From Quanta to Gravitation

– The Science and Life of Christian Møller

By Helge Kragh

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Abstract

The Danish physicist Christian Møller (1904-1980) graduated from Niels Bohr's famed Copenhagen institute in 1929 and remained there until his death in early 1980. He is possibly best known for the eponymous "Møller scattering," a theory of electron-electron scattering based on relativistic quantum mechanics that he announced in 1931. Many physicists will also be aware of Møller's works on Einstein's general theory of relativity and in particular of his influential textbook *The Theory of Relativity* published in 1952. On the other hand, Møller's contributions to elementary particle physics have largely fallen into oblivion, the reason being that his very extensive work in this area did not stand the test of time. Nonetheless, during the period from 1937 to about 1948 Møller was recognised as a leading expert in the theories dealing with mesons and other fundamental particles.

From Quanta to Gravitation is a full biography of Møller based to a large extent on archival and other unpublished material. It covers not only his scientific contributions, but also the social environment in which he grew up and the extensive international network of physicists with which he interacted. On the political and organisational side, the book highlights Møller's work during World War II and his later position as director of Nordita, the Nordic Institute for Theoretical Atomic Physics. Yet another area in which Møller was a key figure concerned the organisation of an international forum for specialists in general relativity theory, what in 1974 materialised as the International Society of General Relativity and Gravitation. Last but not least, Møller was an indefatigable lecturer who for forty years gave courses in theoretical physics to numerous students in Copenhagen. Many of those who listened to his lectures or had him as a supervisor became renowned physicists.

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Preface

The Danish physicist Christian Møller (1904-1980) rarely appears in historical writings on the development of physics in the twentieth century, whether in works written by physicists or by historians of science. And when he does turn up, it is as a relatively minor figure, sometimes reduced to just one of Niels Bohr's faithful lieutenants. I hope this book will show that he was more than that, much more. To the extent Møller is known today, it is primarily for three quite different contributions to physics. One is his quantum-mechanical theory of collisions of fast electrons, so-called Møller scattering, and another is his textbook on relativity theory dating from 1952. The third contribution, better known among quantum chemists than among mainstream physicists, is a theory for calculation of many-electron systems he published in 1934 together with his American colleague Milton Plesset. It is also of some interest that the legacy of Møller lives on in the words 'nucleon' and 'lepton' familiar to all physicists. These he coined in 1941 and 1946, respectively, and in 1939 he was the first to publish a paper with the word 'meson'.

Møller started his career with works in quantum and particle physics, specialising in theories of collision processes, beta decay, and the poorly understood mesons found in the cosmic rays. However, at about 1955 he changed abruptly to studies of general relativity theory, a field which occupied him for the rest of his life. Møller's dual expertise in both quantum and relativity physics makes his scientific life interesting, as it covers two very different and perhaps even irreconcilable areas of fundamental physics. Moreover, with his first paper published in 1929 and the last in 1979 his career spanned the development over half a century, a period during which physics changed dramatically. His life offers a window to this dramatic change. Despite being nearly forgotten today, in his own time Møller was recognised as a major player in international theoretical physics. His professional network was very wide and he interacted in a variety of ways with a large number of better-known physicists, many of whom were or became Nobel laureates. To mention just a few, they included Hans Bethe, Nevill Mott, Rudolf Peierls, Lev Landau, Felix Bloch, Wolfgang Pauli, Subrahmanyan Chandrasekhar, Werner Heisenberg, J. Robert Oppenheimer, Hideki Yukawa, Enrico Fermi, Paul Dirac, Steven Weinberg, George Gamow, Hermann Bondi, Homi Bhabha, and Vladimir Fock.

Of course, Bohr was by far the most important of the numerous physicists which Møller met and with whom he interacted. And yet, although he was deeply influenced by the much-admired Bohr, he was also an independent scientist who defined his own research path without regard to what was considered mainstream by Bohr and his close co-workers in Copenhagen. Much like Piet Hein's little cat, Møller was his own: "Little cat, little cat / walking so alone / tell me whose cat you are / I'm damned well my own." (On Piet Hein, see Section 1.2.)

From Quanta to Gravitation is largely organised chronologically with most of the chapters focusing on Møller's works in theoretical physics. It starts conventionally with his background and youth and ends with a section discussing his general views concerning topics such as philosophy of science and the relations between science and society. Chapter 2 is devoted to Møller's early theory of relativistic electron-electron scattering, which was quickly recognised as an important work and made him known in the international community of theoretical physics. The following chapter deals with his theoretical works in the 1930s on radioactivity, the Møller-Plesset perturbation theory, compact stars, and a few other subjects. Although Møller was not directly involved in the discovery of nuclear fission, he was a witness to it and its aftermaths. Chapter 4 describes in considerable detail his role during the war years 1939-1945, when he for a period acted as co-director of the Copenhagen institute. The sections making up Chapter 5 mostly deal with Møller's scientific work in the 1940s and early 1950s, which to a large extent focused on meson theory but also included an in-depth study of Heisenberg's new *S*-matrix theory and a work on the so-called clock paradox in the theory of relativity.

As mentioned, from about 1955 Møller concentrated on general relativity, which made him an important figure in what is known as the renaissance of Einstein's old theory of gravitation. His extensive work in this area, not only scientifically but also organisationally, is covered in chapters 6 and 7. One of the sections deals with his works on relativistic thermodynamics and another with his ambitious attempt to develop a singularity-free modification of Einstein's gravitational field equations. At the end of his life Møller presented on the basis of his 'tetrad theory' a new cosmological model without a big bang. However, his model was unsuccessful insofar that it was ignored by most relativists and cosmologists. Finally, Chapter 8 deals with Møller's activities related to the public understanding of science and in particular to organisational and institutional aspects of physics. While the first area was restricted to the national level, the latter was international in scope. Møller worked hard to maintain the high scientific status of the Copenhagen institute after World War II, which he did, for example, in his capacity as director of the Nordic Institute for Theoretical Atomic Physics better known as Nordita. He was also a key figure in the establishment of an international society of specialists in gravitation and general relativity.

The book relies to a considerable extent on letters and other archival sources, many of which have not been examined previously. Møller maintained throughout his career an extensive correspondence with other physicists and luckily the major part of the correspondence is collected in the Møller Papers kept at the Niels Bohr Archive in Copenhagen. Apart from these central documents I have also made use of other materials at the Archive, such as the letters to and from Niels Bohr, Aage Bohr, Stefan Rozental, and Léon Rosenfeld. The major collections are abbreviated BSC (Niels Bohr Scientific Correspondence), CMP (Christian Møller Papers), and RP (Léon Rosenfeld Papers). Another useful source is the rich Oral History Interview project of the American Institute of Physics. Møller was interviewed twice by leading historians of physics, in 1963 by Thomas Kuhn and in 1971 by Charles Weiner. Unfortunately, the extant sources have very little to say about Møller as a private person. What I know of him in this respect, and it is admittedly not very much, is pieced together from various sources such as letters, interviews, newspaper articles, and recollections of his colleagues in physics.

Although I did not really know Møller, at least I have met him, namely when I studied at the Niels Bohr Institute 1968-1970 pre-

Fig. 1. Staff and students at the Niels Bohr Institute 1969. In the middle of the front row is Aage Bohr surrounded by the two retired secretaries, Betty Schultz (left) and Sophie Hellmann (right). Other people in the front row are C. Møller, B. Mottelson, J. Bøggild, S. Rozental, and T. Huus. The author is placed near the middle of the fifth row. Author's possession.

paring for my Master's thesis. Møller was giving his lecture course on quantum mechanics, which I dutifully followed and remember as clear, demanding, and not particularly inspiring. I also signed in for some lectures he gave on general relativity, but in this case I dropped out when I discovered how difficult the subject was. At one occasion I asked him if he might possibly act as my supervisor and if he could suggest a thesis project within the history of modern theoretical physics. He was not interested. Many years later and for some reason I no longer remember I got interested in Møller's electron-electron scattering theory on which I wrote a detailed paper in *Archive for History of the Exact Sciences*. At the time I vaguely contemplated to write a full biography but soon abandoned the idea as unrealistic and much too complicated. I still find it to be complicated, but at least it is no longer unrealistic.

During the work with writing this book I have had the great advantage of having an office room at the Niels Bohr Archive and access to the rich archival material located there. I am much grateful to the director of the Archive, Christian Joas, and in particular to its archivist Rob Sunderland who has been of invaluable help. I also acknowledge the assistance of Kader Ahmad, librarian at the library of the Niels Bohr Institute, and of Chris Pethick, former professor at Nordita. Valuable help has been provided by Alexander Blum and Roberto Lalli at the Max Planck Institute for History of Science, and by Helle Kiilerich and Iver Brevik, both of whom knew and worked with Christian Møller.

Helge Kragh, January 2022.

CHAPTER 1.

Becoming a physicist

In late 1904, when Christian Møller was born, Einstein's relativity theory was still in the future. Planck's idea of energy quantisation was known to a few specialists but thought to be relevant only to the field of blackbody radiation. Physicists began to realise that the atom was a composite body, perhaps made up of a large number of electrons distributed in equilibrium positions within a positively charged sphere, such as proposed by J. J. Thomson in England. They did not imagine that Planck's constant of action might have anything to do with the architecture of atoms. The exciting phenomenon of radioactivity attracted intense interest not only among physicists but also among chemists. Somehow radioactive decay seemed to be due to changes in atomic structure, but no-one could say what the connection was. As to gravity, Newton's celebrated theory of 1687 still reigned supreme, unperturbed by anomalies such as the unexplained precession of Mercury's perihelion. Almost all physicists agreed that the concept of the ether was indispensable, indeed that fundamental physics was in the end ether physics. On a foundational level, physics was about the relationship between ether and matter, or even more ambitiously, about how matter could be reduced to manifestations of the ether.

As seen in retrospect, physics at about 1904 was a small business. Historians have estimated that at the turn of the centenary, the total number of academic physicists in the world was between 1200 and 1400.[1] It was almost exclusively a European-American business dominated by physicists from Germany, Great Britain, the United States, and France, who together made up half the world population of physicists. No less exclusively, it was a male business. International meetings in the physical sciences were few and not truly international, as they were largely restricted to physicists from

1. Forman, Heilbron, and Weart (1975). See also Kragh (1999), pp. 3-26 for a general account of physics in the fin-de-siècle era.

Europe and North America. Thus, when the first (and only) International Congress of Physics convened in Paris in July 1900, 789 men and two women registered as participants. Non-whites were represented by papers delivered by one Japanese and one Indian. The congress comprised all branches of physics from electrical measurement technologies over optical experiments to new theories of the electromagnetic ether.[2] How different was the world of physics when Møller passed away eighty years later!

1.1. From Oberrealschule to Bohr's institute

During the winter and spring of 1864 a large Prussian army reinforced by Austrian forces conquered the southern parts of Jutland in what is known as the second Schleswig war. The defeat of the Danish army was crushing and the consequences catastrophic for the Danish kingdom. Not only did the duchies Schleswig and Holstein now become incorporated in Bismarck's new German empire, so did a large part of southern Jutland. At a stroke, the territory of Denmark and its number of inhabitants were drastically reduced, the first with about one-third and the latter with about two-fifths.[3] Only with the Versailles peace treaty following Germany's defeat in World War I did the situation change. As the result of a plebiscite in 1920 the northern parts of Schleswig voted itself back into Denmark and thereby created the current Danish-German border. What in Denmark is referred to as the reunification (*Genforeningen*) was officially celebrated on 9 July 1920. One of the regions liberated from German supremacy was the 312 km^2 island of Als, where Christian Møller was born as a German citizen on 22 December 1904.

Christian was born in the small village Hundslev some 15 km from Sønderborg, the only major town on Als and until 1920 carrying its German name Sonderburg (Hundslev was similarly Hundsleben). His father was Jørgen Hansen Møller (1875-1953), a village

2. For details on the Paris congress and physics in the first decade of the twentieth century, see Staley (2008).

3. See Jespersen (2011), pp. 23-25.

smith, and his mother was the five-year younger Marie Hansen Møller née Terkelsen.[4] Christian did not come to know his mother, who died the year after having given birth to him. At about 1918, when the small family (which included a brother and a sister to Christian) was economically pressed, the father changed to become a merchant in the trade of bicycles for the local area. While Jørgen Hansen Møller avoided being sent to the front during the war, his younger brother was not so lucky. He, Christian's uncle, was one of the approximately 6000 Southern Jutlanders killed in action as German soldiers.

Young Christian first attended a German village school in nearby Notmark and next a public school in Sønderborg. One of his classmates was the one year younger Mads Clausen with whom he formed a lasting friendship. At some point the two technically interested youngsters engaged in inventing an apparatus which from an explosive oxygen-hydrogen mixture should power a battery. Although the grandly conceived project failed, Christian and Mads remained friends for life. In 1933, Mads Clausen, who was trained as an engineer, started a one-man company later called Danfoss which made expansion valves to refrigerators. When Møller died in 1980, the Danfoss Company had become one of Denmark's largest industrial corporations with about 10,000 employees. Throughout the life of Mads Clausen, who died in 1966, he stayed in close contact with his former classmate and would-be inventor. Thus, it was on Møller's initiative that Clausen in 1955 made a substantial donation to the Copenhagen institute for theoretical physics on the occasion of Niels Bohr's seventieth birthday.[5]

To return to Christian Møller's education, in 1917 he was admitted to the Königliche Oberrealschule zu Sonderburg, a school founded in 1865 and turned into a gymnasium in 1910. After the reunification in 1920 it was taken over by the Danish authorities on 26 August and transformed into the still existing Sønderborg

4. On the occasion of his doctorate in 1932, Møller wrote a brief autobiography in *Festskrift Udgivet af Københavns Universitet, November 1933* (Copenhagen, 1933), pp. 106-107.

