursue: physics. She chose the University of Tübingen, one of the first German universities to accept women (although a national law allowed universities to do so since 1904, the law was only implemented in 1907; before that women could attend only as 'guest auditors'). At Tübingen she mastered all the mathematics on offer. There were no theoretical physics classes, but experimental physics were given, by Friedrich Paschen. Sponer benefited from what he had to teach. Nevertheless, as this kind of subject was taught only in the first year and there were no theoretical physics courses, after her first year Hertha transferred to the University of Göttingen, where the proportion of women students was slightly lower than in the rest of Germany (in 1923, 9.4% as opposed to 10.2%). There, starting in the 1918-1919 school year, she was able to learn from Peter Debye, Robert Pohl (a known opponent of allowing women at university) and Woldemar Voigt. After four semesters at Göttingen, Sponer graduated with a dissertation entitled 'Über ultrarote Absorption zweiatomiker Gase' ('On the Infrared Absorption of Diatomic Gases'), directed by Debye, who left Göttingen for Zurich shortly afterward.

With her successful dissertation under her arm, in March 1920 Sponer moved to Berlin, a city swirling with the turbulence of the Weimar Republic. There she worked until spring 1921. She spent her first few months at the Institute of Chemistry and Physics Fritz Haber directed at the Kaiser Wilhelm Society, and in October she switched to the university's Physics Institute. She did not earn a salary at either institute. And at the Physics Institute she met James Franck, the scientist, the man, to whom she was linked for most of the rest of her life, first as a colleague and much later, as I said, as wife.

In Berlin she spent her time studying problems concerning electron collisions. This was the subject of her first publication, which appeared in *Zeitschrift für Physik* (1921), 'On the Frequency of Inelastic Collisions of Electrons with Mercury Atoms'. The paper was related with the work Franck and Hertz had done in 1914. In fact, in the article's introduction Sponer wrote, 'Upon the suggestion of Professor Franck, the yield of inelastic collisions and, to the extent possible, the dependence of the yield on the [electron] velocity was determined in this study'. Sponer's familiarity with Franck is well summarized by a photograph taken during the going-away party thrown when Franck decided to leave Berlin for Göttingen. The picture shows, from left to right: seated, Hertha Sponer, Albert Einstein, Ingrid Franck (James' wife), Lise Meitner, Fritz Haber and Otto Hahn; standing, Walter Grotian, Wilhelm Westphal, Otto von Baeyer, Peter Pringsheim and Gustav Hertz.

One of the perks offered to tempt Franck into settling at Göttingen (in November 1920) was to allow him two assistants. One of the positions went to Hertha Sponer. Her job was temporary at first, but in October 1921 it became a stable post, though it did have to be renewed every two years. Her main function was to take charge of experimental teaching work in the laboratories. And she seems to have been rather strict. Victor Weisskopf said in his memoirs, 'A small incident may have been decisive [in choosing theoretical over experimental physics]. I took a laboratory course in which students were supposed to perform experiments. My immediate supervisor was Hertha Sponer, a rather abrasive woman. One day I had the misfortune of breaking an important part of an instrument, and Miss Sponer said: "You will never become an experimenter." Although her judgment seemed hasty, I took her word for it at the time, and her prediction proved true'.

Sponer continued her research in the field she had taken up in Berlin. Her first article was followed by another 25 papers by 1933, only two of them in partnership with Franck. The second joint paper (1932) also included another famous physicist, Edward Teller, who, as we shall see, was a great friend of Maria Goeppert Mayer. His field was mainly molecular physics.

In October 1922 Sponer gained her habilitation, i.e., *venia legendi*, something that women were only allowed as of 1919. Between 1908 and 1933, 10,595 women took their doctoral degree in Germany. Only 54 of them went on to earn their habilitation (14 in medicine, mathematics and natural sciences). Of these only 24 eventually won the title of 'professor', although only two, botanist Margarethe von Wrangell and sociologist Mathilde Vaerting, attained the senior rank of ordinarius professor (equivalent to a chaired professor) and directed institutes. One of the other 22 was Hertha Sponer, who received her appointment in January 1932; to keep such a position, one usually had to stay in *Herr Professor*'s good graces.

In 1925 Sponer won a Rockefeller Foundation fellowship to spend the 1925-1926 academic year at the Department of Physics of the University of California, Berkeley. There she worked with Raymond T. Birge, with whom she published a paper in the United States' foremost physics journal, *Physical Review*, entitled 'The Heat of Dissociation of Non-Polar Molecules' (1926).

Back in Göttingen, on 1 October 1930 she became head assistant, a position she was supposed to keep for the next five years. But two years later, on 26 October 1932, she was named professor without a chair (*ausserordentlicher Professor*), the second woman appointed to a professorship in the Weimar Republic after Lise Meitner in Berlin, although not at the rank of chaired professor (no women were chaired professors in Prussia at the time).

Sponer stayed in Göttingen until the fateful year of 1933, when Hitler succeeded at forming a conservative-backed government. This was on 30 January. Hitler did not take long –just two months– to start implementing his racial ideology. On 31 March Jewish judges were removed from their duties in Prussia, specifically because they were Jews. A week later, on 7 April, the ominous 'Law for the restauration of the professional civil service' (*Gesetz zur Wiederherstellung des Berufsbeamtentums*) was enacted, designed as a de-facto purge of all ranks of civil servants, including university teachers, of course.

In other words, this law meant that civil servants who had secured their positions during the Weimar Republic and were not of Aryan descent, or whose political activities did not guarantee that they would serve the new regime wholeheartedly, had to leave their jobs. In theory non-Aryans who had entered the civil service before the start of World War I, had fought at the front during the war, or whose fathers or sons had died in action could keep their jobs. In practice, however, they too were quickly thrown out. The theoretical exceptions were eliminated anyway on 15 September 1935 when the decrees known as the 'Nuremberg

Laws' were made; the Nuremberg Laws, officially labelled as 'nationality' laws, stated that Jews were subjects with no rights.

Many Germans of Jewish origin must have reacted to the news just as Victor Klemperer did. Klemperer, a veteran of the Great War, a man of letters and a refined historian, wrote in his diary (*Ich will Zeugnis ablegen bis zum letzten. Tagebücher 1933–1945*; 1995) on 10 April about 'The miserable feeling of "Thank God, I am alive". As a war veteran I am left in my post by the new Civil Service "law", at least for now [...]. But everywhere there is tumult, poverty, fear and trembling'.

Some were not so fearful or 'prudent'. The first scientist to react openly to the affront the new regulations clearly represented was James Franck. Ten days after the decree, on 17 April 1933, he submitted his resignation to the Prussian minister for Science, Arts and Culture. In fact, he could have legally qualified for an exemption, since, as I said before, he had served his country during the Great War. In the 18 April issue, the *Göttinger Zeitung* gave the news including an interview with Franck and some passages from the cover letter he sent the minister with his resignation:

> I have applied to my superior to be allowed to leave my post. I shall try to continue working in science in Germany.
>
> We Germans of Jewish origin are being treated like foreigners and like enemies of the Fatherland. Our children are expected to grow up knowing that they will never be allowed to prove that they are good Germans.
>
> All those who were in the war are allowed to continue serving the State. I decline to benefit from this privilege, although I understand the posture of those who feel it their duty today to remain at their posts.

Hertha Sponer was not Jewish, so she could have stayed in Göttingen. And at first she did, but she no longer had Franck's protection. Instead, she depended on Pohl, who, as I noted, was against women in science. In addition, the Nazi ideology frowned on women's participating in supposedly masculine activities. It was just a question of time before Sponer would decide to leave Germany. And she did. In September 1934 she moved to Oslo (like Blau), although with an official leave of absence from Göttingen. She had been promised the facilities to continue with her research in Oslo, but she felt the actual resources fell short of the promise. In the summer of 1935 she visited Göttingen, and from there she wrote to Ingrid, Franck's wife, 'Göttingen is still falling apart. In all the restaurants and coffee houses one sees indications that Jews are not welcome [...]. I have heard there is even a sign at the railway station now'.

One interesting detail is that in late 1934 Sponer went to Spain as the guest of a certain 'Professor Baberas' (whom I have been unable to identify) of the Madrid

School of Science. She gave a series of lectures there and then spent a week at Christmas visiting Córdoba, Seville and Granada. Volume 33 (1935) of Anales de la *Sociedad Española de Física y Química* contains a paper of hers, 'Los espectros de las moléculas y su aplicación a los problemas químicos' ('The Spectra of Molecules and Their Application to Chemical Problems').

Meanwhile, Franck and his family (which eventually included the family of his daughter and son-in-law, Aryan physicist Arthur Robert von Hippel, a former disciple of Franck's at Göttingen) had settled in Copenhagen, at the Institute of Physics directed by Niels Bohr. But, like other physicists, he, and they, ended up shipping out to the U.S. Franck accepted the offer of a chair at Johns Hopkins University in Baltimore, Maryland, which reached him through Karl Herzfeld. His reasons for taking this step included economic motives, as Franck admitted in a letter to Max Born dated 29 October 1934: 'And aside from the issue concerning the children, at my age it really is becoming important to have the possibility of getting my fingers on a little more money –I estimate $6,000– so that I can take out some life insurance'. The upshot is that Franck joined Johns Hopkins in 1935. Before sailing in June, he visited his mother and sister in Berlin and seized the opportunity to say goodbye to Max Planck, whom he invited to spend a few days with him in Copenhagen before Franck was due to leave Europe. Planck, the most respected of the German physicists, a faithful servant of his homeland, seems to have given the following moving reply: 'No, I cannot travel abroad. On my previous travels I felt myself to be a representative of German science and was proud of it –now I would have to hide my face in shame'.

The shame Planck mentioned had many faces. Obviously, the greatest was the human tragedies; but there were other symbolic faces that also represented the zeitgeist, the spirit of the times. One of such face appeared at the bicentennial celebration of the founding of the University of Göttingen in the summer of 1937. The rector, Friedrich Neumann (appointed by the German government, that is, Hitler) never mentioned Born, Courant or Franck, nor even the ever-independent Hilbert in his commemorative speech; he referred only to the Aryans Gauss, Weber and Felix Klein.

Planck's colleague and friend, Max Born, who was also of Jewish origin, left Göttingen as well. Born went before the Nazi authorities forced him out. On 10 May 1933 he left Göttingen in the company of his wife, Hedi, and his twelve-year-old son, Gustav (his daughter Irene was not in the city at the time). But he soon received invitations. The first, on 26 May, from Léon Brillouin in Paris, followed on the 31st of the month by the offer of a chair in Belgrade. The offer of not a chair but a lectureship from Cambridge, which came in on 15 June, looked more appealing, and Born eventually took it up. Although he spent several years on the banks of the Cam, at the Cavendish Laboratory where physics was king, his last post was in Edinburgh. In October 1936 Born became the Tait Professor of Natural Philosophy (the name still used in the United Kingdom for many physics chairs), replacing Charles Galton Darwin, who had retired early. There he stayed for seventeen years until his own retirement in 1953, when he and Hedi moved back to Germany. They settled in Bad Pyrmont, seventy kilometres south of Hannover, a quiet spot not far from Göttingen, and there he spent his last years. Earlier Born had proposed that James and Hertha Franck might move with them to the vicinity of Lake Constance, but Franck preferred to stay in the United States. Franck's letter to Born turning down the proposal is revealingly infused with sadness, melancholy and practical good sense.

> My dear Born, as tempting as the plan appears, namely to settle down with you both [...] on Lake Constanza for the days to come, it will not work out for us. As for me, a strong feeling of reservation because of what happened in Germany makes me feel uneasy. Although that may not be the deciding factor, there is a whole set of other important factors: my children and grandchildren, who would be so far away if I move back to Germany that I would never see them again; my work, which, despite my emeritus status, continues paying me from year to year (I could not even live off of the pension... and you can understand that not much could be put aside because of all our obligations). Finally, I have no idea how long my earthly life will last and I would like to be buried next to Ingrid in Chicago. I have left Hertha out of my list of reasons up until now but, as if all of those reasons were not enough, the fact that Hertha still has many a year of active productivity before her is probably enough to make the plans you laid out impossible for us. Perhaps you cannot understand this completely, but marriage with Hertha has returned to me a real lust for life [*Daseinsfreude*]. Nevertheless, we only live together a few months per year simply because I could not in good conscience expect Hertha to more or less go hungry after my death in exchange for a couple of years of living together. She needs to keep her professorate and, what is more, she is successful in her work, so much so that, even if it worked out financially (even counting all the money that is going out to Germany), it would simply be disgraceful to tear her away from her life. So I think you understand why your plan isn't feasible for us. I don't reject it lightly because, our old friendship aside, such a cozy corner on Lake Constanza has always seemed to be an ideal spot for old age. A few weeks ago, I had a few postcards from there and thought, with true longing, that the only decent place for us to be was pictured on those cards.

But let us get back to James Franck at Johns Hopkins. Neither the conditions nor the salary there were what Franck needed, so when the University of Chicago asked him to join in 1938, he was quick to accept. In 1941 Franck's wife Ingrid died, and five years later, on 29 June 1946, Franck and Hertha Sponer were married, although they lived apart for most of the year, because Franck remained in Chicago while Sponer had been working as a full professor since February 1936 at Duke University in Durham, North Carolina, the first woman on the university's physics faculty.

For a significant and most telling detail, we have the letter that Robert Millikan, one of the most important, influential physicists in the United States (Nobel Laureate in Physics in 1923), wrote on 24 June 1936 to Duke University's president, William Preston Few, in answer to a request for Millikan's advice on hiring Sponer.

> Dear President Few:
>
> I scarcely know how to reply to your letter of June 11th, but since you ask for a most confidential statement I shall be glad to say a word about how I myself would go about building up as strong as possible a department of physics at Duke University.
>
> I should introduce into the department a number of young men of as pronounced ability as I could find, and then give them every possible opportunity to rise to positions of influence inside and eminence outside. In view of the fact that at least 95% of the ablest minds that are now going into physics are men – indeed, I do not remember that of the several hundred National Research Fellows in physics who have been chosen in the last ten years there have been any women – I should feel that my chance of building a very strong department would be better if I made my choices among the most outstanding of the National Research Fellows or other equally outstanding young men who for one reason or another thought it unwise to become candidates for National Research fellowships. Women have done altogether outstanding work and are now in the front rank of scientists in the fields of biology and somewhat in the fields of chemistry and even astronomy, but we have developed in this country as yet no outstanding women physicists. In Europe Fraülein [sic] Meitner and Madame Curie of Paris are in the front rank of the world's recognized physicists. I should, therefore, expect to go farther in influence and get more for my expenditure if in introducing young blood into a department of physics I picked one or two of the most outstanding younger men, rather than if I filled one of my openings with a woman. I might change this opinion if I knew of other women who had the accomplishments and attained to the eminence of Fraülein [sic] Meitner. I know of no other living woman who has had anything like her accomplishment or has prospects in the future of having such accomplishment.
>
> Also, in the internal workings of a department of physics at a great university I should expect the more brilliant and able young men to be drawn into the graduate department by the character of the *men* on the staff, rather than the character of the women.

> These considerations relate more to the graduate work than to the undergraduate. In a coeducational institution where there are many women students it is undoubtedly also desirable to have for pedagogical purposes women instructors but only in very exceptional cases would I think that the advance of *graduate* work would be as well promoted by a woman as by a man.

Fortunately President Few disregarded Millikan's recommendation, and Hertha Sponer resumed her scientific career at Duke very successfully, consolidating her prestige in the field of molecular spectroscopy, especially in polyatomic molecules. She retired in 1965 and was named emeritus professor.

Charlotte Riefenstahl-Houtermans

The next case I am going to relate, that of Charlotte Riefenstahl (1899-1993), does not concern as distinguished a scientist as Blau or Sponer, but Riefenstahl's story should be told, not only because she studied at the University of Göttingen, but also because of her life as a whole, which was strongly influenced by the man she married in August 1930, Fritz Houtermans (1903-1966). Also, as we shall see, she should be remembered because she knew Maria Goeppert. As she herself admitted in her autobiography (quoted in *Standing Together in Troubled Times*), 'My life was anything but ordinary. I went through trials and tribulations, blows and disillusionment. This was a terrible century, with blood and cruelty. Now, all these events are in the distant past, and they appear in my mind as fragments of an endless movie, as memories and snapshots which no longer hurt me. I have reached the summit. Looking down, in my past, I often think that I was born under a very lucky star'. From the same source: 'My mother wanted me to be a writer, but it was physics that caught me. Most people don't understand this, but physics is like philosophy. Physics answers all these questions about what is happening around you. It is one layer deeper than philosophy. If I had to choose again today, I would still study physics'.

Charlotte started studying at the University of Göttingen in 1922, at a time when 'inflation was growing at such a rate that finally the money one had lost its value. The student body was threatened with starvation. With one meal a day and hardly any cash, we still studied, were excited about physics, love and life in general, and took all these miseries more or less for granted and managed to forget or overlook them.' About the university she said, 'Göttingen University was the best university in Germany, perhaps in the whole world. I took mathematics from such geniuses as Richard Courant and David Hilbert, and physics and physical chemistry from Max Born, Heinrich Hertz, James Franck, Hertha Sponer and Gustav Tammann'. Under Tammann's supervision she earned her doctorate in 1927 at the Institute of Chemistry and Physics. Later in her story she said,

> I was the only woman in the class and it was mostly pleasant. I was surrounded by a jolly crowd of bright young men, who were enthusiasts of science. The only hard thing was I didn't study as much as I could have because I was busy teaching to earn money. I got to know all of them: Robert Oppenheimer, Fritz Houtermans, Walter Elsasser, Viki Weisskopf, Edward Condon, Thorfin Hogness, Patrick Blackett, Michael Polanyi, George Gamow, Robert Atkinson and Leo Szilard… Today these names read as a Who's Who in theoretical physics. To me they were my classmates, friends, and teachers – magnetically attractive, obsessed with their ideas, with politics, with changing the world – with whom I partied and went to concerts.

The list is indeed impressive. One of the men she mentioned, Fritz Houtermans, she married, divorced, remarried and divorced again.

Even before she got her PhD, Charlotte had decided she wanted to teach in the United States for a living. She wrote to the president of Vassar College in Poughkeepsie, New York, for a job on 18 March 1924, describing herself as 'a female student at the Institute of Physical Chemistry of Göttingen University where I am doing work for the doctorate', which, she said, 'I expect to finish in a few weeks'. She got a prompt positive reply from Vassar, but her supervisor, Tammann, did not want to let her go, and he refused to write her a letter of recommendation. Fortunately, Franck and Born did support her, and although by then Riefenstahl was not too happy about leaving Göttingen because she had gotten into a relationship with Houtermans, the two of them decided it would be best for her to spend a year in America. She finally set off for Vassar in the summer of 1927, with her doctorate complete (she defended her thesis on 20 July; it was about recrystallization in silver and gold and the change in electrical resistance). Instead of one year, she stayed at Vassar for two, returning to Europe in 1929.

In early 1930 Riefenstahl and Houtermans were invited by their friend Georg Gamow, a physicist as famous for his scientific accomplishments as for his easy-going humour, to attend a physics conference scheduled to be held in Odessa. As Charlotte later remembered it, the sessions took place at the city hall beneath a huge banner that read in several languages, 'Physicists of the world, unite! In the name of a bright future for all mankind!' After the conference, very much in the propagandistic style with which the Soviet authorities then feted foreign visitors of renown, a cruise was organized to take the conference goers to several sites, starting with Yalta and ending, via another means of transport, in Moscow and Leningrad (now Saint Petersburg). In that heady atmosphere, Fritz proposed. Charlotte accepted immediately, and the wedding was held at the first port they called at, Batumi, in Georgia, with physicists Rudolf Peierls and Wolfgang Pauli as witnesses.

The couple settled in Berlin. Charlotte worked first at the Springer Verlag publishing house and later as an assistant at the Radium Corporation Ltd., while Fritz combined political activities (he was a Communist, a member of the German Communist Party since the 1920s, and convinced Charlotte to join) with applying the atomic spectroscopy experience he had gained under Franck to determine the relative abundance of isotopes. He was, in fact, the first to measure the hyperfine structure of artificially separated isotopes.

Charlotte lived through some rough times with Houtermans. Fritz was a brilliant physicist, but he was also very politically involved, and his political activity used to get him into serious trouble. James Franck, who by then had been living in Chicago for some time, summed up Houtermans' problems in a letter he wrote to the U.S. consul in Berlin in September 1940:

> Dear Sir!
>
> Dr. Fritz G. Houtermans a pupil of mine and a scientist who is very well known everywhere by physicists on account of his very valuable papers applies for an immigration visa to the United States. I understand his application is supported by affidavits but nevertheless a group of us interested in Dr. Houtermans' personality as well as his great ability as a physicist would like to recommend him strongly to you.
>
> Dr. Houtermans was one of my best students in Göttingen; he became afterwards assistant in the Physikalische Institut der Technischen Hochschule in Berlin and Privatdozent. In 1933 he left for England [accompanied by Charlotte and their child] where he got a good position in industry but leaning more to scientific research he accepted [in 1934] a professorship at the Physical Technical Institute in Kharkov U.S.S.R. where he, his wife [who worked as a translator] and his two children [the second was born in Russia] lived until all foreigners were dismissed by decree of the Soviet Government. On his way out of the country he was arrested [on 1 December 1937] without any charges given and spent two and half years in prison before being finally sent to Germany [this was during the brief German-Soviet alliance, when Houtermans was turned over to the Gestapo, but he was released after performing certain services for them].
>
> In this connection I think it would be good to mention that Houtermans was a very young man with a wish to reform the world and played with communistic ideas; but whatever his tendencies may have been the years he spent in Russia including those under most terrible circumstances in the hands of the Soviet Secret Police wiped out all sympathy for communism so far as that not lost already as he became more mature. It is my profound opinion that he would make a loyal and devoted citizen to this country. If you will be able to grant a visa to Dr. Houtermans, you will admit a valuable scientist who will be grateful to live

> here and who will repay his indebtedness to the United States by scientific or industrial work of a high standard.
>
> Mrs. Houtermans who is also a friend of ours from the time she studied in Göttingen and who worked last year as a research assistant in Physics at Vassar College, is of course very anxious to be reunited to her husband and the father of her two children. Dr. Houtermans' mother also is a scientist and teaches in this country [at a private school in Massachusetts].
>
> I am convinced that in making your decision you also will take into account the human side of Dr. Houtermans. We, his friends and colleagues, will do everything which is possible to help Houtermans after his arrival here.

The letter speaks as to Franck's bonhomie, but he does not appear to have sized up Houtermans' complex character accurately, in the light of what really happened.

Charlotte wrangled her way out of the Soviet Union with her two children in December 1937. She reached Copenhagen, where she got help from Niels Bohr, but she did not stay very long; on 24 January 1938 she left for England. Her situation there soon became dramatic, however (Robert Oppenheimer, whom she had known in Göttingen, sent her a cheque for 500 dollars in March). She finally managed to emigrate to the United States, reaching New York on 6 April 1939, where she was taken in by Houtermans' mother, who taught Latin, French and German at the elegant Foxhollow School. Fortunately, her friends at Vassar rallied round; they got her a small one-year fellowship. And when that ended, she was hired at Wellesley College. She stayed there until 1942, but then she was offered a position at Radcliffe College, a women's institution in Cambridge, where she taught in the spring of 1945 (in the 1943-1944 school year she stood in for a Harvard professor on sabbatical, and in 1944-1945 she juggled her classes at Radcliffe with a research job at the Polaroid company's laboratory). In her life as an academic globetrotter, Charlotte was hired for the 1945-1946 school year by another women's school, Wells College, in Aurora (Cayuga County), New York. But she did not linger there.

In the meantime, after the Russians released him, Houtermans spent some time, as I said, in a Gestapo jail, although he was soon set free. His Jewish ancestry was very slight, just a quarter on his mother's side. And he remained in Germany, since he was not granted a visa to emigrate to the United States. Between 1942 and 1943 he published three papers (the most important one was about the photonuclear reaction in beryllium), all in partnership with Ilse Bartz, a chemical engineer with whom Houtermans fell in love. He decided to marry her (they had three children). Obviously, he needed to divorce Charlotte. He did, without telling her; Germany had a law that allowed people who had been separated from their spouse for five years to obtain a 'divorce in absentia' without the need for

Maria Goeppert in her Göttingen years

Max Born appears at the rear of this family snapshot.
Maria is on the left.

Víctor F. Weisskopf, María Goeppert
and Max Born in Göttingen

spousal consent. 'To say that I was ruined,' wrote Charlotte in her memoirs, 'is to say nothing. My love, my plans for the future, my dreams, my years of struggle for Fritz's liberation, my desperation and hope, everything was destroyed in a blink.'

As I said, Charlotte Riefenstahl-Houtermans did not spend very long at Wells College. And this is where Maria Goeppert Mayer steps into the picture. By then she had been living in the United States for years. Maria and Charlotte knew each other from Göttingen. In the words of Riefenstahl-Houtermans, 'I first met Maria Göpper in 1924 in Göttingen. She was seven years younger than me and had just enrolled at the University of Göttingen to study mathematics. There were few girls in exact sciences, so we were destined to meet. Later she switched to physics. One of the examiners on her thesis was James Franck, who was on my examination committee too'. And after outlining Goeppert Mayer's career in the United States (I will get back to that information in another chapter), Riefenstahl-Houtermans explained, 'in 1941 Maria was finally offered her first paid professional position, part-time teaching at Sarah Lawrence College in Bronxville, New York, 15 miles north of Manhattan. Until the end of the war she taught at Sarah Lawrence on and off […]. In 1945, Joe Mayer [her husband] was offered a position at the University of Chicago, which I believe was in the Chemistry Department.' At this point Charlotte added that on 15 August 1945 Maria Goeppert Mayer had written the following letter to Mrs Constance Warren, president of Sarah Lawrence College:

> My husband has accepted a professorship at the University of Chicago and expects to start there on February first […].
>
> In short, I would like to leave the College after the first semester. If, however, no suitable physicist can be found to take my position, I would be willing to stay till the end of the year.
>
> It appears to me to be inadvisable for the College to look for a 'substitute' to fill out for just one semester. […] I do hope that a satisfactory permanent successor for my position can be found.
>
> It might prove easier to find a physicist now, at the beginning of the year. In that case I would be glad to resign immediately. In view of today's news it should not prove too difficult for the College to receive the services of a competent physicist. May I make a few suggestions of names that occur to me?
>
> There is firstly Mrs. Charlotte Houtermans […].
>
> I am very sorry indeed to leave, but I have no choice in the matter.

A wife (like Maria) followed her husband, whether that was best for her or not. But the point here is that Maria recommended Charlotte. And the president of Sarah Lawrence College, Harold Taylor, who had just replaced Constance Warren, called Charlotte right away for an interview. 'He was impressed with my physics credentials,' Charlotte recalled, 'my experiences in the Soviet Union, and my general sophistication'. Taylor offered her a job at once, which she accepted once her previous situation at Wells College was settled. She began teaching in Bronxville in the summer of 1946 and stayed there for 22 years, until her retirement. And even after retiring she kept teaching at Manhattanville College in Purchase, New York, until 1975. Always at women's colleges. 'I was and still am,' she also said in her memoirs, 'a big believer in all-women schools because they help girls to fully develop their intellect without social pressures'.

Her contribution to physics, then, was not through research, as it was for Blau, Sponer and Goeppert Mayer, but through teaching the science she had herself learned from such fine teachers. She can also lay claim to having translated an important 'new physics' book, Gregor Wentzel's *Einführung in die Quantentheorie der Wellenfelder* (*Quantum Theory of Fields*), printed in 1943, from German into English. The translation came out in 1949 and included an appendix prepared by J.M. Jauch.

And I will wind up this section with some notes on how Charlotte's personal life and her relationship with Fritz Houtermans turned out.

In 1951 Charlotte went to Europe to visit her mother and sister. Fritz wrote to say he wanted to see her when he came back from a lecture in Copenhagen. And they met up, after 14 years apart. Fritz, who in 1945 had secured a post in Göttingen and in 1952 a chair at the University of Bern, told her all about his troubles being married to Ilse and said he was still attracted to Charlotte. That was the first step. Fritz got his second divorce, and he and Charlotte were remarried in Bern in August 1953. But their 'second life' lasted only six months; in 1954 they separated again. 'By 1960,' concluded Charlotte in her memoirs, 'my correspondence with Fisl [Fritz], which had already been limited to practical matters, faded away [...]. Fisl's rather heavy drinking habit during the last decade, his chain smoking, finally caught up with him. He became depressed and never again was able to work with the enthusiasm typical of the first eight years of his life in Bern. In 1965 Fisl was diagnosed with lung cancer'. He died of a heart attack in March 1966.

Emmy Noether

Emmy Noether (1882-1935) was an outstanding mathematician, and within her discipline she shone much more brightly than Blau or Sponer did in physics. And she, too, was linked to the University of Göttingen. Noether may be regarded as the 'successor' of other illustrious women mathematicians before her, like Sophie Germain (1776-1831) and Sofya Kovalevskaya (1850-1891).

After eight years' schooling at the Städtischen Höheren Töchterschule (Municipal School of Higher Education for Daughters) in Erlangen, where her father, Max, was a distinguished chaired professor of mathematics at the university, Emmy passed the Bavarian state examinations for English and French teachers with flying colours. That meant she was qualified to teach foreign languages at any educational institution for girls or women. But this was not enough for her; she wanted to go to university. Some family strings may have had to be pulled, but she managed to get into the University of Erlangen, whose senate had declared in 1898 that admitting women students 'would destroy all academic order'. In fact, at first Noether only got permission to audit classes; she was not allowed to take tests before 1903, when the university changed its statutes (only one of Emmy's 985 classmates in the winter 1900 semester was a woman). On 13 December 1907 she earned her PhD *summa cum laude* with a thesis entitled 'Über die Bildung des Formensystems der tornaren biquadratischen Form' ('On Complete Systems of Invariants for Ternary Biquadratic Forms'); her mentor was another signal mathematician, Paul Gordan.

During the following years she worked an unsalaried job at the Erlangen Mathematical Institute, she helped her now-elderly father, and she pursued her own projects, especially in the theory of algebraic invariants. In 1916 she moved to Göttingen. Let Hermann Weyl go on with the story from here, as told in the obituary (1935) he wrote for Emmy:

> During the war, in 1916, Emmy came to Göttingen for good; it was due to Hilbert's and Klein's direct influence that she stayed. Hilbert at that time was over head and ears in the general theory of relativity, and for Klein, too, the theory of relativity and its connection with his old ideas of the Erlanger program brought the last flareup of his mathematical interests and mathematical production. […]. To both Hilbert and Klein Emmy was welcome as she was able to help them with her invariant theoretical knowledge. For two of the most sides of the general relativity theory she gave at that time the genuine and universal mathematical formulation: First, the reduction of the problem of differential invariants to a purely algebraic one by use of 'normal coordinates'; second, the identities between the left sides of Euler's equations of the problem of variation which occur when the (multiple) integral is invariant with respect to a group of transformations involving arbitrary functions (identities that contain the conservation theorem of energy and momentum in the case of invariance with respect to arbitrary transformation of the four world coordinates).

As I said, Klein felt that what Einstein had actually done was to apply the philosophy of his Erlangen programme: If the core idea of special relativity could be interpreted as 'the study of invariants under Lorentz's transformations', then general relativity was just 'the study of invariants under general coordinates transformations'. Under Hilbert and Klein's stimulus, Noether set aside her research into algebraic invariants for a while and turned instead to the relationships in variational principles between symmetries (or invariances) and laws of conservation, with the ultimate goal of elucidating the role of Bianchi identities in the field equations of the general relativity theory. In 1918 she solved the problem, publishing a paper ('Invariante Variationsprobleme'; 'Invariant Variational Problems') containing what came to be known as 'Noether's theorems', a set of mathematical instruments that are splendid not only, or even mainly, for general relativity, but for theoretical physics as a whole. Einstein greeted Noether's papers enthusiastically; on 24 May 1918, he wrote to Hilbert, saying, 'Yesterday I received a very interesting paper by Miss Noether about the generation of invariants. It impresses me that these things can be surveyed from such a general point of view'. And he added, 'It would not have harmed the Göttingen old guard to have been sent to Miss Noether for schooling'. A few months later, on 27 December, after receiving the second of Noether's articles, Einstein repeated how he admired Noether (who, as a woman, was rejected by university faculties) in a letter to Felix Klein: 'What prompts me to write today, though, is a different matter. Upon receiving the new paper by Miss Noether, I again feel it is a great injustice that she be denied the venia legendi. I would very much support our taking an energetic step at the Ministry'.

Going back to Weyl's obituary:

> Still during the war, Hilbert tried to push through Emmy Noether's 'Habilitation' in the Philosophical Faculty in Göttingen. He failed due to the resistance of the philologists and historians. It is a well-known anecdote that Hilbert supported her application by declaring at the faculty meeting, 'I do not see that the sex of the candidate is an argument against her admission as Privatdozent. After all, we are a university and not a bathing establishment'. [According to the 1908 *Privatdozentenverordnung*, only male candidates were allowed (a later protest to the Ministry of Culture was turned down)]. Nevertheless, she was able to give lectures in Göttingen, that were announced under Hilbert's name. But in 1919, after the end of the War and the proclamation of the German Republic had changed the conditions, her Habilitation became possible. In 1922 there followed her nomination as a '*nichtbeamteter ausserordentlicher Professor*'; this was a mere title carrying no obligations and no salary. She was, however, entrusted with a '*Lehrauftrag*' for algebra, which carried a modest remuneration.

She remained in that situation until 1933. Nevertheless, at the same time she was making great strides in her career as a creative mathematician. Here is how Weyl saw the situation, with its intrinsic opposites:

> When I was called permanently to Göttingen in 1930, I earnestly tried to obtain from the Ministerium a better position for her, because I was ashamed

> to occupy such a preferred position beside her whom I knew to be my superior as a mathematician in many respects. I did not succeed, nor did an attempt to push through her election as a member of the Göttinger Gesellschaft der Wissenschaften. Tradition, prejudice, external considerations, weighted the balance against her scientific merits and scientific greatness, by that time denied by no one. In my Göttingen years, 1930-1933, she was without doubt the strongest center of mathematical activity there, considering both the fertility of her scientific research program and her influence upon a large circle of pupils.

With the Nazis' accession to power in January 1933, the situation became even worse for Emmy, who, in addition to being a woman, was a Jew. In April her *venia legendi* and her *Lehrauftrag* (including her pay, naturally) were taken away. In July two women's' colleges, Bryn Mawr, in Pennsylvania, and Sommerville, in Oxford, inquired after her services. Eventually, with financial aid from the Rockefeller Foundation, she accepted a post at Bryn Mawr College for one school year. In October she embarked for the New World.

She was not very lucky in her new stage in life, however. Academically things went well; in February 1934 she began teaching weekly classes at Princeton, not far from Bryn Mawr. Furthermore, Bryn Mawr renewed her contract for another year. But on 14 April 1935 Emmy Noether died in Bryn Mawr Hospital as a consequence of an operation that was not supposed to be very serious.

Maria Goeppert, University of Göttingen Student and Born Disciple

After passing the *Abitur*, when the time came to choose what to study at university, Maria Goeppert picked mathematics. She was quite gifted in the science. The fact that Hilbert was a neighbour and family friend may have influenced her as well, but she seems to have been swayed more by the news that primary and secondary schools could not find enough women maths teachers (as a consequence of the teacher shortage, the number of women studying maths at university rose significantly).

Maria Goeppert was admitted to the University of Göttingen in spring 1924, and except for one semester in England she spent all her undergraduate years there. First she concentrated on mathematics, but one day in 1927 Max Born, a member of the Goeppert family's circle of friends, invited her to attend one of his physics seminars. In fact, a few years earlier, while she was still at the *Frauenstudium*, Maria had already had some contact with the new atomic physics. This contact came to her through Hilbert, who invited her to attend a series of semi-open lectures he was giving on the subject. Maria liked it, but her interest in physics had not yet been truly reawakened. When Born invited her to his seminar, however, the old interest sparked into life, helped along by the idea of joining a lively group

Maria in her student years
in Göttingen

Joseph Mayer with Maria and her
mother in the car he bought shortly
after arriving in Göttingen

Joseph and Maria after their marriage

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of twenty to thirty intelligent peers. On 9 February of that same year (1927), her father died, and that strengthened her close lifelong relationship with Born, who also supervised her doctoral thesis. With Born, and in the Göttingen atmosphere, Maria Goeppert gained a splendid education in quantum mechanics, not only in its mathematical dimensions, but also, thanks to Franck's influence, in a more intuitive, experimental dimension. In the United States this higher knowledge of quantum mechanics would prove extremely useful to her, as the theory behind it was slow to reach some American universities.

In his memoirs, Born recalled Maria as follows:

> Prominent among the German students was Maria Goeppert, the daughter of the professor of paediatrics at Göttingen University who had often attended to our children. Maria was a lovely and lively young girl, and when she appeared in my class I was rather astonished. She went through all my courses with great industry and conscientiousness, yet remained at the same time a gay and witty member of Göttingen society, fond of parties, of laughter, dancing and jokes. We became great friends. After she got her doctor's degree with a very good thesis on a problem of quantum mechanics, she married a young American, Joe Mayer, who worked with me on problems of crystal theory.

Some of the young physics students and researchers at Göttingen were attracted by Maria's sparkling personality. One of them was Victor Weisskopf, as he admitted in his memoirs:

> I was attracted to Maria Göpper, called 'Misi' by her friends. She was the daughter of a well-known professor of pediatrics, and her family belonged to the 'good society' of Göttingen. She also studied theoretical physics, and her thesis was related to my own. For a time we were very fond of each other. Then a young American student came to the university. He impressed us by going to a car dealer, putting down a small pile of money, and driving out with a new car. In

> those days the idea of a student owning a car was preposterous. I don't believe it was the car alone, but pretty soon Misi was going out with the American, Joe Mayer, whom she later married.

Another of the young physicists then at Göttingen who was to leave his mark on science was Walter Elsasser, who also spoke of Maria in his memoirs: 'About the time I came to the end of my thesis, Born [his thesis advisor] acquired a new research student, Maria Mayer, with whom at that time I had little contact […]. She seemed to stand out as the best-dressed woman on the streets of Göttingen. I suspected that she was not subject to the severe economic constraints with which we other young people had to contend. But this did not keep her from progressing in theoretical physics and developing a high degree of expertise in calculations that must have been very useful to Max Born.'

Maria Goeppert completed her PhD work in February 1930. This was shortly after her marriage on 19 January to an American physicist, Joseph Mayer, who, as shall be seen in the next section, was spending a year at Göttingen.

Although by then Maria had already solved many of the problems in her thesis (suggested by Born), she found organizing the written paper a struggle, among other reasons, because her relationship with Mayer was quite the distraction. In her biography of Maria, Joan Dash told how Maria managed to complete her thesis:

> One day Maria and Joe [Mayer] drove in Joe's little Opel to Leyden, to visit the theoretician [Paul] Ehrenfest [who stepped in when H.A. Lorentz voluntarily retired from his chair], who spent much of his time in Göttingen and was one of its most gifted teachers, so gifted that James Franck once remarked: 'I was afraid to ask him a question, because if I asked him a question, it took a terrific time. He didn't let me out of his claws, I must say, until I really understood this thing I have asked and in each detail. Sometimes I didn't want to understand each detail'. Once arrived at Ehrenfest's house […] Ehrenfest demanded to know how Maria would write her dissertation. Maria explained her ideas, Ehrenfest listened, then told her there was no more time to waste, she must go upstairs immediately and not come down again until the entire thesis was committed roughly to paper. Then he led her to his own study, locked the door and left her there; three hours later she had completed an outline that satisfied Ehrenfest.

Maria Goeppert's thesis, which was judged by Born, Franck and Adolf Windaus (Nobel Prize in Chemistry, 1928), consisted of a theoretical study of two-photon processes (the probability that two photons would be emitted in a single atomic transition). Eugene Wigner, who, as we shall see, received half of the Nobel Prize for Physics the same year as Maria and Hans Jensen (who won a quarterprize apiece), declared in her obituary that Maria's thesis was a 'masterpiece of clarity and concreteness'. In the era when she wrote it, the possibility of putting her theoretical predictions to the experimental test was still remote. It was quite a few years later when two-photon phenomena took on experimental importance in both nuclear physics and astrophysics, especially with the development of lasers and nonlinear optics.