5. Clausen to Møller, 27 September 1955 (CMP).

Fig. 2. Sonderburg Oberrealschule, from 1920 Sønderborg Gymnasium, where Christian Møller was a pupil 1917-1923. Photograph from 1917. Credit: Museum Sønderjylland, ISL Mediearkiv.

Statsskole. Christian grew up bilingually, fluent in both Danish and German, which proved to be an advantage for a young man aspiring to become a scientist. "Initially I was interested in language, whereas mathematics was quite foreign to me", he recalled.[6] It took a year or so at Sønderborg Gymnasium until he realised that algebra and geometry were not foreign to him at all. His interest in mathematics and physics was awakened by his gymnasium teachers in these subjects. As he said in an interview with Thomas Kuhn of 1963:

> We had a quite good foundation in the elements of mathematics [but] in physics it was not very much. ... Chemistry was very little. Astronomy we learned a little, just the apparent motions of the stars and also Kepler's laws and some things, but not very much. ... We had a very good teacher in mathematics who by the way also taught us physics

6. Newspaper interview in *Jydske Tidende* of 15 March 1970 on the occasion of Møller's award of the Ørsted Medal. This is also the source for the story about the Møller-Clausen collaboration as juvenile inventors.

> in school, and it was his influence which made me tend to both mathematics and physics.[7]

In another recollection of the same year, Møller (as I shall call him from now on) recalled that he first became vaguely aware of Einstein's general theory of relativity during his time at Sønderborg Gymnasium.[8] The occasion was most likely in connection with the sensational and much-publicised discovery of the Sun's bending of starlight in agreement with Einstein's prediction. As the Danish newspaper *Politiken* announced on 18 November 1919, 'A Revolution in Science: Professor Einstein's Epoch-Making Theories Confirmed. Newton's Law of Gravity Refuted'. On 25 June 1920 Einstein, invited by the Danish Astronomical Society, lectured on 'Gravitation and Geometry' to an invited audience at the Polytechnic College in Copenhagen, and the following day he had a lunch meeting with the prominent Danish author and literary critic Georg Brandes. He also had conversations with Niels Bohr, whom he described in a letter to H. A. Lorentz as a "highly intelligent and excellent man."[9] Einstein's visit attracted massive attention in the newspapers, which brought interviews with the famous German physicist and tried to explain to its readers what his theories were all about. Presumably 15-year old Christian Møller followed the press coverage with much interest, wondering what strange concepts such as curved space and time dilation might mean.

Young Møller's knowledge of and interest in relativity theory did not only stem from newspaper articles but also from the reading of a popular book written by the Norwegian physicist, philosopher and psychologist Harald Schjelderup. He recalled: "In my school days I got hold of a … Norwegian book, written by a man called

7. Interview of 29 July 1963, available on https://www.aip.org/history-programs/niels-bohr-library/oral-histories/4782. This source is referred to as Kuhn (1963) in what follows.

8. Møller (1963a), p. 57.

9. Einstein to Lorentz, 4 August 1920, in Einstein (2006), p. 364. Before visiting Copenhagen, Einstein had given lectures in Oslo. In his letter to Lorentz, he said, "The journey to Kristiania [Oslo] was really beautiful, but the most beautiful was however the time I spent with Bohr in Copenhagen."

Schjelderup. Of course one didn't understand it completely, but it was very fascinating."[10] Many years later, Møller's early encounter with general relativity grew into a serious study of the theory, which since the mid-1950s became his main field of research and turned him into a highly recognised authority in general relativity.

After his graduation in 1923 from the gymnasium in Sønderborg, Møller decided to move to Copenhagen in order to matriculate as a student of mathematics and physics at the city's old university, which at the time was the only university in Denmark.[11] The new University of Aarhus, much closer to Als than Copenhagen, was only established in 1928 and it took nearly thirty years before it included a department of physics. In Copenhagen, Møller obtained a residence at a student college or dormitory called Regensen (Collegium Domus Regiæ) founded in 1623 and located in the central city close to the slightly later Round Tower observatory (Rundetårn). As an alumnus at Regensen he could stay for free, concentrating on his studies. In the fall of 1926, he moved from Regensen to another old college, Borchs Kollegium established in 1691, where he stayed until June 1931.[12] At Borch's Kollegium he was in close contact with two other alumni, Torkild Bjerge and Kaare Grønbech. Bjerge, who was a fellow student at Bohr's institute, became professor of physics at the Polytechnic College, whereas Grønbech, a linguist, became a renowned expert in oriental languages.

At first Møller vacillated between concentrating on mathematics or physics, a choice of no urgent need since the undergraduate school offered the same courses – in mathematics, physics, chemistry, and astronomy – to all students in the exact sciences. These courses were given at the Polytechnic College founded in 1829 by Hans Christian Ørsted and since 1922 with the physicist and inventor Peder Oluf Pedersen as its director. The Polytechnic

10. Kuhn (1963). The book was probably Schjelderup (1921).

11. Until the loss of Schleswig-Holstein in 1864, the even closer University of Kiel was under Danish administration but then became a German university and is today named the Christian-Albrecht University.

12. Mondrup (1943). Borchs Kollegium was named after Ole Borch, a Danish seventeenth-century chemist, philologist, and natural philosopher.

College shared most buildings and professors with the university, and the lectures were attended by both university students and engineering students. At a time Møller thought briefly of becoming an engineer, but he soon came to the conclusion that theoretical physics appealed much more to him than mechanical engineering or electrical technology.

Møller's decision to major in physics rather than pure mathematics was in part the result of courses in rational mechanics and thermodynamics, two branches of science which appealed to him because of the mathematical rigour with which problems could be stated and solved. As he recalled in his interview with Kuhn:

> Also we had a course in what we called 'rational mechanics', that is the old tradition in Europe, 'mécanique rationale' from the French school, and that was taught by a mathematician actually. Well this fascinated me of course very much, because it was the first time that one could calculate something in nature by using differential geometry. So I think it was 'mécanique rationale' and thermodynamics which awoke my interest in physics. I had no idea of quantum theory at that time.[13]

Møller found thermodynamics to be particularly interesting: "It was a fascinating thing to see how one could use mathematics to get the relations between the different thermodynamic quantities and how the whole thing could be formulated in these very few simple laws, the first and the second laws." He recalled with fondness the course in thermodynamics given by Edvard Sextus Johansen, a physicist at the Polytechnic College known as a brilliant teacher and writer of textbooks.

Having passed his undergraduate exam in January 1926 and now determined to specialise in theoretical physics, Møller looked forward to pursue graduate studies. That meant studies at Niels Bohr's already famous institute on Blegdamsvej, which had just been expanded and opened for students aiming at the magister

13. The course in rational mechanics was mainly taught by the mathematician Johannes Mollerup who also, together with Harald Bohr, the younger brother of Niels Bohr, was responsible for the course in mathematical analysis.

Fig. 3. The University's Institute for Theoretical Physics, later renamed the Niels Bohr Institute, as it looked in 1926, the year when Møller started graduate studies. Credit: Niels Bohr Archive, Photo Collection.

degree (roughly equivalent to the degree of Master of Science). The name of the institute founded in 1921 was Universitetets Institut for Teoretisk Fysik, literally meaning The University's Institute for Theoretical Physics. However, it was generally known as just Bohr's institute and would eventually, in 1965, be renamed the Niels Bohr Institute. Since studies at Blegdamsvej only began in the fall semester, Møller, eager to know more about modern physics, decided to register temporarily at the University of Hamburg. At the time he was only vaguely aware, if aware at all, that theoretical physics was experiencing a revolutionary phase with the new quantum mechanics originating in Göttingen and soon to be developed in an alternative version by Erwin Schrödinger in Zurich. His undergraduate studies included only classical physics, with neither quantum theory nor the theory of relativity being taught.

Although Møller had now decided to devote himself to theoretical physics, pure mathematics still attracted him. While in

Hamburg, he followed lectures on modern algebra and elasticity theory given by Emil Artin, a prominent Austrian mathematician best known for his important work on abstract algebra. More importantly, he also listened to Wolfgang Pauli's lectures on the theory of relativity based on his famous review in *Handbuch der mathematischen Wissenschaften* written at the tender age of 21.[14] Pauli, who spent the years 1923-1928 as a lecturer in Hamburg, was at the time deeply immersed in quantum mechanics and maintained his close connections to Bohr. For example, in early April 1926 Pauli visited Bohr in Copenhagen once again. In conversations with Pauli in Hamburg, Møller was informed about Bohr's work on quantum theory and the magic of his institute which was still foreign land to the Danish student. Whereas Møller had not yet met the great Niels Bohr, in the summer of 1926 he met by chance his brother, the mathematician Harald Bohr, who told him about the institute on Blegdamsvej and further wetted his appetite to master modern physics.[15]

During the early phase of Bohr's institute, most of the teaching was undertaken by Hendrik Antonie Kramers, his trusted Dutch assistant, but in the spring of 1926 Kramers left Copenhagen to take up a professorship in theoretical physics at the University of Utrecht. As a replacement young Werner Heisenberg was offered the position as lecturer, which he gladly accepted. Heisenberg began lecturing in May 1926 and continued until the end of 1927, after which he was replaced by the Swedish theorist Oskar Klein. Like Kramers, Klein belonged to the institute's original staff. Trained in physical chemistry as a student of the famous Swedish chemist and Nobel laureate Svante Arrhenius, Klein first met Bohr in 1918 and under the guidance of Kramers he swiftly changed to theoretical physics. When Møller arrived at the institute in the fall semester of 1926, Heisenberg lectured on electrodynamics while other courses (on statistical mechanics, relativity theory, and analytical dynam-

14. English translation in Pauli (1958). See Enz (2002), pp. 25-35.

15. Møller met Harald Bohr on a railroad trip from Sønderborg to Copenhagen. See Møller (1963a), p. 55.

ics) were given by either Klein or Heisenberg.[16] The lectures were in Danish, or in Klein's case presumably in Swedish, a problem Heisenberg had solved by learning himself the Danish language in the course of a few months.

Møller first met Niels Bohr a month or two after having begun his studies at the institute. The meeting made an enduring mark on the young physicist-to-be. Here is his recollection of his encounter in the library of the Bohr institute with the legendary founder of quantum atomic theory:

> I was absorbed in Einstein's famous treatise in *Annalen der Physik* from 1916, where he gave a comprehensive exposition of his general theory of relativity. ... I sat alone in the library and suddenly Bohr entered. Confused, I raised from my seat. Bohr smiled, warmly and friendly, and when he realised what I was reading he told me in a long monologue about Einstein's great contributions in the early part of the century. He emphasized the significance of his analysis of the concepts of space and time and how it had destroyed ingrained philosophical prejudices. I listened tensely and tried to the best of my ability to follow his lines of thought. ... He talked for a long time and explained to me that currently we experienced in atomic theory epistemological revolutions even more profound [than those caused by Einstein's theory of relativity]. Pointing his finger to the opened volume of *Annalen*, he said that now these problems (within the classical theory of relativity) were solved, and he recommended that I should instead study the recent developments in quantum theory initiated by Heisenberg's work the previous year. In this area, he said, there are still many unsolved problems.[17]

Møller was spellbound by his meeting with Bohr: "It was with buzzing head and almost intoxicated by exaltation that I later went home to Regensen along the lakes. It was the first time I experienced the impetus and strange feeling of elevation that one received even

16. Robertson (1979), pp. 110-112.

17. Møller (1963a), p. 57. The paper that Møller studied was Einstein's first full account of his new theory published as 'Die Grundlage der allgemeinen Relativitätstheorie' in the May 1916 issue of *Annalen der Physik*.

from a minor conversation with Bohr."[18] He followed Bohr's advice of focusing on quantum mechanics and for more than a decade shelved his interest in Einstein's theory of general relativity.

Probably in early 1927 Møller studied on Bohr's recommendation a long and difficult paper in which Klein interpreted Schrödinger's new wave mechanics in terms of the correspondence principle rooted in the old quantum theory. By making use of a five-dimensional version of the ordinary Schrödinger equation

$$H\psi = i\hbar\, \partial\psi/\partial t$$

Klein arrived at new formulae for dispersion and Compton scattering.[19] Møller later said about Klein's paper that "it was really the first use of the Schrödinger equation which was done in a way which we would still today recognize as a correct way."[20] Bohr, who was very fond of Klein's work because it highlighted the correspondence between classical electrodynamics and quantum mechanics, thought that only with this work could the transition probabilities in Schrödinger's theory be correctly calculated. Møller was a newcomer to quantum mechanics but he nonetheless quickly digested Klein's difficult paper which proved to be an important resource for his later work on relativistic electron-electron scattering. Half a century later, in an obituary of Klein, he praised the paper in these words:

> The theory, which contains Bohr's frequency condition as a natural element, leads to a simple description of spontaneous and forced emission of light, and also of the photo-electric effect and the dispersion of light. ... As far as most effects are concerned, Klein's simple correspondence-like theory yields in a first approximation the same results as

18. Møller's experience was shared by other young physicists. Otto Frisch recalled that, "when I cycled home through the streets of Copenhagen, fragrant with lilac and wet with rain, I felt intoxicated with the heavy spirit of Platonic dialogue." Quoted in Beller (1999), p. 259.
19. Klein (1927), submitted 4 December 1926 and published 21 January 1927. See also the summary account in Mehra and Rechenberg (2000), pp. 176-180.
20. Kuhn (1963).

the twenty years younger and much more complicated renormalizable quantum electrodynamics.[21]

Less than a month after his first encounter with Bohr, Møller had the experience of listening to a lecture Schrödinger gave in Copenhagen on 'Die Grundlagen der undulatorischen Mechanik' (The Principles of Wave Mechanics). In his capacity as chairman of the Danish Physical Society (*Fysisk Forening*), Bohr had invited the Austrian physicist, who on 4 October 1926 spoke to a general audience of physicists and engineers in the large auditorium of the Polytechnic College. Møller was impressed by the clarity of Schrödinger's talk, which he found to be most exciting. The next day Schrödinger gave a research colloquium at the Blegdamsvej institute, where his views concerning stationary states and the emission of light were severely criticised by Bohr and Heisenberg.[22] Klein was also present during the heated discussion, but Møller was not. After all, he was a quantum neophyte who had only recently started graduate studies. Møller was later told by Bohr about the discussions, where an exasperated Schrödinger was to have proclaimed, "Wenn wir zu dieser Herumspringerei zurückkehren müssen, dann bedaure ich, dass ich mich in die Sache eingemischt habe."[23] About one and a half year later, Møller came to meet the founder of wave mechanics in person.

1.2. Years of apprenticeship

The number of graduate students at Bohr's institute in the late 1920s was very small, typically 6-8 and all of them male students. Apart from Møller, they included Torkild Bjerge, Jørgen Kruse Bøggild, Bengt Strömgren, Mogens Pihl, and Ebbe Rasmussen, all

21. Møller (1977a), pp. 170-171. When Møller in 1974 nominated Klein for the Nobel Prize, it was principally for his old paper in *Zeitschrift für Physik* (Section 8.4).
22. On Schrödinger's visit in Copenhagen, see Bohr (1985), pp. 9-16, Moore (1989), pp. 226-229, and Kragh (2013).
23. Møller (1963a), p. 63. "If we have to return to this jumping around, then I am sorry that I ever got involved with the matter."

of whom proceeded to careers in Danish science and whom we shall encounter in later sections. A little older, Sven Werner worked from 1924 to 1927 as an assistant at the institute after which he obtained a position at the Polytechnic College and later became professor at Aarhus University.

Møller had close relations to Swedish-born Strömgren, a wunderkind who passed his magister exam in 1927 and defended his doctoral thesis two years later at the unusually young age of 21. In 1940 he replaced his father Elis Strömgren as professor of astronomy at the University of Copenhagen. Bengt Strömgren, who would become an internationally renowned astronomer and astrophysicist, started his studies at Blegdamsvej in 1925 with Bohr, H. M. Hansen, and H. A. Kramers as his teachers. He got acquainted with quantum mechanics at about the same time as Møller and in about the same way, by reading research papers. Like Møller, he was exposed to Klein's paper of early 1927, which he found to be most instructive. "One thing for me that was extremely important was Oscar [*sic*] Klein's on the interpretation of the wave picture in terms of probabilities. I learned more from that than from most of the other papers [on quantum mechanics]", he said in an interview of 1976.[24]

Møller and Strömgren continued to interact in a variety of ways until the end of Møller's life.[25] Yet another student at Bohr's institute was Piet Hein, a close friend of Strömgren who studied at the institute from about 1929 to 1932 but without taking his final exam. Hein dropped out of physics and went on to become a celebrated artist, poet, and designer. He wrote thousands of aphorisms in a genre he called 'grooks', one of which (titled *Atomyriades*) goes as follows:

Nature, it seems, is the popular name
for milliards and milliards and milliards

24. American Institute of Physics, interview of 5 May 1976 by Lillian Hoddeson and Gordon Baym. https://www.aip.org/history-programs/niels-bohr-library/oral-histories/5070-1. See also Rebsdorf (2003).

25. Strömgren (1981), a memorial paper on Møller. See sections 8.2 and 8.3 for the Møller-Strömgren connection.

of particles playing their infinite game
of billiards and billiards and billiards.

As a graduate student Møller had to follow courses in topics such as electrodynamics, statistical mechanics, relativity theory, analytical mechanics, and optics. The lectures in optics were given by Bohr's old friend Hans Marius Hansen, an expert in spectroscopy who back in 1913 had directed Bohr's attention to the Balmer spectral series and thus helped to construct the quantum atom.[26] The course in optics was based on a classic text by the German physicist Paul Drude, *Lehrbuch der Optik* first published in 1900 and with an English translation from 1902. Together with his assistants Jacob Christian Georg Jacobsen and Ebbe Rasmussen, Hansen was also responsible for the obligatory course in experimental physics which was part of the exam of all students. Møller chose to do optical experiments on the diffraction of light.[27]

Quantum theory was another subject taught at the institute, but in this case in a more casual and informal manner. To learn about the new theory of atoms and quanta, students had to read and discuss the research papers or seek the advice of more experienced physicists. This was the way Møller and Strömgren learned the craft. Although the first textbooks came out at the end of the decade, no textbook was used in Copenhagen.[28] A regular course in quantum mechanics was only included in the syllabus in early 1928, after Klein had replaced Heisenberg as a lecturer.

The regular courses, whether in theoretical or experimental branches of physics, were not the only way through which the students were trained. No less important, and to most students more exciting, were the colloquia series. There were at the institute two

26. Kragh (2012), pp. 56-57.

27. Weiner (1971a).

28. The first textbook devoted specifically to the new quantum mechanics was George Birtwistle's *The New Quantum Mechanics* (1928), which was followed the same year by Arthur Haas's *Materiewellen und Quantenmechanik* and Hermann Weyl's *Gruppentheorie und Quantenmechanik*. In 1929 Edward Condon and Philip Morse published *Quantum Mechanics*. Paul Dirac's influential and much used *The Principles of Quantum Mechanics* came out in 1930.

such kinds of series, one in which the students reported on an important new (or sometimes older) paper and another a series of research colloquia normally presented by visiting scientists. "The most exciting thing was the colloquia ... where all the foreign guests participated and we tried as students too. It was a difficult field at that time, quantum mechanics. I mean, it was so different from what we had learned in the first three years. ... We learned most by listening to these colloquia."[29] Strömgren, who attended many of the same seminars as Møller, recalled that he was present at "the colloquium where Heisenberg first presented the uncertainty principle."[30] Møller too might have listened to Heisenberg's talk, but since he never mentioned it, I doubt that he did.

In most cases, the topics for the students' colloquia were assigned by Bohr or Hansen. Møller gave a handful of presentations, of which the first was on Louis de Broglie's famous thesis of 1924 (*Recherches sur la Théorie des Quanta*) in which the French physicist introduced the radical idea of matter waves by associating the wavelength $\lambda = h/mv$ to particles of mass m moving with the speed v. This colloquium was followed by a second one on Pieter Zeeman's experiments of 1915 on the aberration of light in a dispersive medium.[31] Of greater interest are two later colloquia he gave on some of Paul Dirac's new and very important papers. Dirac stayed at Bohr's institute from September 1926 to February 1927 and at the end of his stay he completed his pioneering paper on radiation theory titled 'The Quantum Theory of the Emission and Absorption of Radiation'. At some time after it was published in the 1 March 1927 issue of the *Proceedings of the Royal Society*, Møller discussed it in one of the students' colloquia. It was not an easy task, but he succeeded in understanding and presenting Dirac's theory.

Paul Adrien Maurice Dirac, who in the fall of 1925 had developed his own version of quantum mechanics (q-number algebra), arrived

29. Weiner (1971a).
30. American Institute of Physics, interview of 5 May 1976 by Lillian Hoddeson and Gordon Baym. https://www.aip.org/history-programs/niels-bohr-library/oral-histories/5070-1. Heisenberg's unrecorded colloquium was probably in February 1927.
31. See Møller (1952), pp. 63-64, for Zeeman's experiment.

in Copenhagen at the same time that Møller began his graduate studies. Møller and his fellow students were fascinated as well as mystified by the 24-year-old British quantum wizard:

> [Dirac] appeared as almost mysterious. I still remember the excitement with which we in those years looked into each new issue of Proc. Roy. Soc. to see if it would include a work of Dirac. Bohr said, this is what Dirac calls "to think hard." Often he sat alone in the innermost room of the library in a most uncomfortable position and was so absorbed in his thoughts that we hardly dared to creep into the room, afraid as we were to disturb him. He could spend a whole day in the same position, writing an entire article, slowly and without ever crossing anything out. Bohr told that when he once read one of his manuscripts and suggested some changes, Dirac refused as a matter of principle to change anything. According to Dirac, there was only one way in which things could be said, and that was the way he had formulated.[32]

Another and even more important of Dirac's papers, the landmark paper on the linear Dirac wave equation for an electron, was also introduced by Møller in one of the colloquia, probably in February 1928. As he recalled, "the paper had just arrived in the library. ... I think I was the first who talked about the Dirac theory of the electron."[33] Møller further recalled that Klein and the Japanese physicist Yoshio Nishina were present at the seminar and that they were very interested in the new theory. "I came to talk with Nishina about the Dirac equation. He had not studied this paper yet, and I told him about what I had got out of it. Soon after, he and Klein started to make the famous calculations on the Klein-Nishina formula. And there I also got a little job in checking the things." Møller's 'little

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32. Møller (1963a), pp. 59-60. On Dirac's stay in Copenhagen 1926-1927, see also Kragh (1990), pp. 37-43, pp. 120-124, and Farmelo (2009), pp. 107-120.

33. Kuhn (1963) and similarly in Weiner (1971a). The paper was published in *Proceedings* on 1 February 1928. Although Møller may have been the first to report in public on Dirac's theory, it was known to some of the Copenhagen physicists. Dirac had sent a copy of his manuscript to Bohr before publication, and Bohr had handed it over to Klein for closer study. On Bohr's request, Klein went to Cambridge in early 1928 to learn more about Dirac's theory. See Kragh (1990), p. 62, and Klein (1973).

job' was to go critically through the complicated equations in the Klein-Nishina paper submitted in October 1928. As he admitted to Kuhn, the little job was in fact a hard one "because the calculations were much more involved than they became later."[34]

Møller also obtained first-hand knowledge of Compton scattering based on the Dirac equation, something he benefitted from in his later work, by checking the draft of a follow-up paper on this subject by Nishina. In this paper, the last that Nishina wrote while in Copenhagen, Møller found a non-trivial error in the calculation of the Compton formula. In the first reference to Møller ever in the scientific literature, Nishina acknowledged "Mr. Chr. Møller, who kindly has gone through the draft and made me aware of some necessary corrections in the calculations."[35] Concerning the Klein-Nishina theory and Nishina's subsequent paper, Klein recalled: "We sent a letter to *Nature* about it, and then we sent the paper. He [Nishina] went back to Copenhagen then, and there he attacked the question of polarization, and that he did quite alone. I was very busy there, so I never read the paper in detail, but Moller helped me to correct it, so he read everything in it."[36]

In November 1927 the Science Faculty of Copenhagen University announced a prize competition on 'The Analogy Between Mechanics and Optics'. This kind of annual competition went back to 1762, nearly a century before the Science Faculty was established, and had on many occasions served as an entrance ticket for bright students into the academic world. For example, in 1854 the engineer-trained physicist Ludvig Lorenz, who later did important work on electrodynamics, was awarded the gold medal for his essay on the geometrical properties of Fresnel surfaces.[37] The topic of the 1927

34. Kuhn (1963). Klein and Nishina (1929). Nishina stayed at Bohr's institute from April 1923 to late October 1928, only interrupted by shorter stays in Göttingen and Hamburg.

35. Nishina (1929a), p. 877, submitted 30 October 1928 and published 9 January 1929. Nishina similarly referred to Møller's intervention in Nishina (1929b), a note in *Nature* of 9 March.

36. Interview of 28 February 1963 by J. L. Heilbron and L. Rosenfeld. https://www.aip.org/history-programs/niels-bohr-library/oral-histories/4866.

37. Kragh (2018), p. 32.

prize problem appealed to Møller, who decided to write an essay on it. Two years later his efforts were crowned with a gold medal for an extensive essay in Danish with the full title 'A Summary Account of the Analogy between Mechanics and Optics Concerning the Analogy's Significance for the Historical Development of these Sciences as well as the Latest Progress in Atomic Theory'.

The idea of reconstructing mechanics in analogy with a generalised theory of geometrical optics was originally proposed by the Irish physicist and mathematician William Rowan Hamilton in 1833 and later developed by the German mathematician Felix Klein.[38] Of more importance to Møller, in his first communication on wave mechanics in *Annalen der Physik* of March 1926 Schrödinger had derived his quantum wave equation from considerations based on Hamilton's optical-mechanical analogy. Also de Broglie, in his earlier work, had made extensive use of the formal connection between mechanics and ray optics. It was primarily with an eye on quantum mechanics, and especially on Schrödinger's wave mechanics, that Møller entered the competition. In an interview of 1971, he said about the prize essay:

> I studied the old papers of Hamilton and then tried to describe the use of this analogy between mechanics and optics, to lead over to the Schrodinger equation. It was more an historical thing, although I contributed a little myself and extended a little the analogy to the case of anisotropic bodies. ... And I invented a small device to make the treatment of the anisotropic case on the same footing as the isotropic case, by introducing a special metric in the space, which is not the Riemannian metric, but which applies in the case of homogeneous bodies. But a metric which I later found out is called Finsler, the Finsler geometry.[39]

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38. Yourgrau and Mandelstam (1955), pp. 58-64. For a detailed account of the history of the optical-mechanical analogy and Schrödinger's use of it, see Joas and Lehner (2009).