Her first publication was a preview of her thesis, 'Uber die Wahrscheinlichkeit des Zusamenwirkens zweier Lichtquantan in einem Elementarakt'; it appeared in volume 17 (1929) of the journal *Naturwissenschaften*. In 1931 she published another two papers drawn from her Göttingen years, a solo article ('Über Elementarakte mit zwei Quantensprungen', *Annalen der Physik*) and a long, 171-page chapter co-authored with Max Born ('Dynamische Gittertheorie der Kristalle'; 'Dynamic Theory of Crystal Lattices'), which appeared in volume 24 of the *Handbuch der Physik*. She was able to complete this huge amount of work because she spent the summers of 1931, 1932 and 1933 in Göttingen visiting her mother and using her time there to do further work with Born. In 1935 she published another article related with her thesis, this time in English, in the number-one physics journal of the United States (and soon of the world), *Physical Review* (vol. 48): 'Double Beta-Disintegration'.

Joseph Mayer

Joseph Edward Mayer (1904-1983) was a New Yorker who enrolled at the California Institute of Technology (also known as Caltech) in 1921 to study chemistry. In those days outstanding scientists such as Richard Tolman and physical chemist Arthur Amos Noyes were teaching at Caltech, and Linus Pauling was just starting his career there. After getting his BS at Caltech in 1924, Mayer secured a scholarship to another distinguished California seat of learning, the University of California at Berkeley, to learn from the great chemist and physicist Gilbert Newton Lewis. 'He [Lewis] soon became and remains to this day one of my greatest idols', said Mayer in an autobiographical document. Under Lewis's direction he completed his dissertation, 'The Disproof of the Radiation Theory of Unimolecular Reactions'. 'I don't remember exactly', he wrote in the document I have just mentioned, 'when I actually began work on what became my dissertation. It was a fairly difficult experimental stunt, and I think that actually had we started it after we really understood quantum mechanics, we would have thought it not worth doing'.

While at Berkeley Mayer published four papers with Lewis ('Thermodynamics Based on the Laws of Statistics', parts I and II, *Proceedings of the National Academy of Sciences* 14, 1928; 'The Quantum Laws and the Uncertainty Principle of Heisenberg', *Proceedings of the National Academy of Sciences* 15, 1929; 'The Thermodynamics of Gases Which Show Degeneracy', Proceedings of the National Academy of Sciences 15, 1929; and 'The Thermodynamics of Gases Which Show Degeneracy', *Proceedings of the National Academy of Sciences* 15, 1929).

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In autumn 1929 he went to Göttingen on a National Research Council fellowship funded by the Rockefeller Foundation to work with James Franck, who used to be at Berkeley. He also collaborated with Max Born, though; he wrote a paper with Born ('Zur gittertheorie der ionenkristalle', *Zeitschrift für Physik* 75, 1932), the first of a series of articles about the thermodynamics of ionic crystals, a subject he continued to pursue for the next fifteen years. A classmate of Mayer's from Berkeley, who had already spent some time in Göttingen, had advised him it would be much better to get a room in a private home in Göttingen instead of staying at a boarding house, like most American students did. And this friend mentioned that maybe the Goeppert family would oblige, as he thought they would probably be receptive to the idea after Professor Goeppert's death. Indeed, Mrs Goeppert had rented a room the year before to Robert Mulliken, a physical chemist from the U.S. who went on to become famous (he won the 1966 Nobel Prize in Chemistry 'for his fundamental work concerning chemical bonds and the electronic structure of molecules by the molecular orbital method').

Before continuing with Joseph Mayer, I would like to mention what Mulliken wrote in his autobiography about his stay in the Goeppert family home during the summer of 1927:

> While in Göttingen that summer, I lived at the house of Frau Goeppert, widow of a well-known pediatrician. She had a daughter, Maria, who was then a student at the University. One day Maria asked me if I would like to go with her to a social event at the University. Churlishly perhaps, I declined the invitation […].
>
> Maria was a brilliant scientist in physics and chemistry. For her work in explaining the internal structure of nuclei she and J.Y. Jensen of Heidelberg were

> jointly awarded a Nobel prize in physics in 1963 […]. I sometimes wondered how we would have done if I married her. Her knowledge of mathematics and quantum mechanics was far better than mine, and together we might have done well. Scientists often marry other scientists, and this can make for efficiency in their work.

And a little later, he rounded out his practical (or self-serving?) view of life with the following comment: 'My own feeling was that to marry a non-scientist was better for a scientific man than to marry a scientist because it would help to give him broader perspectives and interests'.

When Joseph Mayer knocked on the Goeppert's door, the maid came but told him Mrs Goeppert was ill and could not receive anyone. She would ask if Mrs Goeppert's daughter could see him, though. And, in Mayer's words,

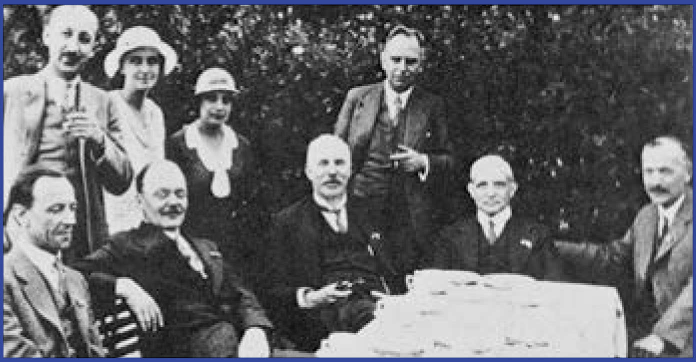
> Well, the daughter came, smiled benignly at my frantic German, and then answered in a beautiful Cambridge English, that her mother was sick, that it was just a cold, but she did not want to see anybody, that I should come back in a day or two, which I did […]. I was much impressed with the daughter and particularly with her perfect English, which I later found she had acquired in one semester at Cambridge on a student fellowship from Germany, in Rutherford's laboratory. She had lived in Girton College while she was in Cambridge, which was the only girl's student house at that time. Well, I was feeling relatively wealthy and I purchased an Opel […], a wonderful car […]. I think the existence of the Opel changed my future life. It was a beautiful machine and I had the only automobile of any of the students or of any of the young faculty. Maria was the belle of Göttingen, as I soon found out. She and the two daughters of Marianna and Herr Professor Landau [mathematician], along with Titi Stein, seemed to make up the acceptable female contingent of every student party.

And he went on in a similar vein, eventually adding, 'In Göttingen I found several letters from Johns Hopkins University offering me a position of associate […]. I responded affirmatively and felt very happy that I had a position assured when I got back to the United States. In the meantime I was getting more and more interested in trying to induce Maria to come back as my wife to the United States. This was not completely trivial; the German immigration quota was filled for several years in advance. However, I found out from the consulate that my wife could get in on a special visa. Well, that worked out. I remember that Maria's favorite aunt, who was not much older than Maria, the wife of the youngest brother of her father, said to her: "You are fortunate in going to America. My sons will be caught up in the next war." They were. One of them survived but was badly wounded.'

And so Maria, now Maria Goeppert Mayer, embarked for America and her destiny.

Franck's farewell party in Berlin. From left to right: seated, Hertha Sponer, Albert Einstein, Ingrid Franck, James Franck, Lise Meitner, Fritz Haber and Otto Hahn; standing, Walter Grotian, Wilhelm Westphal, Otto von Baeyer, Peter Pringsheim and Gustav Hertz

Otto Hahn and Lise Meitner



Participants in the Radioactivity Conference (Münster, Westphalia, 1932). Standing, from the left: von Hevesy, Mrs Geiger, Lise Meitner, Otto Hahn; seated: James Chadwick, Hans Geiger, Ernest Rutherford, Stefan Meyer and Karl Przibram

Meeting at the Niels Bohr Institute, (Copenhagen, 1934). First row (left to right.): W. Pauli, P. Jordan, W. Heisenberg, M. Born, L. Meitner, O. Stern, J. Franck. Second row: M. Oliphant, M. Saha, C.F. von Weizsäcker, F. Hund, F. Reiche, H.D. Jensen, F. London, O. Frisch. Fifth row: E. Teller and V. Weisskopf

CHAPTER 3

The United States

When Maria Goeppert Mayer left Europe in March 1930 on board the transatlantic *SS Europa* in the company of her husband and his sister Kate, who had been visiting Joe, Maria could not ignore the fact that she was on her way to face a very different world. Göttingen was quite closed in many ways, and, while intellectually elitist, it was elitist all the same. Maria was no early-bird emigree; Hitler had not yet gathered up all the strands of power when she left Germany in 1930, and the awful war that would devastate a great deal of the world was not even within the bounds of imagination. Furthermore, her Jewish forebears were few (a grandmother and a great-grandmother), at all events no insurmountable obstacle to an academic career, as we have seen in the examples of Born and Franck. But she surely knew that as a woman she would find it hard, if not impossible, to scale to the heights of a university professorship, although she might still be able to carve out some kind of career in science, like Lise Meitner and Hertha Sponer had done. In fact, as we shall see in this chapter, although her chances were *somewhat* better in Germany (remember Hertha Sponer and the antagonism against the idea of allowing a woman, not to mention a *foreign* woman, to join a university physics department's faculty), Maria´s own situation at many American universities was inhibited by rules against nepotism, which prevented a woman from being hired if her husband was already an employee of the institution (the reverse would have been unimaginable then).

For the right perspective on Maria Goeppert Mayer's life after moving to the United States, we have to look at the situation of American science first.

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Science in the U.S.A

It would be foolhardy indeed to try and sum up a nation in just one phrase, and any attempt to do so is generally doomed to failure. If forced to describe the United States, though, many would resort to one word: *practical.* Indeed, just a quick overview of U.S. history turns up numerous examples of the practical –and pragmatic– inclinations of its citizens, perhaps because it was so hard for them (many of whom were Old World immigrants) to make a living in such a vast country in the 19^{th} century.

The colonization of the west was one of the ruling social forces in the Antebellum period (1861-1865), and a powerful economic force as well. To facilitate expansion into little-known territories and learn more about the land that had already been settled, the federal government found itself obliged to support work in astronomy, hydrography, geophysics, terrestrial magnetism, meteorology, topographical studies, geology, botany, zoology and anthropology. These were the only disciplines with government support back then, since they were key for learning about the nation's physical and human geography. As Hunter Dupree, the great scholar of the history of science and the federal government, put it, 'Sciences in which laboratory work predominated, where discoveries were made in test tubes rather than in distant mountains, were notably absent from the federal government's interest'.

Tellingly, the origins of the American Association for the Advancement of Science also displayed the same kinds of interests. In 1840 ten geologists, most with ties to state topographical and surveying services, met in Philadelphia to create the Association of American Geologists, which two years later took the name of the 'Association of American Geologists and Naturalists'. In 1848 the organization, now more numerous, changed its name, adopting a new charter based on that of the British Association for the Advancement of Science. The recently christened American Association for the Advancement of Science, which at first had only two sections (one for 'general physics, mathematics, chemistry, civil engineering, and applied sciences in general', and another for 'natural history, geology, physiology and medicine'), like its sister organizations in other nations, quickly became an important forum for U.S. scientists. In 1848 the Association had 461 members; in 1854, 1,004.

In the middle of the Civil War, Congress passed a law establishing the National Academy of Sciences. This law was signed by President Abraham Lincoln on 3 March 1863. Many of its proponents entertained the idea that the new institution could liaise between national science and the federal government, or act as the government's deputy. But these ambitions were dashed: the academy was an effective instrument for recognizing scientific merit, but it never became a centre of power or patronage. It did not regularly publish journals, its meetings were infrequent and poorly attended, and furthermore it ran on a shoestring budget (probably the cause of all its other problems).

The period from the creation of the National Academy of Sciences to the outbreak of World War I in 1914 can generally be described as one of moderate although significant growth in American science, without significant public financial aid and without any kind of federal planning.

One discipline where the slow but steady progress of U.S. science during the 19th century can be seen quite clearly is astronomy (which was needed for surveying and keeping good and calendars; it was therefore a *useful* science and was smiled on in America). In 1825 John Quincy Adams became president of the United States, and in his first annual address to Congress he issued one of the most resounding defences of science ever heard from a U.S. president. Among other things, he said that, as an American, he could take no pride in the fact that, while Europe boasted having 130 'lighthouses of the skies', there were none anywhere in the North American hemisphere. He was right. For example, in 1839 Harvard, which was still just a college, did not have the wherewithal to buy astronomical equipment capable of providing any observations worth making. When in 1843 the wave of questions about that year's comet publicly revealed the shortcomings of astronomical observation, a citizens' meeting was held in Cambridge, Massachusetts, chaired by textile magnate Abbot Lawrence, to try and do something about it. As a result of that initiative, in 1847 a splendid astronomical observatory was built and equipped with a telescope that cost 20,000 dollars. Significantly, the most important part of the telescope, the lens, had to be made in Germany.

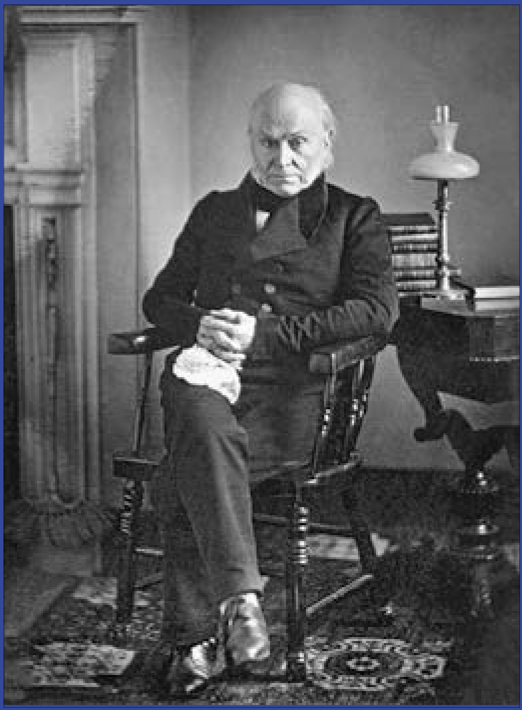
As the century drew to a close, physics and chemistry were far enough along for professional associations to be founded. In 1876 the American Chemical Society was created, although at the time the society was just one of several chemistry associations, essentially serving the interests of chemists in the New York area.

Its membership grew at first, rising from 230 the year of its founding to 314 in 1881, but soon after it dropped, hitting bottom at 204 members in 1889. A series of reforms culminated in a reorganization in 1892, the year when the American Chemical Society may be said to have become a truly national association. In 1895 it had 903 members, and 1,715 in 1900. In 1901 it had 13 local divisions, at least six of which held monthly meetings, and its membership mushroomed: 2,919 in 1905, 5,081 in 1910 and 7,170 in 1914. The figures give some idea of the way the job market was expanding for chemists, fundamentally in the chemical industry, although the federal government also responded to the growth by hiring more chemists. In 1901 22 chemists worked for the government (12 in the Department of Agriculture alone), rising to 32 in 1905 (15 in the Department of Agriculture). In 1911 the figure grew to 292, with the majority, 204, still at Agriculture, but by 1916 chemists were needed in more realms of science, and their number doubled to 716, 397 of whom worked for Agriculture.

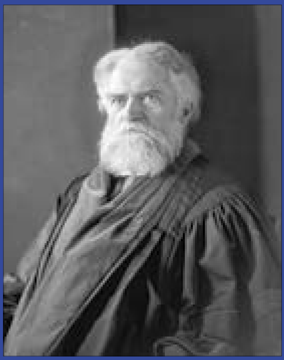
Physicists banded together somewhat later, a detail that may be taken as proof that they were not as useful to the nation as chemists were. Until the last third of the 19th century, the leading figures in American physics were Benjamin Franklin and Joseph Henry, who made major contributions to electricity and electromagnetism. Franklin is especially remembered for having invented the lightning rod, and Henry's contributions included work in telegraphy. It was Arthur Gordon Webster, a Clark University professor who had graduated from Harvard and earned his PhD with Hermann von Helmholtz in Berlin, who saw how little good the National Academy of Sciences was doing, at least for physics. He floated the idea of the American Physical Society in 1899. That same year 38 physicists, including the three leading lights of U.S. physics in that era, namely, Henry A. Rowland (Johns Hopkins), who was elected president, Albert A. Michelson (University of Chicago; he was, remember, the first American Nobel Laureate in Physics, winning the prize in 1907 'for his optical precision instruments and the spectroscopic and metrological investigations carried out with their aid'), and Josiah Williard Gibbs (Yale University, whose work proved fundamental for thermodynamics and statistical mechanics), met at Columbia University in New York to establish the organization formally. And although American physics attained world leadership status in the 20th century, when physics achieved more discoveries fundamental to understanding nature than any other natural science, chemistry still had the jump on physics professionally (which also means 'in terms of social applicability', at least to some extent).

How true this was can be seen by comparing the American Physical Society's membership with the American Chemical Society's roll. In 1909 the APS had 495 members, as opposed to the 4,502 the ACS had that same year. In 1914 the APS

Benjamin Franklin Drawing Electricity from the Sky, Benjamin West, Philadelphia Museum of Art, c. 1816

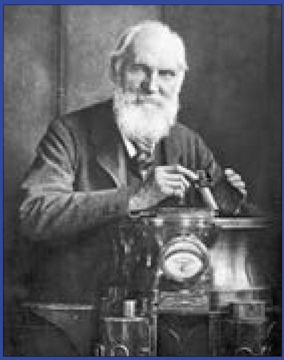


John Quincy Adams, president of the United States. Daguerreotype by Philip Haas, 1843



Simon Newcomb (1905)

Harvard University (College), c. 1638



Lord Kelvin (1902)

Henry A. Rowland

had over 700 members (3,600 in 1939), as compared to the ACS's 7,170 (23,519 in 1939).

To understand why physics and chemistry took off in the U.S. in the early 20th century, we have to look at the country's industry and trade. Manufactured goods came to account for 30 percent more of the national income than agriculture and mining combined. For the first time exports were worth over a billion dollars and outnumbered imports. Like other nations, the U.S. found it needed scientific knowledge to keep up the pace of development. So at least was the feeling of those Americans who had seen the examples of Germany and England. They asked the government to set up a national laboratory to handle the standardization work industry required, and on 3 March 1901 a law was passed creating the National Bureau of Standards.

Science was proved to be a boon for technological advancement (that is to say, business), and private industry reacted faster and more widely to this realization than the federal government. Thomas Edison (well known for inventing the phonograph and the lightbulb) was one of the first to realize at least partially that his business needed science, even though as a self-made inventor he never had a systematic education himself. He spent 1886 to 1888 building a splendid laboratory in West Orange, New Jersey. This laboratory, Menlo Park, had a staff that, while probably far from well rounded, did include a physicist specializing in electricity, chemists who had earned their PhDs in Germany and several former students of colleges that prided themselves on their science programmes. During the first decade of the 20th century, a number of firms in the chemical industry (especially Du Pont in 1902 and Standard Oil of Indiana) opened genuine research laboratories (the first director of the Du Pont laboratory in Repauno, New Jersey, was Charles Reese, a Heidelberg-trained chemist; similarly, when Eastman Kodak founded its laboratory in Rochester, New York, in 1913, it named London-educated chemist C.E. Kenneth Mees as director). At the dawn of the century, the president of the American Chemical Society could say with satisfaction, 'We cannot yet boast like the Germans that a single works employs more than 100 fully trained chemists […], but most of the most important works have teams of 10 to 50 chemists'.

The electricity and communications industries entered the race as well. Their output shot up from 19 million dollars' worth of manufactured goods in 1889 to 335 million in 1914. At the dawn of the new century, the laboratories at General Electric (GE) and American Telephone and Telegraph (AT&T), which had done only routine work in the past, were transformed into research and development centres (GE in 1900 and AT&T in 1904; GE hired Willis Whitney, who had taken his PhD in Leipzig, to direct its lab in Schenectady, New York). The door to innovations in the electric lighting market was thrown wide in 1838, when Belgian

J.B.A.M. Jobard developed a carbon filament in a vacuum, commencing the history of the incandescent light. The industries doing business in this area were therefore forced to seek out physicists, some of whom seized the opportunity to show off their abilities. For example, German chemist and physicist Walther Nernst (whose achievements include the so-called 'third law of thermodynamics') invented a lightbulb with a ceramic filament in 1904. He sold the patent at a handsome profit of a million marks, but his lightbulb was not a success. General Electric hired physicists like Irving Langmuir (who earned his PhD with Nernst) and concentrated on improving tungsten lamps, while AT&T focused on developing new vacuum bulbs. And they were both quite successful. GE came out with a tungsten lamp that was longer lasting, more efficient and cheaper than any other incandescent bulb on the market, thus boosting the company's market share from 25 percent to 71 percent in 1914. AT&T succeeded at developing a highly effective vacuum amplifier that was vital for extending telephone service over long distances.

At first industry had to exert itself to recruit scientists, especially the most creative, who believed their one true calling lay in the halls of ivy. Frank Jewett, who eventually became president of Bell Laboratories (founded in 1925, Bell Laboratories become the world's most famous, most productive science-based industry labs), recalled that when he signed up with Bell his mentor, Nobel Laureate in Physics Albert Michelson, thought 'I was prostituting my training and my ideals'.

Bell Telephone Laboratories was created as a subsidiary of AT&T and Western Electric. Spurred by the need to develop long-distance telephony and meet the challenge posed by radio, the Bell System created its own research branch early, in 1911. AT&T authorized and paid for the basic research, while Western Electric authorized and paid for the development of technology it could apply to its own products. During its first year, Bell Laboratories employed 3,600 people, 2,000 of them technical staff, and it enjoyed a budget of 12 million dollars. Its researchers dealt in radio, electronics, chemistry, magnetism, optics, applied mathematics, sound, energy transformation between electrical and acoustic systems, the generation and modification of electrical currents, instruments, paints and problems concerning the aging and preservation of wood (for telephone poles, of course).

And when GE tried to hire Willis R. Whitney away from the Massachusetts Institute of Technology (MIT) to direct its research lab, it had to promise him he could split his time between GE and MIT. Soon, however, Whitney was absorbed, materially and intellectually, by the problems his new job posed. Many other scientists shared the same experience. Somewhere between 1910 and 1920, industrial research became consolidated as an attractive occupation for U.S. scientists. As we have seen, there were some precedents for this kind of career, especially in Germany and the chemistry of dyes; however, it was in the United States where research laboratories became the most widespread, diversified and entrenched. This was the close of the inauguration of a stage in the history of science and technology that did not fade, but only became more intense as the 20th century rolled on.

The following figures give a good idea of how industrial research laboratories grew. AT&T's research lab increased its workforce from 23 employees in 1913 to 106 in 1916 and raised its budget from 71,000 to 249,000 dollars. When in 1916 the GE lab moved to a new site, it had the finest physics research facilities in the country. Before World War I, physicists employed in industrial laboratories made up only one tenth of the members of the American Physical Society. In 1920, however, they accounted for a quarter of its membership, and by then the number of APS members had doubled. The proportion of papers published by industrial laboratories (which, remember, did not make all their results public) in the country's leading physics journal, *Physical Review,* showed a similar uphill trend: 2% in 1910, 14% in 1915 and 22% in 1920. Twenty years after the creation of GE's research laboratory, over 500 U.S. companies had created their own research facilities.

Science at Universities

Moving on now to universities as the main –or at least traditional– home of science, we find that the American higher education system was, and still is, mostly made up of private schools. At least the most prestigious universities are private. Limiting the list to the oldest institutions only, Harvard (Cambridge, Massachusetts; founded in 1636), Yale (New Haven; 1701), Pennsylvania (Philadelphia; 1740), Princeton (1751), Columbia (New York; 1754), Johns Hopkins (Baltimore; 1875), Cornell (Ithaca; 1865) and Chicago (1890) are all private. These universities all lie on the east coast of the United States, for the obvious reason that it was there the nation's 'colonization' –and its development– began; the west coast caught up later, in the early 20th century (the university foundation dates given here are not hard and fast; they generally indicate that the university's origin can be traced back to that date, which is usually the founding date of the original college that eventually became the university in question).

As explained in the last section, early in the 20th century the U.S. job market in science and technology, particularly physics and chemistry, grew spectacularly. This meant more and more students were going to university to gain an education in these fields. Between 1890 and 1915, American universities conferred roughly 200 PhDs in mathematics, 300 in physics and 500 in chemistry, approximately ten times more than the doctorates given in each of these disciplines in the preceding quarter century. The physical and chemical sciences improved so much in the U.S.

university world that in 1910-1911 the Prussian minister for Education estimated that 12 public and private U.S. universities could compare in quality with Germany's 21 universities. American schools clearly outperformed German institutions in terms of funding; while the average annual budget of a German university was 1.76 million marks, that of a U.S. university was 5.8 million. American expenditure per university had quadrupled since the mid-1890s, while in Germany it had only doubled. True, American universities paid special attention to general education, but, as we have just seen, there were postgraduate programmes. To sum up, the United States was beginning to threaten Germany's leadership on the education front.

Furthermore, at the start of the century some extremely rich men began to take an interest in science and include it in their philanthropic work. In 1901 the Institute for Medical Research was established in New York, funded by the millionaire John D. Rockefeller (in 1956 the institute became a university, Rockefeller University), and in 1902 Scottish industrialist Andrew Carnegie created the Carnegie Institution in Washington, D.C. Both operated with a capital of some 10 million dollars during their early years, which means they produced an amount of interest equivalent to the budget of one of the larger German universities. While Rockefeller's institute focused on biomedicine, Carnegie's provided aid for 'exceptional' researchers in any field. It was, however, in World War I when major foundations really began to assist the physical and chemical sciences in a big way.

Science in the U.S.A was certainly starting to move ahead fast. If we look, for instance, at the money invested in physics, many might be surprised to learn that the United States was the nation that was investing the most, between 1.5 and three times more than Germany, Great Britain and France were each spending. And the distance kept getting bigger. U.S. investments were growing at a rate of 10% per year, while German and British investments were rising 5% per year, and French investments, 2%. These sums are significant, and they help explain the world hegemony the United States later attained in science, quite apart from other 'boosts' the country received, such as the influx of exiled Central European (especially German and Austrian) scientists on the run from Nazi antisemitism. Astronomer Simon Newcomb, president of the Congress of Arts and Science held on the occasion of the 1904 World Fair in St. Louis, which gathered European scientists of such eminence as Henri Poincaré, Wilhelm Ostwald, Ludwig Boltzmann, Ernest Rutherford and Paul Langevin, was not far off the mark when he said in his inaugural address, embellished with all the customary rhetorical flourishes,

> Gentlemen and scholars all! You do not visit our shores to find great collections in which centuries of humanity have given expression on canvas and in marble to their hopes, fears, and aspirations. Nor do you expect institutions and

> buildings hoary with age. But as you feel the vigor latent in the fresh air of these expansive prairies, which has collected the products of human genius by which we are here surrounded, and, I may add, brought us together; as you study the institutions which we have founded for the benefit, not only of our own people, but of humanity at large; as you meet the men who, in the short space of a century, have transformed this valley from a savage wilderness into what is to-day –then may you find compensation for the want of a past like yours by seeing with prophetic eye a future world-power of which this region shall be the seat.

One last anecdote. Before going to St. Louis, Boltzmann spent the summer at the University of California, Berkeley, as part of the school's drive to modernize. Later Boltzmann recounted his impressions of America in *Reise eines deutschen Professors ins Eldorado* (included in his *Populare Schrifthen*, published in 1905). He wrote, 'America will do great things. I believe in these people, even though I have observed a certain clumsiness in them, such as when they handle integral and differential calculus in a theoretical physics seminar. They do it more or less as well as I leap over ditches and climb up and down hills, which one cannot avoid doing on the Berkeley campus'.

The Introduction of Quantum Mechanics in the United States

One important feature of the development and establishment of American science is the fact that European physicists used to visit the United States regularly to lecture and teach, while Americans used to go to European universities to learn. From among the early trips by European scientists, I will mention those made by Irishman John Tyndall in 1872-1873 to give a series of popular lectures; William Thomson (later Lord Kelvin) in 1884 to deliver a course of twenty lectures at Johns Hopkins University, which led to the famous book *Baltimore Lectures on Molecular Dynamics: The Wave Theory of Light* (London, 1904); and J.J. Thomson (director of Cambridge's Cavendish Laboratory) in 1896 to help celebrate the 150th anniversary of the founding of Princeton University, during which he gave four lectures on electrical conductivity in gases, published soon afterward as *The Discharge of Electricity Through Gases* (New York, 1898). Now then, Maria Goeppert Mayer's own particular scientific world was quantum physics, so by what paths did knowledge of her subject reach the United States before she did in 1930?

Before the Heisenberg-Schrödinger-Dirac theory of quantum mechanics (1925-1926), three physicists who were particularly well respected in Europe, Hendrik Antoon Lorentz, Max Planck and Wilhelm Wien, gave lectures at Columbia University, New York, in 1906, 1909 and 1913, respectively. Also, as mentioned in chapter 1, Max Born visited the University of Chicago as Abraham Michelson's guest in 1912 to deliver a series of lectures on relativity. In his autobiography he recorded an interesting point of his trip that not only reflects his impressions and the contrast between old Europe and young America, but also offers some interesting particulars about Michelson's scientific style:

> Almost every British scientist of today has at some time been in the United States, but in Germany before the first world war this was not the case. Very few of my colleagues had crossed the Atlantic. Among my circle, only Max Abraham had been there, spending some years in the State University of Illinois at Urbana, but he did not like the American way of life and had returned. So it was quite an adventure for me to go on this trip in April 1912. I sailed from Bremen on one of the fast German steamers, had a colossal flirtation with a lovely American girl named Mabel but arrived safely in New York.
>
> Though I enjoyed the grandeur of Manhattan's skyscrapers and the turbulent life of the city, I was disgusted by the shocking social contrast between the rich and the poor. I watched the latter in the crowded quarters of the Eastside and Harlem. On the other hand I called on some wealthy families in their beautiful houses near Central Park. These families were mostly those of great Jewish physicians, to whom I had introductions from my father's friends, for instance from old Ehrlich […].
>
> From New York I went straight to Chicago and stayed a few weeks in the Michelson's lovely house. Then I moved into a room in one of the students' dormitories. It was one of the most miserable places I have ever lived in, comparable only with German military barracks: filthy, bleak, depressing. But there was nothing else to be had, for the nearest decent hotel was some miles from the University. When in July I travelled north to visit Mrs Michelson and her children in their summer resort on Lake Michigan, I found the sleeper berth in the train luxurious compared with the dirty and uncomfortable bed in my room at the dormitory […].
>
> Another trip from Chicago took me to Niagara Falls and over to Toronto in Canada.

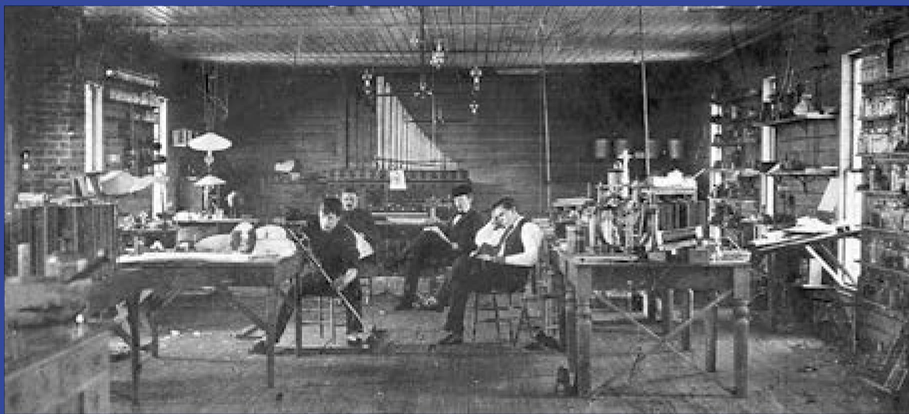
Shortly before leaving Chicago, he attended the Republican National Convention to nominate the next president of the United States, held 'in a colossal hall'. 'The strongest candidate', Born said, 'was Theodore Roosevelt, the first of this name'. 'The crazy procedure of these conventions,' he added, 'has been described often enough, but I do not think anybody who has not seen it can imagine the pandemonium. The walls were plastered with large pictures of the candidates, mainly Roosevelt, and with heads of the elk, his emblem. A number of bands were playing, sometimes different tunes simultaneously.' He then toured the United States before returning home to Europe. What he wrote about Michelson is particularly interesting:

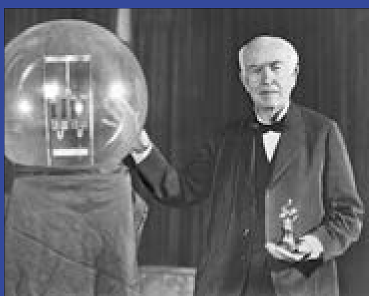
One important feature of the development and establishment of American science is the fact that European physicists used to visit the United States to lecture and teach, while Americans used to go to European universities to learn.

> My work in Chicago consisted of lecturing on the theory of relativity to a group of research students and of younger members of the staff […]. I also worked a little in the laboratory. Michelson gave me one of his wonderful concave gratings and showed me how to use it. So I spent pleasant hours in focusing, observing and photographing spectra of many substances, and I was very happy when I got a plate which Michelson approved of. But he was only interested in the technique of producing faultless photographs and hardly at all in the meaning of all the lines and bands seen on the plates. I could not fail to observe numerous regularities, in particular in the spectrum of the carbon arc, and I asked Michelson whether he knew an explanation. His reaction was very curious. In one of the bands which seemed to me obviously to follow a simple law (I suppose it was one of the CN-bands) he showed me the existence of irregularities, consisting of lines which were displaced from the position where they should have been, and said: 'Do you really think that there is a simple law behind it if horrible things like that happen?' I do not know whether he thought that nature acts in a haphazard way; but he was certainly not interested in the secret behind these phenomena. In fact the first step towards lifting the curtain had at that time already been made, as I soon learned, in the establishment of Deslandre's formula for simple bands which led finally to the explanation of the complicated band system, including those apparent irregularities.

Michelson, in other words, like most U.S. physicists of his day (the main exception being Josiah Willard Gibbs, who had died in 1903), was basically involved in experimentation and did not care much what theoretical system would make sense of his experimental results. And let us not forget that one of the driving forces behind quantum mechanics was precisely the search for a theory to explain the positions of the lines of the spectra of the various chemical elements.

The flow of European visitors to the U.S. resumed after World War I. Hendrik Lorentz returned to America in early 1922 to teach a course at the California Institute of Technology on problems of modern physics. The material was published under the same title as the course, *Problems of Modern Physics* (Boston), in 1927, prepared by English physicist and mathematician Harry Bateman,

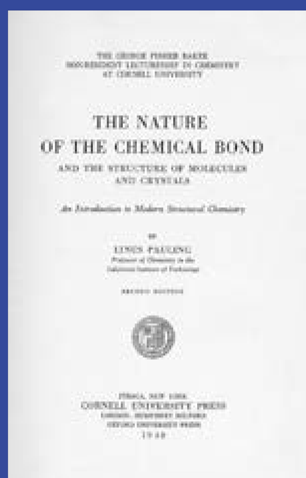




Thomas Edison and his
Menlo Park laboratory

Hendrik A. Lorentz
at the University
of Wisconsin
(1922)

Norbert Wiener and Max Born



THE NATURE
OF THE CHEMICAL BOND
AND THE STRUCTURE OF MOLECULES
AND CRYSTALS

An Introduction to Modern Structural Chemistry

LINUS PAULING

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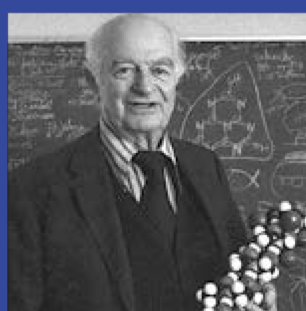


Title page of Linus Pauling's
seminal work and portrait of
the author in his final years

who had been at Caltech since 1917. Through him we know that Lorentz tackled subjects including a good number of issues we might call 'old quantum theory', ranging from the Rutherford-Bohr atom to Niels Bohr's complementarity principle to the movement of quanta in superimposed beams of light. British physicist Charles Galton Darwin, an estimable contributor to quantum physics, also called at Caltech, spending the entire 1922-1923 academic year there. From September 1922 to April 1923, Arnold Sommerfeld made the rounds through various institutions (Wisconsin; Urbana, Illinois; the National Bureau of Standards). From October to December 1923, Niels Bohr visited a number of universities: Amherst College, Harvard, New York, Princeton, Yale (Silliman Lectures), Chicago and Ann Arbor. Peter Debye spent the 1924 spring semester at the Massachusetts Institute of Technology. That same year (1924), Paul Ehrenfest, Arthur Eddington and Hendrik Lorentz also travelled to the United States.

Apart from any interest in getting to know the vibrant 'New World', many top European physicists visited the United States because of the economic consideration they stood to earn. For example, Bohr received 3,000 dollars from Amherst College and 1,250 from Yale, while Sommerfeld got 4,000 dollars for his semester in Wisconsin. The letter that Max's wife Hedwig wrote to Einstein on 2 October 1920 speaks for itself: 'My husband feels an inclination to slay the golden calf in America and to earn enough through lecturing to build a small house in Göttingen to his own requirements. Should you, by any chance, have the opportunity to recommend someone to lecture over there, please suggest Max. He would be able to go there in February, March and April'.

In fact, Max Born was the first to bear tidings of the new quantum theory, Heisenberg's matrix mechanics, to America, in 1925. On 2 November 1925, accompanied by his wife, Born left Göttingen for Cambridge, Massachusetts, to teach a course at MIT that started on 14 November. Maria Goeppert was then just beginning to study physics.

> My lectures at the Massachusetts Institute of Technology [wrote Born in his memoirs] contained in the first part an outline of crystal dynamics, and in the second the elements of quantum mechanics. This was the first systematic presentation of this new field. When I began these lectures, the paper by Jordan and myself, in which matrix calculus was introduced, was still in the press; the big three-man paper [the paper known as the 'Drei-Männer Arbeit'] by Heisenberg, Jordan and myself appeared just at the end of the lecture course.

Therefore, the matrix (Heisenberg) version of quantum mechanics was the first theory known in the United States. Unsurprisingly, Born said nothing about the wave formulation; he was unaware of it, because the first article Erwin Schrödinger published about it, 'Quantisierung als Eigenwerproblem' ('Quantization as an Eigenvalue Problem'), reached the editors of *Annalen der Physik* on 27 January 1926. What Born did know was Paul A.M. Dirac's formulation. 'The day before I left Göttingen', Born explained to Thomas S. Kuhn in an interview on 17 October 1962 (the transcription is at the American Institute of Physics' Niels Bohr Library), 'there appeared a parcel of papers by Dirac, whose name I had never heard. And this contained exactly the same as was to be in our paper. In turning it in, we were about four weeks earlier than him, but not in publication. And I was absolutely astonished. Never have I been so astonished in my life; that a completely unknown and apparently young man could write such a perfect paper. But I didn't know who he was. Only a half year later, when I came to England, I met him'.

During his time at MIT, Born met a then-young Norbert Wiener (1894-1964), who would later become famous for his idea of a new science, cybernetics. Wiener attended Born's lectures and drew the great man's attention by arguing that the matrices of the quantum mechanics proposed by Heisenberg, and developed with the help of Born himself and Pascual Jordan, could be considered operators (a mathematical entity) acting upon vectors in multidimensional spaces, and by suggesting that matrix quantum mechanics be generalized, converting it into a kind of operational mechanics. Together they developed the idea further, publishing a paper ('A New Formulation of the Laws of Quantization of Periodic and Aperiodic Phenomena') that appeared in the January 1926 issue of the *Journal of Mathematics and Physics* (also published in German in *Zeitschrift für Physik*). This was the first article on the new quantum mechanics in the United States.

According to Born, the fees MIT was paying him were not enough to cover his expenses. So, Born wrote a book while he was at Cambridge, to earn some extra money (he drafted it in German, and the teaching assistants assigned to him translated it). The book appeared in 1926 as an MIT publication entitled *Problems of Atomic Dynamics.* In the preface Born explained that it contained exactly what he had covered in class, nothing more. 'The lectures', he added, 'do not purport to be a text-book –for of these we have enough– but rather an exposition of the present status of research in those regions of physics in which I myself had made investigations, and of which I therefore believe that I can take a comprehensive view. In the short time that was at my disposal, I could neither seek for completeness nor consider minutiae. It was my purpose to present methods, objects of investigation, and the most important results. I have avoided references and have only occasionally named individual authors'.

Born's lectures were a success. At least, so we can conclude from the fact that his farewell lecture of 22 January 1926 was attended by 1,000 people. Edwin Kemble, then a young teacher at nearby Harvard University, stated in a review of *Problems of Atomic Dynamics* printed in *Physical Review* (1926), 'It is a happy experience for American physicists that Professor Born was engaged to deliver these lectures on atomic dynamics just as the first accounts of the new matrix mechanics were appearing in Germany. The prompt publication of the text of the lectures with their summary of the first results obtained by this method should be of great service in helping us to keep up with the stream of thought in a field in which we have been prone to lag behind'.