39. Weiner (1971a). Contrary to Riemannian geometry, in Finsler geometry the length of a vector is not restricted to the square root of a quadratic form. This kind of geometry, which has been applied to studies of gravity and elementary particles, was introduced by the German mathematician Paul Finsler in his Göttingen dissertation of 1918.

The prize evaluation committee consisting of Bohr, Klein, and Hansen concluded that Møller's essay was of sufficiently high standard to merit a gold medal, which was awarded him in 1929. They found that the essay demonstrated "a deep knowledge [and] very considerable gifts for independently familiarising himself with a scientific subject which is not at all easily accessible."[40] Unfortunately, Møller's gold medal essay remained unpublished and today it exists only in a single copy kept at the Royal Library in Copenhagen. In the first part, he concluded that "the theory of light quanta leads by necessity ... to the 'complementarity' between momentum-energy and time-space coordinates which Bohr has emphasised so strongly." Later in his essay he commented on Dirac's wave equation: "Given its well-known difficulties with transitions where the electron changes its sign of mass or charge it is most likely only an approximation to the real equation."

Møller took his work with the prize essay very seriously and was particularly interested in meeting Schrödinger and discussing with him the optical-mechanical analogy. In the summer of 1927, Schrödinger had moved from Zurich to Berlin to occupy the chair in theoretical physics at the Friedrich Wilhelm University vacant after Max Planck's retirement (and after Einstein and Sommerfeld had declined the offer). A year later the university in Berlin held a summer semester course in theoretical physics, which offered a golden opportunity for Møller to meet Schrödinger and others of the prominent Berlin physicists. As a Danish student without the required study certificate Møller might not have been admitted to the summer school course, but it turned out that Bohr's recommendation was enough to solve the problem. "Mr. Christian Møller, who studies at this institute, would like to attend the semester course this summer on theoretical physics in Berlin to which you contribute. ...

40. *Festskrift Udgivet af Københavns Universitet 1929* (Copenhagen, 1929), p. 148. Møller's only competitor, Mogens Pihl, received honourable mention (*proxime accessit*). In 1935 Pihl won the University gold medal on a different topic, the interpretation of thermodynamics in terms of quantum mechanics.

Although he has not yet passed his final exam, he is very thoroughly trained in modern theoretical physics", Bohr assured Schrödinger.[41]

Thus, when Møller arrived in Berlin, he was received by the founder of wave mechanics who invited him to dinner in order to get informed about the latest news from Copenhagen. Bohr had recently discussed his principle of complementarity in an article in *Die Naturwissenschaften* which Schrödinger had studied without in any way accepting it. What this principle is, more exactly, is still a matter of debate, but roughly speaking it claims that a full knowledge of phenomena on the quantum level requires concepts that are complementary and mutually exclusive, as in the case of describing an electron as both a wave and a localised particle. Considering complementarity to be a rhetorical device rather than scientific principle, Schrödinger objected to Møller that "Bohr will alle Schwierigkeiten wegkomplimentieren" (Bohr will complement away all difficulties).[42]

Møller recalled that he tried in vain to express to a more than sceptical Schrödinger what Bohr meant by the term 'symbolic', one of Bohr's favourite expressions. Himself uncertain about the meaning, he asked Bohr for help: "The question at stake is what one really understands under the word 'symbolic' – what does it mean that, e.g., the representation of a free particle by means of de Broglie waves is only a symbolic representation and that the analogy between mechanics and optics is just purely formal? I have always had a rough feeling of what it meant; but when I had to express it in words, I ran into difficulties." Møller elaborated:

> We must operate with a particle concept which requires for its definition only a precise determination of at most four quantities. As shown in the professor's article, de Broglie's wave packet has this property when we explicitly use the 'quantum relations' $E\tau = I\lambda =$. Thus, one can say that

41. Bohr to Schrödinger, 26 April 1928, in Schrödinger (2011), p. 453. See also Schrödinger to Bohr, 5 May 1928, and Bohr to Schrödinger, 23 May 1928, in Bohr (1985), pp. 463-467.

42. Møller (1963a), p. 61. The paper in question was Bohr (1928) published in the 13 April issue of *Die Naturwissenschaften*.

> the description of a particle by means of de Broglie waves is a purely mathematical and perhaps elegant expression for a particle's kinematic features as they must be according to the quantum postulate. ... Likewise one can say that Schrödinger's treatment of the interaction problem is a purely mathematical method to describe the kinematic features of an electron in an atom. Presumably, this is the meaning of saying that Schrödinger's analogy between mechanics and optics is purely formal. So, this method admittedly offers a description of facts that occur in nature, but it is unable to base the postulate on the conceptions of classical physics.[43]

A few days later, Bohr replied at length, but not perhaps as clearly as Møller had hoped for. "I am naturally in complete agreement with you that every description of natural phenomena must be based on symbols", Bohr wrote, and then went on:

> To use the word 'symbolic' for non-commutative algebra is a way of speaking that goes back long before quantum theory, and which has entered into standard mathematical terminology. When one thinks about the wave theory, it is, however, precisely its 'visualisability' [*anskuelighed*, German: *Anschaulichkeit*] which is simultaneously its strength and its snare, and here by emphasising the approach's [*behandlingens*] symbolic character, I was trying to bring to mind the differences — required by the quantum postulate — from classical theories, which are hardly ever sufficiently heeded.[44]

As Bohr explained in his letter to Møller, he did not believe that quantum mechanics admits of a realist interpretation in terms of classical models. Although Møller appreciated his contact with Schrödinger and other of the physicists in Berlin, he was much disappointed not being able to meet or listen to the legendary Einstein, who had announced a lecture on the causality principle. However, in February 1928 Einstein had developed a serious heart condition, which put him to bed for four months, and he had not yet fully recovered. Consequently, he was forced to cancel his participation

43. Møller to Bohr, 10 June 1928 (BSC).
44. Bohr to Møller, 14 June 1928 (BSC).

in the Berlin summer school. Among the physicists that Møller did meet in Berlin were also a few Danes participating in the course. One of them happened to be E. S. Johansen, his former teacher during his undergraduate training at the Polytechnic College.

Rather than going straight back to Denmark, Møller extended his stay in Germany by a visit to Göttingen. On Schrödinger's advice he studied in the university library Felix Klein's handwritten lecture notes of 1890 on the analogy between optics and mechanics, which he would make use of in his prize competition essay. Göttingen was the cradle of quantum mechanics and its professor of theoretical physics was the famous Max Born, to whom Møller was introduced. By chance he also met a young physicist with whom he would later collaborate and have close relations with. Since he was not formally introduced to the young man, he did not get his name and only later did he realise that he had run across Léon Rosenfeld, a 24-year-old Belgian theorist who studied under Born. Rosenfeld had wanted to go to Copenhagen but then decided to stay in Göttingen for a while. He only met Bohr in person at the 1929 Easter conference, and about a year later he became one of Bohr's closest and most important collaborators. Another scientist that Møller happened to meet in Göttingen was the Austrian mathematician Otto Neugebauer, who at the time was in charge of the mathematics library and in 1924 had visited Harald Bohr in Copenhagen. At the time he guided Møller around in the Göttingen library, Neugebauer had begun his pioneering research in ancient mathematics and astronomy. Møller came to know him well during the years 1934-1939 when he, as a refugee from Nazi Germany, was employed at the Mathematics Institute in Copenhagen.

Møller described his study of Klein's unpublished Göttingen notes in a letter to Bohr, where he called attention to its relevance for the current formalism of quantum mechanics:

> It is a very interesting work in which he [Felix Klein] … shows that Jacobi's method for integrating the mechanical equations of motion completely corresponds to Huygens' principle in geometrical optics. From this point of view one can regard Dirac's general transformation theory as a generalisation of Jacobi's method since Dirac's method of

> 'integration of the quantum-mechanical equations of motion' will correspond completely to Kirchhoff's principle in optics.

Moreover, Møller pointed out that the idea of complementarity would be part of his forthcoming gold medal work: "In my essay on the analogy between mechanics and optics I hope to demonstrate the necessity of the complementarity principle in quantum theory, such as it clearly appears by the role of the analogy in physics since the days of Newton. The analogy can no longer be taken as support for a 'wave conception' alone, such as has been the case in all works which after Felix Klein have been concerned with the analogy."[45] After having left Berlin on 22 July, he went to his parents' home on Als, where he spent a month combining vacation with work.

When Møller returned to Copenhagen in early September 1928, two new visitors of his age had arrived at the institute. They would both prove important to his early career in physics. One of them was Nevill Mott, a 22-year-old British physicist who worked on the wave mechanics of tracks of alpha particles and also on the consequences of Dirac's new theory of the electron. Mott's closest friend in Copenhagen was George Gamow, the first visitor to the institute from Soviet Russia. Gamow had just submitted a landmark paper explaining alpha radioactivity as a quantum-mechanical tunnelling phenomenon which he had completed during a stay in Göttingen.[46] He arrived in Copenhagen on 22 August 1928 for what he planned to be a brief visit, but thanks to a stipend that Bohr arranged for him he stayed until early 1929.

Gamow, colourful and thoroughly unconventional, made quite an impression on the Copenhageners. In a letter to his mother, Mott wrote: "Though he is a Russian, one wouldn't think it. He ... goes to the cinema rather often, and would love a motor cycle if he had one. And he reads Conan Doyle and doesn't go to concerts, but is a brilliant physicist and hard-working, and gets his results without

45. Møller to Bohr, 17 July 1928 (BSC). While in Copenhagen in the winter of 1927, Dirac gave a seminar on his new transformation theory, most likely with Møller in the audience.

46. On Gamow's theory of alpha decay, see Stuewer (2018), pp. 96-102.

Fig. 4. The Copenhagen 1929 Easter conference. On the first row: N. Bohr, Harald Cramér (a Swedish mathematician), O. Klein, H. A. Kramers, C. Darwin, Ralph de Laer Kronig, P. Ehrenfest, and G. Gamow. On the second row from the right, S. Goudsmit, H. Casimir, C. Møller, and L. Rosenfeld. The second person from the left on the fourth row is Ebbe Rasmussen. Credit: Niels Bohr Archive, Photo Collection, Copenhagen.

using mathematics. And he very seldom stops talking and is about my height."[47] According to Møller's recollections: "We sometimes got the impression that he actually used all of his time on fun and practical jokes, and that the important memoirs he wrote at the time were merely a by-product."[48]

By 1929, the year in which he graduated as a magister in physics and published his first research paper, Møller had become a member of the informal yet tightly knit community of physicists at the Blegdamsvej institute. Bohr recognised him as a promising theorist and coming man in Danish theoretical physics. For example, he was among the few Danish physicists participating in a conference in early April 1929, the first of a memorable series of Copenhagen conferences.[49] Bohr had suggested a reunion conference in a letter

47. Mott (1986), p. 28, who in the letters to his mother extolled Bohr as a semi-god: "Only Bohr knows *everything* that's being done, and has a marvellous knack of finding the sense behind mathematics" (p. 27).

48. Møller (1963a), p. 62.

49. Stuewer (2018), pp. 106-109. Robertson (1979), pp. 136-137.

to Pascual Jordan of 3 March and the week-long meeting took place a month later. Among the participants were not only associates and students of the institute (such as Klein, Jacobsen, Rasmussen, Gamow, and Møller) but also foreign visitors including Pauli, Svein Rosseland, Hendrik Kramers, Paul Ehrenfest, Walter Heitler, and young Hendrik Casimir. As Klein reported in a letter to Nishina: "We have got a new very sympathetic and clever man to the laboratory, Casimir, a Dutch pupil of Ehrenfest. He is only 20 years but knows already a lot of physics."[50]

Some of the participants had been at the institute at previous occasions, while for others it was their first experience. Thus, it was the first time that Rosenfeld – whom Møller had spotted in Göttingen the previous year without realising whom he was – came to Copenhagen. He arrived from Göttingen together with Heitler, and the meeting with Bohr made a deep impression on him:

> What impressed me most about Bohr at this first meeting, was the benevolent radiation from his whole being. There was a paternal air about him ... I don't know how the Athenian delegates for oracle consultation felt on their return from Delos; there are, so far as I know, no autentic [*sic*] records about that. But I imagine their feelings must have been akin to mine after I had listened to Bohr's introductory lecture at the conference.[51]

The successful first conference at the institute ended with a tour of Copenhagen and a visit to Bohr's country house in Tisvilde on the northern coast of Zealand.[52]

Møller also participated in the next Easter conference in 1930, which took place in the first week of April and where one of the new attendees was another young Russian physicist by the name Lev

50. Klein to Nishina, 20 July 1929, in Nishina (1984), p. 9.
51. Rosenfeld, 'My initiation (paraphysical recollections)'. *Journal of Jocular Physics* **2** (1945). On this informal 'journal', see Section 5.4. An English translation not quite following the original is given in Rosenfeld (1979), pp. xxxi-xxxiv.
52. See the vivid description by Bohr's secretary Betty Schultz in her letter to Nishina of 30 April 1929 reprinted in Nishina (1984), pp. 6-8.

Landau. At this conference Møller discussed, among other matters, a new and strange idea of a discontinuous space or 'lattice world' (*Gitterwelt*) that Heisenberg had recently proposed. "There was a paper by Heisenberg in which he tried to avoid the infinities [in quantum electrodynamics] by introducing a ... space-time lattice, but this of course made difficulties with the relativistic invariance."[53] Some of Heisenberg's ideas about a lattice world would later reappear in his theory of the fundamental *S*-matrix, which will be discussed in Section 5.3.

In May 1929, Møller accompanied Bohr on a trip to Cambridge, presumably his first visit to England, and the same year he was trusted to undertake a translation into German of an important lecture that Bohr delivered at the 18th Scandinavian Meeting of Natural Scientists taking place in Copenhagen. 'The Atomic Theory and the Description of Nature', as the title of the lecture was, soon became a key document of the Copenhagen complementarity philosophy and went through many reprints.[54] When the German paper appeared, Møller discovered, probably to his dismay, that Bohr had changed his translation almost completely.

It was Møller's job sometimes to go through copies of manuscripts sent by mail and fill in the formulae that were missing in the typed manuscript (at the time typewriters could not produce mathematical symbols, which consequently were inserted by hand). "I had the tedious job of filling in formulae in a copy of this paper by Heisenberg. Pauli came on a visit here and I asked him if I could read it and he said, 'Well you can fill in the formulae. In this way you will also be able to read it.' So I did that."[55] Møller possibly referred to two long and complicated papers by Heisenberg and Pauli in which they presented an improved theory of quantum electrodynamics that was relativistically invariant and included quantisation

53. Kuhn (1963). See also Pauli (1985), pp. 9-10. Actually, Heisenberg never published his paper, which only circulated in manuscript form. On his idea of a lattice world and its fate, see Carazza and Kragh (1995) and Hagar (2014), pp. 69-72.
54. The German translation appeared as Bohr (1930). The address was originally published in *Fysisk Tidsskrift* in 1929. See Bohr (1985), pp. 219-253.
55. Kuhn (1963).

of both matter and radiation.[56] He found the papers to be "difficult, very difficult to read." Few readers at the time would disagree and so will few modern readers.

As mentioned, Møller graduated as a magister (mag. scient.) in 1929. To be awarded the magister degree, he had to give a public lecture, and he chose to speak of Gamow's theory of radioactivity, a subject which interested him and on which he was preparing a scientific paper (see the next section). Unemployed academics were even more common at the time than they are today – it was at the beginning of the economic depression – and for a time Møller was worried that he might not be able to continue at Bohr's institute. He was in lack of money and needed a job. "I was quite prepared that I would have to go out to the gymnasium as a teacher. But fortunately, I got hold of something which became rather interesting, and Bohr was interested in that ... [and] rather quickly Bohr was able to supply a little more money for me, and he asked me to start to give some lectures on relativity."[57] These lectures covered both the special theory of relativity and elements of the general theory.

In 1931 Møller was appointed scientific assistant at the institute, where he took over the lecture courses previously given by Oskar Klein, who in January moved to Stockholm to become professor in mechanics and mathematical physics at the Royal Technical University. After the death of the mathematician Ivar Fredholm in 1927, the chair was vacant and with strong support from Bohr and Arnold Sommerfeld, Klein was eventually offered the position. In a letter to Nishina of 22 May 1931, Bohr wrote: "As you will know, his [Klein's] departure is a great loss to us, although in Møller, who is developing very promising indeed, we hope to find a good successor to his post."[58] Two years later, after having obtained his doctoral degree, Møller finally got a position as university lecturer (*lektor* in

56. The papers, one published in 1929 and the other in 1930, appeared in *Zeitschrift für Physik* with the same title, 'Zur Quantendynamik der Wellenfelder'. See Enz (2002), pp. 186-193.
57. Weiner (1971a). With "something which became rather interesting" Møller referred to his electron-electron scattering theory.
58. Bohr to Nishina (BSC), reprinted in Nishina (1984), pp. 18-19.

Danish, corresponding to an associate professor), if only a temporary one.[59] With the economic situation improved, on 20 June 1931 he married in the beautiful Søllerød Church north of Copenhagen the three years older Ella Kirsten Johanne Pedersen. Kirsten, as she was usually called, survived her husband with four years. The couple got two children, the boy Ole and the girl Mette. Christian Møller had completed his apprenticeship, and more than that.

The lectureship was not a permanent position as it had to be renewed each year and thus depended on the decision of the Ministry of Education. Eager to keep Møller at the institute, in early 1936 Bohr requested the Faculty of Mathematics and Science to transform the lectureship into a new readership (*docentur*) in mathematical physics, a permanent position associated with its holder and therefore 'extraordinary'. The faculty and the governing body of the university, the Academic Council, approved the request. However, since a new regular position required extra funds, it had to be on the annual state Budget for 1937-1938, and the Ministry declined the application. Bohr repeated his proposal over the next few years, in 1939 arguing for an extraordinary professorship rather than a readership. Only in 1940 did the government allocate the necessary funds for Møller's readership, which officially took its start on 1 April this year. In his carefully worded argument for the new position, Bohr praised Møller's unique qualifications:

> Not only does he possess quite an extraordinary knowledge of modern mathematical physics, he has also contributed to the development of the methods of theoretical atomic physics in a way which has won great recognition. Lecturer Møller is undoubtedly an eminent representative of the younger generation in the area of theoretical atomic physics. I shall not avoid adding that for many years ahead there will not be any other young Danish scientist whose qualifications to fill the readership will be comparable to his.[60]

59. *Aarbog for Københavns Univetsitet 1932-1933*, p. 16.
60. *Aarbog for Københavns Universitet 1939-1940*, p. 63.

Bohr did not give up his attempt to upgrade Møller from a reader (*docent*) to a professor. In May 1942 he repeated his application of 1939, and this time it was accepted. With the funds allocated by the government in its Budget for 1943-1944, Møller was appointed full professor in mathematical physics on 1 April 1943. The term 'mathematical physics' refers generally to the use of advanced mathematical methods to problems of physics, but at the time it was largely equivalent to 'theoretical physics' or what in British and American universities was often called 'applied mathematics'. Møller was a physicist using mathematics, not a mathematician applying his art to physics.

Like the previous readership, Møller's professorship in mathematical physics was 'extraordinary' in the sense that it was new and restricted to him personally. When he retired at the end of 1974, the professorship ceased. After retirement, Møller no longer had teaching obligations or membership of the Academic Council, nor could he draw of university funds for his travels. In other respects, his retirement did not mean much as he continued his work at the institute for theoretical physics and the Nordita institute, which since 1958 had been associated with it (Section 8.3).

Now back to the late 1920s, when Møller was still a graduate student. As mentioned, in his letter to Bohr from the summer of 1928 Møller promised to include in his gold medal essay on the analogy between mechanics and optics some arguments associating it with the complementarity interpretation of quantum mechanics. Indeed, the completed essay contained a discussion of the complementarity principle and a historical reconstruction of the development of the optical-mechanical analogy in light of Bohr's ideas. Bohr was at the time deeply engaged in developing his new principle of complementarity, which formed the background of many of the free-wheeling discussions at the institute and to which Møller could not avoid being exposed. However, although he was thoroughly familiar with the arguments based on the complementarity principle, he was not particularly interested in them and they left almost no trace in his published research works. As he recalled in the interview with Kuhn:

> [Bohr] started to speak about complementarity and all the discussions of Gedanken experiments, that was always very exciting. ... Although we listened to hundreds and hundreds of talks about these things [complementarity and the measurement problem], and we were interested in it, I don't think, except Rosenfeld perhaps, that any of us were spending so much time with this thing. I mean, after all, when Bohr is there, it is very hard to do something better. And also when you are young it is more interesting to attack definite problems – I mean this was so general, nearly philosophical. ... Also of course it was difficult with this complementarity. One could very easily go wrong, and it required Bohr to straighten things out.[61]

Contrary to his colleague Rosenfeld, the 'nearly philosophical' was never a matter of much concern to Møller, who only wrote about the interpretation of quantum mechanics at a few occasions and then restricted to a Danish audience of general readers.[62] The pragmatic attitude and reluctance to deal seriously with complementarity does not mean that Møller was unaffected by the Copenhagen spirit, only that complementarity arguments played almost no role. Other components of the Copenhagen philosophy, and the use of the correspondence principle in particular, were of direct importance to Møller's work and approach to physics. Personally, Møller was very much under the spell of Bohr and strongly indebted to his master.[63] Although his research interests and style of physics differed substantially from Bohr's, he remained throughout his life a loyal and devoted disciple.

And yet, loyal and devoted as he was, 'disciple' may not be the right word. Møller was never one of 'Bohr's boys' and his research was remarkably independent of what Bohr and his closest collaborators implicitly defined as Copenhagen mainstream research. In an

61. Kuhn (1963).

62. See, for example, Møller (1944), a lucid exposition of the epistemological problems in quantum mechanics in full agreement with the ideas of Bohr and Heisenberg. Another example is Møller (1977b), which dealt in part with the philosophical problems related to modern physics. See also Section 8.1.

63. Møller to Bohr, 10 June 1928, and 3 April 1943 (BSC).

interview of 1970, he briefly reflected on his long-time relationship to Bohr:

> I have assisted Bohr on some occasions, but our collaboration was never as close as it was for other people. Most likely, the reason was that Niels Bohr and I were rather different. It required a particular mentality to work with him. One had to shelve more or less one's own ideas and just listen to him. And this was not really my cup of tea. But of course, I admired Bohr very much. He is the greatest I have ever met, not only as a scientist but also as a human being.[64]

It seems that Møller, in so far that he ascribed to a philosophy of physics, was closer to positivism and instrumentalism than was his master Bohr. Thus, it was hardly a coincidence that he chose a sentence paraphrased from Ernst Mach's famous *Die Mechanik in Ihrer Entwicklung Historisch-Kritisch Dargestellt* as the motto for his 1929 gold medal essay: 'Nicht die 'Erklärung', sondern die Beschreibung ist die Aufgabe der physikalischen Theorie' (Description and not 'explanation' is the task of the physical theory).[65] The attitude expressed in these words did not differ from that of many other pragmatically inclined young quantum physicists (such as Hans Bethe, John Slater, and Enrico Fermi), who saw no advantage of mixing physics with the allegedly more obscure complementarity philosophy. Møller never specified his own view, but he may have continued to favour a kind of positivism à la Mach without ever rejecting the central messages of what came to be known as the Copenhagen philosophy of quantum physics. The Danish philosopher David Favrholdt had in the 1950s several conversations with Bohr and some of his collaborators, including Møller. "I discussed a good deal with him", Favrholdt recalled, "and it turned out that he was an adherent of Ernst Mach's so-called neutral monism."[66]

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64. *Jydske Tidende*, 15 March 1970.
65. *Festskrift Udgivet af Københavns Universitet 1929* (Copenhagen, 1929), p. 145.
66. Favrholdt (2009), p. 134. Neutral monism in Mach's sense argues that there is no way to distinguish the mental and the physical; the fundamental constituents of the world are neither mental nor physical.

He thought that Møller was "a great physicist, but philosophically he was on quite a wrong track."

1.3. First scientific papers

Rather than turning his gold medal essay on the optical-mechanical analogy into a publication, which he regrettably never did, Møller decided to enter the physics literature with works that were more in line with frontier research in theoretical quantum physics. This he did by contributing in 1929-1930 with four "not very exciting" papers, three of which appeared in the prestigious *Zeitschrift für Physik*, the chief journal for the new generation of quantum physicists.[67] At the time, one of the hot topics was the application of quantum mechanics to nuclear phenomena, a topic that was cultivated at Bohr's institute by Gamow and Mott in particular.

It was also a topic in which Bohr was much interested, but his concern was of a more general nature and closely related to the severe anomalies that turned up when applying quantum mechanics to the atomic nucleus, which at the time was universally believed to consist of tightly bound protons and electrons. Bohr and several other physicists thought for a while that anomalies such as the continuous beta decay spectrum and the observed spin state of the carbon-14 nucleus, being one and not one-half, indicated the need for a profound revision of physics.[68]

According to Bohr, the strong medicine needed might be to abandon strict energy conservation in the nuclear regime, a radical idea he first contemplated at the end of 1928 and which he somewhat obscurely related to the complementarity principle. Less than a year later, Ernest Rutherford wrote to Bohr: "I have heard rumours that

67. Weiner (1971a). Founded in 1920 by the German Physical Society, *Zeitschrift* was planned to be limited to three annual volumes with no more than 1,440 pages in total. By 1929 it had expanded to seven volumes with a total of 6,094 pages. Most of the important papers in quantum mechanics from 1925 to about 1933 appeared in this journal, many written by non-Germans. Since 1926, *Zeitschrift* accepted only papers written in German language. Kragh (1999), pp. 150-151.
68. For the crisis and Bohr's perception of it, see Darrigol (1988). See also Kragh (2017a) and the literature cited therein.