MIT was not the only place Born preached the new quantum gospel. The same day that he delivered his last lecture there, he caught a train to the headquarters of General Electric's laboratories in Schenectady, where his friend the distinguished scientist Irving Langmuir (among others) worked. From there he made the rounds of Ithaca (Cornell University), Buffalo, Chicago, Pasadena (Caltech), Berkeley, Madison, New York, Princeton and Washington, D.C. Altogether he toured twelve major universities and research facilities. On 23 March 1926 he caught a boat back to Göttingen. He had been a great 'missionary' of the good quantum news.

The appetite of American physicists –and their institutions– for new European knowledge only grew from then on. For example, in 1927 Erwin Schrödinger, Abram Ioffe, Arthur Milne and William L. Bragg spent time in the United States; in 1928, Léon Brillouin, James Franck, Kramers and Weyl; in 1929, Werner Heisenberg, Paul Dirac, Alfred Landé and Friedrich Hund; in 1930, Enrico Fermi, Albert Einstein, Max von Laue, Yakov Frenkel, Otto Stern and Gregor Wentzel. But American universities did not just want guests; they wanted European professors who would come to stay. In the 1920s university scouts were especially anxious to find physicists, particularly theoretical physicists (after all, this was the heyday of theoretical physics). Paul S. Epstein, one of Arnold Sommerfeld's students, was hired by Caltech in 1922; in 1923 Michigan had Oskar Klein as its guest, and when he left the university he convinced Otto Laporte (in 1926) and Samuel Goudsmit and George Uhlenbeck (in 1927) to join the Michigan physics department. Llewellyn H. Thomas (1929) and Alfred Landé (1931) went to Ohio; Karl F. Herzfeld, to Johns Hopkins University (1926), and John von Neumann and Eugene Wigner, to Princeton (1930), first on a part-time basis, and later permanently. Some of these names will reappear in our next chapter in connection with Maria Goeppert Mayer.

Europeans were going to the United States, yes, but the flow went both ways. In the years immediately following the development of quantum mechanics, more young American physicists studied in Germany than ever before, especially theoretical physicists. At least 25 physicists (including some physical chemists) who later attained renown (such as Gregory Breit, Edwin C. Kemble, Robert S. Mulliken, Linus Pauling, John Robert Oppenheimer, Boris Podolsky, Edward U. Condon, Carl Eckart, Howard P. Robertson, Isidor Isaac Rabi, John C. Slater and John H. van

Vleck) attended one of the Central European universities working with quantum mechanics between 1926 and 1930. Most of them flocked to Göttingen, followed by Zurich (ETH, where Pauli and for a time Debye and Schrödinger taught), but some went to Berlin, Leipzig (Heisenberg's school) and Munich (with Sommerfeld). Copenhagen (Bohr) and Cambridge (Rutherford, Dirac) were not as popular.

The American scientists who went to Europe to study quantum mechanics came from a range of institutions, but a great number of them hailed from Harvard, Caltech, Berkeley or MIT. The rest were from other universities (Princeton, Minnesota, Columbia, Michigan, Chicago, Cornell and Yale). Some American foundations actually helped make these trips possible. The Guggenheim Memorial Foundation Fellowship Program, started in 1925, financed 40% of the trips, and the Rockefeller Foundation's International Education Board and the National Research Council also made major contributions.

In the Old World, these young men learned techniques of the new quantum universe and caught a glimpse of its quandaries. And on their return to their homeland many continued pursuing interests and work in the quantum vein. For instance, on their homecoming Slater (who had been in Leipzig and Zurich) and van Vleck (Copenhagen, Cambridge and Oxford) taught quantum mechanics at Stanford University in the summer semesters of 1926 and 1927, respectively. Later, in 1929, Slater taught classes on wave mechanics at the University of Kentucky, and van Vleck continued propagating quantum theories throughout the years he taught at the University of Minnesota and the University of Wisconsin. Breit (Zurich) did likewise at Johns Hopkins, and Condon (Göttingen, Munich), at Columbia. The case of Linus Pauling, the 'Einstein of chemistry', as he has come to be called, is particularly interesting, because it transcends the field of physics (the most typical realm of quantum physics), demonstrating the value of quantum mechanics in other areas, like chemistry (which was, let us not forget, Joseph Mayer's own discipline).

Pauling attended Oregon Agricultural College (now Oregon State University) from 1917 to 1922, when he enrolled at the California Institute of Technology as a doctoral student. He took his PhD in 1925 with a dissertation entitled 'The Determination with X-rays of the Structure of Crystals'. He had by then been interested in the nature of chemical bonds for some time. How that interest came about is something he explained in a manuscript published quite a few years later:

> During my early years as a scientist, beginning in 1919, I had a special interest in the problem of the nature of the chemical bond; that is, the nature of the forces that hold atoms together in molecules, crystals, and other substances. Much of my work during this early period was directed toward a solution of this problem, by application of both experimental and theoretical methods. As soon as quantum mechanics was discovered, in 1925, I began striving to apply that powerful theory to the problem.

After completing his doctorate, Pauling got a fellowship from the John Simon Guggenheim Memorial Foundation to pursue further studies in Europe. His plan was to spend a year in Munich with Arnold Sommerfeld, and to take advantage of the opportunity to visit Niels Bohr in Copenhagen, Max Born in Göttingen and the Braggs (father and son) in Manchester as well. He reached Munich in April 1926. It so happens that Sommerfeld was teaching a class on Schrödinger's recently unveiled wave mechanics. Pauling took the class. While he was in Munich, Gregor Wentzel, who had earned his doctorate with Sommerfeld and at that time was a *Privatdozent*, was using the novel concept of spin and the new quantum mechanics to study complex atoms. In those surroundings Pauling became convinced that quantum mechanics were necessary for solving chemistry problems. He wrote to Arthur Noyes (the Caltech chemistry professor who had helped Pauling get the Guggenheim fellowship) on 12 July 1926, saying, 'I am now working on the new quantum mechanics, for I think that atomic and molecular chemistry will require it. I am hoping to learn something regarding the distribution of electron-orbits in atoms and molecules'.

After a year with Sommerfeld, Pauling spent a month at the Niels Bohr Institute in Copenhagen, but the person who influenced him the most there was not Bohr, but Dutchman Samuel Goudsmit, with whom Pauling published an influential book, *The Structure of Line Spectra* (McGraw-Hill, New York) in 1930 (by which time Goudsmit was teaching at the University of Michigan). Until autumn 1928, when he went back to America, Pauling spent the rest of his time in Zurich, Schrödinger's stomping grounds. But the real connections Pauling forged there were not with Schrödinger, but with two young assistants, Walter Heitler and Fritz London, who published a paper in early 1927 using the recently formulated quantum mechanics to explain the hydrogen molecule's stability. The paper was entitled 'Interaction Between Neutral Atoms and the Homopolar Bond According to Quantum Mechanics'. What Heitler and London did was study the interaction between two hydrogen atoms, with the result that they could find the chemical bond as a consequence of a quantum mechanical 'resonance', a concept that had been introduced in quantum mechanics the year before by Heisenberg in connection with the quantum states of the helium atom.

'I immediately began applying the Heitler-London theory', Pauling wrote years later, 'to more complicated systems, and in 1928 I published a brief paper on the shared-electron-pair theory of the chemical bond [...]. In 1931, stimulated in part by the work of John C. Slater [. . .], I published a detailed discussion of the quantum mechanics of the covalent bond'.

In 1939 he boiled his work down into one great book, *The Nature of the Chemical Bond*. In it he said,

> For a long time I have been planning to write a book on the structure of molecules and crystals and the nature of the chemical bond. With the development of the theory of quantum mechanics and its application to chemical problems it became evident that a decision would have to be made regarding the extent to which the mathematical methods of the theory would be incorporated in this book. I formed the opinion that, even though much of the recent progress in structural chemistry has been due to quantum mechanics, it should be possible to describe the new developments in a thorough-going and satisfactory manner without the use of advanced mathematics. A small part only of the body of contributions of quantum mechanics to chemistry has been purely quantum-mechanical in character; only in a few cases, for example, have results of direct chemical interest been obtained by the accurate solution of the Schrödinger wave equation [...]. The principal contribution of quantum mechanics to chemistry has been the suggestion of new ideas, such as the resonance of molecules among several electronic structures with an accompanying increase in stability.

Quantum chemistry now had a canon textbook of the sort that shapes an entire discipline.

The Emigration of European Scientists to the United States

In previous chapters we have encountered some of the consequences of the racial policies enacted by Adolf Hitler's Nazi government for some of the scientists employed at German universities, and we have seen the cases of Born, Franck, Blau and Sponer. As German rule expanded across the map, so too did the area where racial measures applied. As of March 1938, with the 'Annexation' (*Anschluss*), they included Austria as well. Meanwhile, Italian fascism, mirroring German Nazism and anxious to win German sympathies, culminated its own campaign of antisemitic propaganda. Benito Mussolini began to wage war against the Jews in July 1938 when he instructed the secretaries of the ministries he himself headed (War, the Navy and Aviation) not to admit Jews at military academies. On 14 July the *Giornale d'Italia* published what was known as the *Manifesto della Razza*, in which 'a group of fascist researchers, Italian university professors' who had worked 'with the support of the Ministry of Popular Culture', declared that 'fascism confronts racial problems'. On 17 August a Department of the Interior circular ordered prefects not to appoint Jews to official posts. These orders were supplemented from September through November with others tightening the persecution of Jews, be they Italian or foreign.

Italy's incomparable work in mathematics was especially affected. The famed mathematicians Vito Volterra, Tullio Levi-Civita, Federigo Enriques, Guido Castelnouvo, E.E. Levi and C. Segre were of Jewish origin. Italy also lost its most precious jewel, Enrico Fermi (1901-1954), whose wife was of Jewish descent. As we shall see in the following chapter, Fermi was associated with Maria Goeppert Mayer. When Fermi went to Stockholm to collect his Nobel Prize in Physics in December 1938, he simply did not return to his country. Instead, he went to the United States, where he arrived with his family on 2 January 1939.

Italy also lost other scientists who were less famous at the time, such as Salvador Luria, who in 1969 won the Nobel Prize in Physiology or Medicine for his work on the mechanisms of bacteriophages and viral illnesses. Luria wrote in his autobiography, 'I was torn in 1938 between the call of duty to my parents, which meant for me to stay with them and return to medicine, and the call of freedom, driving me where I could be a scientist. The latter won, I must say, with the full approval of my parents, who felt that once abroad I would be safer myself and might also be of help to them. The news from Germany –the *Kristallnacht* later that year as the outstanding example– made clear to me that the persecution of Jews was not likely, even in Italy, to remain merely a nonviolent humiliation'.

Some of the leading experts in the physical sciences, mathematics and biology who emigrated to the United States

Physicists: Hans A. Bethe (1935), Felix Bloch (1934), Peter Debye (1940), Albert Einstein (1933), Enrico Fermi (1939), James Franck (1933), Philipp Frank (1938), Maurice Goldhaber (1938), Victor F. Hess (1938), Fritz London (1939), Franco Rasetti, Bruno Rossi (1939), Emilio Segré (1938), Otto Stern (1933), Leo Szilard (1938), Edward Teller (1935), Victor Weisskopf (1937).

Astrophysicists: Walter Baade, Rudolph Minkowski (1935), Martin Schwarzschild (1937).

Chemists: Kasimir Fajans (1936), Herman F. Mark (1940), Eugene Rabinowitch (1938; in 1947 he switched to botany and biology).

Mathematics: Emil Artin (1937; he returned to the German Federal Republic in 1958), Salomon Bochner (1933), Richard Courant (1934), William Feller (1939), Kurt Gödel (1940), Karl Menger (1937), Richard von Mises (1939), Emmy Noether (1933), Alfred Tarski (1939), Stam Ulam (1936), Hermann Weyl (1933).

Biochemists and biomedical scientists: Konrad Bloch (1935), Ernst Caspari (1938), Erwin Chargaff (1934-35), Max Delbrück (1937), Heinz Fraenkel-Conrat (1936), Kurt Goldstein (1934), Fritz Lipmann (1939), Otto Loewi (1940), Salvador Luria (1938), Otto Meyerhof (1940), David Nachmansohn (1939), Hans Neurath (1935).

Participants at the 1930 Solvay Conference

E.T.S. Walton,
E. Rutherford
and J. Cockroft

Attendees of the Rome nuclear physics conference (October 1931). Those shown include Millikan, Compton, Curie, Richardson, Marconi, Bothe, Rossi, Stern, Debye, Bohr, Aston, Ellis, Sommerfeld, Perrin, Corbino, Rasetti, Brillouin, Ehrenfest, Fermi and Mott

Most of these scientists wound up making the United States their home. The illustrious German nuclear physicist Hans Bethe (1906-2005) sent Arnold Sommerfeld a letter telling of a magnificent case. Bethe emigrated to Great Britain in 1933, where he spent some time at the University of Manchester and the University of Bristol, after which he went to Cornell in the United States in 1935. He became a nationalized American citizen in 1941. In 1967 he won the Nobel Prize in Physics for his 1938 contributions to the theory of nuclear reactions, especially energy production inside stars. After the war Bethe received from his former teacher Sommerfeld a highly tempting offer to take over Sommerfeld's chair at the University of Munich, one of the peak academic positions in German physics. Here is Bethe's answer, dated 20 May 1947:

> I was very gratified and very honored that you have thought of me as your successor. If everything since 1933 could be undone, I would very happy to accept your offer. It would be lovely to return to the place where I learned physics from you, and learned to solve problems carefully. And where subsequently as your *Assistent* and as *Privatdozent* I had perhaps the most fruitful period of my life as a scientist. It would be lovely to try to continue your work and to teach the Munich students in the same sense as you have always done. With you one was certain to always hear of the latest developments in physics, and simultaneously learn mathematical exactness, which so many theoretical physicists neglect today.
>
> Unfortunately, it is not possible to extinguish the last fourteen years [...]. For us who were expelled from our position in Germany, it is not possible to forget. The students of 1933 did not want to hear theoretical physics from me (and it was a large group of students, perhaps even a majority), and even if the students of 1947 think differently, I cannot trust them. What I hear about the nationalistic orientation of students at many universities starting up again, and about many other Germans as well, is not encouraging.
>
> Perhaps still more important than my negative memories of Germany is my positive attitude toward America. It occurs to me (already since many years ago) that I am much more at home in America than I ever was in Germany. As if I was born in Germany only by mistake, and only came to my true homeland at 28. Americans (nearly all of them) are friendly, not stiff or reserved, nor have a brusque attitude as most Germans do. It is natural here to approach all other people in a friendly way. Professors and students relate in a comradely way without any artificially erected barrier. Scientific research is mostly cooperative, and one does not see competitive envy between researchers anywhere. Politically most professors and students are liberal and reflect about the world outside –that was a revelation to me, because in Germany it was customary to be reactionary (long before the Nazis) and to parrot the slogans of the German

> National [*Deutschnationaler*] party. In brief, I find it far more congenial to live with Americans than with my German *Volksgenossen*.
>
> On top of that America has treated me very well. I came here under circumstances which did not permit me to be very choosy. In a very short time I had a full professorship, probably more quickly than I would have gotten it in Germany if Hitler had not come. Although a fairly recent immigrant I was allowed to work and have a prominent position in military laboratories.
>
> Now, after the war, Cornell has built a large new nuclear physics laboratory essentially 'around me'. And two or three of the best American universities have made me tempting offers. I hardly need mention the material side, insofar as my own salary is concerned and also the equipment for the institute. And I hope, dear Mr. Sommerfeld, that you will understand; Understand what I love in America and that I owe America much gratitude (disregarding the fact that I like it here). Understand, what shadows lie between myself and Germany. And most of all understand, that in spite of my 'no' I am very grateful to you for thinking of me.

Women Scientists in the United States

In women's education, as in many other things, the United States moved along different coordinates to those of the European nations. Its great size, its large number of schools of all kinds, its customs and its lifestyle explain the differences. In fact, the United States had a more open attitude than, say, England, although that does not mean that women trying to pursue a scientific career had it any easier in America.

The movement in favour of higher education for women began to make headway in the United States in the 1860s. Although Oberlin College (in Oberlin, Ohio) was open to both sexes since its foundation in 1833, the real boost to women's education came in 1865 when Vassar College opened in Poughkeepsie, New York. By 1870 many of the state universities accepted female students, especially those schools that had been created using profits from sales of public land. In point of fact, in 1870 Myra Bradwell (1831-1894) wrote to the Illinois Supreme Court for permission to practice law. The court raised objections, but in 1873 the state legislature passed a law declaring that 'no person shall be precluded or debarred from any occupation, profession or employment (except the military) on account of sex'. And in 1880 a woman was chosen to sit on the Supreme Court of the United States. At first Cornell and the University of Michigan were the schools that did the most for women's education in science, but that changed with the establishment of a number of women's colleges (Smith 1871, Wellesley 1875, Bryn Mawr 1885 and the Baltimore College for Women 1885). In addition to paying attention to science, women's colleges employed a good number of women in teaching. Nevertheless, within the field of science, women graduates were not expected to become professional scientists. At most they might dabble in scientific undertakings as amateurs. Even the women themselves displayed a remarkable ambivalence. Williamina P. Fleming, a 'computer' (a woman who performed complicated computations by hand or with very primitive machines) at Harvard College Observatory, about whom I shall speak later, wrote in 1893, 'We cannot maintain that in everything woman is man's equal. Yet in many things her patience, perseverance, and method make her his superior. Therefore, let us hope that in astronomy, which now affords a large field for women's work and skill, she may, as has been the case in several other sciences, at least prove herself his equal'.

Anyway, a fair wind was blowing for women. The 1893 Chicago World's Fair, the great Columbian Exposition designed to celebrate the discovery of the New World, provides a good example. All previous world's fairs had been leveraged by their host nations to show the world the best of what they had, and the United States naturally aimed to do the same. This is why it was so significant that women were one of the topics chosen to figure prominently at the Chicago Fair. One of the exhibition venues was the Woman's Building, where exhibits showcased women's achievements in education, arts, sciences and industry. Moreover, the exposition had a national committee of 115 members, popularly referred to as the Board of Lady Managers. 'Even more important than the discovery of Columbus', said board chair Bertha Honoré Palmer before an audience of over 100,000, 'is the fact that the government has just discovered women'. The World's Congress of Representative Women was also held, starting on 15 May; 330 women spoke before a total audience of 150,000.

What sciences were the first American women undergraduates studying? Margaret Rossiter gathered some data that throw light on American female scientists' degrees before 1920. In that period, 414 women earned degrees in scientific fields at 98 institutions. Their majors break down as follows: botany, 80; zoology, 80; psychology, 67; medical sciences, 45; mathematics, 41; chemistry, 35; geology, 23; physics, 23; astronomy, 18; and anthropology, two. An appreciable proportion of these students (94) were from either Wellesley, Vassar or Smith College.

A similar indicator, but for the interwar period, tells us the number of PhDs awarded to women at U.S. universities.

PhDs earned by Women (1920-1938)

FIELD	TOTAL	W	%
Medicine	1,194	254	21.3
Chemistry	6,052	487	18.4
Physics	1,831	186	14.7
Zoology	2,503	395	15.8
Mathematics	1,954	132	13.8
Botany	1,098	219	19.9
Psychology	1,559	417	26.7
Anthropology	1,197	159	29.9

One of the fields where U.S. women were relatively numerous in comparison to men is astronomy and astrophysics, although, as we shall soon see, women were always cast in supporting roles. The end of the 19th century saw a shift in the balance of the leading astronomical observatories from the Old World to the New. Technology had advanced so far that it was becoming increasingly costly to build ambitious observatories, and in the United States the necessary sums of money could be procured, either from universities or from private benefactors with a penchant for astronomy, such as Percival Lowell, the wealthy Boston investor who made the creation of the Lowell Observatory in 1894 possible. In addition, the prevailing geographical and weather conditions of the more-developed European nations could not easily compete with the variety of conditions to be found in the United States, for instance, at California's Mount Wilson Observatory, with its 2.5-metre mirror, and Mount Palomar Observatory, with its five-metre mirror (Edwin Hubble discovered that the universe is expanding at Wilson Observatory).

As observatory size and complexity increased, so too did the need for assistants, staff specializing in all kinds of duties. One of these duties was to classify photographs, locate stellar objects on photographic plates and perform various sorts of mechanical calculations. Without today's facilities this work was done by hand. These were jobs for underlings. Between 1875 and 1920, at least 164 women worked as assistants at the United States' leading astronomical observatories (decades later, in the days before automatic data analysis, much the same happened in the analysis of big particle accelerators' photographs of subatomic particle collisions: women were hired to measure data for the physicists, mainly men, to study.)

To illustrate how hard it was for women to lead a full scientific research career, let us look at a few examples from the field of astronomy and astrophysics.

Williamina Fleming

In 1881 Edward Pickering, director of the Harvard College Observatory since 1877, became so enraged at his male assistant's clumsy copying and calculations that he is said to have claimed that his maid could do a better job of it. He immediately hired Williamina P. Fleming (1857-1911), a 24-year-old Scottish emigrant, a public school graduate and separated mother. Fleming remained with the observatory for 30 years, eventually becoming a respected astronomer. One of her more outstanding claims to fame is that, of the 23 novae identified in the West from 1572 to 1899, Fleming identified seven (one of them, discovered in 1895, was Z Cen, with a stellar magnitude of 7, and was actually a supernova). Moreover, the 1890 *Draper Catalogue of Stellar Spectra*, a go-to resource for astrophysicists in that era, was largely the result of Fleming's efforts. The catalogue classified spectra and also gave the magnitudes (down to 8) of over 10,000 stars.

One of Fleming's obligations was to hire and direct a group of women assistants who were paid a modest wage to classify photographs of stellar spectra. Between 1885 and 1900 she had 20 such assistants, including graduates of colleges like Vassar, Wellesley and Radcliffe. Henrietta S. Leavitt (who graduated from Radcliffe College in 1892), whom we shall meet again soon, was one of them.

Obviously Pickering thought highly of Fleming... but only up to a point. She was, after all, 'nothing more than a woman'. Fleming in turn respected her director, but not enough to swallow her indignation when she was treated in a way she considered unfair in comparison to her male colleagues. On 12 March 1900, she wrote in her diary (now in the archives of Harvard University, which ran the observatory),

> I had some conversation with the Director regarding women's salaries He seems to think that no work is too much or too hard for me, no matter what the responsibility or how long the hours. But let me raise the question of salary and I am immediately told that I receive an excellent salary as women's salaries stand. […] Sometimes I feel tempted to give up and let him try some one else, or some of the men to do my work, in order to have him find out what he is getting for $1,500 a year from me, compared with $2,500 from some of the other assistants. Does he ever think that I have a home to keep and a family to take care of as well as the men? But I suppose a woman has no claim to such comforts. And this is considered an enlightened age!

Henrietta Leavitt

As I mentioned before, one of Fleming's assistants was Henrietta Swan Leavitt (1868-1921). Her case, too, is illustrative.

In the first decade of the 20th century it became clear that the Magellanic Clouds contained a large number of variable stars (stars whose brightness changes regularly). Leavitt was one of the Harvard College Observatory women tasked with analysing photographs in search of stars of this kind. Her work resulted in a paper published in 1908 in which she analysed photographic plates taken at Harvard's observatory in Arequipa, Peru (southern hemisphere), '1777 Variables in the Magellanic Clouds' (*Harvard College Observatory Annals* 60, No 4, pp. 87-108, 1908). Far from just looking for new variable stars, Leavitt also set her sights on determining the periods at which their brightness varied. In this 1908 paper she reported the periods of 16 variable stars, noting, 'attention was called to the fact that the brighter variables have the longer periods'. By 1912 she had extended her calculations to 25 stars and established that there was a roughly linear relationship between the logarithm of a star's period and its apparent brightness (as observed on Earth). The relationship Leavitt had discovered was of great potential value, because a number of mathematical operations and certain observations could be applied to deduce the absolute (intrinsic) brightness of a star, which in turn meant astronomical distance could be found. Leavitt seems to have realized this but was not allowed to do anything about it. That job was reserved for astronomers like Harlow Shapley and Edwin Hubble, both of the Mount Wilson Observatory. Shapley used Leavitt's relationship to calibrate the absolute magnitudes of Cepheids, thus taking the decisive step toward using Cepheids as distance indicators. In fact, he himself determined how far away the star clusters surrounding the Milky Way lie. Hubble used Cepheids in 1924 to put an end to a centuries-long debate by demonstrating that our galaxy, the Milky Way, is not the end of the universe, but that the universe is populated by galaxies that are separated from each other. Some years later, in 1929, using new Cepheids, he proved that the universe is expanding.

Cecilia Payne-Gaposchkin

In 1923 Harvard rolled out a graduate studies programme in astronomy to address a shortage that had been one of the observatory's greatest weaknesses since its creation. The fact that approximately a third of the astronomy PhDs conferred in the United States between 1930 and 1945 were from Harvard gives an idea of the programme's success. The very first person to earn a doctorate under that programme was a woman, Cecilia Helena Payne-Gaposchkin (1900-1979), an Englishwoman who had attended Newnham College, Cambridge.

Payne-Gaposchkin's dissertation was entitled 'Stellar Atmospheres' and was published in 1925 as the first issue of the *Harvard Observatory Monograph*. Until 1925 the general belief was that the sun and stars contained the same elements as the Earth, and in approximately the same proportions. This assumption rested on spectroscopic observations and was consistent with the belief that our planet had been formed with material from the sun. In 1920 physicists M.N. Saha, R.H. Fowler and E.A. Milne began using quantum physics and statistical mechanics to estimate the temperature of stellar atmospheres based on observed spectra. Cecilia Payne took these theories a step farther and proved that they could be used to estimate the chemical composition of a star's surface. Her most astounding finding was that stellar atmospheres are fundamentally made up of hydrogen and helium.

After completing her doctorate, unable to find any jobs in astronomy in England, Cecilia Payne remained at the Harvard Observatory. In 1934 she married Sergei Gaposchkin, an astronomer of Russian extraction whom she had met on a trip to Germany and whom Cecilia had helped find work at Harvard. In 1956, after having had three children, Payne-Gaposchkin became the first female full professor in the history of Harvard University.

While these particulars are true, they give no notion of the difficulties that beleaguered Cecilia Payne. Fortunately, she left us a record of them in her autobiography. The quote below is long, but it should be included nonetheless, because, in addition to explaining the kind of discrimination and difficulties aspiring women scientists had to face in England, some of the details Payne-Gaposchkin related about her career in the U.S. applied equally well to Maria Goeppert Mayer.

> A woman knows the frustration of belonging to a minority group. We may not actually be a minority, but we are certainly disadvantaged. Early experience had taught me that my brother was valued above me. His education dictated the family moves. He must go to Oxford at all costs. If I wanted to go to Cambridge I must manage it for myself. Early I learned the lesson that a man could choose a profession, but a girl must 'learn to support herself'. Presumably, this would be until she found a husband. But it was early impressed upon me that I could scarcely hope to do that, as I had 'no money of my own'. Such was the Victorian social code in which I grew up.
>
> In my case the real obstacle in marriage was that I met no men at all. There was an unwritten law in our house that if my brother should bring any of his friends home, his sisters must make themselves scarce. This was part of the social code of the contemporary public school boy – another aspect of sex discrimination.
>
> Once or twice I was asked to a dance, given for some school friend as a 'coming-out party'. This was a concentrated agony. I did not know how to dance. My clothes, too, were an embarrassment, for they were hand-me-downs from the daughter of a wealthy friend. I still remember my horror when I learned that one of my dancing partners knew her, and thought with crimson shame that he probably recognized the dress I was wearing. Even when I fell back on conversa-

tion it was a disaster. A friend of my brother, whom I had tried thus to entertain, remarked to him later: 'Fancy! A girl who *reads* Plato for *pleasure!*' I simply did not know how to behave at a dance.

Matters did not improve when I went to Cambridge. Women were segregated in the lecture room. Even in the laboratory they were paired off if possible, and (did I imagine it?) treated as second-class students. It might have been different if I had been gay and attractive and had worn pretty clothes. But I was dowdy and studious, comically serious and agonizingly shy. The *Demostrator* in the Advanced Physics Laboratory told someone (who kindly repeated it to me) that I was 'slow'. It did not occur to me to protest. Ignorant and uncouth I might be, but not *slow!* I decided to pay no more attention to anything Henry Thirkill said: he was simply not noticing. Unluckily for me, he was one of the final Examiners in the [*Mathematical*] *Tripos* [the highly competitive Cambridge examination that 'classified' students], and I believed him responsible for placing me in the second class. I heard through the grapevine that the other Examiner, William Brag whom I adored, had wished to place me higher. Henry Thirkill had put my back up [...].

The attitude to women that oppressed my childhood and youth was typical in England at the time. Fifty years have not mended matters much. Although my work was well known by the time I was 30, I am sure that I stood not the slightest chance of obtaining a position in England between the time I went to Harvard to the time I retired in 1965 [...]. But though I had gone to the Right University, I had read the wrong subject. One could not have become an astronomer in England without having obtained a First Class in the Mathematical Tripos. And, of course, I was a woman. The Royal Observatory was administrated by the Admiralty. The redoubtable H.H. Turner recorded that when a candidate for the position of Chief Assistant at Greenwich was asked what qualifications he had had for the job, he replied: 'Among other things I had to climb a rope'. I should have failed the test; rope-climbing has never been my strong point. A restriction to the male sex no longer dominates the Royal Observatory, but something else still has a stranglehold on Astronomy in England.

Here Cecilia Payne recalled how things went for her in the United States:

We manage things better in the United States. Even 50 years ago a woman might do astronomical research and even make a name by publication. She might hold a position –without a title and ill-paid, it is true– and she could meet on equal terms with any astronomer in the world. In my early days at Harvard, everyone who was anybody (and many more besides, who were going to be somebody in the future) came through and argued, and fraternized. Those were glorious days. We got to know Lundmark, Milne and Unsöld, Hund, Carathéodory and [Paul] ten Bruggencate. How we argued, how we walked about the streets and sat talking in restaurants until the manager turned off the lights in despair! We met as equals; nobody condescended to me on account of sex and youth. Nobody ever thought of flirting. We were scientists, we were scholars (neither of these words has a gender). In that heady atmosphere a woman did not degenerate into the abominable stereotype of the *Femme savante*, that combination of conscious erudition and affected coyness that suggests 'It's really not *womanly* to know as much as I do'. How different from the attitude described by one of my English friends: 'With my education, I never could expect to marry'. Yes, we do things better here [...].

I spent many years at Harvard, research and writing my main interests, with an undercurrent of editing that gradually took more and more of my time, and incidentally taught me much about the craft of writing. I had no official status, as little as that of the students who provided the 'girl-hours' in which Shapley counted his research expenditures. I was paid so little that I was ashamed to admit to my relations in England. They thought I was coining money in a land of millionaires. But I had the run of the Harvard plates. I could use the Harvard telescopes (a dubious boon, this, in the climate of Cambridge) and I had the library at my fingertips.

Then came the time when Shapley organized the Department of Astronomy, and began to attract doctoral candidates. The first of these students was Frank Hogg, and (with or without status) I was to direct his research. Lectures began, informally at first, then more organized, and of course I had to lecture. The new Department called for a Chairman, a Professor. I could have done it; who knew the ropes better? But it was 'impossible'; the University would never permit it. Only a few years earlier, Theodore Lyman had refused to accept a woman as candidate for the PhD, and Shapley had somehow circumvented the difficulty. But this time it was not to be. I do not know what he tried to do, but he reported that President Lowell had said that 'Miss Payne should never have a position in the University while he was alive'. Perhaps Shapley did make the attempt. But my nameless status remained nameless. Harry Plaskett was brought from Victoria to head the new Department [...].

I was not jealous of him, although the students assigned to me soon transferred their allegiance to him. I was sorry, but I considered that it was their loss; and it left me more time for research [...].

Only some years later, when Harry Plaskett was called to Oxford to succeed H.H. Turner, did I feel jealous of him. Of course I had no right to aspire to the Savilian Professorship, but I felt that I should have been as well qualified as he. Not for the first time, I felt I had been passed over because I was a woman.

Computers at the Harvard Observatory. Fleming is shown standing, and Henrietta Leavitt is the third from the right

Henrietta Leavitt

Williamina Fleming

Cecilia Payne-Gaposchkin

> When Plaskett left Harvard there was a search for a successor. Shapley said to me at this time: 'What this Observatory needs is a spectroscopist'. I replied indignantly that *I* was a spectroscopist, though I was being pushed against my will into photometry. I protested to no avail: a spectroscopist must be imported. The position was offered to Otto Struve, and he told me many years later why he had refused it. Shapley told him, he said: 'Miss Payne shall give up spectroscopy', thus assuring him a free hand. He refused to accept the position on those terms. He had a noble, generous heart; he was one of the giants of his time. If only it had been my lot to work with him!
>
> It was then that Donald Menzel was called to Harvard, after having made a name for himself at Lick Observatory. Again I was asked, 'how much it would disturb me?' The groundwork for the 'Divide and Rule' system had been laid long before. It was not for many years, on Shapley's retirement, that I found that Menzel and I could form an alliance, rather than existing in a state of armed truce. This was a grave loss to me, and perhaps to science too [...].
>
> Years passed and Lowell was no longer President of the University. Under James Conant the status of women at the Observatory underwent a change. Miss [Annie Jump] Cannon [1863-1941] was as famous as any astronomer in the world, and justly so. For many years she had enjoyed the ambiguous title 'Curator of the Astronomical Photographs', which carried no status in the University. Now she was appointed Astronomer, and I received the same title. It was a step forward for me, for now I had a position, though still at a regrettable salary. My duties, research, lecturing, guidance of students, were actually those of Professor, but at least I now had a University position [...].
>
> Another lapse of years, another President of the University, and the time came for Shapley to retire as Director of the Observatory. After an agonizing time of indecision, Donald Menzel finally succeeded him. To Donald I owe the advancement that was finally accorded to me. The finances of the Observatory had been a closely guarded secret, and when he learned what salary I had been getting, he told me that he was shocked. He promptly raised it, and soon doubled it. Moreover, he succeeded where Shapley had failed (though I shall never know how hard he had actually tried): I was made Phillips Professor and Chairman of the Department of Astronomy. Such was the generous treatment I was accorded by the man from whom I had been systematically estranged for many years. He did not let my sex, or my less-than-cooperative attitude, stand in my way.

Cecilia Payne-Gaposchkin eventually found a permanent workplace at the Harvard Observatory. Behind her stretched a long history that had rendered women familiar figures in astrophysics. Even so, it took her another 31 years to make full professor.

Generally speaking, at least until well after World War II, U.S. university science departments were reluctant to allow women onto their research staff or faculty. The underlying idea was that a man was preferable, and naturally there was almost always a man available somewhere.

And now we are ready to move on to Maria Goeppert Mayer's story in the United States.

CHAPTER 4

Maria Goeppert Mayer in the United States (1930-1945)

On arriving in Baltimore, Maryland, Joseph Mayer joined the chemistry department at Johns Hopkins University as an assistant professor, but Maria found that her excellent credentials and wealth of knowledge about quantum mechanics, which put her far ahead of any faculty member, did her little good. The rules designed to prevent nepotism plus the effects of the depression that followed the terrible stock market crash of 1929 made it impossible for a teacher's wife to get anything like a decent academic position at Johns Hopkins. All she could manage was a few hundred dollars a year for helping a member of the Department of Physics with his German correspondence. That did entail the privilege of a place for her to work, though, which was nothing to be sneezed at, and it gave her access to the university's facilities and the activities going on in the department, where experimental research outranked theoretical research. The department's main exception was the brilliant theoretician Karl Herzfeld, an expert in thermodynamics and the kinetic theory of gases. Herzfeld was at that time interested in physical chemistry (or, if you prefer, chemical physics), the same field as Joseph Mayer (recall from chapter 2 that it was Herzfeld who passed Johns Hopkins' job offer along to Mayer).

Fortunately, the 'long hand' of Göttingen reached Johns Hopkins, too. According to Frank Rice, a member of the physics department, Max Born had asked Herzfeld, 'Will you take care of her?' And, even more importantly, the department's most prominent member, Robert W. Wood, a great expert in optics whom Maria considered 'the most important experimental physicist in the world, the king of Baltimore, like Hilbert at Göttingen', who was an old friend of James

Maria found that her excellent credentials and wealth of knowledge about quantum mechanics, which put her far ahead of any faculty member, did her little good. The rules designed to prevent nepotism plus the effects of the depression that followed the terrible stock market crash of 1929 made it impossible for a teacher's wife to get anything like a decent academic position at Johns Hopkins.

Franck, greeted her warmly from the start. 'With me', Maria once said, 'he was incredibly kind and civilized. He even used to take his pipe out of his mouth'.

Her chance to work with Herzfeld, and in her husband's own field, slowly drew Maria Goeppert Mayer into the field of physical chemistry. However, as we shall see, she never made it her permanent scientific home, even though the knowledge she gained in the discipline later proved highly useful. The fact that this was so, that she produced papers in different fields, was due mostly to the precarious conditions reigning throughout a good portion of her scientific career. She had to seize any opportunity to work with well-established scientists at the schools her husband's career took her to. As will become apparent, in more ways than one Maria Goeppert Mayer's research had to 'go with the flow', depending on which scientists made research opportunities available to her. If the metaphor serves, for a long, long time Maria Goeppert Mayer was a fragile sailboat, finely built but depending on whatever winds and currents it could find. There is, nevertheless, a connecting link, a common 'skeleton' to all the papers she published throughout her career: the quantum mechanics theory she had learned at Göttingen. Fortunately, the theory had –and has– an extraordinary range of applications. It ended up leading her to the field of nuclear physics, where Maria achieved her greatest scientific success.

Karl Herzfeld

Karl Herzfeld (1892-1978) was born in Vienna and studied physics and chemistry at the University of Vienna (1910-1912). After that he attended the ETH in Zurich, where he met Otto Stern (Nobel Prize in Physics, 1943), who introduced him to the study of thermodynamics.

In 1913 he left Zurich for Göttingen, returning later to Vienna, where he took his doctorate in 1914. Herzfeld volunteered for the Austro-Hungarian Army in World War I (during this period he published six papers on statistical physics applied to physics and chemistry problems). On his discharge he returned to the University of Vienna, but the university was in economic straits, so he moved on to Munich with the idea of studying analytical chemistry and getting a job in the chemical industry. In Munich he became an assistant at the physical chemistry laboratory run by Kasimir Fajans, who had worked with Rutherford in Manchester. It was then when theoretical physics won him over, and he formed ties with Arnold Sommerfeld's group, earning his habilitation in theoretical physics and physical chemistry. He spent nearly two years in Munich (1925-1926) as full (extraordinarius) professor. There he directed Walter Heitler's thesis; Heitler would later become a leading researcher in quantum electrodynamics and quantum field theory, contributing to quantum field theory with an influential book, *The Quantum Theory of Radiation* (1936). Herzfeld also met Linus Pauling in Munich; as we saw in chapter 3, Pauling spent most of his postdoctoral European stay in Munich. During his time in the Bavarian capital, Herzfeld published a paper –in volume 9 of the Springer-Verlag publishing company's series *Handbuch der Physik*– on kinetic theory and statistical physics, *Klassiche Thermodynamik* (1926), which came to be used as a graduate text at German-speaking universities. It was that same year, 1926, when he went to Johns Hopkins University, first as a visiting professor and eventually as a regular professor.

Physical Chemistry, the Synthesis of Ammonia and Quantum Chemistry

Physical chemistry was a relatively young discipline. It was founded in the late 19th century by Wilhelm Ostwald (1853-1932), Svante Arrhenius (1859-1927) and Jacobus Henricus van't Hoff (1852-1911). Physical chemistry really came into its own, though, in the years leading up to World War I, when countries strove to synthesize ammonia, NH3. Plants need large quantities of nitrogen, which is their main food source. Air contains theoretically unlimited amounts of nitrogen (N2), but plants cannot access it directly. Only those plant species that live in symbiosis with certain bacteria capable of converting atmospheric nitrogen into ammonia can get at it. Therefore, nitrogen fertilizers have to be used, especially when farmers want to increase the number of crops they can grow per year.

But fertilizer availability was a problem, especially in the second half of the 19th century. Germany's population had grown from 25 million people in 1800 to 55 million in 1900. In 1913 Germany was consuming 200,000 tons of nitrogen a year, and 110,000 tons of that were imported in the form of nitrate, mainly from Chile, but also from Peru and Bolivia. Most of it was used for intensive farming.