Fig. 5. C.V. Raman and wife at a visit to Bohr's institute in 1930, shortly after having received the Nobel Prize in Stockholm. Niels Bohr had nominated him for the prize in 1929 and again in 1930. From left to right: O. Klein, N. Bohr, C. V. Raman, M. Bohr, G. Gamow, C. Møller, R. Adler (Bohr's maternal aunt), L. Raman, J. C. G. Jacobsen. Credit: Niels Bohr Archive, Photo Collection, Copenhagen.

you are on the war path and wanting to upset the Conservation of energy, both microscopically and macroscopically. I will wait and see before expressing an opinion."[69] Bohr was indeed questioning the fundamental principles of energy and momentum conservation not only in some nuclear processes but also in the interior of the stars. Without giving any details, he thought that the enigma of stellar energy generation might be solved in this way. Most likely, during the 1929 Easter conference Møller and the other participants listened to and critically discussed Bohr's heretical views, although we do not know for sure or to what extent. Unfortunately, there is no record of the conference.

Instead of engaging in Bohr's speculations, Møller chose a definite problem of his own that he could analyse in mathematical

69. Rutherford to Bohr, 19 November 1929 (BSC).

detail, namely a generalisation of Gamow's new theory of alpha radioactivity. In a classical paper that appeared in *Zeitschrift für Physik* in mid-October 1928, Gamow used the equations of wave mechanics to explain how an alpha particle pre-existing in the nucleus could penetrate the potential barrier and escape with an energy smaller than the maximum height of the potential.[70] He found that the penetration or 'tunnelling' probability was given by an exponential expression from which he derived the characteristic decay constant λ related to the half-life by $T_{½} = \ln 2/\lambda$. Moreover, from the theoretical decay constant he derived the so-called Geiger-Nuttall law formulated by Hans Geiger and John Nuttall in 1912. According to this empirical law there is a linear relationship between the logarithm of the decay constant and the energy of the emitted particles, namely

$$\log\lambda = a + b \log R$$

where R is the range in air and a and b are constants. Unknown to Gamow, the two American physicists Ronald Gurney and Edward Condon, at Princeton University's Palmer Physical Laboratory, came simultaneously and independently to the same results. Their paper in *Nature* actually appeared about a month before Gamow's longer and more detailed paper. The Gurney-Condon theory was almost identical to Gamow's except that the two Americans concluded, contrary to Gamow and wrongly as it turned out, that the theory did not apply to the inverse problem of an alpha particle entering the nucleus.[71]

From conversations with Gamow in Copenhagen, Møller knew about his theory before it appeared in print. While this theory, and also the corresponding one of Gurney and Condon, was nonrelativistic and based on the ordinary Schrödinger equation, Møller's aim was to generalise it to the relativistic domain. In this work he was assisted not only by discussions with Gamow, but also with Klein and Bohr. His paper, the first of many, was submitted in late April

70. Gamow (1928).

71. Gamow's paper was dated 29 July 1928, the one of Gurney and Condon 30 July. See Stuewer (1986) and Stuewer (2018), pp. 96-105, for a detailed historical account of the two theories.

1929 – just a week before the Easter conference – and it appeared in the 20 June issue of *Zeitschrift für Physik*. At the time relativistic quantum mechanics meant either the new linear Dirac theory or the older Klein-Gordon theory from 1926. According to the latter theory, a free particle was governed by a quantum equation of the form

$$-\frac{1}{c^2}\frac{\partial^2\psi}{\partial t^2}+\nabla^2\psi=\left(\frac{mc}{\hbar}\right)^2\psi$$

where $\hbar = h/2\pi$. In Dirac's theory the differential operators were of the first order and the wave function ψ consisted of four components of which two formally corresponded to states of negative energy. As Møller pointed out, the alpha particle had presumably zero spin and for this reason might not be described by Dirac's equation valid for electrons. He consequently carried through his elaborate calculations first on the basis of the Dirac equation and subsequently changed to the Klein-Gordon equation. In both cases he assumed for simplicity the nuclear potential barrier to be rectangular with a height U.

In Gamow's formula for the decay constant there appeared an exponential function with the term $\sqrt{2m(U-E)}$ in the exponent, where E is the energy of the emitted alpha particle. According to Møller's relativistic version the term was slightly different, namely given by

$$\sqrt{2m(U-E)-c^{-2}(U-E)^2}$$

and with a factor that differed slightly according to which of the two relativistic wave equations was applied. "For the problem of radioactivity it does not matter which of the two theories one uses, since the deviations ...turn out to be [empirically] meaningless."[72] In both cases, Møller's more complicated formulae for the decay constant reduced to Gamow's in the limit of a vanishing velocity of light. About two years later Sisirendu Gupta, a physicist at the University College of Science in Calcutta, improved on Møller's

72. Møller (1929), p. 452. The only references in the paper were to works by Gamow, Klein, Dirac, and the Hungarian physicist Johann Kudar. Strangely, he did not refer to the work of Gurney and Condon.

calculations by using a Coulomb nuclear potential and a different method of computation.[73]

The relativistic corrections derived by Møller were small and unmeasurable, hence of no interest to the experimenters. But this was of little relevance to Møller, whose debut paper, filled with elaborate formulae rather than words, was primarily an exercise in advanced mathematical physics. It proved to his peers that he fully mastered the new relativistic quantum mechanics and its associated mathematical machinery. Further proof followed in the next three papers published in 1930, which all related to scattering theory, another field of concern to the Copenhagen physicists.

While experiments played no role in Møller's paper of 1929, in his two next papers he faced a well-known experimental anomaly related to the structure of the atomic nucleus. As Gregor Wentzel proved in 1926, Ernest Rutherford's famous scattering formula for alpha particles originally derived on a purely classical basis in 1911 could be deduced also from the new quantum mechanics. However, new and more delicate experiments made in 1925 by Rutherford and James Chadwick at the Cavendish Laboratory showed puzzling deviations from the formula for light elements.[74] The anomaly attracted much attention and Møller was only one out of a dozen physicists or so who entered the problem.

Møller considered a model of the nuclear field in which the potential followed Coulomb's law outside the nucleus and attained a constant value within it. With a model of essentially this kind he calculated the nuclear radius and the height of the potential barrier by using the fundamental quantum-mechanical scattering theory that Born had developed in two important papers of 1926. It was in these papers that Born suggested the probability interpretation of the wave function ψ, which can be expanded in terms of the proper functions of the Schrödinger equation, meaning that $\psi = \sum c_n \psi_n$. According to Born, the quantity $|c_n|^2$ denotes the probability that the system is in the state given by ψ_n. "We have a

73. Gupta (1931).

74. Wentzel (1926). Rutherford and Chadwick (1926).

general method from the experimental data to determine the potential in the nucleus with any desired accuracy", Møller wrote in one of the papers. "The performance of this work demands, however, more accurate measurements than are yet at hand."[75] Comparing his revised scattering formula with experimental data Møller found a reasonable but only qualitative agreement. Like other physicists analysing the anomaly, he used Born's general scattering theory only in its first approximation. "It remains to be proved that the higher approximations of Born's method can be neglected", he wrote. "I hope to return to this point in a later paper."[76]

And this he did. In another paper in *Zeitschrift*, predominantly of a mathematical nature, Møller worked out the second order approximation for alpha particles explicitly and also considered the elastic scattering of electrons from neutral atoms.[77] He concluded that Born's method in its first approximation was unsuitable for alpha particles, whereas it described correctly electron-atom scattering if a certain condition was satisfied. The condition was that

$$\pi Z e^2 / h v \ll 1$$

where v is the velocity of the electron and Z the atomic number of the target atom. Møller pointed out that in the case of helium the condition was satisfied only for relatively high electron velocities, whereas it was invalid for small velocities and large atomic numbers. His conclusion may have stimulated experiments at Bohr's institute, where Sven Werner investigated electron scattering in helium at the energy range 40-300 eV. Werner, who presumably discussed his work with Møller, found deviations from Mott's scattering theory at low energies, which "is to be expected from the remarks that Møller makes at the end of his work."[78] At the time Møller submitted his

75. Møller (1930a). Issue of 22 March, dated 21 February
76. Møller (1930b). Issue of 12 May, received 15 March.
77. Møller (1930c). Issue of 17 December, received 24 October. As shown by a reference in Møller's paper, he was at the time familiar with Dirac's famous textbook *Principles of Quantum Mechanics* which Oxford University Press announced for sale in the late summer of 1930.
78. Werner (1931), p. 209.

paper on the Born approximation he had begun contemplating a more original and ambitious work that would qualify as his doctoral thesis.

Fig. 6. Møller drinks a toast with Bohr in 1940, possibly on the occasion of Møller's appointment as reader at the University of Copenhagen. The person in between is Hans Henrik Bohr, the second son of Niels and Margrethe Bohr. Credit: Niels Bohr Archive, Photo Collection, Copenhagen.

CHAPTER 2

Møller scattering

Coinciding with the one hundredth anniversary of Goethe's death, the most famous of the many conferences at Bohr's institute took part in early April 1932. It featured a memorable performance of a humorous play known as the 'Copenhagen Faust' or sometimes 'Blegdamsvejens Faust'.[1] Among the participants, apart from Bohr, were well-known physicists such as Heisenberg, Pauli, Kramers, Ehrenfest, and Dirac. Also Møller's friends Rosenfeld, Strömgren, and Piet Hein participated. Although Møller does not figure on the iconic photograph taken in the auditorium and also does not appear in the Faust parody, he did take part in the conference. He recalled that he told about his forthcoming stopping theory at the meeting and that he was "rather intimidated by seeing Pauli and Ehrenfest on the first row."[2] However, he kept a low profile and may not have participated in the entire conference. If so, it was for good reasons, for at the time he was intensely occupied with preparing a lengthy and difficult article that at the end of the year would earn him a doctorate and turn him into a full-blown theoretical physicist.

To pursue a career in Danish science and eventually become a professor, Møller needed a doctoral degree, a dr. phil. degree of the same kind that Bohr had obtained in 1911 with his thesis on the electron theory of metals. However, Møller had the advantage that the rules for obtaining a doctoral degree at the University of Copenhagen had been changed in the meantime. While Bohr was forced to write his dissertation in Danish – the only alternative was Latin – by a regulation of 1921 it was made possible that in special cases it could be written in a foreign language such as German, English, or French. Moreover, according to the old rules a work which had already been published did not qualify as a doctoral thesis or a part of it. Fortunately, this rule was abolished too, in this

1. Segré (2008). Halpern (2012).
2. Møller (1963a), p. 64.

case by a new regulation of 1927.[3] According to the yearbook of the University of Copenhagen for 1932: "On 28 November 1932 mag. scient. Christian Møller (magister degree in physics from September 1929) defended for the philosophical doctor degree his work 'Zur Theorie des Durchgangs schneller Elektronen durch Materie'. The official opponents were Professor, Dr Niels Bohr and Professor Dr Oskar B. Klein from Stockholm; there were no opponents among the audience. The degree was conferred on 9 December 1932."[4]

2.1. Stopping theories and electron-electron collisions

Møller's dissertation was judged only by Bohr and Klein, whereas usually there were three official opponents. The dissertation was also highly unusual by consisting of just a single paper published in a German journal supplemented by a survey in Danish. As a newspaper pointed out, it was probably the slimmest dissertation ever presented in the country.[5] Møller recalled: "Since I was not very wealthy and it was a rather expensive thing to write a thesis, Bohr managed to get this paper in the *Annalen der Physik* recognized as a thesis. I then only had to write a Danish survey of what was contained in this paper, and I did that, and these two things were coupled together and formed the thesis."[6] It was a hard and time-consuming work to write and revise the paper for *Annalen*, but by mid-July he had revised the second and last proof. After a well-deserved vacation, which he spent with his wife camping in Northern Zealand and in his family's house in Hundslev on Als, Møller returned to Copenhagen. As usual he mixed his holidays with studies, such as he indicated to Bohr: "I have read van der Waerden's excellent book *Die Gruppentheoretische Methode in der*

3. See Smith (1950) for the history of doctoral dissertations at the University of Copenhagen. The doctorates conferred in the natural sciences were traditionally dr. phil. (*doctor philosophiae*) or dr. med. (*doctor medicinae*) degrees. The title dr. scient. (*doctor scientiarum*) was only introduced in 1977.

4. *Aarbog for Københavns Universitet 1932-1933*, p. 113.

5. 'Elektrondisputatsen'. *Ekstrabladet*, 29 November 1932.

6. Weiner (1971a).

Quantenmechanik, which has given me much pleasure."[7] Møller wrote the obligatory Danish summary account while in Copenhagen, finishing it in October. In this Danish part of the dissertation, he compared his theory with experimental data too recent to be included in the *Annalen* paper.

At the oral defence Bohr expressed his happiness about this first dissertation in theoretical physics from the institute and praised Møller for his insight in what he called "the symbolic treatment" of physical phenomena, "a method of such power and beauty that it always must lead to something beautiful and good."[8] Bohr and Klein considered the dissertation to be "an instructive example of the peculiar correspondence-like connection between the quantum theory and the classical mechanics" they had often emphasised, and they stressed the author's "superior mastery of the mathematical methods on which the work rests."[9]

The subject of Møller's doctoral thesis was a theory of the passage of fast electrons through matter based on a correspondence method of calculating the electron-electron cross section in a way that satisfied relativistic invariance. This theory he first presented in an important paper in *Zeitschrift für Physik* in 1931, and the next year he followed it up by a more elaborate treatment in *Annalen der Physik*, the other of Germany's leading physics journals. It was this latter paper comprising 55 pages of dense calculations that served as his thesis.[10]

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7. Møller to Bohr, 25 July 1932 (BSC). The book published in 1932 by the Dutch mathematician Bartel van der Waerden helped disseminate group theory to the community of theoretical physicists.

8. 'En diskussion mellem lærde'. *Berlingske Tidende*, 29 November 1932.