Producing ammonia and ammonia compounds from atmospheric nitrogen and hydrogen was one of the problems German chemists threw themselves at in the early days of the 20th century. It was a problem for physical chemistry. In about 1900 Wilhelm Ostwald came very close to solving the problem, but it was Fritz Haber –whom we met in chapter 2– who achieved what Ostwald thought he had done. Haber benefitted in this from other German scientists' work, such as Walther Nernst's research into the rules of thermodynamics. Nernst, another of the greats of physical chemistry, had been a disciple of Ostwald in Leipzig. The theorem, or third law of thermodynamics, which Nernst presented in 1905, provided the means for calculating the values of specific heats. This was especially important for Haber.

Haber's work reached its culmination in 1908. Haber was then a professor of electrochemistry in Karlsruhe, assisted by Robert le Rossignol, an Englishman who had studied ammonia with William Ramsey before settling in Karlsruhe. It was then that Haber succeeded at synthesizing ammonia using osmium and uranium as catalysts, working at very high pressure and moderate temperature.

Getting a process for synthesizing ammonia before war broke out in 1914 was a lucky break for Germany. The country was able to produce the artificial fertilizers it needed to maintain and even increase its farm output, since it could not ship traditional natural fertilizers through the wartime blockade.

The next step in the consolidation of physical chemistry as an important part of chemistry was the introduction of quantum mechanics concepts in physical chemistry. While the ties between chemistry and physics are familiar to us today, it took a long time for scientists to see the relationship. The electron, identified in cathode ray experiments in 1897 by Joseph John Thomson, director of the Cavendish Laboratory in Cambridge, soon became a common bridging element between physics and chemistry. Unsurprisingly, Thomson himself was one of the first to introduce it in chemistry, with his model of the atom as a plum pudding: he suggested that electrons (the raisins in the pudding) are distributed regularly throughout a positively charged mass. He particularly endeavoured to explain the organization of the periodic table of elements according to the number of electrons that he believed existed in concentric circles in his model of the atom. One of Thomson's books, *The Electron in Chemistry* (1923), summing up five lectures he delivered at the Franklin Institute in Philadelphia, opened with a preface containing a passage that fits nicely here:

> It has been customary to divide the study of the properties of matter into two sciences, physics and chemistry. In the past the distinction was a real one owing to our ignorance of the structures of the atom and the molecule. The region inside the atom or molecule was an unknown territory in the older physics, which had no explanation to offer as to why the properties of an atom of one element differed from those of another element. As chemistry is concerned mainly with these differences there was a very real division between the two sciences.
>
> In the course of the last quarter of a century, however, the physicists have penetrated into this territory and have arrived at conceptions of the atom and molecule which indicate the way in which one kind of atom differs from another and how one atom unites with others to form molecules. These are just the problems which are dealt with by the chemists and thus if the modern conception of the atom is correct the barrier which separated physics from chemistry has been removed.

Niels Bohr took another important step across the bridge between physics and chemistry when in 1920 he began to use his quantum model of the atom to explain the periodic table (Bohr utilized a series of quantum numbers that characterized the different electron layers). The development of quantum physics thus became intertwined with a fundamental part of chemistry, the organization of the elements.

The fundamental step toward comprehending the *chemical bond,* a fundamental element of any theory of chemistry, was taken by Gilbert Newton Lewis (Joseph Mayer's dissertation supervisor at Berkeley, as mentioned before) and Walther Kossel, a German. In 1916 they shook the chemistry community with the publication of two papers (Lewis's in the *Journal of the American Chemical Society*, and Kossel's in *Annalen der Physik*). What Lewis and Kossel stated in their theory (which was based on the electron) is that there are two ways a chemical bond can be made: either when electrons are transferred between atoms (electrovalence) or when atoms share electrons (covalence), although some cases may fall in between. Electrovalence was eventually found to be the main process in inorganic chemistry, while covalence is the more frequent process in organic chemistry. It was Lewis's ideas that gained the upper hand, especially after he published a book that was to become a classic, *Valence and the Structure of Atoms and Molecules* (1923). Previously, however, a countryman of Lewis's, Irving Langmuir, who worked in industrial laboratories and had studied in Germany with Walther Nernst, made significant contributions to the 'reworking', or popularization, of the new type of bond. In fact, it was Langmuir who introduced the word 'covalence' in a paper ('The Arrangement of Electrons in Atoms and Molecules') published in 1919 in the *Journal of the American Chemical Society*. In the section entitled 'Valence, Coördination Number and Covalence' he wrote, 'To distinguish between the valence thus found and that assumed in the ordinary valence theory we shall denote by the term "covalence" the number of pairs of electrons which a given atom shares with its neighbors'.

Langmuir is interesting not only because of his numerous major contributions (he received the Nobel Prize in Chemistry in 1932), but because he is an example of the kind of scientist who was young enough when quantum physics was developed to jump into the new scientific world with both feet. He clearly saw what quantum physics could mean for chemistry, as shown by what he wrote in an article published in 1921, 'Future developments of theoretical chemistry', where Langmuir discussed Rutherford and Bohr's atom and mentioned the contributions of William H. and William L. Bragg, Van den Broek and Moseley. He said,

> And all these things mark the beginning, I believe, of a new chemistry, a deductive chemistry, one in which we can reason out chemical relationships without falling back on chemical intuitions. Chemical science in the past has been in a way like biology, botany, geology and so on, in which we deal with general relationships, where we cannot express results quantitatively. We have had, of course, certain fundamental quantitative laws, thermodynamic laws, laws of combining multiple proportions, for example; but although these have been of great importance, they have been capable of accounting for only an insignificant proportion of chemical phenomena.
>
> Now, if we look ahead what do we see as the most probable development in purely theoretical chemistry during the next few years? I believe the field of atomic structure will dominate theoretical chemistry, because a knowledge of atomic structure will enable us to deduce the chemical and physical properties of atoms and molecules.

Very soon theoretical chemistry would have a more powerful tool, quantum mechanics. At first it seemed that solving Schrödinger's equation for the system made up of the atoms in a molecule would do the trick and clear up all the problems having to do with molecules, including, of course, bonds. Paul Dirac, one of the creators of quantum mechanics, assertively said so in an 1929 article ('Quantum Mechanics of Many-Electron Systems'): 'The general theory of quantum mechanics is now almost complete, the imperfections that still remain being in connection with the exact fitting in of the theory with relativity ideas [...]. The underlying physical laws necessary for the mathematical theory of a large part of physics and the whole of chemistry are thus completely known, and the difficulty is only that the exact application of these laws leads to equations much too complicated to be soluble.'

The origin of the specialty that would be known as quantum chemistry is generally associated with the 1927 publication of a paper by Walter Heitler and Fritz London explaining the stability of the hydrogen molecule within the recently formulated quantum theory. The paper was entitled 'Wechselwirkung neutraler Atome und homopolare Bindung nach der Quantenmechanik'. More precisely it concerned the interaction between two hydrogen atoms, obtaining the chemical bond as the result of a quantum mechanic 'resonance'. The concept of resonance had been introduced in quantum mechanics the year before by Heisenberg in connection with the quantum states of the helium atom.

And this is where Linus Pauling steps in. As I explained in chapter 3, Pauling turned out to be one of quantum mechanics' leading figures. According to one of his commentators (Buckingham), for Pauling chemistry was 'a quantum phenomenon, or rather, a great collection of quantum phenomena'. Other scientists also made noteworthy contributions to the field, however. Physicist John Slater was just as much a giant in the field as Pauling.

Pauling and Slater's initiatives and leadership helped quantum chemistry flourish more strongly in America than anywhere else. The formulation of quantum mechanics came at a time when the physical and chemical sciences were coming of age in the United States. And the new ideas attracted young physicists, especially in the beginning; newcomers like John van Vleck, Edward Condon, David Dennison, Ralph Kroning, Isidor I. Rabi, Philip M. Morse, Oscar Rice and George E. Kimball all at one time or another made contributions to quantum chemistry.

Maria Goeppert Mayer was part of this group for a while.

The Johns Hopkins Years

It was practically inevitable that Maria Goeppert Mayer would be forced to set aside her own pet subjects, at least partially, for a while, and take up physical chemistry: her husband was a physical chemist, and all the theoretical physics work being done at Johns Hopkins was run by Herzfeld, who, as we have seen, was interested in physical chemistry at the time. In addition, her knowledge came in handy for both her husband's work and Herzfeld's scientific pursuits, inasmuch as physical chemistry could also benefit from the new quantum physics, and Maria knew quantum physics far better than Joseph or Herzfeld. In fact, it was she who taught her husband quantum mechanics. She produced the following papers, published in partnership with Herzfeld: 'Energieübertragung an adsorbierte Moleküle', *Zeitschrift für Physikalische Chemie* (1931); 'The Polarizability of Ions from Spectra', *Physical Review* (1933); 'On the States of Aggregation', *Journal of Chemical Physics* (1934); 'On the Theory of Fusion', *Physical Review* (1934); 'Behavior of Hydrogen Dissolved in Palladium', *Zeitschrift fur Physikalische Chemie, B* (1934); and 'On the Theory of Dispersion', *Physical Review* (1936). With Joseph Mayer and Stephen Brunauer she wrote 'The Entropy of Polyatomic Molecules and the Symmetry Number', *Journal of the American Chemical Society* (1933).

She also published papers with other scientists: with Albert May, 'Some Lattice Sums Involved in the Calculation of Elastic Constants', *Physical Review* (1936), a paper that belonged to what would soon be termed 'solid-state physics' (later 'condensed matter physics'); with Herzfeld's student Alfred Sklar, 'Calculations of the Lower Excited Levels of Benzene', *Journal of Chemical Physics* (1938), one of the first calculations –using the methods of group theory, the matrix version of quantum mechanics and the Hund-Mulliken approximation to calculate the molecular levels of benzene– of the complicated electron structure of benzene. Her partnership with Sklar developed from Herzfeld's interest in the problem of how a substance's chemical structure determines its optical properties, like colour. Herzfeld proposed that Sklar tackle the subject as his doctoral dissertation. The problem required complex mathematical techniques associated with the most satisfactory methods then known for determining molecular spectra, and Herzfeld suggested asking Maria for help. Sklar, who completed his dissertation in 1937, used the Heitler-London method instead of the approximate Hund-Mulliken method.

For the first three months of their time in Baltimore, the Mayers lived in a student dormitory. They intended to buy a home, but, since they were scheduled to spend the summer at the University of Michigan's summer physics symposium in Ann Arbor, they thought it best to wait. They did buy a car, the great American convenience that was often more 'must-have' than 'convenience', given the distances between places. They drove to Ann Arbor in their new Buick.

The eight-week Summer Symposium in Theoretical Physics had been a regular event since the mid-1920s, and it was held each year right up until the start of World War II. A great many top physicists used to go, either to speak or to listen. Most of them worked in the United States, but some foreign physicists attended as well. Every year around 50 scientists went to the symposium, few enough that people mingled informally. The length, informality and low number of participants made this symposium different from scientific conferences, which lasted only a few days and were usually attended by a crowd of scientists. The renowned Solvay Conferences on Physics, which were held in Brussels and began in 1911, were very different, because they were for the elite only, generally physicists of a certain age.

Johns Hopkins University, in Baltimore

Robert Atkinson, Enrico Fermi and Maria Goeppert Mayer at the Summer Symposium in Theoretical Physics, 1930

Joseph Mayer, Maria Goeppert Mayer and Karl Herzfeld at Johns Hopkins

Maria Goeppert Mayer, Joseph Mayer, Robert Atkinson, Paul Ehrenfest and Lars Onsager at the summer physics symposium in Ann Arbor, 1930

George Uhlenbeck, a Dutchman and a disciple of Ehrenfest in Leiden, who together with Samuel Goudsmit introduced the quantum concept of spin in 1925, was, like Goudsmit, an assistant professor at the University of Michigan (where he remained until 1935, when he succeeded to H.A. Kramers' chair at the University of Utrecht, although he returned to America in 1938 and to Ann Arbor in 1939). Uhlenbeck said the summer symposiums at Ann Arbor 'had, I believe, a strong influence on the development of physics, and they surely influenced me! Often in the fall I continued […] to try to digest what I had learned in the summer!' He particularly mentioned that his own involvement in beta decay theory was all due to the symposium.

The headline participants of the 1930 symposium were Enrico Fermi and Paul Ehrenfest. Fermi taught a class on the quantum theory of radiation, where he introduced quantum field theory, which was very new to some of the participants. Again according to Uhlenbeck, '[T]hese lectures are the best introduction to quantum electrodynamics which are available' (the leading lecturers at the 1931 symposium were Hendrik A. Kramers and Wolfgang Pauli, with shorter talks by Arnold Sommerfeld and Robert Oppenheimer).

In the brief memoirs Joseph Mayer published in the *Annual Review of Chemical Physics* (1982), he recalled the Ann Arbor experience:

> That particular Ann Arbor summer session [1930] was enormously successful. Both Enrico Fermi and Paul Ehrenfest were extremely good lecturers. Each sat in the front row when the other was lecturing and corrected the other's English, much to the amusement of the audience. But both were extremely clear. The audience included Robert Atkinson, an English astronomer and physicist, Lars Onsager, Serge Korff, Donald Andrews, Charles Squire and of course Sam Goudsmit and George Uhlenbeck, both professors at Ann Arbor at the time.
>
> We became particularly good Friends of the Fermi's. Laura Fermi was always a delight and Enrico was always interesting and informative.

Accustomed to mingling with promising young physicists in Göttingen, Maria Goeppert Mayer must have felt right at home in the environment at Ann Arbor. What is more, this was an institutionalized meeting where firmly established scientists mixed with younger ones, like her, who still had to prove their mettle, and that made the experience so much better than what she knew from Göttingen. Meeting Fermi and his wife Laura, with whom Maria got along well, turned out to be especially important. Later we shall see what Fermi meant to Maria when they were both at the University of Chicago.

When the symposium closed, Joseph took Maria sightseeing across the United States. Again according to Mayer's memoirs,

> [The trip] included Black Hills, Tetons, the Yellowstone and Glacier Parks, and then went further west to Seattle, stopping at Mt. Rainier, which I climbed. In Seattle, we visited Henry Frank […]. On driving up to the village at the bottom of Mt. Rainier, as far as we could go in a car, we passed Nisqually Glacier, which was then down almost to the road […].
>
> We drove south from Seattle, stopped at Crater Lake, and […] from there to the San Francisco Bay and Berkeley. At Berkeley we visited old friends of mine, including Robert Oppenheimer and his wife Kitty. In the last years that I was at Berkeley, Oppenheimer had come back from Germany and gave a lecture on quantum mechanics. I don't think I understood anything of it but I was enormously impressed […]. I was amused recently at reading an article in the Cal Tech magazine by Carl Anderson who had listened to Oppenheimer's lectures on quantum mechanics in the same year at Cal Tech. According to Anderson's story, the class kept getting smaller until he was the only one left. Oppenheimer then came to him and said: 'Please, don't leave, I can't go on with nobody in the class. Let me have at least one student to the end of the quarter'. At Berkeley there were several of us who went through the whole semester, or whatever the length of time that Oppenheimer was scheduled to lecture, and we enjoyed it, but I think we were pretty well snowed. Oppenheimer was not a good lecturer at that time. His great facility for making things clear came only later […].
>
> From Berkeley we went down the coast to Pasadena where we visited the Paulings. We camped in the Pauling yard. They had a house very close to Cal Tech […]. The most exciting thing that happened on the trip back from Pasadena to Baltimore was simply a series of tire failures which depleted the ready cash that we had. At that time it was almost impossible to cash checks, although I think we had some money in the bank in Baltimore.

When they had a home of their own, Joseph and Maria held frequent get-togethers where there was no shortage of drinks, even during the famous Prohibition, which was passed by Congress in 1917, ratified by three fourths of the states in 1919 and only abolished in 1933. The Mayers liked to drink and were heavy smokers.

The couple's first child, Marianne, was born in the spring of 1933. Not long before that Maria became an American citizen. After Marianne's birth Maria focused entirely on caring for the baby, setting her research aside for a while. She chafed under the circumstances. In fact, after a year she went back to her research, which she never again left; in the end this meant her relationship with her daughter was strained. But her situation at the university was more than rocky itself. She was merely tolerated, though she managed to take charge of around half a dozen graduate physics classes (on statistical physics and classical mechanics).

Irritated by Maria's situation (in all the years she was at Johns Hopkins, she was never paid more than 200 dollars a year), Karl Herzfeld sent the following letter to the university's president, Joseph Ames:

> Let me take this occasion to state that in my opinion [Maria Mayer] does at least one third of the work of a full time associate, both as a teacher and in research. She teaches usually two hours for half a year, an advanced course in theoretical physics, and is besides active, on equal footing with Dieke and myself, in two seminars throughout the year.
>
> In addition, she gives, together with her husband and Dr. Andrews, a two hour seminar in chemistry throughout the year. So far as her research is concerned, she publishes several papers a year, usually in conjunction with [Joseph] Mayer and myself [...]. In conclusion, I think she is a very valuable member of the department, both as a teacher and as far as the publications emanating from the department go. From the estimate made before, the adequate amount of remuneration would be $1,000.

The letter did not get her a raise. Instead, they cut her pay. In fact, she sometimes refrained from cashing her monthly cheques. As she wrote to her mother, 'The university has so little money that I'm always afraid that it will wind up going bankrupt'.

One bit of good news was that James Franck, who had been staying in Copenhagen with Bohr since he left Göttingen, decided to take up an invitation from Johns Hopkins University, with an economic assist from the Rockefeller Foundation. Franck joined the university in 1935. So, the 'Göttingen circle', or a smaller version of it, was re-joined in Baltimore. Franck found his old friend and partner Robert Wood at Johns Hopkins, and in 1936 they published a joint paper in the *Journal of Chemical Physics*, 'Fluorescence of Chlorophyll in Its Relation'. He also worked with Herzfeld; together they published a paper entitled 'An Attempted Theory of Photosynthesis' (1936), also in the *Journal of Chemical Physics.* In this paper they mentioned that they had consulted with Edward Teller, another illustrious immigrant, to whom I shall return shortly. Franck stayed at Johns Hopkins until 1938, when he accepted an offer from the University of Chicago that far outbid Baltimore, economically and scientifically. He remained there until retirement.

When Maria got pregnant again in 1937, she decided to stop teaching (she seems to have disliked the idea of what students would think). Instead, she and Joseph decided to write a statistical mechanics textbook. They figured they would finish it in two years, but they were wrong, as it was published in 1940: Joseph Edward Mayer and Maria Goeppert Mayer, *Statistical Mechanics* (John Wiley & Sons, New York; Chapman and Hall, London).

Despite all her constraints at Johns Hopkins, some students understood just how capable she was from the quality of her research. Her first doctoral student was Robert G. Sachs (1916-1999), who later led a brilliant career (in 1968 he became a full professor at the University of Chicago and director of the Enrico Fermi Institute, a post he kept until his death in 1999, although as director emeritus; and from 1973 to 1979 he directed Argonne National Laboratory). Sachs defended his dissertation, 'Nuclear Spins and Magnetic Moments by the Alpha-Particle Model', in 1940 at Johns Hopkins, even though Maria was then in New York, as we shall see. Actually Goeppert Mayer shared the job of advising on Sachs' dissertation with another illustrious physicist, Edward Teller, who, as I already said, played an important role in Maria's own career.

In the obituary of Maria Goeppert Mayer published in the *Biographical Memoirs of the National Academy of Sciences*, Sachs recalled his early relationship with her:

> When as her first bona fide student I turned to her for guidance in choosing a research problem, nuclear physics was on the rise; and she told me that that was the only field worth consideration by a beginning theorist. She took me to Teller to ask his advice about possible research problems. Our resulting joint work was her first publication in the field of nuclear physics. My thesis problem on nuclear magnetic moments was also selected with Teller's help, and she gave her guidance throughout that work, suggesting application to this problem in nuclear physics of techniques of quantum mechanics in which she was so proficient.

The article to which Sachs referred, which he and Goeppert Mayer published together, was entitled 'Calculations on a New Neutron-Proton Interaction Potential', and it appeared in 1938 in the *Physical Review*.

Edward Teller

Edward Teller (1908-2003) was a member of an extraordinary generation of scientists born in Hungary: mathematicians John von Newman and George Pólya, physical chemists Georg von Hevesy and Michael Polanyi, physicists Eugene Wigner and Leo Szilard and the slightly older aeronautical engineer Theodore von Kármán. Teller was a great physicist whose fame was marked by his radical political ideas in defence of a larger atomic arsenal for the United States, the country of which he eventually became a citizen. He is known especially as 'the father of the H-bomb'. In 1935 Teller, who had been in England for some time, received word through his friend Russian physicist George Gamow that The George Washington University in Washington, D.C., was offering Teller a full professorship. While in

Washington, Teller used to visit Johns Hopkins University regularly. Let us see what he wrote about this in his autobiography, *Memoirs:*

> In addition to my daily activities with Geo [Gamow] and my students, I also developed ties to the larger community of physicists in the area. I took part in the weekly seminar the Bureau of Standards held in their old building on Connecticut Avenue; I made what contributions I could to the discussions, and occasionally suggested speakers. Twice a month, I traveled to Baltimore [a distance of 62 kilometres by road] for a seminar.
>
> Papa Franck, as he was known to the large group of young scientists whom he had befriended, had organized the twice-monthly seminar. Because the ranks of émigré scientists in the United States were continuing to grow, the seminar developed the flavor of reunions. James Franck, my old sponsor from Göttingen days (who had resigned his position there in 1933 in protest against Hitler's policies), had accepted a position at Johns Hopkins University. Hertha Sponer, my formidable friend from Göttingen, had found a position at Duke, as had Lothar and Traute Nordheim, my friends from the pension on Stegemühlenweg.
>
> At the seminar, I was reintroduced to Maria Goeppert Mayer [...]. In addition to being an extremely able physicist, Maria was also very beautiful. Slender and blond, she had a natural delicacy and grace as well as a considerable strength of mind.

Teller then talked about Maria's marriage to Joseph Mayer, not entirely accurately adding that 'in the following years, [she] had been occupied with her two small children'. And he continued, more correctly (remember, he dealt with physical chemistry first, not physics), 'Maria had not yet returned to physics because the body of knowledge had grown considerably since her student days. I gave her some brief tutorials and encouragement, and soon became good friends'. Indeed, Teller helped her get into the new field of nuclear physics.

Nuclear Physics

Knowledge of the structure of the atom –an atom is made up of an electron sheath comprising different levels through which electrons 'travel' (the quantum reality is more complex than the word 'travel' suggests; it is actually a question of 'probability clouds'), and this sheath surrounds a nucleus made up of protons and neutrons (which have no charge)– is so widespread today that we tend to think that, once Rutherford proposed the planetary model, this was how it worked. And yet the neutron did not appear in either Rutherford's model or Bohr's. In fact, it had not even been discovered yet. Once Rutherford proposed the planetary model and Bohr presented his model of the atom, the general belief was that the nuclei of atoms were made up of protons and electrons. This framework certainly explained why radioactive substances emitted beta rays (electrons): 'beta decay' apparently meant electrons were leaving their nuclei. Theorists did not yet know that neutrons existed, or neutrinos, or that beta decay, a common form of radioactivity, takes place because of weak interaction (the interaction responsible for radioactive processes), in a process in which one neutron from the atom's nucleus becomes a proton, emitting a very fast-moving electron and a neutrino (this may also involve the transformation of a proton into a neutron, emitting an antielectron, or 'positron', and an antineutrino). It was Enrico Fermi who explained beta decay correctly in 1933-1934.

While Irène and Frédéric Joliot-Curie came close to identifying the neutron (a component of the atomic nucleus first predicted by Rutherford in 1920), they puzzled over how to interpret the meaning of an experiment they ran in January 1932 in which they found that the radiation emitted by a polonium source caused the emission of protons from a layer of paraffin wax. 'Interpretation of the phenomenon', they wrote at the end of their note, 'is hampered by the fact that the energy of the emitted photons can be only very imperfectly estimated [...]. Whatever interpretation can be given to this phenomenon plausibly takes place for all very high quantum energy radiations, particularly for cosmic rays, if they are electromagnetic in nature [...]. It thus seems established for these experiences that a high-frequency electromagnetic radiation is able to liberate, in hydrogenated bodies, very high-velocity animated protons'. Thus they missed a one-of-a-kind chance, leaving the way clear to one of Rutherford's colleagues at the Cavendish Laboratory, James Chadwick (1891-1974).

In 1911, the same year Rutherford published his atomic model paper, Chadwick graduated from Manchester. He remained there to work in the field of radioactivity under the management of Rutherford himself. In 1913, shortly after receiving his Master of Science degree, he went to Berlin to work with Hans Geiger, a former member of Rutherford's group in Manchester. However, this was not a good time to travel to Germany. World War I soon broke out, and Chadwick was interned. He remained at Ruhleben, a horseracing track-cum-internment camp about four kilometres west of Berlin, very near the industrial suburb of Spandau, until the war was over (1918).

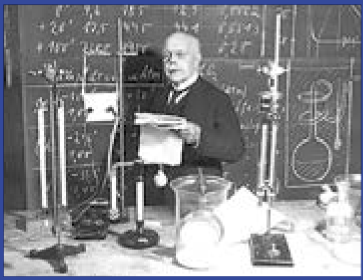
After the war Chadwick returned to Manchester with Rutherford. When Rutherford was appointed to direct the Cavendish Laboratory in April 1919, he asked Chadwick to go with him, and Chadwick agreed. It was during his years at Cambridge when Chadwick discovered the neutron. What triggered his discovery was Irène and Frédéric Joliot-Curie's article in the 18 January 1932 issue of *Comptes rendus*. Chadwick never doubted the Joliot-Curies' observations; what he did question, however, was their explanation. To him it was obvious that the protons were being knocked out as the result of neutron collisions. He announced his surmise in a note published in *Nature* on 27 February 1932 entitled 'Possible Existence of a Neutron'. This article was followed by a longer paper in the *Proceedings of the Royal Society*, with the more assertive title 'The Existence of a Neutron'. As he explained in the note in *Nature,* 'These results, and others I have obtained in the course of the work, are very difficult to explain on the assumption that the radiation from beryllium is a quantum radiation, if energy and momentum are to be conserved in the collisions. The difficulties disappear, however, if it assumed that the radiation consists of particles of mass 1 and charge 0; or neutrons. The capture of the α-particle by the ^{9}Be nucleus may be supposed to result in the formation of a ^{12}C nucleus and the emission of the neutron'.

The neutron thus made its debut in the universe of the elementary particles, which was still quite the elitist club (just electrons, protons and photons, plus perhaps the neutrino); and its discoverer, James Chadwick, was rewarded with the 1935 Nobel Prize in Physics.

That same year, in 1932, a new member joined the tiny group of elementary particles: the positron, the first example of antimatter, a state whose existence was deduced from the relativistic equation for the electron that Paul Dirac came up with in 1928.

In 1931 American physicist Carl Anderson (1905-1991) used a cloud chamber to take the first photographs of the mysterious cosmic rays that had been observed for the first time in 1911 by Austrian physicist Victor Hess, who published his results a year later. Anderson noted that tracks appeared in his photographs with a similar frequency. This must correspond to particles with an opposite charge, he reasoned, but both he and Robert Millikan (his boss at the California Institute of Technology and a man with ideas of his own about the nature of cosmic radiation) believed the particles with a positive charge were protons. However, more detailed studies (measurements of the ionization and curve of the tracks) showed that at least some of the particles' tracks could not be due to protons. At last Anderson published a brief paper in the 9 September 1932 issue of *Science,* the journal of the American Association for the Advancement of Science. It bore the title 'The Apparent Existence of Easily Deflectable Positives' an ambiguous and rather obscure title revealing the reservations he still felt. Even so, he wrote:

> For the interpretation of these effects [the tracks in his photographs] it seems necessary to call upon a positively charged particle having a mass comparable with that of an electron, or else admit the chance occurrence of independent tracks on the same photograph so placed as to indicate a common point of origin of two particles. The latter possibility on a probability basis is exceedingly unlikely.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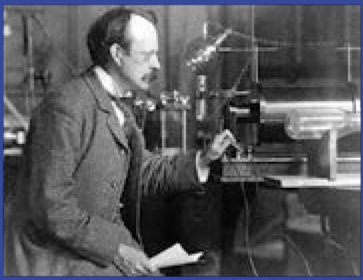
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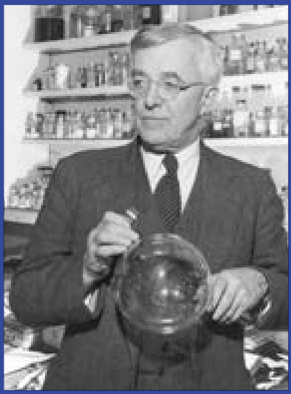

Irving Langmuir


Hans Bethe


James Chadwick


Fréderic Joliot and Irène Curie (1934)


Otto Hahn and Lise Meitner

A few months later, on 28 February 1933, a new paper of his appeared, entitled 'The Positive Electron', although in this article he used the word 'positron'. The doubts in his note in *Science* –motivated, he said, 'in view of the importance and striking nature of the announcement'– were gone.

Another breakthrough for nuclear physics came from chemistry, not physics, in 1932: the discovery of heavy hydrogen, i.e., deuterium, a stable hydrogen isotope. Its discoverer, Harold Urey (1893-1981), who influenced Maria Goeppert Mayer's career as we shall see later, announced the discovery in an article ('A Hydrogen Isotope of Mass 2') in *Physical Review* co-written with his partners F.G. Brickwedde and G.M. Murphy. Only two years later, Urey received the Nobel Prize in Chemistry (while the most abundant isotope of hydrogen –also called 'protium' – has only one proton in its nucleus, deuterium has a proton and a neutron; the nucleus of deuterium –that is, what is left when a deuterium atom is stripped of the electron orbiting its nucleus– is called a deuteron.)

During 1936 and 1937 Hans Bethe published three papers in *Review of Modern Physics* summing up and reviewing the status of nuclear physics at the time. The papers were collectively dubbed 'Bethe's Bible'. In the first (co-authored with R.F. Bacher; the second was a solo paper, and the third was written with M. Stanley Livingston) Bethe explained the importance of the deuteron for nuclear physics thus:

> The deuteron plays in nuclear physics the same role as the hydrogen atom in atomic physics. It consists of two elementary particles, one proton and one neutron. It is well known that any two-body problem can be integrated explicitly if the force between the two particles is a known function of the distance of the particles. Thus the theoretical results about the deuteron are free from approximations made to simplify the mathematical treatment. They are, as we shall see, also to a very large extent independent of the assumptions we make about details of the force between neutron and proton [...]. The theory of the deuteron is thus more suitable for quantitative comparisons with experiment, and therefore for a check of the underlying ideas about nuclear structure, than any other part of nuclear theory.

Yet another development took place in 1932 that furthered the study of atomic nuclei, i.e., nuclear physics. For a long time alpha particles were the only means available for disturbing atoms in a controlled fashion, but the only way scientists had to make alpha particles left them at the mercy of naturally radioactive elements. All available sources of radiation were too weak to penetrate any farther into the mysteries of the nucleus. One gram of radium emitted 37 billion alpha particles per second (plus other products of decay). Only one alpha particle out of 100,000 led to a transformation. That was not enough to enable the resulting substances to be chemically separated for examination. In addition, the energy of these alpha particles was barely strong enough to get past the electrical repulsion of their target nuclei (which had a positive charge). Rutherford's group at the Cavendish Laboratory in Cambridge discovered that, the faster alpha particles travelled, the more transformations they generated. It was therefore vital to create machines that could increase the number of particles and their velocity. And since alpha particles have a charge, one way to do this was to subject them to large differences in potential. It was not until the 1920s when instruments began to appear that could do this, and it was only in 1932 when John Cockcroft and Ernest Walton designed a device (the voltage multiplier) that produced 125,000 volts, enough to prove that bombarding lithium isotopes, whose atomic mass is 7, with protons would break the lithium into two alpha particles (which are, remember, helium nuclei). This was the first artificial atomic decay.

Before the Cockcroft-Walton generator, in about 1928, Merle Tuve of the Carnegie Institution's Department of Terrestrial Magnetism in Washington, D.C., used a transformer invented by Nikola Tesla to reach a potential difference of three million volts (V). Tuve and his partner Gregory Breit used this method to accelerate protons and electrons. After a short job at an electrical plant in Alabama, Robert J. Van de Graaff designed an electrostatic generator of his own. He spent a year at Oxford on a scholarship, after which, at Princeton (where he went in 1928) he adapted his generator to accelerate particles. Soon his prototype was producing 80 kV (1 kV=1,000 V), rising to 750 kV in 1931. If he used two spheres, he could achieve a potential difference of 1.5 MV (1 MV=1,000 kV). By 1937 there were Van de Graaff generators that stood five metres tall and could reach five million volts. In 1933 Tuve and his group used a million-volt Van de Graaff generator and a discharge tube featuring improvements of their own to observe the disintegration of lithium and boron. But the most important initiative, the one whose development did the most to mark a new era in physics, was the initiative associated with the name of Ernest Orlando Lawrence (1901-1958).

After graduating from Yale, Lawrence got a job as associate professor of physics at Berkeley in 1928. The next year, while skimming an issue of *Archiv für Elektrotechnik,* he ran across a paper by Norwegian engineer Rolf Widerøe (1902-1996), a doctoral student at the Aachen Technical University. Lawrence did not know much German, but the paper's illustrations suggested the idea of the cyclotron. Widerøe –who had been working with notions of the sort since at least 1922– had built a *linear* tube to accelerate particles. The tube was divided into two parts and powered by a 25,000-volt alternating current (in 1922, while in Karlsruhe, he had considered a circular tube, which he called *Strahltransformator,* or 'ray transformer', for the same purpose). When the charged particles entered the tube, they passed into the first area, where the electrical field accelerated them with a 25,000-volt thrust; at that point, the field in the second area aimed in the opposite direction, but by the time the particles reached the second area, the field had changed polarity, so the particles got another 25,000-volt push. Altogether, 50,000 volts from a 25,000-volt difference in potential.

In essence what Lawrence did was focus on the circular layouts Widerøe had first considered for his accelerating chamber, but with a number of tweaks. One was the use of a magnetic field to enable the particles to move along circular trajectories. By making the electrical field switch polarities every half turn to keep the right tangential thrust, the device could increase the particles' energy with each revolution. Naturally, since the particles were moving faster and faster, they would also trace bigger and bigger circles, but no matter what their speed, it turned out that they always took the same time to describe each revolution (i.e., the particles' frequency of rotation was independent of the radius of their orbit). So, the voltage inversion frequency could remain steady, as it was always in resonance with the particles' cycles. This 'principle of resonance' was actually what made the construction of the cyclotron possible. 'Cyclotron' was, by the way, a term that Lawrence used informally for some time, preferring the more formal ring of the term 'magnetic resonance accelerator'. 'Cyclotron' did not become the device's official name until 1936.

The idea was tested with a handcrafted prototype (made by Nels Edlefsen, one of Lawrence's students, at an estimated cost of about 25 dollars). Further development required bigger studies. In this pursuit, Lawrence had the cooperation of his students, especially Stanley Livingston, who made it the subject of his doctoral work. In late 1930 Lawrence and Livingston completed the construction of the cyclotron. It measured about 12 centimetres in diameter. They tested it on 2 January 1931, and by applying 2,000 volts they managed to produce protons with 80 kV of energy.

Lawrence announced right away that he hoped to reach a million volts, and even more if he could just build more elaborate devices. The university's president proceeded to make Lawrence a full professor over the opposition of the faculty's non-scientists. Lawrence was then just 29 (it was at this stage of his developments that he learned of Cockcroft and Walton's successful experiment).

The race was on, one of the most intense and in many ways innovative races in the history of science. Innovative not so much because of the scientific ideas that came out of it as because of the methodology, the attitude needed to tackle the task of organizing cyclotron construction. A character like Lawrence's was required: enterprising, ambitious, unrelentingly forward-looking, striving toward increasingly powerful machines, ready to scour for the economic wherewithal to finance his projects, open-minded and tech-savvy. Lawrence's qualities were just the ticket for making progress in the direction that physical reality had dictated to scientific research in the realm of the elementary particles, i.e., 'high energies'. The picture was far from complete with electrons, protons, neutrons, photons, positrons and neutrinos; with each new energy leap, matter reacted, unveiling new elements, new elementary particles. A nature like Lawrence's was needed to shoulder the task of building ever-bigger, ever-more-powerful machines (Big Science), but also to craft the model of how to steer, how to handle, in this sense, this new science.

Nuclear physics can therefore well be said to have gotten underway in 1932, a genuine *annus mirabilis*.

And now we can get back to Maria Goeppert Mayer.

Columbia University

When *Statistical Mechanics* (the textbook the Mayers wrote during Maria's second pregnancy) came out, the couple was no longer at Johns Hopkins. Joseph Mayer had been an associate professor, and at most American universities that would have meant he was on the tenure track to an eventual full professorship. Johns Hopkins was not prepared to renew him, though. The Great Depression was probably the reason. The country was still struggling economically, and universities were no exception. Mayer could have taken the university to court over its decision, but he did not, among other reasons, because more or less at the same time he received two job offers, one from the University of Chicago and the other from Columbia University (in New York). And although Johns Hopkins gave him a two-year grace period, Joseph resigned immediately and took up Columbia's offer, which featured twice the salary he was earning in Baltimore. Peter Mayer was born in 1938, a few months before his mother and sister moved to New York.

Harold Urey, the physical chemist who, as we said, discovered deuterium, played a role in Columbia's proposal. Urey, like Joseph Mayer after him, had done his PhD work at Berkeley under the supervision of G.N. Lewis. His interest in physics led Urey to the Niels Bohr Institute in Copenhagen in 1924 on an American-Scandinavian Foundation fellowship. On his return to the U.S., although he had a fellowship at Harvard (where he stayed just a month), Urey accepted a position Lewis had secured for him at Johns Hopkins University. He remained there until 1929, when he moved on to Columbia University, where he 'made' the deuteron. So, he was in Baltimore at the same time as Joseph and Maria Mayer. And he was impressed by both.

Maria's position at Columbia was in some ways worse than in Baltimore. In fact, when *Statistical Mechanics* was on the brink of publication, someone raised the point of how to list the authors in the credits, since Maria did not have a job title. At a meeting of the Department of Chemistry (of which Joseph was a member), Harold Urey asked the department to offer Maria an honorary appointment, but they said no. Urey then had Maria teach a number of chemistry classes that semester just so the book could list her as 'Lecturer in Chemistry. Columbia University'. Joseph Mayer (whose name came first) was listed as 'Associate Professor of Chemistry. Columbia University'. The book was highly successful, by the way; it hit its sixth reprint in 1954.

Though Maria held no position or title at Columbia, the director of the Department of Physics, George Pegram, ordered a little office for her. And despite the hurdles set in her way, Maria made strides as a physicist at Columbia. She was there at the same time as some quite extraordinary physicists: Isidor Rabi, Jerrold Zacharias and most of all Enrico Fermi, whom she already knew from the 1930 Ann Arbor symposium. She published three papers between 1940 and 1946 (that may not sound like many, but the bare numbers mean very little because, among other reasons, during the war years she was busy at work that could not be published, as we shall see). She wrote the first of these papers (1940, *Journal of Chemical Physics*) with Brother Gabriel Kane of Manhattan College, New York. It was a brief note, 'Lattice Summations for Hexagonal Close-Packed Crystals', and was related with the crystal fusion paper she had published with Herzfeld in 1934 in *Physical Review*. The second paper, 'Rare Earth and Transuranic Elements', *Physical Review* (1941), was a solo production, and it is particularly interesting. It stemmed from Fermi's suggestion that she try and predict the structure of the valence shell of as-yet-undiscovered transuranic elements (remember, Fermi's research programme in Italy launched slow neutrons against the various chemical elements in the hope that, when he bombarded the last elements in the periodic table, he would obtain elements heavier than uranium, i.e., 'transuranic' elements.) Making use of a very simple version of the semiclassical model developed by Llewellyn Thomas and Enrico Fermi (the Thomas-Fermi or Fermi-Thomas model) to study the electron structure of systems of many bodies, Goeppert Mayer reached the conclusion that the transuranic elements would form a new series of chemically rare earths. Despite her simplifications, her model proved accurate enough in terms of the predictions that could be drawn from it concerning the qualitative behaviour of these chemical elements. The third of her papers she co-authored with Kenneth J. McCallum of the Columbia Department of Chemistry, 'Calculations of the Absorption Spectrum of Wurster's Salts', *Review of Modern Physics* (1942).

Maria made strides as a physicist at Columbia. She was there at the same time as some quite extraordinary physicists: Isidor Rabi, Jerrold Zacharias and most of all Enrico Fermi.

As said in chapter 3, when Enrico Fermi went to Stockholm in December 1938 to collect his Nobel Prize in Physics, which he had won 'for his demonstrations of the existence of new radioactive elements produced by neutron irradiation, and for his related discovery of nuclear reactions brought about by slow neutrons', he never returned to his homeland. Instead, he and his family moved to the United States. Like Joseph and Maria Mayer a few months later (in autumn 1939), the Fermis went to Columbia University.

Because they happened to join Columbia at almost the same time, the Mayers and the Fermis became fast friends. Laura Fermi, a gifted writer, explained in *Atoms in the Family* (another major book of hers is *Illustrious Immigrants*) that during the first six months they lived in Manhattan, not far from Columbia. Enrico wanted to stop renting, however, and buy a house instead, so they went to cast their eye over Leonia, a little town in nearby New Jersey where some of their colleagues lived. One of those colleagues was Harold Urey. They paid him a visit. 'Harold Urey', Laura Fermi recalled, 'was a good orator and sold Leonia to us. Besides, I was anxious to go live where the dirt on my children's knees would not be gray, as in New York, but an honest brown'. And it so happened that just when the Fermis bought a home in Leonia, the Mayers did likewise. And Urey played a part in their decision, too. In Joan Dash's words, 'One day her husband told her: "Several of my colleagues live in a town called Leonia. It is in New Jersey, just across the George Washington Bridge [...]. Let's go see what it looks like". It was February, and an icy-cold afternoon. As we got off the bus at the stop-light in Leonia, a gust of wind blew in our faces and blinded us. We did not know where to go'. And they decided to drop in on Harold Urey. 'The Ureys were in their large living-room and had a fire going. Our visit was a success [...]. Dr. Urey talked at length to us, in his serious, slightly professorial tone, about Leonia and its excellent public schools, about the advantages of living in a middle-class town where one's children may have all that other children have [...]. By the following summer we were the happy owners of a house on the Palisades, with a large lawn, a small pond, and a lot of dampness in the basement'.

As often happened throughout her life, during her time at Columbia Maria Goeppert Mayer established close relationships with distinguished scientists. In this case what she had was the 'social triangle' of the Ureys, the Fermis and the Mayers, reinforced by their being neighbours.

The Discovery of the Fission of Uranium

Practically at the same time the Fermis were arriving in New York, a scientific development of enormous socio-political consequences was taking place in Germany: the discovery of the fission of uranium, or more precisely, the isotope uranium-235.

In autumn 1938 Otto Hahn, who was doing research at the Kaiser Wilhelm Society Institute of Chemistry in Dahlem, and his partner Fritz Strassmann performed a series of experiments using the same Nobel Prize-winning method that Fermi had employed in Italy: bombarding uranium with slow neutrons. To their surprise Hahn and Strassmann observed that they obtained barium, a much lighter element than uranium –almost half its weight (uranium's atomic number is 92, and barium's is 56). The uranium nucleus seemed to have split in two, to have *fissioned.* But nothing like this had ever been observed before. All atomic transmutations discovered so far involved one element transforming into another element nearby in the periodic table. On 6 January 1939, they published a paper in *Naturwissenschaften* voicing their doubts about their 'peculiar findings […]. As chemists we must assert that the new products are barium […]. However, as nuclear chemists who work in close proximity with the physics field, we cannot quite take so drastic a step, which goes against all the experiments performed heretofore in nuclear physics. Perhaps there has been a series of rare coincidences that have given us false indications'.

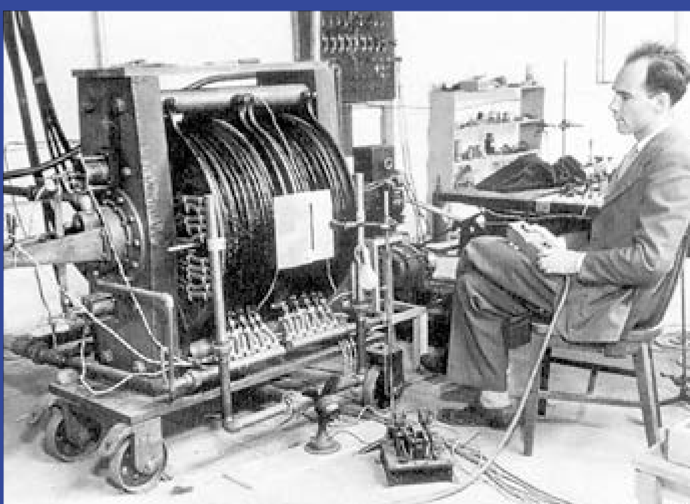
Lise Meitner, who worked with Hahn for 30 years but was forced due to her Jewish origin to leave the institute and Germany when Germany annexed Austria, was then in Stockholm. She was the first to hear about what had happened, from a letter Hahn sent her. Her nephew, physicist Otto Frisch, was spending his Christmas holiday with his aunt in the little city of Kungälv, near Gothenburg. He and Meitner would later interpret the new results together. Frisch described Lise's reaction and the events that immediately took place thus:

> When I came out of my hotel room […] I found Lise Meitner studying a letter from Hahn and obviously worried by it. I wanted to tell her of a new experiment I was planning, but she wouldn't listen; I had to read that letter. Its content was indeed so startling that I was at first inclined to be skeptical […].
>
> Was it a mistake? No, said Lise Meitner; Hahn was too good a chemist for that. But how could barium be formed from uranium? No larger fragments than protons or helium nuclei (alpha particles) had ever been chipped away from nu-

Ernest O. Lawrence with the huge magnet required by one of his cyclotrons

Instruments Hahn and Strassmann used to discover the fission of uranium

Ernest O. Lawrence with one of his first cyclotrons



Carl Anderson

> clei, and to chip off a large number not nearly enough energy was available. Nor was it possible that the uranium nucleus could have been cleaved right across. A nucleus was not like a brittle solid that can be cleaved or broken; George Gamow had suggested early on, and Bohr had given good arguments that a nucleus was much more like a liquid drop. Perhaps a drop could divide itself into two smaller drops in a more gradual manner, by first becoming elongated, then constricted, and finally being torn rather than broken in two? We knew that there were strong forces that would resist such a process, just as the surface tension of an ordinary liquid drop tends to resist its division into two smaller ones. But nuclei differed from ordinary drops in one important way: they were electrically charged, and that was known to counteract the surface tension.

This idea proved key. Frisch and Meitner immediately started scratching numbers in the snow, finding that the charge of a uranium nucleus was indeed enough to almost entirely overcome the surface tension effect, so the uranium nucleus could really be compared to a very trembly, unstable drop ready to split apart at the slightest provocation, such as the impact of a single neutron. With the help of Einstein's formula $E=mc2$, they were able to explain other details. They reached the conclusion that they could understand the phenomenon Hahn and Strassmann had discovered, all before the discoverers had even published it! Frisch and Meitner's paper was sent to *Nature,* where it was published on 18 March 1939.

Two days after solving the riddle, Frisch was off to Copenhagen, anxious to brief Niels Bohr about developments. The Dane grasped the idea at once and was greatly excited. It so happened that Bohr was just about to leave for the United States to spend three months at Princeton University in New Jersey, where he was scheduled to explain the quantum theory of measurement. His son Erik and his close associate Léon Rosenfeld were sailing with him. On 16 January the ship docked in New York, where John Wheeler and Enrico and Lauri Fermi were waiting. As soon as he landed, in fact before he even left the pier, Bohr said a few words to John Wheeler about Frisch's news. Niels and Erik then left with the Fermis for a brief stopover in New York before heading on to New Jersey, while Wheeler took the train with Rosenfeld to Princeton. On the way Wheeler convinced Bohr's partner to give a talk at Princeton about Hahn and Strassmann's discovery, Meitner and Frisch's interpretation and the conclusion Bohr had worked out with Rosenfeld during the crossing (they had found that the new process fit neatly into the compound nucleus theory Bohr had created to explain nuclear reactions), all of which was unknown in the United States. Rosenfeld's description caused a huge stir, but Bohr became upset when he found out about it, since he had intended to protect Meitner and Frisch's work until it could be published (as it would be on 11 February), likewise a number of experiments Frisch was conducting in Co-

This idea proved key. Frisch and Meitner immediately started scratching numbers in the snow, finding that the charge of a uranium nucleus was indeed enough to almost entirely overcome the surface tension effect, so the uranium nucleus could really be compared to a very trembly, unstable drop ready to split apart at the slightest provocation, such as the impact of a single neutron.

penhagen; that was why he had not said anything to Fermi. In Rosenfeld's words, 'The effect of my talk on the American physicists was more spectacular than the fission phenomenon itself. They rushed about spreading the news in all directions, and very soon the fission fragments had been seen at the oscilloscope in several laboratories in the United States, a very striking demonstration that was quite easy to produce'.

Indeed, once the news was out, American physicists rushed to mine the new vein, proving that Bohr was right to be apprehensive. One member of Fermi's team at Columbia, Herb Anderson, prepared to run the same experiments that Frisch was doing in Copenhagen to study the products of fission more closely, and on 26 January Fermi spoke about fission at a small theoretical physics meeting in Washington, D.C., sponsored by The George Washington University and the Carnegie Foundation, without mentioning Frisch. This enraged Bohr, who was attending the meeting as well. By the end of the month, the story was in the newspapers; on the 29th, for instance, the *New York Times* ran a piece on it.

The news spread to the West Coast, too. On 28 January Robert Oppenheimer, the future leader of the Manhattan Project, wrote to William Fowler, 'The U [uranium] business is unbelievable. We first saw it in the papers, wired for more dope, and have had a lot of reports since. You know it started with Hahn's finding that what he had taken for Ra [radium] in one of the U activities fractionally crystallized with Ba [barium] […]. Many points are still unclear […]. In how many ways does the U come apart? At random, as one might guess, or only in certain ways? And most of all, are there many neutrons that come off during the splitting or from the excited pieces?'

This last point was essential, because, if the reaction Hahn and Strassmann discovered produced more than one neutron, then it might conceivably unleash a chain reaction (the released neutrons could collide with more than one uranium nucleus, freeing energy and neutrons each time, and repeating this process over and over). Moreover, a huge amount of energy might possibly be produced in a fraction of a second, which meant that, in the case of a more-or-less uncontrolled chain reaction, a tremendously powerful weapon could be built or, should the reaction be controllable and gradually released, an energy source could be constructed, a nuclear reactor that could be used for peaceful means. But for the physicists involved, although these ideas could be imagined and thus were perhaps possible, implementation seemed to be a matter for the far distant future. As phrased in the October 1939 issue of the mainstream science magazine *Scientific American*, '...power production by means of nuclear fission would not be beyond the realm of possibility. But under present conditions, the process is as inefficient as removing the sand from a beach a grain at a time'.

One major problem was that the fission of uranium using neutrons took place in uranium-235, nature's least abundant uranium isotope (U-235 makes up 0.72 percent of the world's uranium, while U-238 accounts for 99.28 percent). If the process was ever going to be used for anything but experiments, this problem had to be solved.

The question of the number of neutrons thrown off in each fission was quickly tackled. In Paris (Joliot-Curie) a mean of 3.5 neutrons was found, while Columbia (Fermi) recorded two neutrons. The road to the chain reaction therefore remained open. But it was still too early for scientists to really make any headway. First the United States had to enter World War II, although some scientists soon saw that they had to move things along as soon as possible. Leading the pack was an active, insightful, original Hungarian physicist of Jewish origin, Leo Szilard (1898-1964).

When Hitler came to power, Szilard (then a *Privatdozent* at the University of Berlin) had no doubt about what was going to happen, and he kept his suitcases packed. A few days after the Reichstag fire (27 February 1933), he took an almost empty train from Berlin to Vienna. A day later the same train was completely packed. After Vienna, Szilard settled in London. Shortly after he reached the English capital, the annual meeting of the British Association for the Advancement of Science was held (September 1933), and Szilard read in the newspapers that Rutherford had told the meeting that anybody talking about the industrial use of energy produced from atoms was little less than a lunatic. This comment set Szilard to thinking about the subject and, as he recalled in an autobiographical document, '[I]t suddenly occurred to me that if we could find an element which is split by neutrons and which would emit *two* neutrons when it absorbed *one* neutron, such an element, if assembled in sufficiently large mass, could sustain a nuclear chain reaction'. Not long afterward Frédéric and Irène Joliot-Curie discovered artificial radioactivity, and Szilard saw more clearly how the possibility of a chain reaction could be explored, although he never managed to rouse much enthusiasm in others. First he tried with beryllium, but that failed. Despite his inability to find a candidate, he had the business sense (and political awareness) to apply for a patent for the laws that he thought governed chain reactions in the spring of 1934. Szilard knew just what his ideas implied, and he wanted to keep access to them restricted, so he resorted to the only way of keeping his patent from spreading: he assigned it to the British Admiralty. The patent ('Improvements in or relating to the transmutation of chemical elements') was accepted on 12 December 1935. Until late 1937 Szilard remained more or less permanently in England, where he managed to snag a small fellowship at Oxford. Practically on his own, he continued mulling over chain reactions.

When Szilard saw war looming on the horizon, he moved to the United States. He arrived, jobless, on 2 January 1938. In the U.S. Eugene Wigner passed him word of the discovery Hahn and Strassmann had made late that year. Few people in the world could appreciate the consequences of their findings better than he. That was the start of an intense, complicated stage of Szilard's life when he got busy among his American colleagues while corresponding with others in England and France (Joliot-Curie). Briefly, Szilard wanted someone to check immediately whether fission produced neutrons, as he believed it did, and if so he wanted them not to publish their results, so the Germans would not know. Fermi, who soon ascertained that fission did indeed produce neutrons, was not in favour of Szilard's tactics; Edward Teller was. Eventually Fermi was brought around to Szilard's point of view, but while they were still talking it over, in March 1939 Joliot-Curie and his colleagues Hans von Halban, Jr., and Lew Kowarski published a note in *Nature* clearly stating that neutrons were emitted from the fission of uranium and furthermore saying that this could lead to a chain reaction. Fermi then took a clear stance in favour of publishing. The same month when the note appeared in *Nature,* March 1939, German troops took over what was left of free Czechoslovakia.

In April the Paris and Columbia groups' experiments continued, finding that the number of neutrons was two or three. This further confirmed the possibility of a chain reaction. That same month U.S., British, German and French scientists appealed to their respective governments to help with fission research and asked them to keep a close eye on their uranium supplies. The scientists did not think a highly explosive chain reaction was possible (that idea was entertained only by a very few), but some of them did believe fission could lead to an energy source that could be used in industry or submarines.

In July, after three months at Columbia as a guest scientist participating in the Fermi group's experiments, Szilard again found himself out of work, so he joined Wigner in New York. The two Hungarian scientists became increasingly convinced the danger was real, and they began to worry over uranium supplies, particularly what might happen if the Germans gained access to the big deposits in the Belgian Congo. They brainstormed over the channels they might use to warn the Belgian government not to sell uranium to Germany, and it struck Szilard that Einstein knew the queen of Belgium. At once he told Wigner the two of them could go see Einstein, tell him all about the situation and ask him to consider writing to the queen.

Einstein was on holiday in Peconic, New York, but they soon tracked him down. It was the first time the physics genius had ever heard tell of the possibility of a chain reaction, Szilard recalled years later, but he grasped the idea and its implications at once. However, he did not care for the idea of writing to the queen, though he did offer to write to a man he knew in the Belgian government. He was going to do so when Wigner suggested it would be improper to address a foreign government without first contacting the American State Department. They all decided that Einstein would send a draft of the letter he meant to write to the Belgians together with a note saying that, if he did not hear from the State Department in two weeks, he would send the letter to Europe. Having agreed on this course and arranged to find out how to approach the State Department, they split up.

Again it was Szilard who found a way. He managed to get in touch with a director at the Lehman Corporation, Alexander Sachs, who in point of fact had already heard about the new developments before Szilard's visit and had even pointed out their importance to the president. Sachs advised them to have Einstein write directly to President Roosevelt and volunteered to deliver the letter personally.

Szilard used Einstein's draft to prepare a version for President Roosevelt, which he submitted to Einstein (still on holiday) on 30 July. Szilard asked Edward Teller to drive him up to Peconic ('I entered history as Szilard's chauffeur', Teller once joked). The two talked over the contents of the letter to Roosevelt with Einstein. The famous missive was finally written on 2 August 1939, and the full text is as follows (Einstein's letter was accompanied by another from Szilard to Sachs, dated 15 August, enclosing a four-page memorandum to the president, also from Szilard and bearing the same date):

> Sir:
>
> Some recent work by E. Fermi and L. Szilard, which has been communicated to me in a manuscript, leads me to expect that the element uranium may be turned into a new and important source of energy in the immediate future. Certain aspects of the situation which has arisen seem to call for watchfulness and, if necessary, quick action on the part of the Administration. I believe there-

fore that it is my duty to bring to your attention the following facts and recommendations:

In the course of the last four months it has been made probable –through the work of Joliot in France as well as Fermi and Szilard in America– that it may become possible to set up a nuclear chain reaction in a large mass of uranium, by which vast amounts of power and large quantities of new radium-like elements would be generated. Now it appears almost certain that this could be achieved in the immediate future.

This new phenomenon would also lead to the construction of bombs, and it is conceivable –though much less certain– that extremely powerful bombs of a new type may thus be constructed. A single bomb of this type, carried by boat and exploded in a port, might very well destroy the whole port together with some of the surrounding territory. However, such bombs might very well prove too heavy for transportation by air.

The United States has only very poor ores of uranium in moderate quantities. There is some good ore in Canada and the former Czechoslovakia, while the most important source of uranium is the Belgian Congo.

In view of this situation you may think it desirable to have some permanent contact maintained between the Administration and the group pf physicists working on chain reactions in America. One possible way of achieving this might be for you to entrust with this task a person who has your confidence and who could perhaps serve in an unofficial capacity. His task might comprise the following:

a) to approach Government Departments, keep them informed of the further development, and put forward recommendations for Government action, giving particular attention to the problem of securing a supply of uranium ore for the United States;

b) to speed up the experimental work, which is at present being carried on within the limits of the budgets of University laboratories, by providing funds, if such funds be required, through his contacts with private persons who are willing to make contributions for this cause, and perhaps also by obtaining the co-operation of industrial laboratories which have the necessary equipment.

I understand that Germany has actually stopped the sale of uranium from the Czechoslovakian mines which she has taken over. That she should have taken such early action might perhaps be understood on the ground that the son of the German Under-Secretary of State, von Weizsacker, is attached to the Kaiser-Wilhelm-Institute in Berlin, where some of the American work on uranium is now being repeated.

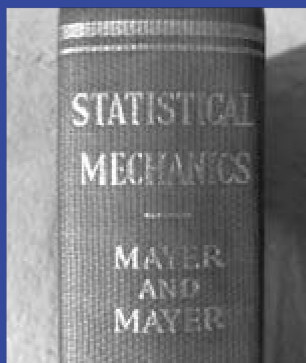
Einstein's letter, which Sachs was only able to deliver to Roosevelt in October, combined with the United States' feelings about fission and the development of atomic and nuclear physics, produced results. In October 1939 the president appointed the Advisory Committee on Uranium, chaired by the director of the National Bureau of Standards, Lyman J. Briggs, to coordinate research into splitting uranium isotopes and achieving a sustained chain reaction. Wigner and Teller, representatives from the Army and the Navy, and Richard Roberts of the Carnegie Institution sat on the committee. On 1 November the committee informed the president that chain reactions were a possibility but there was no actual demonstration, and that in spite of the uncertainties the government ought to support detailed research and immediately buy four tons of pure graphite (one of the potential moderators, or substances to control neutron proliferation) and 50 tons of uranium oxide, in case the preliminary research turned up good reasons to carry on with the project. The president acknowledged the recommendation, but no action was taken.

Once again Szilard seized the initiative. He wrote an article for *Physical Review,* the leading U.S. physics journal, which also had a worldwide readership. There Szilard described how a chain reaction could take place with uranium, using graphite as a moderator (with the article he enclosed a letter to the editor asking him to hold off on publishing the article until further notice). Szilard again appealed to Einstein for help, and on 7 March 1940 Einstein wrote Sachs, informing him of the article and saying that if something was not done the article would be published. The middleman spoke to the president on 15 March. Roosevelt answered that the best course would be for the Uranium Committee to meet again. Sachs then asked Briggs to call a meeting; Briggs assented, requiring Sachs to attend also. Sachs asked why Szilard and Fermi were not invited as well. Briggs' answer is surprising but interesting, as it reveals some of the 'hostile' feelings toward emigrants I referred to in the last chapter. He said, 'Well, you know, these matters are secret and we do not think that they should be included'. The matter was nevertheless fixed up, and both Szilard and Fermi attended the meeting.

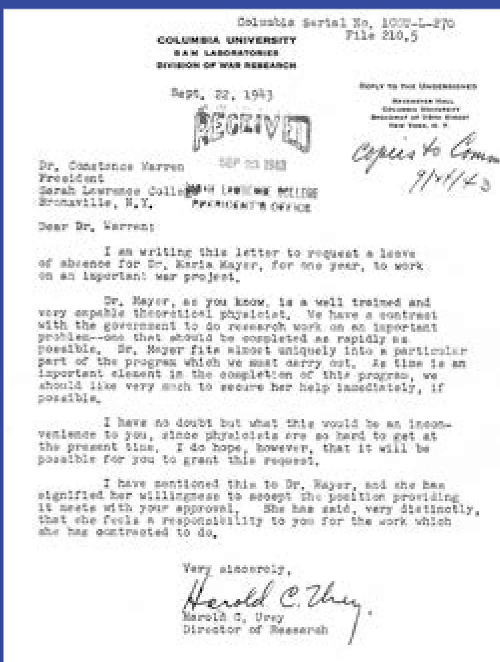
Although some progress was made from that point (in July 1940, for example, the War Department and the Department of the Navy approved a programme aimed at splitting the two uranium isotopes), it was not much. The main goal was to build a reactor capable of sustaining a chain reaction and to find a method for separating U-235 from U-238 to procure enough fissionable uranium. After all, there were not too many reasons to get into a frantic race. An entire ocean lay between the Americans and the war in Europe.

Marianne, the Mayers' first child, born in 1933

Edward Teller, Maria Goeppert Mayer, Joseph Mayer and James Franck



Spine of Joseph and Maria's *Statistical Mechanics,* published in 1940



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File 210.5

COLUMBIA UNIVERSITY
SAM LABORATORIES
DIVISION OF WAR RESEARCH

Sept. 22, 1943

RECEIVED
SEP 23 1943
SARAH LAWRENCE COLLEGE
PRESIDENT'S OFFICE

Copies to Comm
9/24/43

Dr. Constance Warren
President
Sarah Lawrence College
Bronxville, N.Y.

Dear Dr. Warren:

I am writing this letter to request a leave of absence for Dr. Maria Mayer, for one year, to work on an important war project.

Dr. Mayer, as you know, is a well trained and very capable theoretical physicist. We have a contract with the government to do research work on an important problem--one that should be completed as rapidly as possible. Dr. Mayer fits almost uniquely into a particular part of the program which we must carry out. As time is an important element in the completion of this program, we should like very much to secure her help immediately, if possible.

I have no doubt but what this would be an inconvenience to you, since physicists are so hard to get at the present time. I do hope, however, that it will be possible for you to grant this request.

I have mentioned this to Dr. Mayer, and she has signified her willingness to accept the position providing it meets with your approval. She has said, very distinctly, that she feels a responsibility to you for the work which she has contracted to do.

Very sincerely,

Harold C. Urey

Harold C. Urey
Director of Research

Harold Urey's letter asking Sarah Lawrence College to release Maria Goeppert Mayer from her duties there so she could work on the Manhattan Project

Harold Urey

Maria Goeppert Mayer (second from the right) with colleagues from Sarah Lawrence College in 1943

The first really significant steps toward establishing an atomic bomb programme were taken in the organization of the new agency President Roosevelt set up on 28 June 1941, the Office of Scientific Research and Development (OSRD), which held authority over all scientific defence work.

The Manhattan Project

The first really significant steps toward establishing an atomic bomb programme were taken in the organization of the new agency President Roosevelt set up on 28 June 1941, the Office of Scientific Research and Development (OSRD), which held authority over all scientific defence work. The OSRD was headed up by Vannevar Bush, an engineer known for his contributions to applied mathematics and electrical engineering. During World War I, Bush worked in submarine detection; in 1939 he resigned from the vice presidency of MIT to become president of the Carnegie Institution in Washington. Soon he rose from a mere member of the National Advisory Committee for Aeronautics (NACA) to committee chairman, and, on seeing the effects of the war that was breaking out across Europe, he laid plans for a committee to coordinate the nation's scientific research efforts aimed at wartime applications. In early June 1940, when the German army was pushing into France, Bush persuaded Roosevelt to place him at the head of the National Defense Research Committee (NDRC), which was officially created on 27 June of that same year, ten days after the fall of France. One of the NDRC's goals was to look for new ways to apply science to the needs of war, and it had the power to call on the National Academy of Sciences, the National Bureau of Standards and other federal laboratories for assistance.

The creation of the Office of Scientific Research and Development a year later (approximately half a year before the U.S. entered the war) put the NDRC under the OSRD, which meant the NDRC's powers were scaled back. James B. Conant, chemist and president of Harvard, was named Bush's successor at the helm. Half a year later, on 6 December 1941 (that is, one day before Japan attacked Pearl Harbor), it was decided to reorganize the Uranium Committee (known as S-1). Conant took over the chair, and the committee was made up of Lymann Briggs (director of the National Bureau of Standards), Eger Murphree (research director at the Standard Oil Development Company), Ernest Lawrence, Arthur H. Compton and Harold Urey. Urey was in charge of directing the work on isotope separation using the method of gaseous diffusion and the work of producing heavy water. Lawrence focused on the initial production of small samples of fissionable elements, the electromagnetic method of isotope separation and certain experimental work having to do with the properties of the plutonium nucleus. Compton directed the basic physics work studying the chain reaction and was moreover authorized to explore the possibility of producing plutonium in useful quantities using controlled chain reactions. In January 1942 Compton reorganized the research in progress at Chicago under the obviously camouflaged name of the 'Metallurgical Laboratory' and hired more staff. He also supervised other laboratories' work. This included Fermi's work on graphite piles and neutrons at Columbia University in New York (which was moved some months later to Chicago, where such work was centralized) and the research being done under Gregory Breit's leadership at several locations to learn about fast-neutron reactions for their possible application to explosives. Thus it was in Chicago, on 2 December 1942, at what was dubbed 'Chicago Pile One' (whose construction began on 16 November) that Fermi's team achieved history's first controlled, self-sustaining chain reaction ('self-sustaining' meaning that it produced enough energy to keep itself going). It was allowed to run for four and a half minutes.

Half a year earlier, on 18 June 1942, Colonel James C. Marshall of the Army Corps of Engineers received orders to form a new Corps district for a special job, making atom bombs. The district was officially created on 13 August and dubbed the 'Manhattan Engineer District', since Marshall had set up his headquarters in Manhattan, New York. For security reasons the district's work was referred to as 'the DSM' (Development of Substitute Materials). This was what eventually was known simply as 'the Manhattan Project'. On 17 September an extremely well-qualified officer of the Army Corps of Engineers, Coronel (later General) Leslie R. Groves, was placed at the head of the Manhattan Project. Groves' name would be joined forever to that of the American atomic project.

Tracing the history of the Manhattan Project would take me too far away from Maria Goeppert Mayer. Furthermore, most people have at least a rough idea of it already. I will therefore just mention a few points of very special interest that affected Maria Goeppert Mayer's situation.

First of all, the fact that the DSM was assigned to the U.S. Army Corps of Engineers had consequences that were not truly appreciated at the time but went on to mark the development of post-war science and, more indirectly, world socio-political history from the 1950s on. To put it in a nutshell, civil society surrendered at least part of its sovereignty over science to the military. True, the handover was supposed to be temporary, for the duration of the exceptional wartime conditions, but in the end the military authorities clearly saw that mid-century science (especially the physical sciences, which include electronics) contained real here-and-now developments and future potential that made science indispensable for the military's ongoing mission: to be war-ready under the best possible conditions. What is more, if war did happen, it would probably be waged or start (in the event of Soviet missile attacks with nuclear warheads) somewhere far enough away from America that the military would need electronics to cope with the distances involved. As a consequence, the handover ended up being more permanent that it was supposed to be. In that sense the Manhattan Project was a watershed moment in the history of contemporary science and consequently in general history itself.

As work progressed, the project's scope became increasingly clear. Scientific capacity was not all that was needed; industrial and technological capacity were required, too, as was military coverage, which provided essential infrastructure and organization. There was the Chicago Metallurgical Laboratory, whose reactor contained almost 400 tons of graphite, six tons of uranium metal and 58 tons of uranium oxide, under Fermi's supervision. There was also Ernest Lawrence's Radiation Laboratory at Berkeley. Furthermore, the Westinghouse Electric and Manufacturing Co. was handling the production of uranium metal, and the Mallinckrodt Chemical Works in St. Louis was preparing uranium oxide, assisted by the National Bureau of Standards. The National Carbon Co. and the Speer Carbon Co. were producing high-purity graphite, following the suggestions of technical experts from the National Bureau of Standards. Researchers from the Carnegie Institute of Washington, the National Bureau of Standards, the Rice Institute, Cornell University, Purdue University, the University of Chicago, the University of Minnesota, the University of Wisconsin, the University of California, Stanford University and the University of Indiana were participating in Gregory Breit's fast-neutron reaction studies. And scientists from other universities, such as the University of Virginia, Brown and Yale, and from facilities like the Rockefeller Institute for Medical Research and Iowa State College were taking part in other tasks. The M.W. Kellogg company stepped up to study the gaseous diffusion of uranium-235 and -238 isotopes so a pilot plant could be designed.

The problem of separating uranium-235 and uranium-238 was fundamental, because only U-235 was fissionable. For a good summary of the methods used to separate the isotopes, one of the members of the Los Alamos Laboratory, Robert Serber (a theoretical scientist from Berkeley and a protégé of Oppenheimer), prepared a series of five lectures to bring the people at Los Alamos and new arrivals up to speed regarding the situation and its problems. This text, classified as a Top Secret Limited Document, was not declassified until 1965. It was only published in 1992, with notes and commentary added by the Serber himself, as *The Los Alamos Primer.* The following passages are drawn from one of the author's commentaries:

> In order to make an atomic bomb with uranium the United States had to separate the 1/140th part of U^{235} from the 139 parts of U^{238} in natural uranium when the only difference between the two for purposes of separating them was their mass. Most of the two billion dollars that the wartime program to develop the atomic bomb –the Manhattan Project– spent was invested in building the vast machinery necessary to separate uranium. One system, gaseous diffusion [created by Gustav Hertz, who received the Nobel Prize in Physics in 1925 for developing processes to separate isotopes via gaseous diffusion], converted natural uranium to a gas and then relied on the two isotopes' differing rates of diffusion across a porous barrier [...]. The building that held the gaseous-diffusion plant in Oak Ridge, Tennessee, was correspondingly large –a U-shaped structure with each leg of the U nearly half a mile long. Another system, electromagnetic separation, relied on the fact that an electrically charged atom traveling through a magnetic field moves in a circle at a radius determined by its mass. Ions of a vaporous uranium compound projected through a strong magnetic field inside a curved tank separate into two beams, with lighter U^{235} atoms following a narrower arc than heavier U^{238}. Metal pockets set at the end of the thousands of tanks built at Oak Ridge collected each beam of isotopes separately in the form of metal flakes. The system was notoriously inefficient, but it got the job done. Most of the uranium used in the Hiroshima bomb was separated this way.

Serber did not mention the third separation method, which was based on the centrifugal force that separates bodies of different masses in a spinning cylindrical machine, or centrifuge. In 1934 Jesse Beams of the University of Virginia used this method to separate two isotopes of chlorine, chlorine-35 and chlorine-37. In 1940 Beams himself received government funding to try and apply the procedure to uranium isotopes using groups of high-speed centrifuges, but the method was not a success, as it required a huge amount of energy. The centrifuge approach was eventually abandoned in 1944.

Plutonium is another fissionable element, the element used to make the bomb released over Nagasaki, but it had to be made artificially. Because of plutonium's extremely short half-life (much shorter than uranium's), there was practically none remaining on the Earth. The Stone and Webster Engineering Corporation and Du Pont were called in to build a plant in Oak Ridge, Tennessee, known as the Clinton Engineering Works, to develop methods for producing plutonium. The plant began operating on 4 November 1943. Another plant, known as the Hanford Engineer Works, was built for large-scale plutonium production on the Columbia River in Hanford, in central Washington state. Research also had to be done into the problems of corrosion, cooling, shielding, protection and biomedical consequences. Numerous companies and universities had a hand in all these issues.

The bombs that were dropped on the Japanese cities of Hiroshima on 6 August 1945 and Nagasaki on 9 August 1945 contained uranium and plutonium, but it was thought unnecessary to test the uranium first. The plutonium did have to be tested, because it used a new kind of fuse to trigger the explosive reaction. The test results are part of history: seven minutes after the explosion, the bomb's cloud stood 11.5 kilometres tall. The rest, as they say, is history.

Lastly, there was Los Alamos, which tackled the problem of using all the materials, devices and knowledge produced at facilities like those I have just listed to actually make the atom bomb, the ultimate goal of all that preliminary work.

In the summer of 1942, Oppenheimer set up an encounter at Berkeley to explore the theoretical aspects of nuclear explosions. The participants included Hans Bethe, John van Vleck, Edward Teller, Robert Serber and Felix Bloch. In November a location was chosen for the atom bomb laboratory: Los Alamos, New Mexico, roughly 48 kilometres from Santa Fe. The greatest advantage of the site, which could only be reached by a winding road, was that it had considerable acreage available for possible testing. The laboratory was awarded to the University of California, and Oppenheimer was its director from day one. Oppenheimer arrived in Los Alamos in March 1943, followed by various groups that came together into the most impressive team of scientists the history of science had ever seen. Los Alamos was the long- or short-term home of people like Von Neumann, Bohr, Richard Feynman, Bethe, Fermi, Teller, Emilio Segré, Weisskopf, Luis Alvarez, Edwin McMillan, Rabi, Tolman, Lawrence, Compton, Edward Condon, Norman Ramsey and Stanislaw Ulam.

At last, after all the work done at facilities across the country, the first test of a nuclear explosion took place on 16 July 1945. It was code named 'Trinity'. In the early morning hours, on the desert stretches of Jornada del Muerto (97 kilometres from Alamogordo, New Mexico, and 400 kilometres from the Los Alamos Laboratory), they set off a bomb based on plutonium, chemical element number 93 on the periodic table, a material that, as I said, had to be recreated in the laboratory because it was almost non-existent on Earth. The bombs that were dropped on the Japanese cities of Hiroshima on 6 August 1945 and Nagasaki on 9 August 1945 contained uranium and plutonium, but it was thought unnecessary to test the uranium first. The plutonium did have to be tested, because it used a new kind of fuse to trigger the explosive reaction. The test results are part of history: seven minutes after the explosion, the bomb's cloud stood 11.5 kilometres tall. The rest, as they say, is history.

And Maria Goeppert Mayer had a hand in the Manhattan Project, although her part was very modest.

The Fermis' and the Mayers' Concerns

Before Maria got involved in wartime research (which she only did after the U.S. entered the war, after Japan attacked Pearl Harbor in 1941), both she and the Fermis were keenly aware of the danger posed by Hitler's Germany, then in the middle of its avalanche of conquests. Maria knew, because she was well informed of what was happening in Germany by her family; and the Fermis had left Italy so that Laura, who was of Jewish descent, would not be persecuted in Mussolini's fascist Italy, which was Germany's ally. In her book *Atoms in the Family*, Laura Fermi included a number of passages illustrating how worried the Fermi and Mayer families were about possible future events. Looking back the idea seems too childish for such intellectually capable people to have entertained, but humans in danger react in ways outside the normal criteria of rational thought. This is what Laura Fermi wrote:

> Our friends the Mayers were as concerned as we were. We had first met them in Ann Arbor in 1930, when we had been on our first visit to America. They had then been newly wed; Joe a tall, blond, American boy; Maria a blond, medium-sized German girl from Göttingen, where they had met and married. Both were scientists, he a chemist, she a physicist. Because Joe had joined the faculty of Columbia University in the fall of 1939, they had bought a house in Leonia at about the same time we did.
>
> Maria, who still had many relatives in Germany, was well informed of what happened there and knew what naziism meant. The Mayers and the Fermis determined to leave the United States together if naziism should become established in this country. During the many evenings spent with the Mayers between the fall of France and America's entry into the war, we made plans together. Between a philological argument on the origin of some English word and a piece of advice on gardening that the Mayers passed down to the Fermis, we prepared to become modern Robinson Crusoes in some faraway desert island.
>
> We made plans as soundly conceived in the theory, as carefully worked out in all details, as might be expected from a group which included two theoretical physicists and a practical, American-raised chemist.

> Joe Mayer was to be our sea captain, a role in which he was not excessively experienced. Enrico's knowledge of currents, tides and stars, would help. His delight at the prospect of experimenting with compass and sextant was encouraging. Yet Joe felt we should practice navigation in the Florida waters at the first opportunity.
>
> Meanwhile, there was much we could do. Maria Mayer and Enrico could consult and determine what part of our civilization was worth saving. Accordingly, Maria could collect the best-suited books. Enrico, the descendant of farmers, could study the agricultural problems of our refuge. It was my task to see that our colony would not go naked in years to come. I might decide on cotton seed and spinning wheels or on bolts of cloth. It did not matter, so long as everyone was clothed. A few scientifically selected persons would be invited to join our expedition: we ought to have a doctor, we ought to have children of such age, sex, and heredity that they could later marry ours and people the island.
>
> What island we would make ours was still to be determined. In a war in which the United States would in all likelihood participate on the side against Germany, the Atlantic Ocean was out of the question. The Pacific Ocean is sown with islands. In the temperate zone between the Hawaiians and the Philippines there were numberless islets large enough for us. We would search for a desert island among them. We could not foresee Pearl Harbor, and we could not foresee the Japanese!
>
> While envisaging adventure, Enrico and I did not neglect more practical precautions. Historical knowledge and personal experience had taught us that when war breaks out in a country, the assets of enemy aliens are immediately frozen. We could not predict the extent of American tolerance; we did not know that the financial restrictions would allow sufficient leeway for ample living. So decided to bury a 'treasure' in our basement […].
>
> The 'treasure' proved unnecessary. We dug it up only when we left Leonia.

The Fermis' and Mayers' reaction reminds me of something that happened to Erwin Schrödinger. He laid the following proposal before Spanish physicist Blas Cabrera in a letter he wrote on 24 February 1939 at the Grand Hotel de Bruxelles. Schrödinger had left his professorship at the University of Graz after Germany annexed Austria, and he was looking for a place to settle. In September 1939 he made his home in Dublin. Cabrera had been exiled from Spain since September 1936 and was in Paris.

> Dear friend: It is many months that I have not had news directly from you, although when in Oxford I understood that you continued living in Paris at the campus and that not much had changed until then.

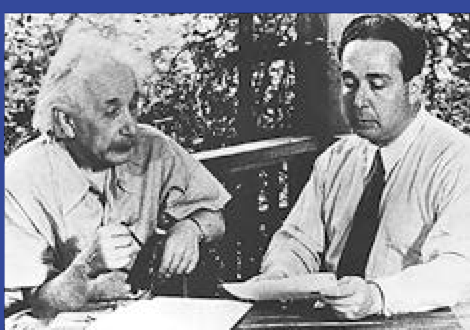
Max Planck speaking at the 25th anniversary of the creation of the Kaiser-Wilhelm Gesellschaft (1936)

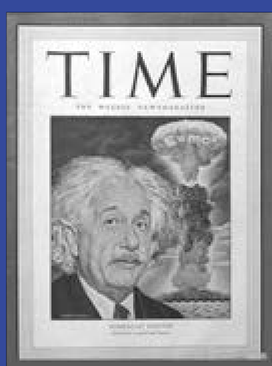
From Enrico Fermi ‘s group in Rome (June 1934). left to right:
O. D’Agostino, E. Segrè,
E. Amaldi, F. Rasetti and E. Fermi

Photographs of the Hiroshima and Nagasaki bombs in a contemporary publication

R. Oppenheimer, E. Fermi and E.O. Lawrence at Berkeley, 1936

Albert Einstein and Leo Szilard, discussing the letter to Roosevelt





Covers of *Time* magazine devoted to Albert Einstein (1946) and J. Robert Oppenheimer (1954)

The newspapers have just informed me that you have found the same fate (it might even be called *luck!*) as happened to me in the early days of September.

This new event is much more grave for you and your wife than for us! It is ever so painful for me. It was clear nothing else could have happened, either in your case or in mine, given the regrettable success of political situations. We have each of us lost his homeland. And much more! I mean that I have lost yours as well. And I am again so sorry for that. We have lost Italy. We have lost most all the parts of Europe worth living in.