9. Bohr to K. Jessen, 28 June 1932 (Bohr General Correspondence, NBA). Bohr to Klein, 28 June 1932 (BSC). In his letter to Klein, Bohr referred to Heisenberg's still unpublished "very beautiful and interesting" treatise which "assumes protons and neutrons as the only constituents of the nucleus and describes β–ray emission as a disintegration of the neutron." Heisenberg (1932b).

10. Møller (1931), received 21 May, published 29 July. Møller (1932), received 3 May, published 15 August. This chapter relies on two earlier studies on the origin and history of Møller scattering. See Kragh (1992) and Roqué (1992), where further details and references can be found.

Møller's scattering theory built upon established traditions within stopping theory and electron-electron interaction which he transformed into a single coherent theory based on correspondence arguments. The penetration of charged particles through matter was a subject of great interest to Bohr, who in his early career had contributed significantly to it. In 1913, and more elaborately in a paper published two years later, he developed a theory in which a charged projectile's encounter with atomic electrons was treated by means of a perturbation method. The paper of 1913 is noteworthy from a historical point of view because it preceded his atomic model and was the first one in which he referred to Rutherford's nuclear atom. Moreover, he related the frequency of the harmonically bound electrons to Planck's quantum hypothesis.[11]

Bohr argued that the energy loss of charged particles entering matter was essentially due to collisions with atomic electrons, whereas collisions with the nuclei only added negligibly to the energy loss. For a projectile of mass m, charge e, and initial velocity v passing a distance Δx through a substance with N atoms per unit volume he found that only those collisions in which the transferred energy was smaller than

$$S = 2\pi N e^4 \Delta x / m v^2$$

would contribute significantly to the energy loss. For the average energy loss in one-electron atoms he obtained the result

$$\Delta_S T = \frac{2\pi N e^4}{m v^2} \Delta x \log\left(\frac{k v^2 m v^2 S}{4\pi^2 \nu^2 2 e^4}\right)$$

where T denotes the kinetic energy and ν the frequency of the bound electron; the quantity k is a constant of value ca. 1.12. In his paper of 1915 Bohr showed that his theory could be extended to the relativistic domain and that the result would then be

$$(\Delta_S T)_{rel} = \Delta_S T - \log(1 - v^2/c^2) - v^2/c^2$$

11. Bohr (1913). Bohr (1915). See also Bohr (1987) for his series of works on stopping theory.

Bohr's classical theory accounted well for experimental data and formed the background for much of the following development of stopping and scattering theory. After the emergence of quantum mechanics, John A. Gaunt at Cambridge University developed in 1927 a semi-classical theory which gave a stopping formula almost the same as Bohr's. Whereas Gaunt treated the projectile classically and the atom quantum-mechanically, shortly later Walter Elsasser and others produced the first scattering theories fully based on quantum mechanics.[12] Elsasser's result for the energy loss of charged particles in hydrogen agreed with Bohr's formula.

These early studies were greatly extended and given a more rigorous treatment in a 76-page long paper written by Hans Bethe in the spring of 1930.[13] In this *tour de force* article, 23-year-old Bethe applied Born's approximation method to give a comprehensive account of nonrelativistic fast-collision processes in which the speed of orbital electrons was negligible compared to that of the projectile. For the quantity $\Delta_S T$ he deduced the formula

$$\Delta_S T = \frac{2\pi N e^4}{m v^2} \Delta x \log\left(\frac{2 m v^2 S}{a^2 R^2 h^2}\right)$$

where $a = 1.105$ and R, Rydberg's spectral constant, corresponds to the classical frequency appearing in Bohr's formula by $\nu = cR$. As Bethe pointed out, the difference between the logarithmic terms in the two formulae relates to the extra factor $(4\pi e^2/hv)^2 = (2\alpha c/v)^2$, where α is the fine structure constant given by $2\pi e^2/hc$. For fast electrons ($v > c/70$) this factor is smaller than one, which agreed with available empirical data. The superiority of Bethe's theory to Bohr's was substantiated a few years later by experiments made at Manchester University.

Bethe became aware of Møller's 1931 paper some time after having published his nonrelativistic theory. By using Møller's method he saw a way in which he could extend his own theory to the relativistic domain. The two young physicists had not met, but they ex-

12. Gaunt (1927). Elsasser (1928).

13. Bethe (1930), published 10 June 1930.

changed letters on what was their common research interest. In the autumn of 1931, Møller told Bethe about his plan for a comprehensive scattering theory which he planned to use for a doctoral dissertation. "Since I will now try to use the same method to calculate the stopping of ultrafast particles (β-rays, cosmic radiation)", he wrote, "I would be very happy if you would send me a reprint of your work on the same matter according to Schrödinger's theory." Møller further reported: "Mr. Rosenfeld has completed a very interesting generalisation of the method used in my work; he derives quite general relativistic expressions for the interaction of an arbitrary number of particles, although of course only in the domain where the interaction can be considered small, i.e., where the relativistic many-body problem has a correspondence-like meaning at all."[14]

By December Møller had made progress and derived formulae that yielded Bethe's results in the nonrelativistic limit. This is what he reported in a letter to Heisenberg, where he also mentioned that his formulae gave surprisingly high ionisation for very fast electrons.[15] It is interesting if not of great importance that Møller chose to title his forthcoming paper, which essentially made up his dissertation, almost the same as Bethe's article. In fact, the only difference was that Møller substituted 'Elektronen' for Bethe's more general 'Korpuskularstrahlen' (corpuscular rays). In his interview by Kuhn of 1963, Møller stressed that his works of 1931-1932 were triggered by Bethe's work to which he was directed by a conversation with Lev Landau:

> Bethe had treated the collisions and stopping phenomena in the non-relativistic case. He had written the matrix element for the transition in such a way that it looked as if one particle in its transition creates a charge distribution which then acts on the other through a Coulomb potential. And then the rather obvious idea came to do it relativistically; instead of using the Coulomb static potential, to introduce a retarded

14. Møller to Bethe, 30 September 1931 (CMP). Rosenfeld (1931b).
15. Møller to Heisenberg, 4 December 1931, a response to a letter from Heisenberg to Møller, 28 November 1931 (CMP).

potential corresponding to the charge and current which corresponds to such a transition.[16]

As regards the role played by Landau, Møller stated: "Actually it was Landau who mentioned that probably one could do such a calculation by a purely correspondence treatment. ... It was not Bohr who gave me the idea to this, it was actually this remark by Landau." Indeed, when Møller's paper appeared in print it acknowledged Landau "for some considerations which have been essential for this work." According to an anecdote, Landau got upset and said to Møller that the paper should have been signed by both of them, to which Møller answered: "You know, Dau, I want to get married, but my fiancée's father will not agree if I am not an associate professor at the university." Landau replied: "If this is the case, it is yours. I can write another article for you."[17]

Having studied in detail Møller's paper of 1931, Bethe wrote him: "During the last days I have occupied myself very thoroughly with your important work on the scattering of electrons of relativistic velocity. I find it wonderful that you can treat the problem in such a simple way!"[18] Apparently, Bethe had only recently realised that he was duplicating, more or less, what Møller was working on and about to finish. Aware of the possible competition, Bethe wanted to know if Møller had already derived the stopping formula or was on his way to do so. Møller suggested that Bethe might publish a brief paper in *Die Naturwissenschaften*, or that the two might jointly write the paper, but nothing came out of it. Bethe did write a paper, but the manuscript was rejected by *Die Naturwissenschaften* as too

16. Kuhn (1963). And in Weiner (1971a): "It was absolutely crucial that Landau was here because he — well, first of all, he brought me into the scattering problem — recommended me to read a paper by Hans Bethe. That was the non-relativistic scattering. Then I noticed that the way he had formulated it could be formally at least generalized to relativistic treatment, and I talked with Landau about this, and he said, 'Yes, that is fine'."

17. The story is told in a Russian online article about Landau: https://russkiymir.ru/en/publications/251919/. Christian Møller and Kirsten Pedersen married a month after the paper was published.

18. Bethe to Møller, 25 March 1932 (CMP).

long, and so it happened that the two papers, covering the same ground, were published separately.[19]

Due to the different speeds of publication, Bethe's paper submitted to *Zeitschrift* one day after Møller's to *Annalen* appeared first, which was probably a reason why Bethe alone was often credited with the stopping formula. Bethe actually referred to Møller's as yet unpublished paper: "We will only briefly sketch the derivation of the braking formula, since an extensive derivation, as we learned, will soon be published by Møller."[20] His work was considerably shorter and less detailed than Møller's, and moreover it relied on his knowledge of the latter paper. In his letter to Møller of 30 April 1932, Bethe wrote:

> Unfortunately, it was not possible to publish the stopping formula as a note in Naturwissenschaften, such as you suggested. … I have now submitted the work to Zeitschrift für Physik and added a brief derivation which, however, is essentially limited to collisions with small scattering angles and even in this case is not very complete. I hope that in this way I have not offended your work, to which I have referred appropriately; I also hope that you will find the form of my note to be correct.

As three physicists pointed out much later, Bethe's paper "would likely have met resistance in getting published in the present system of peer review."[21]

Møller arrived in his 1932 *Annalen* paper at a formula for the mean energy loss in hydrogen, which can be compared with the ones derived by Bohr and Bethe. Møller's formula was

$$\Delta_S T = \frac{2\pi N e^4}{m v^2}\Delta x\left[\log\left(\frac{2mv^2 S}{a^2R^2h^2}\right) - \log(1 - v^2/c^2) - v^2/c^2\right]$$

which reduces to Bethe's nonrelativistic formula for $v/c \ll 1$.

19. Møller (1932), with an added reference to Bethe (1932) published 7 June 1932. Bethe to Møller, 30 April 1932 (CMP). Schweber (2012), pp. 220-221.

20. Bethe (1932), p. 294.

21. Fontes, Bostock, and Bartschat (2014), p. 518, a detailed analysis of Bethe's paper and comparison with Møller's papers of 1931 and 1932.

The collision between two electrons was satisfactorily explained in terms of wave mechanics by a theory proposed by Mott in 1930. However, Mott's theory was limited to collisions at low energy, and for high-energy interactions it was realised that electron-electron collisions were governed by Dirac's quantum equation rather than the one of Schrödinger. The problem of extending Dirac's one-electron equation to two interacting electrons was first addressed by Gaunt and in 1929 more thoroughly by Gregory Breit, a Russian-born American physicist at the Carnegie Institution of Washington.[22] For the interaction energy Breit derived the expression

$$V = \frac{e^2}{r}\left[1 - \frac{\boldsymbol{\alpha}_1 \cdot \boldsymbol{\alpha}_2}{2} - \frac{(\boldsymbol{\alpha}_1 \cdot \boldsymbol{r})(\boldsymbol{\alpha}_2 \cdot \boldsymbol{r})}{2r^2}\right]$$

where the $\boldsymbol{\alpha}$ symbols refer to the 4×4 Dirac matrix vectors of the two electrons and $\boldsymbol{r}$ to the distance between them. Breit's corresponding wave equation accounted for magnetic interactions and retardation effects to the order of $(v/c)^2$ and it also agreed well with experiments. However, the agreement was incomplete and from a theoretical point of view it was a blemish that the equation did not satisfy relativistic invariance. Still by early 1931 the problem of a fully relativistic electron-electron interaction was unsettled. It would be solved some months later by Møller, who in his *Zeitschrift* paper mentioned Breit's treatment in a footnote without citing its source. In a later paper Breit referred to Møller, noting that the results stated in the latter's paper of 1931 were derived "without using the formalism of the quantum electrodynamics."[23]

2.2. The correspondence approach

The old semi-classical quantum theory was to a large extent based on Bohr's correspondence principle, which also served as an important tool in the creation of the Heisenberg-Born-Jordan quantum mechanics in 1925. In a broad sense the correspondence principle stipulates that quantum theory must contain classical mechanics

22. Breit (1929). Roqué (1992), pp. 209-212.
23. Breit (1932), p. 617.

and electrodynamics as limiting cases. In works from about 1920 Bohr developed a quantitative version of the principle to derive the intensity and polarisation of light emitted from atoms. In this way he and others extended the original atomic theory to yield predictons of transition probabilities and the structure of many-eletron atoms.[24] To the extent that Bohr's correspondence principle lived on in the post-1925 era it was mainly in alternative versions of radiation theory, as a temporary substitute for quantum electrodynamics, or as a general principle of a more qualitative nature. After the new mechanics had been secured a satisfactory formulation, the correspondence principle was largely replaced by arguments based directly on quantum mechanics. On the other hand, the principle did not vanish instantly or completely from quantum physics.[25]

As mentioned in Section 1.2, the most sophisticated and consistent use of correspondence arguments was made by Klein in 1927, when he formulated a quantum theory of radiation based on a direct connection of Maxwell's equations with the five-dimensional relativistic version of wave mechanics he had recently proposed. Applying a method inspired by Bohr's calculation of the Einstein probability coefficients of spontaneous emission, Klein found expressions for the charge and current densities and evaluated these by means of correspondence rules to obtain the fields emitted by perturbed atoms. In this way he succeeded in giving a correspondence interpretation of wave mechanics and explanations of, for example, the photoelectric effect, dispersion, and the Compton effect.