What will you do? I, for now, have a six-month position, a very well-paid position. Afterward I have –most probably– the chance of [work?] a [full professorship?] in the very north of the continent or, I should rather say, on one of its islands. Although the people there are very friendly to me, it seems to me a gloomy decision for a man who loves the outdoors, who loves the South, who loves the Mediterranean. I beg you not to tell anybody because it is very likely I will have to do it anyway. However I keep mulling over other possibilities. I think formally of South America, I mean the countries where Spanish is spoken. I have thought if, putting our two names together, which are well known in the world (at least in the physics world), we were to offer to transplant European physics to a remote location, in Peru, for example, bringing some disciples with us, do you believe we could obtain possible conditions and at last acquire a new homeland? Is that a vain dream?

I tell you this so you can think it over. I do not know your situation right now. It is possible that it is very embarrassing. I hope it is not for now. But in any case it seems to me that in the years to come the possible happiness of life will be a function d+n of d, distance from Europe, where n is a rather high exponent. Do you not think so?

I beg you give my deep respects to your lady wife and believe me always your most attached and devoted friend

E. Schrödinger.

Maria Goeppert Mayer and the Manhattan Project

The day after the attack on Pearl Harbor, Maria Goeppert Mayer received her first real job offer since her arrival in the United States: a part-time position at Sarah Lawrence College in Bronxville, New York. The students were all women, and if she wanted it she had to start immediately. The pay looked good, 1,500 dollars a year for two days' work. She accepted, even though it meant she needed a car to get to work. At the college she taught a science course of her own invention covering the various sciences. Joseph Mayer joined the Army practically at the same

The day after the attack on Pearl Harbor, Maria Goeppert Mayer received her first real job offer since her arrival in the United States: a part-time position at Sarah Lawrence College in Bronxville, New York. The students were all women, and if she wanted it she had to start immediately. In the spring of 1942 Maria received a new offer, this time from her friend Harold Urey, who was putting together a working group at Columbia to try and solve one of the Manhattan Project's core problems.

time. He was posted to the Aberdeen Proving Ground in Maryland to research conventional weapons, a task that kept him away from home five days a week (and on Saturdays he worked at Columbia). In February 1945 he was posted to the Pacific for several months to see how the soldiers who had trained at Aberdeen were using their weapons. In the spring of 1942 Maria received a new offer, this time from her friend Harold Urey, who was putting together a working group at Columbia to try and solve one of the Manhattan Project's core problems, how to separate the fissionable uranium-235 isotope from uranium-238 (as we have seen, the S-1 Committee tapped him in late 1941 to direct the work on separation via gaseous diffusion). Maria's knowledge of physical chemistry made her a good pick for the project. She said yes on the condition that it had to be a part-time job, she must never have to work on Saturdays, and she must be able to stay home if either of her children fell ill (in practice, however, her work was anything but part-time). She applied for leave from Sarah Lawrence, although she still managed to teach there occasionally throughout the war. Even so, the stress of her work and her husband's absence eventually caused her to become distanced from her children, as science was always very important to her; the children were left in the care of a nanny they did not like very much. From then on her relationship with her children, especially her daughter Marianne, was difficult. Her Manhattan Project work –to which I shall refer shortly– made the situation worse.

Although the Urey group's big objective was to use the gas diffusion method, Urey was not sure it would work, and he decided to explore another method at the same time, separation through photochemical reactions. In one of her conversations with Joan Dash, Maria said, 'Urey usually assigned me not to the main line of research of the laboratory, but to side issues, for instance to the investigation of the possibility of separating isotopes by photochemical reactions. This was nice, clean physics although it did not help in the separation of isotopes'.

The photochemical method required a thorough knowledge of the spectra of U-235 and U-238. Since Maria had worked in spectroscopy before –recall, for example, her and Kenneth McCallum's 1942 paper, 'Calculations of the Absorption Spectrum of Wurster's Salts'– Urey had her direct the theoretical work. But soon her task load grew, as shown in a letter she wrote later to Born telling him she had started as a theorist consulting on a small project and ended up directing an experimental staff of fifteen, mostly chemists. She started by gathering and comparing published data about the spectra of uranium compounds and found that there were many unknowns, which she set her chemists to track down. But this method led nowhere, and in 1943 Urey decided to rule out the photochemical procedure (only years later, when lasers were available, did it become feasible).

Maria then switched over to the gaseous diffusion method, focusing on investigating the thermodynamic properties of uranium hexafluoride. Uranium hexafluoride, UF6, is the only gas that stays a gas at moderate temperatures, and it is the heaviest known gas, which should have made it a good choice to use in the centrifuge method. Maria's work consisted in pinning down the range of temperatures in which the gas was stable and establishing its exact chemical structure. She did this by using measurements of its spectrum to calculate the gas's thermodynamic properties. The fact that she could predict the behaviour of UF6 at different temperatures was important for the continuing development of the gaseous diffusion method.

During the war years, she did not publish, as was the rule for military research at the time. The work she did then was, however, indubitably significant for a paper she published after the war with Jacob Bigeleisen, a chemist who also worked at Columbia on the uranium isotope separation project (after the war, he worked at Ohio State University and then the University of Chicago, before joining Brookhaven National Laboratory in 1948), entitled 'Calculation of Equilibrium Constants for Isotopic Exchange Reactions' (*Journal of Chemical Physics,* 1947). She also wrote a paper with Bigeleisen, Peter Stevenson and John Turkevich, 'Vibrational Spectrum and Thermodynamic Properties of Uranium Hexafluoride Gas' (*Journal of Chemical Physics,* 1948). The data on the spectra of uranium compounds were published by the Atomic Energy Commission after the war. They included a detailed analysis of fluorescence and the absorption spectra of the compounds, with a 75-page appendix of spectroscopic data, most of it prepared by Goeppert Mayer.

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The Opacity Project

In the summer of 1941, while waiting for atomic bomb research to really get under way, Edward Teller settled at Columbia University, seizing the opportunity to advise Urey's group on isotope separation. This enabled him to forge a stronger relationship with the Mayers. In his memoirs he recalled, 'Our closest friends that year, the Fermis, the Mayers, and the Ureys, all lived outside New York City, in Leonia; therefore, although we lived near the university, Mici [his wife] and I did a considerable amount of commuting.' This friendship, and pure scientific respect, played a part in Teller's asking Maria Goeppert Mayer in 1943 to join the Opacity Project, an endeavour related with the bombs being prepared in the Manhattan Project.

Teller joined the Los Alamos Laboratory in 1943. He did not like the fact that Oppenheimer had put Hans Bethe at the head of the Theoretical Division, because he thought he had greater merits and more experience as regards the idea of using the fission of uranium to make bombs. The situation worsened when Bethe tried to get Teller to do work that Teller did not care for. To solve the problem, Oppenheimer gave him a different assignment, which Teller explained in his memoirs as follows:

> A few weeks later [after his clash with Bethe], Oppie [Oppenheimer] gave me an assignment I welcomed because it involved a unique privilege: travel. We could discuss our work openly within the confines of Los Alamos, but security measures prevented an exchange of information among the laboratories working on various aspects of the program. Because an exchange of information was badly needed by the laboratories at Hanford, Oak Ridge, Chicago and Columbia, Oppie delegated four of us to be spokespeople; I was largely responsible for communications with Columbia.
>
> Part of the reason for my selection, I suspect, was that I repeatedly raised a question about the transport of energy within the bomb [he did this at least since 1942]. I knew from my work with Gamow that *opacities* –the ease or difficulty with which electromagnetic radiation can be transferred through a material–

play an important role in determining the time needed for the energy released in the center of the sun to move to a point at the surface of the sun.

> Given the temperatures in the bomb, I believed that radiation transfer –in this case, by means of X-rays– might play an important role in determining the effectiveness of the bomb. When opacity is low, radiation escapes rapidly. That means the bomb would develop less energy because the pressure building up during an explosion would be lower. A bomb with more time to develop energy would more efficient; therefore, easy escape of radiation, or to use the technical term, *low opacity*, might have important effects on the atomic bomb. Superficial estimates suggested that the loss of energy through radiation would not play a great role. Although Oppie agreed that precise statements would be much better, no one at Los Alamos had time to do the calculations needed to prove or disprove the question.

And as Teller was in charge of supervising the work done at Columbia, he thought his friend Maria Goeppert Mayer would be a choice candidate to participate in the project, with her solid knowledge of quantum mechanics. He felt,

> Even though Maria had not left Germany because she was in danger (only her grandmother or great-grandmother was a Jew), she hated Nazism. I believed that she would be delighted if she could contribute to the war effort [actually, she already was, by working on isotope separation] and told Oppie so. In November 1943, Oppie made me the intermediary to propose the task to her and, if she accepted, to supervise that effort.

Because of the nature of the work, Maria had to be issued authorization. Oppenheimer approved, but it was not he who had to give official permission. For that Goeppert Mayer and Teller travelled to Washington, D.C., where she secured her authorization. In the spring of 1945 Maria was invited to spend a few months at Los Alamos to get a more complete picture of the project and work closely with Teller. Teller referred to her time there in his memoirs:

> Seeing Los Alamos through Maria's eyes, I realized what a wonderful and remarkable place it was, and my admiration of Oppenheimer's talents as an administrator deepened. A few weeks after she arrived, Maria got word that, with the end of the campaign in Okinawa, Joe [Mayer] was coming home. Stanley Frankel and I drove Maria down the canyons to Albuquerque. She had decided on the extravagant but speedy adventure of flying home; at that time, more than a half a century ago, neither Santa Fe nor Los Alamos had an airport.

And so Maria came into her domain, the domain of the properties of matter and radiation at extremely high temperatures, which in fact had a great deal to do with another scientific endeavour on which Teller would later work very hard:

making a hydrogen bomb. At the same time, Maria strengthened her relationship with Teller, which eventually led to further work together, as we shall see in the next chapter. Though she could hardly have suspected it, the research path she took then would end up leading her to her great scientific success, the achievement that won her the Nobel Prize.

CHAPTER 5

The Road to the Nobel Prize

Lessons of War

Just a few days after atom bombs destroyed Hiroshima and Nagasaki and World War II reached its end, three representatives from the University of Chicago went to Santa Fe, the city near Los Alamos Laboratory. There they spoke to Fermi and a few other scientists about the university's plans to create a nuclear research institute where chemists, biologists and engineers would work alongside physicists. The goal would be to continue down the trail blazed during the war, since Chicago had done some important atom bomb work.

The University of Chicago's initiative fell within the broader context of the post-war importance of scientific research (especially but not only research in physics and, within physics, nuclear physics). The military authorities, and through them the political authorities, now thoroughly understood how science had given the nation an edge that helped it achieve victory, and they realized that wartime efforts ought to be continued. The examples of how the military and the federal government took an interest in scientific research abound. Let us look at two of them.

The first example I have selected involves the Hungarian engineer Theodore von Kármán, one of the 20th century's foremost experts in the physics of fluids and aerodynamics. Early in the 1930s, von Kármán left the institute he directed in Germany for the California Institute of Technology, where he directed the Daniel Guggenheim Graduate School of Aeronautics. There he became the top U.S. authority in the field.

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As an eminent applied scientist specializing in aeronautics (which proved crucial during the war), von Kármán received many demands from the armed forces, which he normally accepted, even long before World War II began. Accordingly, it is unsurprising that Henry Harley Arnold, Commanding General of the Army Air Forces and the leading defender of the idea that the U.S. should have an air force just as strong as its army and its navy, was frequently in contact with the California Institute of Technology, home to one of the nation's major aeronautical schools.

One of Arnold's challenges as head of the country's armed forces was, as he said in his autobiography *Global Mission* (1949), 'to get the best brains available, have them use as a background the latest scientific developments in the air arms of the Germans and the Japanese, the R.A.F., and determine what steps the United States should take to have the best Air Force in the world twenty years hence'. Arnold particularly wanted to find somebody to head up a committee of scientists – 'practical scientists', he said– and engineers with experience in 'sonics, electronics, radar, aerodynamics, and any other phases in science that might influence in any way the development of aircraft in the future'. Robert Millikan, who was responsible for bringing von Kármán to Caltech, recommended von Kármán to Arnold, and the general took his advice.

Arnold and von Kármán's relationship began in 1944, when the end of World War II was near, although they had known each other since 1936. It was not long before von Kármán arrived in Washington, and soon new scientists began to trickle in after him. 'I told these scientists', Arnold continued in his autobiography, 'that I wanted them to think ahead twenty years They were to forget the past; regard the equipment now available only as the basis for their boldest predictions. I wanted them to think about supersonic speed airplanes, airplanes that would move and operate without crews; improvements in bombs, so that we could use smaller bombs to get greater effects; defenses against modern and future aircraft; communication systems between airplanes themselves in the air; television, weather, medical research; atomic energy, and any other phase of aviation which might affect the development and employment of the air power to come'.

Von Kármán became Arnold's direct subordinate. Arnold put his wish list in black and white on 7 November of that very year, 1944, in a memo to von Kármán that should be quoted here, as it explains the attitude that came to drive the American forces in the air and, through them, a large measure of the scientific policy of the more-developed nations. Although the U.S. Air Force is not the only branch of the nation's armed forces, it probably wielded more post-war influence than any other military organization in the design and establishment of defence and attack tactics and in federal research and development policy, because of its potential and its dependence on technology (a copy of the memo, along with von Kármán's answer and reports, which I will quote later, lie in box 175.4 of the *Th. von Kármán Collection* at the Robert A. Millikan Library, California Institute of Technology Archives).

MEMORANDUM FOR DR. VON KÁRMÁN:

Subject: AAF [Army Air Forces] Long Range Development Program

1. I believe the security of the United States of America will continue to rest in part in developments instituted by our educational and professional scientists. I am anxious that Air Force's postwar and next-war research and development programs be placed on a sound and continuing basis. In addition, I am desirous that these programs be in such form and contain such well thought out, long range thinking that, in addition to guaranteeing the security of our nation and serving as a guide for the next 10-20 year period, that [*sic*] the recommended programs can be used as a basis for adequate Congressional appropriations.

2. To assist you and your associates in our current concepts of war, may I review our principles. The object of total war is to destroy the enemy's will to resist, thereby enabling us to force our will on him. The attainment of war's objective divides itself into three phases: political, strategic and tactical. Political action is directed against the enemy's governing power, strategic action against his economic resources, and tactical action against his armed forces. Strategical and tactical actions are our main concern and are governed by the principles of objective, surprise, mass, offensive, movement, economy of forces, cooperation and security.

3. I believe it is axiomatic that:

a. We as a nation are now one of the predominant powers.

b. We will no doubt have potential enemies that will constitute a continuing threat to the nation.

c. While major wars will continue to be fought principally between the 30th and 60th parallels, north, global war must be contemplated.

d. Our prewar research and development has often been inferior to our enemies [*sic*].

e. Offensive, not defensive, weapons win wars. Counter-measures are of secondary importance.

f. Our country will not support a large standing army.

g. Peace time economy requirements indicate that, while the AAF now receives 43% of current War Department appropriations, this allotment or this proportion may not continue.

h. Obsolete equipment, now available in large quantities, may stalemate developments and give Congress a false sense of security.

i. While our scientists do not necessarily have the questionable advantage of basic military training, conversely our AAF officers cannot by necessity be professional scientists.

j. Human-sighted (and perhaps radar or television assisted) weapons have more potential efficiency and flexibility than mechanically assisted weapons.

k. It is a fundamental principle of American democracy that personnel casualties are distasteful. We will continue to fight mechanical rather than manpower wars.

l. As yet we have not overcome the problems of great distances, weather and darkness.

m. More potential explosives, supersonic speed, greater mass offensive efficiency, increased weapon flexibility and control, are requirements.

n. The present trend toward terror weapons such as buzz bombs, phosphorous and napalm may further continue toward gas and bacteriological warfare.

4. The possibility of future major wars cannot be overlooked. We, as a nation, may not always have friendly major powers or great oceanic distances as barriers. Likewise, I presume methods of stopping aircraft power plants may be soon be available to our enemies. Is it not now possible to determine if another totally different weapon will replace the airplane? Are manless remote-controlled radar or television assisted precision military rockets or multiple seekers a possibility? Is atomic propulsion a thought for consideration in future warfare?

5. Except perhaps to review current techniques and research trends, I am asking you and your associates to divorce yourselves from the present war in order to investigate all the possibilities and desirabilities for postwar and future

war's development as respects the AAF. Upon completion of your studies, please then give me a report or guide for recommended future AAF research and development programs. May I ask that your final report also include recommendations to the following questions:

a. What assistance should we give or ask from our educational and commercial scientific organizations during peacetime?

b. Is the time approaching when all our scientists and their organizations must give a small portion of their time and resources to assist in avoiding future national peril and winning the next war?

c. What are the best methods of instituting the pilot production of required nonrevenue equipments of no commercial value developed exclusively for the postwar period?

d. What proportion of available money should be allocated to research and development?

Von Kármán took Arnold's assignment very seriously. He formed a group that was known as the Army Air Force Scientific Advisory Group (later the U.S. Air Force Scientific Advisory Group). A little over a year later, on 15 December 1945, he turned in his answer. He told the general that in 'cooperation with a group of selected associates, experts in various branches of the sciences involved', he had endeavoured to 'review the scientific requirements involved in the functions of the future Air Forces'. The results of the Caltech professor's team took the shape of a report entitled *Toward New Horizons*, which remained secret until the late 1950s (in its Torrejón campus library, the Spanish National Institute of Aerospace Technology –Instituto Nacional de Técnica Aeroespacial, INTA– has a copy of this document translated into Spanish by Antonio Pérez-Marín. It is a 166-page mimeographed limited edition –the one I consulted was number 6– published in 1959 under the heading 'Traducción del Informe del año 1945 al General H. H. Arnold', 'Hacia Nuevos Horizontes', 'Ciencia, llave de la supremacía aérea', by 'Teodoro von Kármán'). The document is divided into thirteen parts. The first contains a rather general discussion of the relationship between science and aerial war and an analysis of the Air Forces' main research problems. The following twelve parts include 32 scientific monographs on a wide range of subjects, such as 'Aerodynamics and Aircraft Design', 'Future Trends in the Design and Development of Solid and Liquid Fuel Rockets', 'High Temperature Materials', 'Automatic Control of Flight', 'Heat and Television Guided Missiles', 'Radar Aids for the Guidance of Missiles', 'Properties of High Explosives', 'The Use of Radar in Air Force Operations', 'Future Trends of Research in Aviation Medicine' and 'Psychological Research in the Army Air Forces'.

The first three general conclusions of von Kármán's report were these:

> 1. The discovery of atomic means of destruction makes a powerful Air Forces even more imperative than before […].
>
> 2. The scientific discoveries in aerodynamics, propulsion, electronics, and nuclear physics, open new horizons for the use of air power.
>
> 3. The next ten years should be a period of systematic, vigorous development, devoted to the realization of the potentialities of scientific progress, with the following principal goals: supersonic flight, pilotless aircraft, all-weather flying, perfected navigation and communication, remote-controlled and automatic fighter and bomber forces, and aerial transportation of entire armies.

Von Kármán himself wrote the report's first paper, 'Science, the Key to Air Supremacy', which concluded by stressing the need for a more powerful air force,

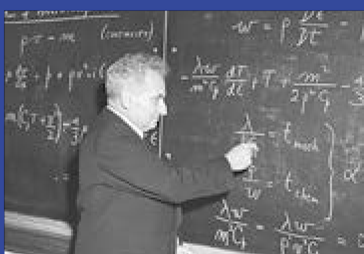
> a. Reaching remote targets swiftly and hitting them with great destructive power.
>
> b. Securing air superiority over any region of the globe.
>
> c. Landing, in a short time, powerful forces, men and firepower, at any point of the globe.
>
> d. Defending our own territory and bases in the most efficient way.

And obviously 'only an air force which fully exploits all the knowledge and skill which science has available now and will have available in the future, will have a chance of accomplishing these tasks'.

Von Kármán's report to General Arnold is a magnificent example of how militarization threatened the future of U.S. science. That 'threat' was made good, too, at least partially. But there were other trends, other forays into what science could offer future society once the war was over. Two big names spring to mind, that of President Roosevelt and that of Vannevar Bush, director (as we know) of the Office of Scientific Research and Development. On 17 November 1944, Roosevelt sent Bush the following letter, which was included in the publication containing Bush's report, *Science, the Endless Frontier. Report to the President on a Program for Postwar Scientific Research* (United States Government Printing Office, Washington 1945):

> Dear Dr. Bush: The Office of Scientific Research and Development, of which you are the Director, represents a unique experiment of team-work and cooperation in coordinating scientific research and in applying existing scientific knowledge to the solution of technical problems paramount in war. Its work has been conducted in the utmost secrecy and carried on without public recognition

Maria Goeppert Mayer studying isotope abundance



Theodore von Kármán

Vannevar Bush and President Harry Truman (1948)

Hans D. Jensen, who co-authored *Elementary Theory of Nuclear Shell Structure* with Maria Goeppert Mayer of any kind; but its tangible results can be found in the communiques coming in from the battlefronts all over the world. Someday the full story of its achievements can be told.

There is, however, no reason why the lessons to be found in this experiment cannot be profitably employed in times of peace. The information, the techniques, and the research experience developed by the Office of Scientific Research and Development and by the thousands of scientists in the universities and in private industry, should be used in the days of peace ahead for the improvement of the national health, the creation of new enterprises bringing new jobs, and the betterment of the national standard of living.

It is with that objective in mind that I would like to have your recommendations on the following four major points:

First: What can be done, consistent with military security, and with the prior approval of the military authorities, to make known to the world as soon as possible the contributions which have been made during our war effort to scientific knowledge?

The diffusion of such knowledge should help us stimulate new enterprises, provide jobs for our returning servicemen and other workers, and make possible great strides for the improvement of the national security.

Second: With particular reference to the war of science against disease, what can be done now to organize a program for continuing in the future the work which has been done in medicine and related sciences?

The fact that the annual deaths in this country from one or two diseases alone are far in excess of the total number of lives lost by us in battle during this war should make us conscious of the duty we owe to future generations.

Third: What can the Government do now and in the future to aid research activities by public and private organizations? The proper roles of public and of private research, and their interrelation, should be carefully considered.

Fourth: Can an effective program be proposed for discovering and developing scientific talent in American youth so that the continuing future of scientific research in this country may be assured on a level comparable to what has been done during the war?

New frontiers of the mind are before us, and if they are pioneered with the same vision, boldness, and drive with which we have waged this war we can create a fuller and more fruitful employment and a fuller and more fruitful life.

> I hope that, after such consultations as you may deem advisable with your associates and others, you can let me have your considered judgment on these matters as soon as convenient –reporting on each when you are ready, rather than waiting for completion of your studies in all.
>
> Very sincerely yours
>
> Franklin D. Roosevelt

The letter clearly reveals President Roosevelt's breadth of vision. He did not allow himself to forget that in future there would be, or should be, something more than military confrontations between powers, potential wars, and that therefore scientific research should not be restricted, or severely curtailed, to the military applications of its immense possibilities. Roosevelt's intention was to place science in the public domain as soon as possible, and this goal shines clearly in his letter to Bush.

Bush completed Roosevelt's assignment, produced the requested report and sent it to the president –President Truman, since Roosevelt had died– on 5 July 1945. Unlike von Kármán's report to Arnold, the Bush report was made public that same year. It was titled *Science, the Endless Frontier. Report to the President on a Program for Postwar Scientific Research*.

I will simply quote some of Bush's conclusions and recommendations, which hewed faithfully to the lines laid out by Roosevelt.

With regard to medicine, Bush said the 'Government initiative and support for the development of newly discovered therapeutic materials and methods can reduce the time required to bring the benefits to the public', adding that 'it is clear that if we are to maintain the progress in medicine which has marked the last 25 years, the Government should extend financial support to basic medical research in the medical schools and in the universities, through grants both for research and for fellowships. The amount which can be effectively spent in the first year should not exceed 5 million dollars. After a program is under way perhaps 20 million dollars a year can be spent effectively'.

The director of the OSRD did not overlook science's relationship with the armed forces, but he did emphasize the need for some kind of civil control: 'Military preparedness requires a permanent independent, civilian-controlled organization, having close liaisons with the Army and Navy, but with funds directly from Congress and with the clear power to initiate military research which will supplement and strengthen that carried on directly under the control of the Army and the Navy'.

Some of what he wrote on the value of science to industry is just as applicable today: 'A nation which depends upon others for its new basic scientific knowledge will be slow in its industrial progress and weak in its competitive position in world trade, regardless of its mechanical skill'.

And I would also like to mention Bush's recommendations with respect to higher education: 'Publicly and privately supported colleges and universities and the endowed research institutes must furnish both the new scientific knowledge and the trained research workers [...]. It is chiefly in these institutions that scientists may work in an atmosphere which is relatively free from the adverse pressure of convention, prejudice, or commercial necessity [...]. If the colleges, universities, and research institutes are to meet the rapidly increasing demands of industry and Government for new scientific knowledge, their basic research should be strengthened by use of public funds'.

Many of the ideas Bush laid out in his report about what universities ought to do in future came to pass, although some universities did establish institutions to do research that ventured outside the 'civil dimension' into military interests. In fact, the armed forces were some of the top funders of universities and research.

In February 1946 Joseph and Maria Mayer, like Fermi, Urey and Teller before them, went to the University of Chicago. Before we get into that, however, this is a good place to discuss a research programme that indirectly involved some of Maria Goeppert Mayer's work, as I said at the end of the last chapter.

Edward Teller and the H-Bomb

On 3 September 1949, one of the samples taken by the B-29s the U.S. Air Force was using to analyse the air over Japan, Alaska and the North Pole found evidence of the first Soviet nuclear explosion in the North Pacific near Japan. The explosion had taken place on 29 August ('Joe 1', the Americans called it). The explosion of the Soviet atom bomb meant the start of an *atomic race.* In the United States, Ernest Lawrence and Edward Teller argued that the country had to counterattack by developing a new weapon to counteract the Soviet's. They were talking about a superbomb much more powerful than the bombs used in 1945, a hydrogen bomb, i.e., a fusion bomb using processes similar to the thermonuclear reactions that take place inside stars, where light elements combine to produce heavier elements in a blast of energy.

Teller's idea of a fusion bomb dated back to the early days of the wartime atomic project. At a July 1942 seminar on the theory behind the atom bomb, Teller had suggested the possibility that a fission bomb might be used to deto-

The explosion of the Soviet atom bomb meant the start of an *atomic race*. In the United States, Ernest Lawrence and Edward Teller argued that the country had to counterattack by developing a new weapon to counteract the Soviet's.

nate a fusion bomb, and in 1944 he defended the idea that an implosion bomb could be made using deuterium and tritium as fuel. But Los Alamos had been created to build a fission bomb, and Teller's proposal met with a cool reception. Teller decided to pursue his own studies into a possible thermonuclear bomb. And when the war was over and the effects of Hiroshima were known, few scientists were eager to continue nuclear weapons research. As a consequence, no progress had been made toward developing the bomb Teller wanted, for lack of a suitable programme to back it (the Soviets, to the contrary, embarked on such a project in 1948). In 1949 the American scientific community was no longer as united as it had been a few years before, and views were clearly split. Oppenheimer was against it, which meant that the Atomic Energy Commission (or AEC, the civil organization that took over U.S. nuclear affairs in January 1947 when the Manhattan Engineer District was disbanded) ended up declaring him a security risk in 1953-1954, denying him access to atomic secrets. Lawrence, on the other hand, campaigned fiercely for the new bomb, personally visiting the AEC, the Joint Committee on Atomic Energy, the Department of Defense and even Congress. In the post-war years, especially in the United States, Big Science (almost all of which had military applications) was considered a matter of state and as such was debated in all kinds of forums. Remote indeed lay the times when practically everything hinged on the initiatives of scientists themselves or their patrons, the Siemenses, Carnegies and Rockefellers. Teller made an aggressive appeal to his colleagues in 1950, which was published in the *Bulletin of the Atomic Scientists* under the title 'Back to the Laboratories'. He compared the international situation in the wake of the Soviet nuclear bomb with the situation in 1939. For the Hungarian physicist, the decision to use something like the hydrogen bomb was the responsibility of politicians, not scientists. In his opinion the man of science was not 'responsible for the laws of nature. It is his job to find out how these laws operate. It is the scientist's job to find the ways in which these laws can serve the human will. However, it is not the scientist's job to determine whether a hydrogen bomb should be constructed, whether it should be used, or how it should be used'. In a clear allusion to the sorts of activities other physicists (like Pauli) were then engaged in, Teller added, 'our scientific community has been out on a honeymoon with mesons. The holiday is over. Hydrogen bombs will not produce themselves'. Luis Álvarez issued a similar pronouncement in June 1951: 'Anyone who now takes the time to work on mesons is little less than a traitor'.

For a better grasp of what things were like at the time, we have to factor in not only the U.S.'s relations with the Soviet Union, but also the fact that Communists led by Mao Tse Tung seized power in China in 1949. Because of these events, the U.S. National Security Council dug in behind the idea that the United States was facing a period of extreme danger and should fully rearm for its own security. The council's recommendation to the president called for approval of a superbomb budget of 100 to 200 million dollars. On top of everything else, British spy Klaus Fuchs confessed that he had been spying for the Soviet Union and passing on hydrogen bomb data since 1942, and this too helped swing the balance. On 31 January 1950, Truman approved the proposal. That same day the president made the following public statement: 'It is part of my responsibility as Commander-in-Chief of the armed forces to see to it that our country is able to defend itself against any possible aggressor. Accordingly, I have directed the Atomic Energy Commission to continue its work on all forms of atomic weapons, including the so-called hydrogen or super bomb.' Two years and nine months later, the United States set off a bomb ('Mike') that was 1,000 times more powerful than the bombs of 1945. Three years and a few weeks later, the Soviets detonated their own 'Mike' in central Asia.

The American nuclear programme's budget skyrocketed under Truman's order. The programme's expenses increased by almost sixfold between 1947 (318.3 million dollars) and 1952 (1,766.4 million dollars), with the real quantitative change starting in 1949 (631.9 million dollars).

Sheltered by military interests, civil research into fusion (that is, research aimed at developing technologically and economically viable systems for producing energy) flourished. It was hardly by chance that the United States, the Soviet Union and Great Britain were the countries that made the most progress in this scientific field. In the case of the U.S., because of Truman's decision to build the superbomb, two fusion laboratories were opened at Princeton University in 1951. One was dubbed 'Matterhorn B' ('B' for 'Bomb') and directed by John A. Wheeler, an old colleague of Niels Bohr's, teacher of Richard Feynman and Manhattan Project veteran, who was convinced of the need for superbombs. The other was named 'Matterhorn S' ('S' for 'Stellarator', the 'Star Machine', the name given to the instrument developed to study the plasma from nuclear fusion) and was directed by astrophysicist Lyman Spitzer, who unsurprisingly (given his specialty) was deeply interested in controlled thermonuclear reactions. Wheeler was researching explosive reactions, but he needed the basic knowledge generated in Spitzer's lab, where important progress was in fact made in the theory of controlled fusion and the design of the stellarator, although ultimately the stellarator proved less advantageous than the Soviet device developed for the same purpose, the tokamak.

The case of the hydrogen bomb also affords an understanding of the mindset, the reasons why tip-top scientists (and some of those involved in the effort were indeed the very best in the U.S. as well as in the U.S.S.R.) decided to lend a hand in solving the problems of building the bomb even when not embroiled in a world war. Naturally, there are as many answers to this question as there are scientists, so diverse is the universe of individual minds. I will just cite the reasons of an especially exceptional scientist, Soviet physicist Andrei Sakharov (1921-1989), who contributed decisively to the Soviet Union's H-bomb but ended up as a significant opponent of the Communist regime and recipient of the Nobel Peace Prize in 1975.

In his memoirs Sakharov recalled how he started work in this field:

> In 1948, no one asked whether or not I *wanted* to take part in such work [helping to build a hydrogen bomb]. I had no real choice in the matter, but the concentration, total absorption, and energy that I brought to the task were my own. Now that so many years have passed, I would like to explain my dedication –not least to myself. One reason for it (though not the main one) was the opportunity to do 'superb physics' (Fermi's comment on the atom bomb program). Many people thought his remark cynical, but cynicism ordinarily presupposes duplicity, whereas I believe Fermi was quite sincere, although he may have been begging the real question. It should not be forgotten that Fermi's complete sentence –'*After all,* it's superb physics'– implies the existence of another side to the matter.

That Sakharov considered thermonuclear fusion 'superb physics' was shown where he said, 'The physics of atomic and thermonuclear explosions is a genuine theoretician's paradise [...]. A thermonuclear reaction –the mysterious source of the energy of sun and stars, the sustenance of life on Earth but also the potential instrument of its destruction– was within my grasp. It was taking shape at my very desk [...]. What was most important for me at the time, and also, I believe, for Tamm and the other members of the group, was the conviction that our work was *essential*'. Of course, he realized 'the terrifying, inhuman nature of the weapons we were building. But the recent war had also been an exercise in barbarity, and although I hadn't fought in that conflict, I regarded myself as a soldier in this new scientific war'.

Sakharov's words show us just how attractive good scientific research is to scientists, frequently outweighing practically any other consideration. Even for scientists who have moral sensitivity and civic bravery, as Sakharov eventually proved. The 20th century was witness to numerous examples of a similar sort.

And now we are ready to get back to Maria Goeppert Mayer.

Maria Goeppert Mayer at the University of Chicago

The idea of creating an institute of nuclear studies at the University of Chicago as mentioned at the start of this chapter predated the end of the war. The Metallurgical Laboratory associated with the Manhattan Project and directed by Arthur Compton, as explained in chapter 4, was soon shut down, making way for Argonne National Laboratory. The idea, in the light of the experience, was to replace it with a similar interdisciplinary facility where some of the scientists who had been drawn into the Metallurgical Laboratory during the war years could work. This notion was promoted by astronomer Walter Bartky, who replaced Compton as head of Chicago's physics department. Bartky wanted Fermi to direct the future institute, but Fermi did not want to get involved in administrative work, so the job eventually went to Samuel Allison. Fermi did agree to settle down at the University of Chicago, however.

But Harold Urey played a more important role than Bartky in the new institute's creation. What the discoverer of deuterium wanted was to keep the University of Columbia group –Fermi, Joseph and Maria Mayer and Teller– together. With their okay, Urey looked first at the North Pacific coast, where living conditions were better than in the central states or the east coast. In July Urey informed Teller that he had met the president and members of the Department of Chemistry of the University of Washington (in the state of Washington), where his idea was warmly received. However, on second thoughts, Urey realized that there would be trouble if the institute were set up at a state university that had no wartime experience with research contracts and nobody who knew how to create a research institute. Furthermore, since the University of Washington was a public university, any initiative had to be authorized not only by the university's regents, but also by state legislators, and that would take lots of time and effort. So, the University of Chicago was a much better option.

When he left Los Alamos after the end of the war, Urey travelled through Chicago on his way back to New York, where he was still teaching at Columbia. In Chicago he received a firm offer to establish an institute of nuclear studies where he would have a position shared with a professor from the chemistry department. They told him they had also offered Teller a job in the physics department and were seriously considering making Joseph Mayer an offer. And at this point I can resume the story where we left it at the start of this chapter, how three University of Chicago representatives paid a call in Santa Fe. Laura Fermi described the events in her book *Atoms in the Family* as follows:

> The idea of a research institute was born in Chicago during the spring preceding the end of the war. Arthur Compton had pondered for a while how best to keep together some of the physicists, biologists, chemists, engineers, and even metallurgists, whom he had first gathered at the Metallurgical Laboratory […].
>
> By the middle of July [1945] it was felt that correspondence was no longer adequate, that a meeting was necessary between representatives of the university and a few scientists. Harold Urey, Samuel K. Allison, Cyril S. Smith and Fermi ought to be consulted. But the last three were extremely busy in Los Alamos during that July of 1945. They could not go to Chicago. Vice-president of the University Gustafson, Walter Bartky, dean of the division of the physical sciences, and Harold Urey were willing to take a trip to New Mexico, but they had no pass to Los Alamos. The six men met in Santa Fe […].
>
> Over a lunch of sandwiches packed at Fuller Lodge on the mesa the policies for the future institute were discussed. It would not be divided into departments. It would provide a meeting ground for science and industry. The industry might give financial support to the institute and in return receive scientific advice and information on progress of research.
>
> The new institute needed a director, and the six men consulted with one another. Harold Urey said he had tried administrative work, and he felt he was not suited for it. Fermi had never done administrative work, but he was sure he was not suited. Cyril Smith was a metallurgist who had been in industry before joining the uranium project. He had no experience in university work, he said. Sam Allison could not think of a good excuse and was named director on the spot. Yet Sam Allison voiced some doubts: the responsibility of directing research in biology and metallurgy, besides that in physics and chemistry, was too great. Biology and metallurgy were remote from his field of research. Three Institutes for Basic Research were established: the Institute for Nuclear Studies, the Institute of Metals, and the Institute of Radiobiology. Allison remained the director of the first [...].
>
> The Institutes for Basic Research started to function at the beginning of 1946. And so we came to live in Chicago.

The most hesitant member of Urey's planned group was Teller. Teller hoped to keep working on the H-bomb project at Los Alamos, but, as he explained in his memoirs, the day after the surrender of Japan, 'Oppenheimer came to my office to tell me that "with the war over, there is no reason to continue the work on the hydrogen bomb". His statement was unexpected. It was also final. There was no way I could argue; no way I could change Oppenheimer's mind'. The George Washington University was still waiting for him after four years' absence, but in the end he took up Chicago's offer. Its initiative 'to continue nuclear research by collecting Manhattan Project scientists in a special institute led by Fermi' held a powerful attraction for Teller. He also said in his memoirs, 'In addition to Fermi, the university had signed up Harold Urey, James Franck [who, as we have seen, joined the university before the war started], Leo Szilard, Cyril Smith and Joe and Maria Mayer. The invitation to join my friends was too tempting to turn down'. Others joined the institute later on, including Gregor Wentzel and Subrahmanyan Chandrasekhar.

Joseph and Maria Mayer did indeed also leave Columbia for the University of Chicago. Joseph was offered a full professorship and membership in the Institute for Nuclear Studies. He took it. This time Maria also entered the deal; she was offered an associate professorship, but Chicago, like other schools, refused to pay her on the grounds of its rules against nepotism. Nevertheless, for the first time she was to have an office, and there would be nothing against her participating in university activities, including the activities of the Institute for Nuclear Studies. 'This', she said years later, 'was the first place where I was not considered a nuisance, but was greeted with open arms'. The Chicago years were her scientific golden age.

At almost the same time, the newly created Argonne National Laboratory offered her a position, a paid position, as senior physicist; no doubt it helped that the laboratory's director was her former student Robert Sachs. Argonne was founded on 1 July 1946 when the Metallurgical Laboratory was disbanded, and its mission was to carry out research in basic nuclear science and develop uses for nuclear energy, particularly (but not only) peaceful uses. It depended on the University of Chicago under a contract with the Atomic Energy Commission. It was located in the Argonne Forest, about 40 kilometres southwest of Chicago. Maria accepted the offer and became the first person to use an electronic computer to find the solution to the problem of the criticality (the state where a reactor's chain reaction is self-sustaining) of a liquid metal breeder reactor. She used the Monte Carlo method to program the ENIAC (Electronic Numerical Integrator And Computer), the first electronic computer, which was located at the Aberdeen Proving Ground's Ballistic Research Laboratory (a U.S. Army facility) in Maryland (a summary of this work was published in 1951 as part of the *U.S. Department of Commerce Applied Mathematics* series).

One intriguing detail that shows how important joining Chicago's Argonne National Laboratory was for Maria and how her colleagues at the university influenced her is the fact that, when she arrived, she knew little of nuclear physics.

Joseph and Maria Mayer also left Columbia for the University of Chicago. Joseph was offered a full professorship and membership in the Institute for Nuclear Studies. He took it. This time Maria also entered the deal; she was offered an associate professorship, but Chicago, like other schools, refused to pay her on the grounds of its rules against nepotism. The Chicago years were her scientific golden age.

Her speciality was quantum mechanics, and that was a necessary tool for nuclear physics, but nuclear physics went much farther, delving into the atomic nucleus. She confessed to Joan Dash that she arrived in Chicago with 'very little knowledge of Nuclear Physics! It took me some time to find my way in this, for me, new field [not really so very new, since in the 1930s she published two papers that had to do with nuclear physics]. But in the atmosphere of Chicago, it was rather easy to learn nuclear physics.' And not because books were not available: 'Never [did I learn much] from books. I read books occasionally, but I didn't sit down with a book and learn it'. She learned through absorbing knowledge at the Argonne's cosy weekly seminars. In view of the fact that Maria became part of physics history for solving a nuclear physics problem, the vast importance of this new twist in her career is evident, even though her own career once more had to take a back seat to her husband's.

Teller and Goeppert Mayer on the Origin of the Elements

When she moved to Chicago, Maria planned to keep working with Teller on the Opacity Project, and she did, as a consultant, while teaching at the university. But a year after having settled in Chicago Teller displayed a particular interest in a new problem that fascinated him, the origin of the elements. He wanted someone to work with him, someone who had a solid knowledge of mathematics. Maria was obviously the ideal candidate. And she said yes.

It comes as no surprise that Teller was interested in the problem, given how important making a hydrogen bomb was for him. And this meant considering a problem in element synthesis: the reaction in which two hydrogen isotopes produce helium, and the energy produced, a process that takes place inside stars.

His interest dated from years before, and it coincided with the interest of his friend and colleague at The George Washington University, George Gamow (in fact, it was Gamow who insisted on Teller's being hired as a condition of his own acceptance of a professorship). In the spring of 1938, Gamow and Teller decided to make the problem of stellar thermonuclear energy sources the subject of their university's yearly conference. One of the conference goers was Hans Bethe. Bethe knew nothing about what was inside stars when he arrived, but at the end of the conference he came up with a possible diagram of nuclear reactions involving hydrogen and carbon and producing enough energy to explain solar radiation observations (Bethe would later become one of the finest specialists in stellar nucleosynthesis; in 1967 he received the Nobel Prize in Physics 'for his contributions to the theory of nuclear reactions, especially his discoveries concerning the energy production in stars'). Shortly after the conference, one of Gamow's students, Charles Critchfield, proposed another theory of the energy production process dubbed the 'proton-proton reaction' (or H-H reaction), which began with the collision of two protons that formed a deuteron emitting a positron and a neutrino. We could say that it was then when the field of stellar nucleosynthesis was born.

Along with the problem of the transformations of light elements and the energy produced inside stars, there was the problem of the abundance of chemical elements in the universe. In his autobiography, *My World Line: An Informal Autobiography* (1970), Gamow explained, '[I]n the 1940s it was believed, not quite correctly, that the universe as a whole was chemically homogeneous, and that the relative abundance of different elements was fairly well represented by the constitution of our sun, the neighboring stars, and the interstellar material. About 99 per cent of matter was assumed to be formed by hydrogen and helium in nearly equal quantities (by weight), the remaining 1 per cent being accounted for by heavier elements in amounts decreasing with increasing atomic weight. It was natural to assume that the observed universal abundances of chemical elements do not result from the nucleosynthesis within the individual stars, which would lead to a great variety of chemical constitution, but go back to the early "prestellar" state of the universe, when matter was distributed completely homogeneously through the universe'. A paper of Gamow's in *Physical Review* (vol. 70, 1946) entitled 'Expanding Universe and the Origin of Elements', began with the following words: 'It is generally agreed at present that the relative abundances of various chemical elements were determined by physical conditions existing in the universe during the early stages of its expansion, when the temperature and density were sufficiently high to secure appreciable reaction-rates for the light as well as for the heavy nuclei'.

Photograph taken in the 1950s

Maria Goeppert Mayer in her office at the University of Chicago

With members of the University of Chicago at a New Year's Eve party, c. 1960

Scientists clustering around the carved rock memorial of the creation of Argonnne National Laboratory

With Argonne National Laboratory colleagues

The same year that Gamow published this paper, he became dissertation advisor to Ralph Alpher, who had taken his master's degree in 1945 at The George Washington University. Alpher, who was juggling a job he had held at the Johns Hopkins University Applied Physics Laboratory since 1944 with Navy work (which he had been doing since 1940), completed his dissertation in 1948. It was called 'The Origin and Relative Abundance Distribution of the Chemical Elements'. Even before submitting his dissertation, Alpher had prepared a paper, co-written with Gamow, that released some of his results. The article is now famous, not as much for its contents as for its, shall we say, 'circumstance'. It was published in volume 73 (1948) of *Physical Review* under the title of 'The Origin of Chemical Elements', and the 'circumstance' is that its authors are listed as Ralph Alpher, Hans Bethe and George Gamow. Bethe actually had nothing to do with the paper, but Gamow was an inveterate joker and included him unbeknownst to Bethe, because he wanted to 'complete' the sequence of αβγ (α for Alpher, β for Bethe and γ for Gamow). By the way, Maria Goeppert Mayer and Edward Teller's paper had practically the same title as the αβγ paper: 'On the Origin of Elements', and it too was published in *Physical Review* (vol. 76, 15 October 1949; received by the journal on 22 June).

According to the abstract preceding the article by 'Maria G. Mayer and Edward Teller' (credited like that, and signed in that order), the paper concerned the following: 'The abundances of elements and isotopes indicate that heavy and light elements have been produced by different processes. The origin of heavy elements is discussed in detail. It is assumed that the heavy elements were formed by a fission process from a neutron-rich nuclear fluid. Simple assumptions are made about this fission process and isotopic abundances are calculated for $62 \leqq Z \leqq 78$ [Z is the atomic number; the 62 corresponds to samarium, and the 78, platinum]. The properties of the neutron-rich liquid and possible details of the fission process are discussed'.

In his book *Cosmology and Controversy*, Helge Kragh gives this clear explanation of the contents of the Goeppert Mayer-Teller paper:

> Not all the nuclear-physical models of element formation were based on the hot primordial gas assumed by Gamow, Alpher and Herman [Robert Herman was a colleague of Alpher's at the Johns Hopkins laboratory who joined Gamow and Alpher's work]. In 1948-49 Maria Goeppert Mayer and Edward Teller, then at the University of Chicago, suggested that whereas the light elements may have been formed by thermonuclear equilibrium reactions, the mechanism of heavy-element formation was very different […]. The starting point of Mayer and Teller was a condensed fluid of cold nuclear matter consisting of or having a large excess of neutrons. This primordial hypothetical object, a 'polyneutron', differed from Gamow's somewhat similar speculations of 1942

> in not comprising the entire mass of the universe, but having a mass less than that of a star. The early universe would thus have had to comprise a multitude of polyneutrons, the origin of which Mayer and Teller did not account for. The polyneutron was assumed to break up in a kind of fission process, first to very heavy fragments with an excess of neutrons. The breakup of the polyneutron was not an ordinary fission, however, but the formation of small droplets which would break off from the surface of the polyneutron. After a series of nuclear reactions involving beta decay and neutron evaporation, heavy stable elements would be formed from the droplets. Mayer and Teller found that this mechanism was able to lead to a distribution of heavy isotopes ($Z>24$) in rough agreement with observations.

The year when the Goeppert Mayer-Teller paper appeared, George Gamow published a book, *Theory of Atomic Nucleus and Nuclear Energy Sources*, with Charles Critchfield, the former student mentioned before, who also worked at Los Alamos during the war. One of the subjects the book dealt with was the origin of the elements, and it is interesting to read what Gamow and Critchfield had to say about the Mayer-Teller theory (which they said Goeppert Mayer and Teller had told them about before publishing): 'We now turn to another, no less, fantastic picture of the origin of the elements proposed recently by Goeppert Mayer and Teller. In contrast to the theory described above [the theory Gamow proposed in 1946 and later enlarged upon with Alpher], these authors assume that, in its most compressed state, the universe was filled with a *neutral nuclear fluid*. As expansion began this nuclear liquid must have broken up into a number of drops of various sizes'. And here they addressed the possible behaviour of such drops, which, they noted, could be '1 Ångström, 1 centimetre or 1 kilometre in diameter', concluding that 'Goeppert Mayer and Teller were able to show that the masses of the newly formed nuclei will be of the order of a few hundred proton-masses. It must be admitted, however, that this point of view does not explain the observed abundance-curve any better than any of the other theories, since it leads to a broad Gaussian distribution of relative abundance among the heavy elements, and offers no explanation whatsoever for the extremely high abundance of the lighter elements'.

The Goeppert Mayer-Teller theory does not seem to have won over many fans. A letter from Rudolf Peierls in Birmingham to Ed Salpeter, dated 26 January 1950, reads:

> [D.] Wroe has turned over to cosmology and the origin of elements. It now looks as if the general idea of Teller and Mayer can be rescued assuming that the universe was at one time so small that it was completely filled with matter and nuclear density and low temperature, and then expanded. This leads to condensation more or less as in the cloud chamber and the rest proceeds as in Teller's

> [and Goeppert Mayer's] picture. There are, of course, very many complications to be allowed for and one cannot yet be sure of the answer.

Peierls continued investigating these possibilities, and in 1952 he, K.S. Singwi and D. Wroe published a paper in *Physical Review*, 'The Polyneutron Theory of the Origin of the Elements', where they assumed 'as an early stage in the expansion of the universe a homogeneous fluid of nuclear density and low temperature' and proved that, 'for reasonable values of the constants, this will, on expansion, leave the matter in the form of droplets of the same properties as those found in the Mayer-Teller "polyneutron". However, this model leads necessarily to an abundance curve in which the amount of heavy elements is at least comparable to that of the light elements, contrary to experience'.

The problem, the huge problem with all these theories, starting with Gamow's and continuing with Goeppert Mayer and Teller's, is that they started off on the wrong foot. Today it is thought that only light elements were formed in the initial moments of the Big Bang, basically hydrogen and a smaller amount of helium, while the heavier elements are 'manufactured' inside stars, whose pressures and temperatures make this possible. When stars are ripped apart in colossal supernovas, the heavier elements inside them are then scattered across the universe. It was not long before scientists began developing this other view of the origin of the chemical elements, a view, a theory, that found solid support in a 127-page article published in October 1957 in *Review of Modern Physics*. It was entitled 'Synthesis of the Elements in Stars', and it was signed by Margaret and Geoffrey Burbidge (a married couple), William Fowler and Fred Hoyle (in 1983 Fowler received the Nobel Prize for Physics, shared with Subrahmanyan Chandrasekhar, 'for his theoretical and experimental studies of the nuclear reactions of importance in the formation of the chemical elements in the universe'; in my opinion –and that of many other scientists and historians– Fred Hoyle should have won also).

From the Origin and Abundance of the Elements to the Shell Model

The last section showed us how nuclear physics was used to look into the origin and abundance of the chemical elements that populate the world, and this led in turn to a better understanding of the universe. Well, cosmology was not the only discipline that led to inquiries into the abundance of the elements; a new science, geochemistry, did the same.

The origin of geochemistry dates back to the 1920s, although it did not become an independent discipline until around 1950. The Finns Kalervo Rankama and Thure Sahama wrote what was possibly the first modern geochemistry text-

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book, *Geochemistry* (1950). The book stressed the new discipline's relationship with nuclear physics: 'There exists between nuclear physics and geochemistry a contact zone in the abundance studies [of the chemical elements], and it is evident that the two fields of study are able to contribute much to each other'. *Geochemistry* was in fact published by the University of Chicago Press, and one of the chemistry professors at the University of Chicago was William Draper Harkins (1873-1951), one of the first chemists to inquire into nuclear physics, in addition to pioneering geochemistry. His accomplishments include having deduced the basic process of nuclear fusion, which was essential for Teller's longed-after H-bomb. Some of Harkins' work also provides a glimpse into certain aspects of Maria's later contribution to the structure of the nucleus as well. In a paper he published in 1917 ('The Evolution of Elements and the Stability of Complex Atoms', *Journal of the American Chemical Society*), Harkins concluded that on average elements with an even atomic number were about 70 times more abundant in meteorites than elements with an odd Z. This ratio became known later as the 'Harkins rule' (Harkins used meteorites because he believed they were the best available example of the elements existing in the universe and therefore their analysis could lead to conclusions about the distribution of the abundance of the different chemical elements).

He also said that the first seven elements (in order of abundance) had an atomic number, Z, that was even, and that they accounted for almost 99 percent of the matter in meteorites. Later he proposed other rules, such as the rule that atoms with a mass number, A, that is even and an odd number of nuclear electrons, E, are extremely rare. Note, however, that at the time (before the discovery of the neutron) the nuclei of atoms were assumed to be made up of protons and electrons, so A represented the number of protons; now, however, A=Z+N, where N is the number of neutrons (for Harkins, E=A-Z). I have been unable to trace any relationship between Joseph or Maria Mayer and Harkins, but I have mentioned him because, as I explained, some of the ideas Harkins was dealing with, such as the ideas I have just outlined, from part, in an updated form, of the ideas Maria dealt with in arriving at her nuclear shell model.

Harkins was not the only scientist treading these paths. Another traveller was Viktor Goldschmidt (1888-1947), the Swiss Norwegian who is regarded as the founder of modern geochemistry. In 1926 Goldschmidt pointed out the odd scarcity of lithium, beryllium and boron, suggesting that it might explain nuclear physics. Goldschmidt was well abreast of nuclear physics, as proved by one of his reference works, *Geochemische Verteilungsgesetze der Elemente* (1923-1938; *The Geochemistry of the Distribution of the Elements*), published in the proceedings of the Norwegian Academy of Science; one part of it, the portion printed in 1937, contains references to recent work in nuclear physics, including 'Bethe's Bible'.

It was this environment (or, if one prefers, this tradition of scientific research) that proved so conducive to Maria Goeppert Mayer's successful work. This and the research she did with Teller into the origin of the chemical elements. But for a good explanation of what Maria did, first we need to know about the concept of 'magic numbers' in nuclear physics. According to Steven Moszkowski, one of Maria Goeppert Mayer's students, the term 'magic number' was coined by Eugene Wigner: 'Wigner too believed in the liquid drop model, but he recognized, from the work of Maria Mayer, the very strong evidence for the closed shells. It seemed a little like magic to him, and that is how the words "Magic Numbers" were coined'.

Perhaps the best explanation of magic numbers appears in a paper by Maria G. Mayer herself, published in the March issue of *Scientific American* in 1951 ('The Structure of the Nucleus'). She wrote,

> Every nucleus (except hydrogen, which consists of but one proton) is characterized by two numbers: the number of protons and the number of neutrons. The sum of the two is the atomic weight of the nucleus. The number of protons determines the nature of the atom; thus a nucleus with two protons is always helium, one with three protons is lithium, and so on. A given number of protons may, however, be combined with varying numbers of neutrons, forming several isotopes of the same element. Some isotopes are stable; others decay by radioactivity. Some of the stable isotopes readily add a neutron; others are less inclined to do so. Now it is a very interesting fact that protons and neutrons favor even-numbered combinations; in other words, both protons and neutrons, like electrons, show a strong tendency to pair. In the entire list of some 1,000 isotopes of the known elements, there are no more than six stable nuclei made up of an odd number of protons and an odd number of neutrons. The other odd-

odd nuclei break down radioactively by emitting a negative or positive electron; this change in charge transforms a neutron into a proton, or a proton into a neutron and creates a more stable even-even combination of protons and neutrons. Moreover, certain even-numbered aggregations of protons or neutrons are particularly stable. One of these magic numbers is 2. The helium nucleus, with two protons and two neutrons, is one of the most stable nuclei known. The next magic number is 8, representing oxygen, whose common isotope has 8 protons and 8 neutrons and is remarkably stable. The next magic number is 20, that of calcium. Calcium, with 20 protons, has 6 stable isotopes, ranging in neutron number from 20 to 28. This is an unusually large number of stable isotopes from the lower region of the periodic table. Among these light elements the relative stability can be determined very accurately in terms of binding energy. The net mass of a nucleus is always smaller than the combined masses of the protons and neutrons of which it is composed. The binding energy is calculated from this 'mass defect' by means of Einstein's famous relation, E=mc2, with *m* representing the mass defect and *c* the velocity of light. Such calculations show conclusively that the nuclei with the magic numbers 2, 8 and 20 have much greater binding energies than their neighbors. But for the heavier elements above calcium the binding energies are not accurately determined, and we must judge their relative stability by indirect evidence. One such piece of evidence is the number of stable (*i.e.*, nonradioactive) nuclei that are found to exist with a given number of protons or neutrons. Another is the relative abundance of a given nucleus in the universe, since it seems reasonable to assume that the most abundant isotopes are the most stable. By these tests the number 50 joins the list of magic numbers. Tin, with 50 protons, has 10 stable isotopes, more than any other element, and it is much more abundant than the neighboring elements in the periodic table. The same is true, to a somewhat lesser degree, of the number 28. Another magic number is 126: an isotope with 126 neutrons holds them much more strongly than one with 127 or 128. Perhaps the most remarkable magic number of all is 82. There are 7 stable nuclei containing 82 neutrons, ranging from isotopes of xenon to samarium. The barium isotope with 82 neutrons accounts for 72 per cent of the abundance of that element, and cerium's 82-neutron isotope represents 88 per cent of all the cerium. Finally, 82 protons means lead, and lead is the stable end-product of the decay of all the heavy radioactive elements that may be found in nature. There are other indications of the special stability of these magic numbers. For instance, nuclei containing 50, 82 or 126 neutrons do not like to add an extra neutron: their absorption cross-sections for fast neutrons are smaller by several factors of 10 than those of an average nucleus of nearly the same weight.

The list of magic numbers, then, is: 2, 8, 20, 28, 50, 82 and 126.

"One of the main nuclear features which led to the shell structure is the existence of what are usually called the magic numbers. That such numbers exist was first remarked by Elsasser in 1933. What makes a magic number is that a configuration of a magic number of neutrons, or of protons, is unusually stable whatever the associated number of the other nucleons."

I will now quote from the opening paragraphs of the lecture Maria Goeppert Mayer gave in Stockholm when she accepted the Nobel Prize, explaining how she arrived at her prizewinning research.

> One of the main nuclear features which led to the shell structure is the existence of what are usually called the magic numbers. That such numbers exist was first remarked by Elsasser in 1933. What makes a magic number is that a configuration of a magic number of neutrons, or of protons, is unusually stable whatever the associated number of the other nucleons. When Teller and I worked on a paper on the origin of elements, I stumbled over the magic numbers. We found that there were a few nuclei which had a greater isotopic as well as cosmic abundance than our theory or any other reasonable continuum theory could possible explain. Then I found that those nuclei had something in common: they either had 82 neutrons, whatever the associated proton number, or 50 neutrons. Eighty-two and fifty are 'magic' numbers. That nuclei of this type are unusually abundant indicate that the excess stability must have played a part in the process of the creation of elements.
>
> My attention was then called to Elsasser's papers written in 1933. In the year 1948 much more was known about properties of nuclei than was available to Elsasser. The magic numbers not only stood up in the new data, but they appeared more clearly than before, in all kind of nuclear processes. It was no longer possible to consider them as due to purely accidental coincidences.
>
> The magic numbers, as we know them now are:
>
> 2, 8, 20, 28, 50, 82, 126
>
> and more important, they are the same for neutrons and protons.

As this quote suggests, it was her research with Edward Teller that steered Maria Goeppert Mayer in the direction of magic numbers, but her starting point was the work done earlier by Elsasser.

So, what did Elsasser's paper say? Walter Elsasser (1904-1991) was in Göttingen at the same time as Maria Goeppert, as mentioned in chapter 2. While he was interested in physics (he moved to the United States in 1935, and in the 1940s he switched first to geophysics and later to systems biology), he came very close to making two outstanding Nobel-worthy discoveries. The first occurred in 1925, while he was at Göttingen. In a brief note published in *Naturwissenschaften,* he shared calculations that favoured the idea that electrons also behaved like waves, something that was demonstrated by experiment two years later in 1927 by Clinton Davisson and Lester Germen and by George Paget Thomson (Davisson and Thomson received the Nobel Prize in Physics for it in 1937). His other discovery took place in Paris, at Frédéric Joliot-Curie's laboratory, where Elsasser went in 1933 after Hitler's rise to power. This discovery was a little more remote, but it was the one Maria Goeppert Mayer mentioned in her Nobel lecture. Elsasser reported it thus in his autobiography, *Memoirs of a Physicist in the Atomic Age* (1978), page 187:

> An atomic nucleus consists of a certain number of 'nucleons' (a collective term embracing both protons and neutrons, particles of comparable mass). The number of nucleons can vary from one, the proton itself that can be the nucleus of a hydrogen atom, to well over two hundred for the heaviest elements known. At that time Niels Bohr's school in Copenhagen had decided that the nucleus was a homogeneous agglomeration of nucleons without further internal structure: this was known as the 'liquid drop' model of the nucleus. There was considerable empirical evidence that some truth inhered in this model; but I had developed certain doubts (on evidence that would be too long to quote) and I thought that eventually the nucleus would be found to have a degree of internal structure. I decided to follow up this idea, and a large part of my efforts in France was spent on it. In the fall of 1933, K. [Kurt] Guggenheimer, a physical chemist, came to Paris from Berlin. He found a temporary position in a laboratory of the Collège de France. Since everyone in physics at that time was beginning to question how the nucleus was held together, he and I couldn't help meeting on this topic. He had a great deal of knowledge of how molecules are held together starting from atoms. There are many analogies with nuclei but no identity, since the energies involved in the nuclear case are a hundred thousand times larger than in the molecular case. Still, from ordinary chemical reaction kinetics, one thing was clear. Variations of binding energies of the nucleons would in many cases be reflected in nuclear 'abundances'. Abundance is a technical term for the relative proportions of different kinds of nuclei. This was significant information because the abundances of many sorts of nuclei had been measured. I proposed a joint piece of research, but we were unable to agree [not entirely true, as they published a joint paper, 'Sur les anomalies dans

> les proportions des éléments et sur l'origine des corps radioactifs', in volume 197 (1933) of the journal of the Académie des Sciences, *Comptes rendus*]; and in the summer of 1935 [*sic*] Guggenheimer by himself published two articles on the binding energies of the nucleons in the *Journal de Physique* ['Remarques sur la constitution des noyaux atomiques I' and 'Remarques sur la constitution des noyaux atomiques II', published in the 1934 volume of *Journal de Physique*; part I reached the editors on 9 May of that year, and part II, on 9 July]. Some months later he told me that he had found a place in England. He disappeared, and since neither of us was much of a correspondent, I soon lost track of him. I had, in 1935, found a trick to obtain, at least approximately, the binding energies of individual protons or neutrons from the directly measured disintegration energies of the very heavy, naturally radioactive nuclei. This enabled me to show in detail how beyond the end of a nuclear shell the binding energy of a nucleon suddenly decreases to as little as a third or quarter of the preceding value. I was satisfied that I had established the existence of shells, although it soon became clear that they were not simply analogous to the shell of atoms. Later, the numbers of nucleons at which shells were closed, 2, 8, 20, 28, 50, 82, 126, became known as 'magic numbers'. But since I was not a party to the jargon of Los Alamos, I know not much about the origin of this term.

At this point Elsasser commented that he wrote some other theoretical papers on nuclei in that period (one co-authored with Francis Perrin), but not in the field that would later become the definitive 'shell model'. And he wound up with an admission of the limits of what he could do at the time:

> The deeper physical understanding of the forces between nucleons that brought about the nuclear shell structure became possible only two decades later when as a result of the Manhattan Project and the tremendous growth of nuclear research in universities all over the world that followed it, the forces between nucleons began to be understood in quantitative detail. In 1963 the Nobel Prize for physics was divided between Eugene Wigner, who obtained one-half which he deserved long before, and two people who had between them worked out the theory of nuclear shell structure. They were a German theoretical physicist, Hans Jensen, and an American, Maria G. Mayer […]. In an article that Maria Mayer wrote in the journal *Science* (vol. 165) in 1964, she duly quotes my earlier contributions to this problem but also points out, perfectly correctly, that the underlying mathematical theory could not possibly have been understood before the knowledge of nuclear interactions had sufficiently advanced, which occurred only in the 1950s.
>
> I have been asked often in my life whether I did not regret having 'come so close' to the Nobel prize. My answer has always been that there would have been too high a price to pay for a purely external decoration. After all, this would

> have implied that I would have had to remain a full-time nuclear specialist and would have been involved in all the activities that led to Hiroshima, Nagasaki and to all other untold disasters that still hang over the heads of mankind.

But what was Elsasser's contribution? He attempted to formulate a theory of the nuclear structure ('Sur le principle de Pauli dans les noyaux I' and 'Sur le principle de Pauli dans les noyaux II', *Journal de Physique*, vols. 4 and 5, 1933 and 1934) to explain the apparent nuclear shells James H. Barlett had observed ('Structure of Atomic Nuclei', *Physical Review*, vol. 41, 1932) in light nuclei possessing two, eight, 18 or 32 nucleons; Guggenheimer, who studied the problem from the chemistry standpoint, added the numbers 50 and 82 to the list. Importantly, Elsasser did not restrict his work to the light elements. He included the heavy elements in his considerations, which he based on the use of quantum wells. The foremost opponent of nuclear shell models in those years was Niels Bohr, who was busy developing his compound atom model ('Neutron Capture and Nuclear Constitution', *Nature*, vol. 137, 1936), which I referred to in chapter 4. And the situation became no better when George Gamow associated his own liquid drop model with the compound atom model, whose mathematical aspects were developed by Gregory Breit and Eugene Wigner.

Basically, Bohr's argument was that measurements of nuclear interaction suggested that the force between two nucleons was of the same order as the force between the nucleus and a single nucleon; and consequently it was impossible to treat nucleons separately and assign quantum values to them, something that was considered in any nuclear shell model, where nucleons were treated like the electrons in Bohr's atom, which moved in stable orbits (energy levels) in the potential produced by the nucleus and were assigned quantum parameters. For the nuclei of atoms to contain something like that (that is, shells of nucleons), nucleons had to be regarded as independent particles with quantum parameters (like spin and magnetic momentum) similar to the quantum parameters of electrons; and furthermore –pursuing the analogy– it was necessary to assume that the interaction between individual nucleons was less than the bond energy produced by a central nuclear potential.

The vast amount of empirical information on the abundance of the chemical elements that Maria Goeppert Mayer had amassed in the course of her research with Teller into the origin of the elements far outweighed the information available to Elsasser or Guggenheimer. This led her into magic numbers, and she tried to interest Teller in them as well, but he had already shifted his attention to the development of nuclear weapons. Her husband helped her, supporting her interest and offering his viewpoint as a chemist, which in some ways made him a better sounding board for discussing issues involving regularities for which no theoretical explanations were known (as had happened in the classic case of valence theory, which was introduced before quantum physics provided the right theoretical backdrop).

Maria Goeppert Mayer presented the findings of her systematic analysis of isotope abundance data, which supported the idea of the existence of magic numbers in atomic nuclei, in a paper that appeared in the 1 August 1948 issue of *Physical Review* (vol. 74, pp. 235-239; received on 16 April 1948), 'On Closed Shells in Nuclei'. She gave a fine explanation of the paper's contents in the introduction, which read:

> It has been suggested in the past that special numbers of protons or neutrons in the nucleus form a particularly stable configuration [W. Elsasser, *J. de phys, et rad. 5,* 625 (1934)]. The complete evidence for this has never been summarized, nor it is generally recognized how convincing this evidence is. That twenty neutrons or protons (Ca^{40}) form a closed shell is predicted by the Hartree model. A number of calculations support this fact [E. Wigner, *Phys. Rev. 51,* 847 (1937); W.H. Barkas, *Phys. Rev. 55,* 692 (1939)]. These considerations will not be repeated here. In this paper, the experimental facts indicating a particular stability of shells of 50 and 82 protons and of 50, 82 and 126 neutrons will be listed.

Now then, knowing about the existence of magic numbers and their relationship with nucleus stability was one thing; coming up with a theoretical explanation to back it up was quite another. And Maria Goeppert Mayer's great achievement was that she found the explanation. She had on her side her familiarity with nuclear 'numerology' plus her solid knowledge of quantum mechanics, which placed her in a good position to tackle the problem. Yet she still needed a little 'push', a suggestion. And this she got from Enrico Fermi, whose office was near hers at the University of Chicago. Maria Goeppert Mayer herself explained the history of Fermi's suggestion in her Nobel 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 [this must have been in late 1948 or early 1949] as Fermi was leaving my office he asked: 'Is there any indication of spin-orbit coupling?' Only if one had lived with the data as long as I could one immediately answer: 'Yes, of course and that will explain everything'. Fermi was skeptical, and left me with my numerology.

The consequence of spin-orbit coupling is that there is a split in the nucleons' energy levels. This type of interaction was known to take place in an atom's electrons due to the interaction of magnetic momentum associated with the spin of electrons as they orbit the nucleus (produced by the central field, caused by the positively charged protons), but the split is so tiny compared with the total energy

Her husband helped her, supporting her interest and offering his viewpoint as a chemist, which in some ways made him a better sounding board for discussing issues involving regularities for which no theoretical explanations were known.

of the bond due to the central potential that it is generally dismissed, except in very heavy atoms. The Fermi-Mayer idea that there might be a strong spin-orbit interaction in the case of nucleons meant, to start with, that protons and neutrons orbited within the nucleus.

In her article 'Maria Goeppert Mayer: Atoms, Molecules and Nuclear Shells' (*Physics Today*, 1986), Karen Johnson shared a simple explanation Maria Goeppert Mayer used to explain spin-orbit coupling to her daughter.

> Think of a room full of couples waltzing. They are moving around the room in circles, each circle enclosed within another. Each circle corresponds to an energy level. In addition to orbiting, though, each couple is also spinning like a top. Now suppose that while orbiting counterclockwise some couples are spinning clockwise, and the rest counterclockwise. Those spinning counterclockwise will find the going easier than those spinning clockwise [...]. Everybody who has ever danced the fast waltz knows that it's easier to dance one way around than the other. Therefore, for a given circle of dancers, the energy necessary to orbit will be different for couples spinning in opposite senses. In the same way, nucleons of a given orbital angular momentum have two opposite energies, depending on whether their spin is parallel or antiparallel to the orbital motion. This splitting of the energy level is called spin-orbit coupling.

A more technical explanation of the model appears in chapter IV ('Individual Orbits in the Nucleus') of the book *Elementary Theory of Nuclear Shell Structure* (1955), which Maria Goeppert Mayer co-wrote with Hans Jensen (with whom she shared the Nobel Prize). I shall return to this book later, but since this is a crucial point, I shall quote it here:

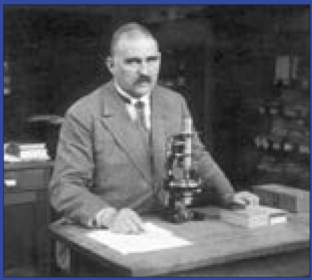
> Our knowledge of the forces between the nucleons is far from complete. The only fact of which we are certain is that they are of short range. They are very different in character from the long-range Coulomb forces which govern the constitution of the electronic cloud of the atoms. Therefore, one may have serious doubts whether it is useful to approximate the nucleus by the same type of model as leads to such a comprehensive understanding of the atomic consti-

> tution. However, inasmuch as we know nearly as little about the nuclear forces as we did 20 years ago, there is not much sense in trying to judge from a priori arguments how satisfactory a description of nuclear structure can be obtained by any approximation. It is worthwhile, therefore, simply to construct a nuclear model which makes essentially the same assumptions as those inherent in the Bohr-Pauli approximation of the periodic table, and then to investigate to what extent the known data about the nuclei can be correlated in this way. Furthermore, the phenomena related to the magic numbers are most easily stated in terms of a shell closure. The well-established fact that those magic numbers are the same for protons as for neutrons can be considered an empirical indication that the nucleons inside the nucleus retain a sufficient degree of individuality. Consequently we expect that an approximation which considers individual orbits of nucleons in the quantum-mechanical sense might describe at least some simple properties of the nucleus. In fact, as we shall see, this simple model accounts for a large number of nuclear data surprisingly well.
>
> Let us assume, then, that each nucleon moves in an average field of force V(r), of spherical symmetry, and independent of the exact instantaneous positions of all the other nucleons. The possible 'nucleon orbits' are characterized by a set of quantum numbers (*n, l, j, m*j), just as in the electronic case, and the corresponding energy levels are successively filled with protons and neutrons. According to the Pauli principle, each proton level of quantum number *j* [*j* represents the total angular momentum: the sum of the orbital angular momentum, *l*, and the angular momentum of the spin, *s*; the angular momentum is associated to the motion of rotation of an object] can contain no more than $2j + 1$ protons; the same holds for the neutrons. Proton and neutron levels characterized by the same set of quantum numbers (*n, l, j, m*j) do not exactly coincide, because the Coulomb force, acting only on the protons, shifts the protons levels to higher energy. Even the sequence of the levels might differ slightly for protons and neutrons, because the orbits of lower *l* penetrate deeper into the charged nuclear core, and the proton interaction with the repulsive Coulomb field is stronger in these orbits. Consequently, at least for nuclei with high charge, lower *l* values are less favored for protons than for neutrons.

Maria Goeppert Mayer presented her arguments for the importance of spin-orbit coupling in a letter to the editor printed in *Physical Review,* volume 75 (15 June 1949; pp. 1969-1970; received on 4 February 1949). She entitled it 'On Closed Shells in Nuclei, II'. In it she thanked Fermi for his help; in fact, she seems to have offered to have him sign the letter with her, but Fermi refused on the grounds that, since he was the more famous of the two, everybody would think he had done the lion's share of the work.

Walter Elsasser

Hans Suess



Viktor M. Goldschmidt

Edward Teller and
Eugene Wigner

Maria Goeppert Mayer at the 1963
Nobel Prize Award Ceremony.
On her left, Hans Jensen

Alfred Nobel

With the King of
Sweden at the Nobel
Prize Award Ceremony

Her knowledge of known experimental data and her mathematical mastery of quantum mechanics and the theory of rotation groups were vital for reaching the conclusion explaining magic numbers. In 'The Shell Model', the lecture she gave in Stockholm on receiving the Nobel Prize, she referred to this interrelationship between theory and experiment in nuclear physics in these words:

> There are essentially two ways in which physicists at present seek to obtain a consistent picture of atomic nucleus. The first, the basic approach, is to study the elementary particles, their properties and mutual interaction. Thus one hopes to obtain a knowledge of the nuclear forces.
>
> If the forces are known, one should in principle be able to calculate deductively the properties of individual complex nuclei. Only after this has been accomplished can one say that one completely understands nuclear structures [...].
>
> The other approach is that of the experimentalist and consists in obtaining by direct experimentation as many data as possible for individual nuclei. One hopes in this way to find regularities and correlations which give a clue to the structure of the nucleus [...].
>
> The shell model, although proposed by theoreticians, really corresponds to the experimentalist's approach.

Simultaneous Discovery

In the first lines of 'On Closed Shells in Nuclei, II', Maria Goeppert wrote, 'The spins and magnetic moments of the even-odd nuclei have been used by Feenberg and Nordheim to determine the angular moment of the eigenfunction of the odd particle'. But the interesting thing about this is the two scientists she named, Eugene Feenberg (Washington University, St. Louis) and Lothar W. Nordheim (Duke University; German by birth, PhD from Göttingen in 1923). They reacted separately to Goeppert Mayer's paper of 1 August 1948 with articles published on 15 October 1948 (Feenberg), 1949 (Feenberg and Hammarck), and 1949 (Nordheim) in *Physical Review,* putting forward ideas that represented the main contributions to the shell model. Maria received preprints of Feenberg's and Nordheim's papers, and they made her doubt whether to publish 'On Closed Shells in Nuclei, II'. Maybe Feenberg and Nordheim had drawn inspiration from her first article, 'On Closed Shells in Nuclei', but maybe they had not. 'I'll wait', she told her husband. 'I'll write to the editors of *Physical Review* and ask when those papers will come out, and send them something that I will ask them to print at the same time. I must not take advantage because I saw their preprints'. Joseph was not of the same mind. He argued that courtesy toward colleagues was one thing, but over-punctiliousness was another, and she owed nothing to their work.

Maria Goeppert Mayer presented her arguments for the importance of spin-orbit coupling in a letter to the editor printed in *Physical Review*, volume 75. She entitled it 'On Closed Shells in Nuclei, II'. In it she thanked Fermi for his help; in fact, she seems to have offered to have him sign the letter with her, but Fermi refused on the grounds that, since he was the more famous of the two, everybody would think he had done the lion's share of the work.

Physical Review informed her that Feenberg's and Nordheim's papers were due to appear in the June 1949 issue and, in view of the diverse papers that had been written on the shell model, they suggested she write a brief note so the different approaches could be compared.

In the end Maria did so, and she wrote 'On Closed Shells in Nuclei, II'. And as she had asked, her article was held back until the 15 July 1949 issue and was printed at the same time as Feenberg's and Nordheim's. But Feenberg's and Nordheim's theoretical interpretations concerning the presence of magic numbers proved incorrect. While this was going on, on the other side of the Atlantic, a similar paper was in the works, this time completely independent from Maria Goeppert Mayer's.

The 'Letters to the Editor' section of the 1 June 1949 issue of *Physical Review* (vol. 75) printed an article/letter less than a page long (p. 1766) that reached the editors on 18 April (remember, Goeppert Mayer's came to the editors on 4 February, but it was being held). It was entitled 'On the "Magic Numbers" in Nuclear Structure', and it was signed by Otto Haxel (Max Planck Institute, Göttingen), J. Hans D. Jensen (Institute of Theoretical Physics, Hamburg) and Hans E. Suess (Institute of Theoretical Physics, Hamburg). 'A simple explanation of the "magic numbers" 14, 28, 50, 82, 126', the abstract reads, 'follows at once from the oscillator model of the nucleus [H.A. Bethe and R. Bacher, *Review of Modern Physics* 8 82, 1937], if one assumes that the spin-orbit coupling in the Yukawa field theory of nuclear forces leads to a strong splitting of a term with angular momentum l into two distinct $j = l \pm ½$'.

At a symposium held at the University of Minnesota in May 1977 (the contents were later made into a book, *Nuclear Physics in Retrospect. Proceedings of a Symposium on the 1930s*), one of the scientists signing that article/letter, Hans Edward Suess (1909-1993), explained the paper's origin. I shall quote a few passages from his explanation.

> There are a few points I could make. One is that what we did in Hamburg and what Maria [Goeppert Mayer] did in Chicago were completely separate and unrelated. We did not know a thing about each other. Yet, from a completely different approach, and under completely different circumstances, we were led to precisely the same result. But the big difference can be characterized best by what happened: Jensen once remarked that if he had known more theoretical nuclear physics, he would never have believed a word of what I had told him. It was really a difficult job for a mere chemist, who uses different methods than a theoretical physicist, to convince him. I used what is generally considered the 'circumstantial evidence'. Chemists are used to considering simultaneously a number of facts and then deriving a conclusion from them, whereas theoretical physicists usually wish to consider the result of one single experiment, or one phenomenon they wish to interpret. I didn't really know a thing about what I was actually doing. I just had these magic numbers –not from Maria but from the great mineralogist Viktor Moritz Goldschmidt, who is not mentioned in our paper but who published them in the Proceedings of the Norwegian Academy in 1938. I could see from this information that there was 'circumstantial evidence' that without question had to have a physical meaning. So I started playing with these numbers and found that they were indeed magic –I mean all kinds of things could be done with them, such as deriving mathematical progressions of these numbers. Next I simply looked up in a textbook the solutions of the Schrödinger equation for the three-dimensional harmonic oscillator [he did this because he assumed, as others had assumed before him, that nucleon movement inside the nucleus could be explained as due to the action of a central potential, for which the case of an oscillator was considered]. (I did not go through the mathematics, because it could be looked up in any textbook.) Then I wrote down the levels one gets, sorted them in sequence according to the angular momentum, and used the Pauli principle to see how many particles would fit into each level. This gave the wrong numbers, but all one had to do was to reverse the usual sequence of the spin values, and to start with the highest spin. If one includes the highest spin in the previous shell, one gets precisely the magic numbers. Well, I then thought that there might be something to this –maybe it's not just a magic trick. I looked up the paper by Smith to see whether the parities of the empirical spin values for the odd mass-number species would fit into such a scheme. I drew this up and showed it to Hans Jensen, because to me it was rather convincing that there was something behind it. Jensen said: 'Oh, that I

> have to draw up myself', and he went through the literature and plotted each spin value in the diagram.

In other words, Suess had found that there were some relationships between magic numbers and the abundance of isotopes according to Goldschmidt. However, he was at sea to explain why they existed, and it was then that he asked Jensen for help. Right off the bat –again, according to Suess– he said, 'That's all nonsense –it doesn't fit'. But soon the light dawned.

> The following day he came to me and said: 'Well, if there is something to it –if the scheme you drew up means something– it would mean that there is a strong spin-orbit coupling'. I asked why there should be a strong spin-orbit coupling. He replied: *'Das hat der liebe Gott so gemacht'* ['that is how the good God made it']. He then wrote a note to *Die Naturwissenschaften*, but I said: 'Wait a minute' –I had an agreement with Haxel that we would publish whatever came into our minds together. So he agreed that that we should put Haxel's name on it also. Haxel too had been worrying so much about magic numbers and had realized that magic numbers had to have some meaning. Thus, it was actually just an accident that we in Germany had this idea of spin-orbit coupling. I certainly did not realize the deeper meaning it had for basic nuclear physics.

The note in *Die Naturwissenschaften* (vol. 35, p. 376, 1948) that Suess speaks of was signed by O. Haxel, J.H.D. Jensen and H.E. Suess, and it bore the title of 'Zur Interpretation der ausgezeichten Nukleonenzahlen im Bau der Atomkerne' ('On the Interpretation of the Magic Numbers of Nucleons in the Structure of the Atomic Nucleus'). It was the prelude to the aforementioned *Physical Review* article, 'On the "Magic Numbers" in Nuclear Structure'. Bearing in mind that Suess acknowledged that the paper's core idea, the existence of strong spin-orbit coupling, was Jensen's, it is fair for him to have received the Nobel Prize together with Maria Goeppert Mayer.

Jensen gave his own version of how they reached the discovery in a lecture he delivered in Stockholm when he collected his Nobel Prize.

> The war years and also the first few years thereafter brought the physicists in Germany into a stifling isolation, but at the same time they gave us some leisure to pursue questions off the beaten trails. At that time I had many discussions with Haxel in Berlin, later Göttingen, and with Suess in Hamburg on the empirical facts [about magic numbers] which singled out the above-mentioned numbers. To Suess they became more and more significant, primarily in his cosmo-chemical studies: he found that in the interval between the numbers already mentioned, the numbers Z and N=50 and N=82 were also clearly prominent. Haxel, at first quite independently, encountered the same numbers in the study of other nuclear data.

Although my two colleagues tried hard to convince me that these numbers might be the key to the understanding of nuclear structure, at first I did not know what to make of it. I thought the name 'magic number', whose origin was unknown to me, to be very appropriate. Then, a few years after the war, I had the privilege of returning to Copenhagen for the first time. There, in a recent issue of the *Physical Review*, I found a paper by Maria Goeppert Mayer, 'On closed shells in nuclei' [the 1948 article], where she too had collected the empirical evidence pointing out the significance of the magic numbers. That gave me the courage to talk about her work, along with our results, in a theoretical seminar. I shall never forget that afternoon. Niels Bohr listened very attentively and threw in questions which became more and more lively. Once he remarked: 'But that is not in Mrs. Mayer's paper!'; evidently Bohr had already carefully read and pondered about her work. The seminar turned into a long and lively discussion. I was very much impressed by the intensity with which Niels Bohr received, weighted, and compared these empirical facts, facts that did not at all fit into his own picture of nuclear structure. From that hour on I began to consider seriously the possibility of a 'demagification' of the 'magic numbers'.

At first I tried to remain as much as possible within the old framework. To begin with, I considered only the spin of the whole nucleus since there appeared to exist a simple correlation between the magic nucleon numbers and the sequence of nuclear spins and their multiplicities. I first thought of the single-particle model with strong spin-orbit coupling (fortunately, I was not too well versed in 'Bethe's bible' and I did not remember the old arguments against a strong spin-orbit coupling too well) during an exciting discussion with Haxel and Suess, in which we tried to include all available empirical facts in this scheme. As we did this it turned out that, because of the spin-orbit coupling, the proton- and the neutron-number 28 should also be something like a magic number. I remember our being elated when we found some hints in the still meagre data that was available at that time. Nevertheless, I did not feel very happy about the whole picture, and was not really surprised when a serious journal refused to publish our first letter, stating 'it is not really physics but rather playing with numbers'. Only when I thought of the lively interest in the magic numbers which Niels Bohr had shown did I dare send the same letter to Weisskopf who forwarded it to the *Physical Review*. Yet it was not until later, after I had presented our ideas in a Copenhagen seminar and been able to discuss them with Niels Bohr, that I finally gained some confidence. One of Bohr's first comments seemed remarkable to me: 'Now I understand why nuclei do not show rotational bands in their spectra'. With the accuracy of measurement available at the time, one had looked for such spectra in lighter nuclei, which according to the liquid drop model or a similar model should have relative small moments of inertia and therefore widely separated rotational levels. As we know today, these light nuclei, like many others, in fact show no rotational levels.

Two Seminal Articles by Maria Goeppert Mayer

'On Closed Shells in Nuclei, II' was actually nothing more than a not-necessarily-convincing sketch, a sort of intuitive suggestion of the relationship between magic numbers and spin-orbit coupling. Joseph Mayer and Edward Teller both urged Maria to enlarge upon her letter to the editor with a meatier paper developing a true theory of the shell model. It was hard going, but she did it. Not with a single article, but with two, which appeared back to back in the 1 April 1950 issue of *Physical Review* (vol. 78, pp. 16-21 and 22-23). The papers were received by the editors on 7 December 1949 and were entitled 'Nuclear Configurations in the Spin-Orbit Coupling Model, I. Empirical Evidence' and 'Nuclear Configurations in the Spin-Orbit Coupling Model, II. Theoretical Considerations'.

In the opening lines of part I, Goeppert Mayer referred succinctly to earlier papers in footnotes (which I give between brackets).

> Nuclei containing 2, 8, 20, 28, 92 or 126 neutrons or protons are particularly stable [W. Elsasser, *J. de phys. et rad.* 5, 625 (1934); M.G. Mayer, *Phys. Rev. 74,* 235 (1948)]. These closed shells have been explained in different ways [E. Feenberg and K.C. Hammack, *Phys. Rev. 75,* 1877 (1949). L.W. Nordheim, *Phys. Rev. 75,* 1894 (1949)]. It has also been pointed out that the 'magic numbers' can be explained on the basis of a single particle picture with the assumption of strong spin-orbit coupling [Haxel, Jensen, and Suess, *Phys. Rev. 75,* 1766 (1949); M.G. Mayer, *Phys. Rev. 75,* 1969 (1949)]. The detailed evidence supporting this point of view will be discussed in this paper.

And in the second article she offered the theoretical explanation.

> No attempt is made to explain the strong spin-orbit coupling. The object of this article is to investigate if there are any theoretical reasons for these empirical rules. For this purpose, it was assumed that an attractive potential acts between identical nucleons.

Without the publication of these two articles, Maria Goepper Mayer's contribution to the nuclear shell model would most likely have been much less appreciated and could have gone unnoticed. Jensen, Suess and Haxel hesitated less to expand upon their first article in *Naturwissenschaften*. The same year that their letter was printed in *Physical Review,* they published two more articles in the German journal: Otto Haxel, J.H.D. Jensen and H.E. Suess, 'Zur Interpretation der augsezeichneten Nucleonenzahlen im Bau der Atomkerne: II. Mitteilung', *Naturwissenschaften* 36, 153-155 (1949); and J.H. Jensen, H.E. Suess and O. Haxel, 'Modellmässige Deutung der ausgezeichneten Nucleonenzahlen im Kernbau' ('Interpretation of the Nuclear Structure Based on the Model of the Magic Numbers of Nucleons'), *Naturwissenschaften* 36, 155-156 (1949).

Maria Goeppert Mayer and Hans Jensen, Partners

The history of science is riddled with squabbles over who made a given discovery first. The famous –and all-around shameful– clash between Isaac Newton and Gottfried Leibniz for priority in the invention of infinitesimal calculus springs to mind. In the case of the shell model, something similar could well have happened between Maria Goeppert Mayer and Hans Jensen, though it probably would not have been as violent as the fight between the two giants. But nothing like that ever did occur, even though both probably regretted at first not having been the only one to make the breakthrough. What did happen is that they soon started corresponding. Maria used to write about 'our theory', and Hans used to answer with references to 'your theory'.

Johannes Hans Jensen (1907-1973) had studied in Freiburg and Hamburg, where he took his doctorate in 1934. He received his habilitation in 1936. He was linked to several associations in Hitler's National Socialist Party: 1933, member of the Nationalsozialistischer Deutscher Dozentenbund (German Socialist League of University Lecturers, or DSDDB); 1934, member of the Nationalsozialistischer Lehrerbund (National Socialist League of Teachers; NSLB); 1937, member of the Nationalsozialistische Deutsche Arbeiterpartei (National Socialist German Workers' Party, or NSDAP). Despite membership in so many organizations and the fact that he was in on the German nuclear project during World War II (for instance, he worked with Paul Harteck to develop double centrifuges to separate uranium isotopes; Otto Haxel and Hans Suess worked on the atomic project as well, although they were not members of the Nazi party), Jensen was far from being a Nazi. During a 1943 visit to Copenhagen, he indirectly passed information about Heisenberg's nuclear reactor work to Niels Bohr, implying that Germany was far from being able to make an atom bomb, which was very valuable intelligence for the Allies. In 1943 he was named associate professor of theoretical physics at Hannover Technical University, where he was promoted to full professor in 1946. He left Hannover in 1949 when he was given a full professorship in Heidelberg.

Goeppert Mayer and Jensen met in person in the summer of 1950, when the Mayers went to Germany as State Department consultants tasked with easing the resumption of relations between U.S. and German physicists and chemists. They arrived in August and stayed in Germany for three months. Jensen seized the op-

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portunity for his school, the University of Heidelberg, to invite Maria to visit the following summer (in 1950 she was elected a member of the Heidelberg Academy of Sciences). The Mayers returned the favour, and in 1951 Jensen toured the United States for a few months as a guest lecturer, stopping off in Chicago between lectures, where he stayed at Maria and Joseph's house. Altogether he spent about two months with them. It should then come as no surprise that they decided to write a book together on the implications of the shell model. Jensen seems to have come up with the idea first. Later he confessed to Maria Goeppert that he had an ulterior motive: he wanted to increase the chances of a Nobel Prize for both of them, not just by 'selling' the shell model, but also by getting their two names into the public eye. Otherwise, he believed, they would have a problem: Suess and Haxel had helped Jensen gather the experimental data, and that would make four candidates for the prize, but the Nobel Foundation's statutes did not allow four people to share a Nobel Prize. Three was the maximum.

It was also important to spread the word about the shell model, prove its usefulness, because for some time not all physicists believed in it. Although Fermi was convinced, as was Weisskopf (who had forwarded Jensen, Suess and Haxel's letter to *Physical Review*), others looked at it askance. Joan Dash's biography of Maria Goeppert Mayer tells the story of what happened at a seminar that Maria Mayer gave at Princeton, which was directed by Oppenheimer:

> Her voice was whispery, as always, the German accent competing with the British one. She was nervous and shy and she chain-smoked as usual; turning to the blackboard, she took up a piece of chalk and became confused as to which was chalk and which was a cigarette. A young theorist who heard her lecture found it hard to take seriously. 'I might accept it more readily if she didn't always call them 'magic numbers', he said to himself. 'It sounds like hocus-pocus, numerology. Why not stable numbers? That you could take seriously'.

After four years' work together, Goeppert Mayer and Jensen finally completed their book, *Elementary Theory of Nuclear Shell Structure.* It was published in

1955 in New York by John Willey & Sons and in London by Chapman & Hall. The authors were listed in order as Maria Goeppert Mayer (Argonne National Laboratory and University of Chicago) and J. Hans Jensen (Universität Heidelberg), and the book was dedicated 'To our most patient and most constructive critic, Joseph Edward Mayer'. The book contained thirteen chapters and four appendices, a total of 269 pages. For our purposes, it suffices to quote part of the eminently instructive preface:

> In the last two decades nuclear physics has grown so rapidly that the fact that this monograph is concerned only with one partial aspect of this diversified field may not need justification. The specification of this limited field, however, by sensible and well-defined demarkation lines offered some difficulties.
>
> There are two ways in which physics tries to obtain a consistent picture of the structure of the atomic nucleus. One of these is the study of elementary particles, their properties and mutual interactions. Thus one hopes to obtain a fundamental knowledge of the nuclear forces, from which one can then deductively understand the complicated nuclear structures. The other way consists in gaining, by direct experimentation, as many different data as possible for individual nuclei, and examining the relations among these data. One expects to obtain a network of correlations and connections which indicate some elementary laws of nuclear structure. These two ways have not yet met to establish a complete understanding of the nucleus, although many connections have been found.
>
> The state of nuclear physics today is somewhat analogous to that of the concepts of the structure of matter *before* quantum mechanics. At that time, the physics of electrons, and quanta of the electromagnetic field, on the one hand, and the facts and rules of chemistry, on the other, had not yet been united in a common picture. A chemist at that time could not wait until quantum mechanics was completed. He developed for his orientation a system of models which still retain their own importance.
>
> In the same way, the nuclear physicist who follows the second of the ways mentioned above is compelled to employ useful models to keep his orientation in the ever-growing mass of experimental information. To this purpose, above all, this book is dedicated. Besides, we hope, some features appear which are of fundamental importance for the theory of elementary nuclear forces.
>
> The recollection of the history of the development of chemistry may perhaps remove the magic aspect of the success that the shell model has had in correlating so many experimental facts, and in making some rather precise and definite predictions, although only very general and qualitative assumptions about the nuclear forces are used. We therefore trust that it may be worth while to bring together in one monograph much of the scattered work which has been

done in recent years, and in which the shell model has helped to systematize experimental knowledge and to gain insight into the structure of the nucleus.

Goeppert Mayer and Jensen's book certainly helped the shell model become known, accepted and used by nuclear physicists. A review published in *Nuclear Physics* in 1956 (vol. 6, pp. 670-671), signed only with the initials 'L.R.', emphasizes this point. Because it helps understand both the book's contents and the reaction it aroused, I will reproduce it in its entirety.

> The two physicists who independently recognised the importance of spin-orbit coupling in characterising the individual nucleon states, and thus for the first time devised a workable shell model of the nuclear structure, have joined forces to survey the rich field opened up by their discovery. The well-co-ordinated picture of nuclear properties afforded by the model is made up of data supplied by highly specialised experimental investigations of the most varied nature. It has therefore been for some time a compelling task to present the fundamental features of the model and a comprehensive summary of the results hitherto achieved in a fashion palatable to the experimental physicists (who, as is well-known, affect to find mathematical arguments difficult to follow). Several excellent survey articles have already appeared in response to this need, but the present book, with its larger scope and fuller treatment, will be welcomed, not only as a serviceable guide to those already engaged in research in this vast field, but more generally as an authoritative introduction into it for those who wish to acquaint themselves with one of the chief problems of nuclear physics. Even the more theoretically minded will find the book extremely helpful; after all, the theoretical techniques which are not treated in it are of a straightforward kind and can easily be looked up in the original papers if need be (I am not speaking, of course, of the symplectic group, the mysteries of which require an exclusive initiation anyhow!).
>
> The general set-up of the book follows a straightforward plan and covers the whole ground extensively. The exposition is clear and concise, and well illustrated by examples and diagrams. The latter are mostly familiar from the published papers, which does not mean that they always provide the best representation of the point at issue. There is a full tabular presentation of the relevant data, which is most useful; the inclusion of a few more graphs, however, would have enhanced still more the practical usefulness of the book. Excellent appendices offer in a nutshell the main theoretical concepts and formulae, most conveniently collected for direct application.
>
> If we now scrutinise the detailed treatment of the various aspects of the subject, we do not find everywhere, unfortunately, the same degree of excellence. What the authors sometimes lack is certainly not competence or critical judgment, but simply sufficient information about the work of others. They

Soon after arriving in San Diego, Maria had a stroke. It did not incapacitate her, but it did reduce her faculties, and she had continuous health trouble the rest of her life. Even so, she kept teaching and actively participating in the presentation and development of the shell model.

> themselves confess, in the preface, to an 'arbitrary selection' in this respect, and somewhat lightheartedly afford as an excuse their aim of writing an introduction and not an 'exhaustive compilation'. This would be all right if they did not too often ignore contributions of really significant value to the very arguments they present. This is not the result of carefully considered restraint, but of sheer negligence. It is small consolation to observe that such neglect of foreign work is an attitude not confined to our authors, but widespread among American physicists. The chaotic conditions of publication outside the United States have no doubt something to do with this deplorable state of affairs. Is it too much to hope that *Nuclear Physics* might help to restore some balance in the appreciation and utilisation of valuable contributions to the common endeavour by making them more easily accessible?

Full Professor at the University of California, San Diego

On 28 November 1954 Enrico Fermi died, a victim of stomach cancer. At his loss some members of the Institute for Nuclear Studies left Chicago. Teller had already gone in 1952, and Urey departed in 1958 for the University of California's new San Diego campus. The year after that Maria and Joseph Mayer received the offer of a full professorship each at San Diego, no doubt at Urey's recommendation. The band of scientist friends was back together again. And Maria finally held a major official position, a full professorship. Significantly, 24 hours after learning about the University of California's offer, the University of Chicago countered by offering Maria the very sort of position it had refused to give her before. Apparently its rules against nepotism were not so strict after all.

Shortly before the Mayers moved to San Diego, their daughter Marianne, who had no interest in being a scientist, married an astrophysicist, Donat Wentzel, the son of an old friend of the Mayers, Gregor Wentzel (Donat was born in 1934 in Zurich, where his father taught at the ETH). Maria spared no effort in making the wedding a success. Her son, Peter, began by studying physics but eventually

With Werner Heisenberg and Eugene Wigner, Lindau, 1968

Joseph Mayer and Maria Goeppert Mayer visiting Max Born in Bad Pyrmont, Germany, 1964

Maria Goeppert Mayer in the 1960s

Plaque in honour of Maria Goeppert Mayer in Katowice, where she was born switched to economics. He became a professor of economics, the eighth generation of university professors in Maria's family. Marianne never wanted or needed a job. She devoted herself entirely to her family.

Unfortunately, soon after arriving in San Diego, Maria had a stroke. It did not incapacitate her, but it did reduce her faculties, and she had continuous health trouble the rest of her life. Even so, she kept teaching and actively participating in the presentation and development of the shell model. The reduction of her scientific activity can be estimated from the simple fact that after 1959, the year she joined the University of California, the only papers she published were 'Harmonic Oscillator Wave Function in Nuclear Spectroscopy' in *Physical Review* 117, 174-84 (1960) (with R.D. Lawson) and her last publication, a review of the shell model written with Hans Jensen, 'The Shell Model. I. Shell Closure and jj Coupling', in *Alpha-, Beta- and Gamma-Ray Spectroscopy,* Kai Siegbahn, ed. (North Holland, Amsterdam, 1965), p. 557. There was also her Nobel lecture, 'The Shell Model', which was published in three places in 1964, twice in English and once in German: *Science* 145, 999-1006; *Les Prix Nobel en 1963* (The Nobel Foundation, Stockholm), and 'Das Schalenmodell des Atomkerns', *Angewandte Chemie* 76 (7), 729-37.

Earlier, between 1951 and 1958, apart from her book with Jensen, she had the following publications to her credit: a highly acclaimed article in *Scientific American* (March 1951), 'The Structure of the Nucleus'; 'Nuclear Shell Structure and Beta Decay', *Reviews of Modern Physics* 23, 315-21 (1951), with S.A. Moszkowski and L.W. Nordheim; 'Report on a Monte Carlo Calculation Performed on the ENIAC', U.S. Department of Commerce, *Applied Mathematics*, Ser. 12, 19-20 (1951); 'Electromagnetic Effects Due to Spin-Orbit Coupling', *Physical Review* 85, 1059 (1952), with J.H. Jensen; 'Radioactivity and Nuclear Theory', *Annual Reviews of Physical Chemistry,* 3, 19-38 (1953); a review of the shell model published in *Proceedings of the International Conference of Theoretical Physics, Tokyo* (Science Council of Japan, 1954), pp. 345-355; 'Classification of Beta Transitions', in *Beta and Gamma Ray Spectroscopy*, chapter 16.1 (North Holland, Amsterdam, 1955); 'Twin Neutrino Theory. A Modified 2 Component Theory', *Physical Review* 107, 1445-47 (1957), with V.L. Telegdi; and 'Statistical Theory of Asymmetric Fission, Part VII', in *Proceedings of the International Symposium on Transport Processes in Statistical Mechanics, Brussels* (Interscience Publishers, New York, 1958), pp. 187-191.

The Nobel Prize

Maria Goeppert Mayer was proposed for eight years and nominated 26 times for the Nobel Prize in Physics and once for the Nobel Prize in Chemistry. In 1955 she

Half of the Nobel Prize was for Eugene Wigner 'for his contributions to the theory of the atomic nucleus and the elementary particles, particularly through the discovery and application of fundamental symmetry principles', and the other half was shared by Maria Goeppert Mayer and Hans Jensen 'for their discoveries concerning nuclear shell structure'.

was nominated by Max Born and J. van Staveren. In 1956, by E. Justi, M. Kholer, Harold Urey, Karl Johann Freudenberg, G. Rathenau and James Franck. In 1957, by James Franck and Karl Johann Freudenberg. In 1958, by James Franck and Eugene Wigner. In 1959, by Karl Johann Freudenberg and Max Born. In 1960, by B. Flowers, Emilio Segrè, Harold Urey and Karl Johann Freudenberg. In 1962, by James Franck, M. Kohler, Willard Libby, Heinz Maier-Leibnitz, Torsten Gustafson and Lamek Hulthén. And in 1963, the year she became a laureate, by Amos de Shalit and Torsten Gustafson. In 1958 she was proposed for the Nobel Prize in Chemistry by Willis Eugene Lamb.

Hans Jensen received 29 nominations and was proposed for nine years. In 1955, by Max Born. In 1956, by E. Justi, M. Kholer, Harold Urey, Karl Johann Freudenberg, G. Rathenau and James Franck. In 1957, by James Franck and Karl Johann Freudenberg. In 1958, by James Franck and Eugene Wigner. In 1959, by Karl Johann Freudenberg and Max Born. In 1960, by B. Flowers, Emilio Segrè, Harold Urey and Karl Johann Freudenberg. In 1961, by Karl Johann Freudenberg. In 1962, by Karl Johann Freudenberg, James Franck, M. Kohler, Willard Libby, Heinz Maier-Leibnitz, Torsten Gustafson and Lamek Hulthén. And in 1963, the year he became a laureate, by Karl Johann Freudenberg, Amos de Shalit and Torsten Gustafson. In 1958 he was proposed for the Nobel Prize in Chemistry by Willis Eugene Lamb.

Curiously enough, Eugene Wigner, who won half of the prize, had been proposed fewer times, 24.

It was in 1963 when the three finally became Nobel laureates. As I have already said, half of the prize was for Eugene Wigner 'for his contributions to the theory of atomic nucleus and the elementary particles, particularly through the discovery and applications of fundamental symmetry', and the other half was shared by Maria Goeppert Mayer and Hans Jensen 'for their discoveries concerning nuclear shell structure'.

Wigner's Nobel lecture was entitled 'Events, Laws of Nature, and Invariance Principles'; Goeppert Mayer's, 'The Shell Model'; and Jensen's, 'Glimpses at the History of Nuclear Structure Theory'.

Maria Goeppert Mayer was not the first woman to win a Nobel Prize in science (Physics, Chemistry or Physiology or Medicine) after Marie Curie became a laureate in Physics (1903) and Chemistry (1911). She was preceded by Irène Joliot-Curie (Chemistry, 1935), one of Marie's two daughters, and Gerty Cori (Physiology or Medicine, 1947). Both, by the way, shared their prizes with their respective husbands, Frédéric Joliot-Curie and Carl Ferdinand Cori (the Coris also shared their prize with Argentinian physiologist Bernardo Houssay). And the year after Maria Goeppert became a laureate, Dorothy Crowfoot Hodgkin won the Nobel Prize in Chemistry for having used X-ray diffraction to determine 'structures of important biochemical substances'. Before Goeppert Mayer won the Nobel Prize in Physics, Bertha von Suttner (1905), Jane Addams (1931) and Emily Greene Balch (1946) had won the Nobel Peace Prize, and Selma Lagerlöf (1909), Grazia Deledda (1926), Sigrid Undset (1928), Pearl S. Buck (1938) and Gabriela Mistral (1945) had won the Nobel Prize for Literature, a relevant fact because of what it means about women's capability: if women can lead in other fields, why not in science?

One interesting detail Margaret Rossiter highlighted is that Maria Goeppert received much more attention for winning the Nobel Prize than Gerty Cori ever did. Cori went almost unnoticed. Local and national U.S. papers meanwhile compared Maria Goeppert to Marie Curie. They also dwelt a good deal on her family life, though, a subject that would probably not have been mentioned in the case of a male Nobel laureate. *Science Digest*, for example, entitled one of its articles 'At Home with Maria Mayer' and dwelled on her beauty during her Göttingen days and the fact that her husband said what a good housewife she was. A ladies' magazine, *McCall's*, repeatedly emphasized that she was an elegant hostess and described in detail the gown she planned to wear to the Nobel Prize Award Ceremony. And the *San Diego Evening Tribune* headed its story with 'S.D. Mother Wins Nobel Physics Prize'. That she was a distinguished physicist, a full professor of the University of California, San Diego, was less important than being a good homemaker.

Since Maria Goeppert Mayer won the Nobel Prize in Physics, only two other women have done the same, Canadian Donna Strickland, professor at the University of Waterloo, who shared half the prize in 2018 with Gérard Mourou 'for their method of generating high-intensity, ultra-short optical pulses' (the other

Her memory has withstood the passage of time. After her death the American Physical Society created the Maria Goeppert Mayer Award for young women physics PhDs who are just beginning their career. Argonne National Laboratory and the University of California, San Diego, have memorialized her. The first confers a yearly prize to an outstanding young woman scientist or engineer, and the second hosts the annual Maria Goeppert Mayer Symposium.

half went to Arthur Ashkin), and Andrea Ghez, who won it in 2020 jointly with Reinhard Genzel 'for the discovery of a supermassive compact object at the centre of our galaxy' and with Roger Penrose, the latter, 'for the discovery that black hole formation is a robust prediction of the general theory of relativity'.

Death

Photographs from the Nobel Prize Award Ceremony clearly show that Maria Goeppert Mayer had entered a physical decline; her condition had been worsened by a number of heart attacks. She died on 20 February 1972. Joseph Mayer remarried soon afterward (his second wife, Margaret Griffen, was not a scientist). He lived for another eleven years. He died in 1983.

Her memory, however, has survived the passage of time. After her death the American Physical Society created the Maria Goeppert Mayer Award for young women physics PhDs who are just beginning their career. The winner receives money and the opportunity to speak about her research as a guest lecturer at four leading institutions. Two of the facilities Goeppert Mayer was related with, Argonne National Laboratory and the University of California, San Diego, have memorialized her. The first confers a yearly prize to an outstanding young woman scientist or engineer, and the second hosts the annual Maria Goeppert Mayer Symposium for women researchers to discuss current issues in science. A 35-kilometre-wide crater on Venus bears her name. And in 2011 she was placed on a commemorative stamp by the U.S. Postal Service.

She deserved these memorials and homages. She still does. I have tried to show in this book that her scientific career was not easy, that her ‘condition’ made it impossible for her to establish and pursue a consistent, lifelong programme of research of her own choosing. She had to make do, adjust to whatever she could get from the schools where her husband taught. And every time she shone and left her mark. She was, without any shadow of a doubt, a great scientist.

Bibliography

Biographies of Maria Goeppert Mayer

JOAN DASH, *A Life of One's Own. Three Gifted Women and the Men They Married* (Harper & Row, New York, 1973).

JOSEPH P. FERRY, *Maria Goeppert Mayer. Physicist* (Chelsea House Publishers, Philadelphia, 2003).

ULLA FÖLSING, *Mujeres Premio Nobel* (Alianza Editorial, Madrid, 1993).

KAREN E. JOHNSON, 'Science at the Breakfast Table', *Physics in Perspective 1*, 22-34 (1999).

PETER C. MAYER, *Son of (Entropy)2. Personal Memoirs of a Son of a Chemist, Joseph E. Mayer, and a Nobel Prize Winning Physicist, Maria Goeppert Mayer* (AuthorHouse, Bloomington, 2011).

ROBERT G. SACHS, 'Maria Goeppert Mayer, 1906-1972', *Biographical Memoirs National Academy of Sciences* 50, 310-328 (1979).

EUGENE WIGNER, 'Obituary: Maria Goeppert Mayer', *Physics Today* 25 (May 1972), pp. 77-79.

Chapter 1

MAX and HEDWIG BORN, *Ciencia y conciencia en la era atómica* (Alianza Editorial, Madrid, 1971).

MAX BORN, *My Life. Recollections of a Nobel Laureate* (Charles Scribner's Sons, New York, 1978).

WERNER HEISENBERG, *Encuentros y conversaciones con Einstein y otros ensayos* (Alianza, Madrid, 1985); *Diálogos sobre la física atómica*, in José Manuel Sánchez Ron, dir. (1996), *Heisenberg·Bohr·Schrödinger. Física cuántica* (Círculo de Lectores, Barcelona, 1996).

ROBERT S. MULLIKEN, *Life of a Scientist* (Springer-Verlag, Berlin, 1968).

MAX PLANCK, *Autobiografía científica y otros escritos* (Nivola, Madrid, 2000).

JOSÉ MANUEL SÁNCHEZ RON, *Historia de la física cuántica, I. El período fundacional (1860-1926)* (Crítica, Barcelona, 2001).

Chapter 2

EDOARDO AMALDI, *The Adventurous Life of Friedrich Georg Houtermans, Physicist (1903-1966)* (Springer, Heidelberg, 2012).

MAX BORN, *My Life. Recollections of a Nobel Laureate* (Charles Scribner's Sons, New York, 1978).

AUGUSTE DICK, *Emmy Noether, 1882-1935* (Birkhäuser, Boston, 1981).

WALTER M. ELSASSER, *Memoirs of a Physicist in the Atomic Age* (Science History Publications, New York, 1978).

NANCY THORNDIKE GREENSPAN, *The End of a Certain World. The Life and Science of Max Born* (Basic Books, New York, 2005).

FELIX KLEIN, *Lecciones sobre el desarrollo de la Matemática en el siglo XIX* (Crítica, Barcelona, 2006).

JOST LEMMERICH, *Science and Conscience. The Life of James Franck* (Stanford University Press, Stanford, 2011).

MARIE-ANN MAUSHART, *Hertha Sponer. A Woman's Life as a Physicist in the 20th Century* (Department of Physics, Duke University, Durham, North Carolina, 2011).

CONSTANCE REID, *Hilbert* (Springer-Verlag, 1970).

MARGARET W. ROSSITER, *Women Scientists in America. Struggles and Strategies to 1940* (The Johns Hopkins University Press, Baltimore, 1982).

DAVID E. ROWE, *A Richer Picture of Mathematics. The Göttingen Tradition and Beyond* (Springer, 2018).

ARNE SCHIRRMACHER, *Establishing Quantum Physics in Göttingen. David Hilbert, Max Born, and Peter Debye in Context, 1900-1926* (Springer, 2019).

M. SHIFMAN, *Standing Together in Troubled Times* (World Scientific, Singapore, 2017).

BRIGITTE STROHMAIER and ROBERT ROSNER, *Marietta Blau. Stars of Disintegration. Biography of a Pioneer of Particle Physics* (Ariadne Press, Riverside, California, 2006).

VICTOR WEISSKOPF, *The Joy of Insight. Passions of a Physicist* (BasicBooks, New York, 1991).

HERMANN WEYL, 'Emmy Noether', *Scripta Mathematica* 3, 201-220 (1935).

RICHARD WILLSTÄTTER, *From My Life* (W.A. Benjamin, New York, 1965).

The Collected Papers of Albert Einstein, vol. 8, part B ('*The Berlin Years: Correspondence 1918*'), Schulmann, Robert, Kox, Anne J., Janssen, Michel and Illy, József, eds. (Princeton University Press, Princeton, 1998).

Chapter 3

PHINA G. ABIR-AM and DORINDA OUTRAM, eds., *Uneasy Careers and Intimate Lives. Women in Science, 1789-1979* (Rutgers University Press, New Brunswick, 1987).

MAX BORN, *My Life. Recollections of a Nobel Laureate* (Charles Scribner's Sons, New York, 1978).

MAX BORN, *The Born-Einstein Letters, 1916-1955* (Macmillan, New York, 2005).

A. HUNTER DUPREE, *Science in the Federal Government* (The Johns Hopkins University Press, Baltimore, 1986; first edition 1957).

D. FLEMING and B. BAILYN, eds., *The Intellectual Migration* (Harvard University Press, Cambridge, Massachusetts, 1969).

DANIEL KEVLES, *The Physicists. The History of a Scientific Community in Modern America* (Alfred A. Knopf, New York, 1978).

KATHERINE HARAMUNDANIS, ed., *Cecilia Payne-Gaposchkin: Her Autobiography and Other Recollections* (Cambridge University Press, New York 1984).

SALVADOR LURIA, *Autobiografía de un hombre de ciencia* (Fondo de Cultura Económica, Mexico, 1986).

C. MEAD and T. HAGER, eds. (2001), *Linus Pauling, Scientist and Peacemaker* (Oregon State University Press, Covallis, 2001).

SIMON NEWCOMB, 'The evolution of the scientific investigator', in *Congress of Arts and Science. Universal Exposition, St. Louis, 1904,* Howard J. Rogers, ed. (Houghton, Mifflin and Co., Boston, 1905), vol. 1; reproduced in *Physics for a New Centu*ry, Katherine R. Sopka, comp. (American Institute of Physics, New York, 1986).

LINUS PAULING, *The Nature of the Chemical Bond and the Structure of Molecules and Crystals* (Cornell University Press, Ithaca, 1939).

NATHAN REINGOLD, ed., *Science in Nineteenth-century America. A Documentary History* (Macmillan, London, 1966).

MARGARET W. ROSSITER, *Women Scientists in America. Struggles and Strategies to 1940* (The Johns Hopkins University Press, Baltimore 1982) and *Women Scientists in America. Before Affirmative Action, 1940-1972* (The Johns Hopkins University Press, Baltimore, 1995).

JOSÉ MANUEL SÁNCHEZ RON, *El poder de la ciencia. Historia social, política y económica de la ciencia (siglos xix y xx)* (Crítica, Barcelona, 2007; second edition 2010).

DAVA SOBEL, *The Glass Universe. How the Ladies of the Harvard Observatory Took the Measure of the Stars* (Viking, New York, 2016).

KATHERINE RUSSELL SOPKA, *Quantum Physics in America* (Arno Press, New York, 1980).

STUEWER, ROGER H., 'Nuclear physicists in a new world. The émigrés of the 1930s in America', *BerWissenschaftsgesch* 7, 23-40 (1984).

Chapter 4

HANS A. BETHE, ROBERT F. BACHER and M. STANLEY LIVINGSTON, *Basic Bethe. Seminal Articles on Nuclear Physics, 1936-1937* (Tomash Publishers-American Institute of Physics, 1986).

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

OTTO R. FRISCH, *Del átomo a la bomba de hidrógeno. Recuerdos de un físico nuclear* (Alianza Editorial, Madrid, 1982).

KAREN E. JOHNSON, 'Maria Goeppert Mayer: Atoms, molecules and nuclear shells', *Physics Today* 39, 44-49 (September 1986).

JOSEPH MAYER, 'The way it was', *Annual Review of Chemical Physics* 33, 1-23 (1982).

LÉON ROSENFELD, 'Nuclear reminiscences', in *Cosmology, Fusion and Other Matters: George Gamow Memorial Volume,* Frederick Reines ed. (Colorado Associated Universities Press, Colorado, 1972).

JOSÉ MANUEL SÁNCHEZ RON, *El poder de la ciencia. Historia social, política y económica de la ciencia (siglos XIX y XX)* (Crítica, Barcelona, 2007; second edition 2010).

ROBERT SERBER, *The Los Alamos Primer. The First Lectures on How to Build an Atomic Bomb* (University of California Press, Berkeley, 1992).

MATTHEW SHINDELL, *The Life and Science of Harold C. Urey* (The University of Chicago Press, Chicago, 2019).

ALICE K. SMITH and CHARLES WEINER, eds., *Robert Oppenheimer. Letters and Recollections* (Harvard University Press, Cambridge, Massachusetts, 1980).

ROGER STUEWER, ed., *Nuclear Physics in Retrospect. Proceedings of a Symposium on the 1930s* (University of Minnesota Press, Minneapolis, 1979).

LEO SZILARD, 'Reminiscences', in *The Intellectual Migration. Europe and America, 1930-1960* (Harvard University Press, Cambridge, Mass. 1969).

EDWARD TELLER, with JUDITH SHOOLERY, *Memoirs. A Twentieth-Century Journey in Science and Politics* (Perseus Publishing, Cambridge, Massachusetts, 2001).

Chapter 5

WALTER M. ELSASSER, *Memoirs of a Physicist in the Atomic Age* (Science History Publications, New York, 1978).

GEORGE GAMOW, *My World Line. An Informal Autobiography* (The Viking Press, New York, 1970).

GEORGE GAMOW and C.L. CRITCHFIELD, *Theory of Atomic Nucleus and Nuclear Energy Sources* (Clarendon Press, Oxford, 1949).

MARIA GOEPPERT MAYER and J. HANS D. JENSEN, *Elementary Theory of Nuclear Shell Structure* (John Wiley & Sons, New York, 1955).

RICHARD G. HEWLETT and FRANCIS DUNCAN, *Atomic Shield. A History of the United States Atomic Energy Commission*, vol. II ('1947-1952') (University of California Press, Berkeley, 1990).

KAREN E. JOHNSON, 'Maria Goeppert Mayer: Atoms, Molecules and Nuclear Shells', *Physics Today* 39, 44-49 (September 1986).

KAREN E. JOHNSON, 'From Natural History to the Nuclear Shell Model: Chemical Thinking in the Work of Mayer, Haxel, Jensen, and Suess', *Physics in Perspective* 6, 295-309 (2004).

HELGE KRAGH, *Cosmology and Controversy* (Princeton University Press, Princeton, 1996).

HELGE KRAGH 'An Unlikely Connection: Geochemistry and Nuclear Structure', *Physics in Perspective* 2, 381-397 (2000).

MARGARET W. ROSSITER, *Women Scientists in America. Struggles and Strategies to 1940* (The Johns Hopkins University Press, Baltimore, 1982).

ANDREI SAJAROV, *Memorias* (Plaza & Janes/Cambio 16, Barcelona, 1991).

EDWARD TELLER, 'Back to the Laboratories', *Bulletin of the Atomic Scientists* 6, 71-72 (1950).

José Manuel Sánchez Ron

Maria Goeppert Mayer: From Göttingen to the Nobel Prize in Physics

Maria Goeppert Mayer (1906-1972) was one of the four women to have won the Nobel Prize in Physics so far: Marie Curie (1903), Maria Goeppert Mayer (1963), Donna Strickland (2018) and Andrea Ghez (2020). In this book emeritus professor of the history of science at the Autonomous University of Madrid and member of the Spanish Royal Academy José Manuel Sánchez Ron tells about Maria Goeppert Mayer's life story and contributions within the context of the scientific and national worlds she lived in (Germany and the United States) and reconstructs the highs and lows of her career, which bore her from the University of Göttingen to Johns Hopkins, Columbia University, the University of Chicago and finally the University of California, San Diego. As especially gifted for theoretical physics as she was, the 'circumstances' of her life prevented her from pursuing any consistent or continuous plan of research. The main 'circumstance' of her professional life was her marriage to a scientist, Joseph Mayer, a point that most of the institutions that employed him seized upon as a reason not to hire her or pay her for her work. As a result, she had to conform to the scientific interests of the researchers at the institutions where her husband taught. These scientists (like Karl Herzfeld, Edward Teller and Enrico Fermi) recognized her talent, as had Max Born and James Franck at Göttingen before. And every time, at every institution graced with her presence, she left her mark. So it was until she achieved her great success, the nuclear shell model, which secured her the Nobel Prize.

José Manuel Sánchez Ron received his licentiate degree in physics from the Complutensian University of Madrid and his doctorate from the University of London. Since 2019 he has been emeritus professor of the history of science at the Autonomous University of Madrid, where he was named head professor of theoretical physics in 1994. He is the author of many influential works on the history of international and Spanish science. In 2015 he received the Spanish National Literature Award in the Essay category for *El mundo después de la revolución. La física de la segunda mitad del siglo XX* (The World After the Revolution. Physics in the Second Half of the 20th Century), the first Spanish National Literature Award ever to go to a book on the history of science. In 2016 he received the Julián Marías Award from the Community of Madrid for his scientific career in the humanities. He was inducted into the Spanish Royal Academy in 2003.

This book commemorates the fortieth anniversary of the Spanish Nuclear Safety Council. Our organization has been serving nuclear safety and radiological protection in Spain for 40 years. Its ongoing mission is to protect workers, the population and the environment from the harmful effects of ionizing radiation, ensuring that nuclear and radioactive facilities are run safely under the correct preventive measures.

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