Klein's correspondence approach, with its unmistakable imprint of the Copenhagen spirit, was held in great esteem at Bohr's institute, where it was not only further developed by Møller but also by Rosenfeld in two papers of 1931. One of them, received by *Zeitschrift für Physik* on 24 October 1931, started with a reference to Møller's new paper: "Møller has recently treated the collision between two particles by taking into account retardation and using a reason-

24. Jammer (1966), pp. 109-118.

25. Kragh (2012), pp. 217-220. In his comprehensive work on the principles of wave mechanics published in 1933, Pauli included a lengthy section on the correspondence approach. He cited Klein but not Møller. See Pauli (1946), pp. 201-210.

able approximation approach closely related to Klein's well-known correspondence method."[26] Under the influence of Klein and the Belgian mathematical physicist Théophile de Donder, young Rosenfeld considered the correspondence principle an essential element in wave mechanics even before coming to Copenhagen. In works from the late 1920s on a possible reconciliation of wave mechanics and general relativity, he referred explicitly to Bohr's principle and used it in his search for a five-dimensional theory incorporating both quanta and gravitation.[27]

In Copenhagen, the attitude to the new and exceedingly complicated theory of quantum electrodynamics proposed jointly by Heisenberg and Pauli in 1929 was somewhat sceptical.[28] The Heisenberg-Pauli theory was impressive by being relativistically invariant and including quantisation of both radiation and matter waves. On the other hand, it was infected by paradoxes and divergent quantities, of which the infinite self-energy of the electron was the most severe and the most discussed. In retrospect, the theory of Heisenberg and Pauli was a progressive step towards the future quantum electrodynamics, but around 1930 it was controversial for both empirical and conceptual reasons. Physicists at Bohr's institute tended to look upon the fundamental and ambitious theory with some reserve and were generally reluctant to participate in its early development.

Møller was well acquainted with the Heisenberg-Pauli theory but not tempted to follow up on it. Much later, Kuhn asked him: "To what extent ... was the correspondence principle approach thought of as an approximate substitute in view of the infinities, and to what extent did it look like just as fundamental approach?" Møller answered:

> I think one looked upon this as a preliminary thing. I mean something like the Heisenberg-Pauli theory would always appear as something

26. Rosenfeld (1931a). Rosenfeld acknowledged discussions with Møller and Delbrück.

27. On Rosenfeld's early works, see Peruzzi and Rocci (2018), especially pp. 216-221.

28. Heisenberg and Pauli (1929). See also Schweber (1994), pp. 39-55.

> more fundamental. But it didn't give so many new results … All these formulas, I mean the Delbrück scattering and the Klein-Nishina formula and scattering between fast particles and between electrons and positrons, as Bhabha did, could be done by these correspondence methods. But certainly, I think we all had the feeling that the field is something real and must be treated like a quantum-mechanical system by means of *q*-numbers and commutation relations and so on.[29]

Whatever the precise attitude to the correspondence approach in Copenhagen, physicists at Bohr's institute looked with more sympathy on this approach than did physicists elsewhere. To apply it to electron-electron scattering at high energy, as Møller did, was quite in the tradition of the Copenhagen spirit.

Although the correspondence principle or better correspondence-like considerations appeared prominently in several publications from 1927 to about 1933, in most cases it was in versions that had little in common with Bohr's original principle dating from 1918. References to the correspondence principle by name rarely implied use of Bohr's principle in its authentic version and sometimes the authors only used the principle in a rhetorical sense. Møller's scattering theory merely related a classical concept, the four-current distribution in Maxwellian electrodynamics, to a corresponding concept in quantum mechanics, namely the matrix elements. In fact, the critical dependence of Møller's method on correspondence arguments did not appear very explicitly in his papers. One looks in vain for terms such as 'correspondence principle' or 'correspondence-like' (*korrespondenzmässig*) in his 1931 paper, and in his more detailed paper of 1932 they only appeared once. Nonetheless, correspondence arguments did play a crucial role in his work, which was recognised by several contemporary physicists including Dirac, Rosenfeld, and J. Robert Oppenheimer.

Dirac rejected the Heisenberg-Pauli theory on methodological grounds, arguing that the field should be treated as something more elementary than particles and not, as Heisenberg and Pauli

29. Kuhn (1963). As suggested by Roqué (1992), Møller's evaluation of the attitude of the Copenhagen physicists involves some degree of post hoc rationalisation.

had done, as a dynamical system on the same footing as the particles. He described the interaction between electrons solely by a wave function, meaning that the interaction energy only entered implicitly, as a consequence of the theory. When Dirac published his alternative to the Heisenberg-Pauli quantum electrodynamics in May 1932, he stressed its connection to the correspondence principle and compared it to Møller's theory, which he summarised as follows:

> A definite advance in the relativistic theory of the interaction of two electrons is contained in a recent paper by Möller, where it is shown that in the calculation of the mutual scattering of two colliding electrons by Born's method of approximation, one may describe the interaction with retarded potentials and use relativistic ideas throughout, without getting any ambiguity in the scattering coefficients to the first order of approximation. This lack of ambiguity is ground of presumption of the correctness of the result. When, however, one tries to apply similar methods to the higher approximations or to more general problems, one meets very definitely with ambiguities.[30]

As Dirac further pointed out, "The method by which Möller obtained his result may be compared with the methods of the Correspondence Principle ... for calculating Einstein's A and B coefficients from classical models." Despite his praise of Møller's theory, Dirac concluded that it was too special to qualify as a proper alternative to the Heisenberg-Pauli quantum electrodynamics: "It would be useless to try to extend Möller's method by setting up rules to provide a definite interpretation for ambiguous quantities. Any attempts in this direction would be just as futile as the attempts made in the pre-Heisenberg epoch to calculate Einstein's A's and B's from some sort of mean of classical quantities referring to the initial and final states." Dirac consequently developed his own alternative, a theory of quantum electrodynamics which only relied peripherally on Møller's scattering theory. In a critical work on Dirac's 1927 radiation theory, Rosenfeld referred to Møller's new

30. Dirac (1932), p. 455. The reference to Møller's theory was to his first work, Møller (1931).

scattering theory, and it may have been this work which drew Dirac's attention to Møller's paper.[31]

Dirac discussed his new theory when staying for two weeks in Copenhagen in April 1932 in connection with the Easter conference celebrating the tenth anniversary of Bohr's institute. Møller was most likely among the discussants, and Heisenberg, Klein, and Rosenfeld certainly were. As Rosenfeld proved shortly later, Dirac's theory was mathematically (but not conceptually) equivalent to that of Heisenberg and Pauli, which made Dirac's theory much less appealing.[32] Although Dirac accepted Rosenfeld's equivalence proof as formally correct, he maintained that mathematical equivalence did not imply physical equivalence and therefore continued to develop his approach. By the summer of 1932 it was established that Møller's interaction formula could be obtained not only from Dirac's theory, such as shown by the Leningrad physicist K. Nikolsky, but also from quantum electrodynamics in either Fermi's formulation or the Heisenberg-Pauli version.[33]

2.3. Møller's scattering formula

As mentioned, it was discussions with Landau in late 1930 that stimulated Møller to take up the problem of extending Bethe's stopping theory into the relativistic domain by means of the correspondence approach previously developed by Klein. To do so he made use of Dirac's new quantum theory and a retarded four-potential (ϕ, $\boldsymbol{A}$) corresponding to the charge and current densities (ρ,$\boldsymbol{j}$). The idea of expressing the finite velocity of the propagation of electrical action by means of retarded potentials goes back to a paper of 1867 written by Ludvig Lorenz, the only Danish mathematical physicist before

31. Rosenfeld (1931b). See Kojevnikov (2002) for the Møller-Rosenfeld-Dirac connection.

32. Rosenfeld (1932). Dirac to Bohr, 23 March 1932 (BSC). Dirac to Rosenfeld, 6 May 1932, reproduced in Kragh (1990), p. 136.

33. Nikolsky (1932). Some years later, Nikolsky (or Nikol'skii), a critic of the Copenhagen school of quantum mechanics, became involved in a dispute with V. Fock. See Graham (1966), p. 72.

Møller.[34] In his work of 1931 Møller considered the non-radiative interaction of two electrons initially described by two of the four components in Dirac's wave function, ψ_1 and ψ_2. The other two components ψ_3 and ψ_4 referred to the controversial negative energy states, and as Møller tersely stated in a footnote, "We shall look apart from the solutions with negative energy." At the time Møller wrote his paper, Dirac still associated the negative energy states with the much heavier protons ($\bar{e} = p^+$), a view he was largely alone to defend. He only introduced the positively charged antielectron as a new elementary particle ($\bar{e} = e^+$) in another landmark paper published in late May 1931.

To obtain the interaction matrix element corresponding to a transition from an initial to a final state, Møller followed the method of Klein's theory of 1927.[35] He ended up with an expression symmetric in the two particles, which was satisfying given that the symmetry was not built into the method on which his theory was based. "He [Landau] remarked that the final result was symmetrical in the two particles, and this gave me great confidence."[36] Another reason for his confidence was that for small velocities, the derived interaction energy reduced to the one found by Gaunt in 1929, namely

$$V = \frac{e^2}{r}(1 - \boldsymbol{\alpha}_1 \cdot \boldsymbol{\alpha}_2)$$

where $\boldsymbol{\alpha}_1$ and $\boldsymbol{\alpha}_2$ are the Dirac matrix vectors of the two electrons. The first term in Gaunt's expression is the Coulomb interaction and the second one the interaction due to the electrons' spin. Møller's formula for V included a factor of $\exp(ikr)$, where $k = 2\pi\Delta E/hc$ and ΔE is the energy difference between the initial and the final state. In the fall of 1931 there seemed to be no possibility of testing the predictions of the theory. Møller realised that in order to turn it into a testable theory he had to transform it into a proper scattering theory, which he did with his theory of the following year.

34. Kragh (2018), pp. 126-128, 141-142. Lorenz used the retarded potentials to develop an electrical theory of light independently of but equivalent to Maxwell's field theory.
35. See Roqué (1992) for details on Møller's derivation.
36. Weiner (1971a).

"The object of the present work is to treat the passage of hard β rays through matter in agreement with quantum theory and relativity. All the physical phenomena associated with the passage of rapidly moving electrons through matter ... can be reduced to the interaction of the electrons with the atoms."[37] This is how Møller introduced his paper of 1932 in which he derived a general formula giving the number of beta particles of a certain energy scattered a certain angle. After having corrected some errors pointed out by Heisenberg, Møller reported his final scattering formula in letters of late January 1932 to Mott and also to the Cambridge physicist Frank C. Champion.[38] Møller's electron-electron scattering formula gives the cross section for electrons scattered between the angles θ and $\theta + d\theta$. With γ denoting the Lorentz factor

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

and μ the quantity

$$\frac{2 - (\gamma + 3)\sin^2\theta}{2 + (\gamma - 1)\sin^2\theta}$$

the formula reads

$$I(\theta)d\theta = 4\pi \left(\frac{e^2}{mv^2}\right)^2 \frac{\gamma + 1}{\gamma^2} \left[\frac{4}{(1 - \mu^2)^2} - \frac{3}{1 - \mu^2}\right.$$

Remarkably, it contains no terms including Planck's constant h.

As Møller showed, when one applied the second-order Klein-Gordon equation instead of the Dirac equation, the same formula came out but without the last term in the square bracket. The term

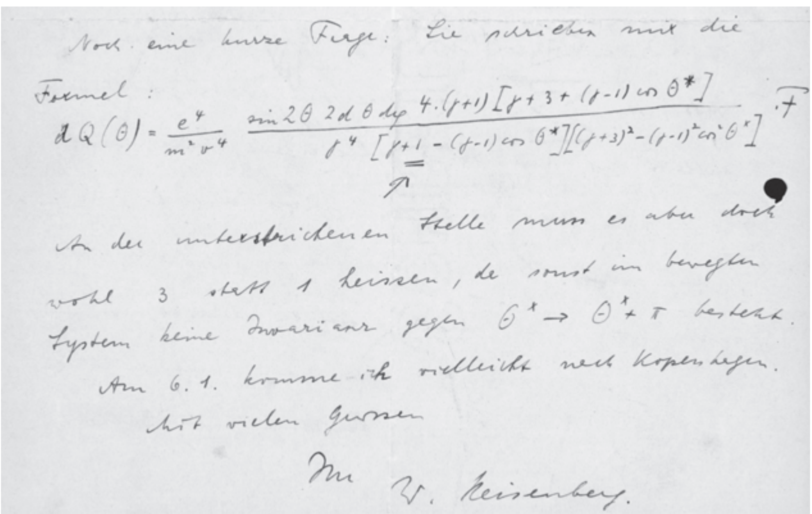
$$\left. + \frac{(\gamma - 1)^2}{4\gamma^2} \left(1 + \frac{4}{1 - \mu^2}\right)\right]$$

37. Møller (1932) p. 531.

38. Møller to Mott, 25 January 1932 (CMP). Møller to Champion, 25 January 1932 (CMP). Heisenberg to Møller, 28 November and 12 December 1931 (CMP).

is thus the contribution made by the spin of the two electrons taking part in the collision process. Perhaps surprisingly, Møller did not appraise or pay much attention to his scattering formula. Once deduced, he went on applying it to the problem of stopping or energy loss. The formula that Møller found for this problem in his 1932 paper is given in Section 2.1.

Heisenberg was aware of and interested in Møller's theory at an early date, primarily because of its potential relevance to the high-energy component of the cosmic rays on which subject Heisenberg was preparing an article. His aim was to confront the remarkably high energies with theoretical predictions and in this way to explore the possible failures of existing quantum electrodynamics. In his correspondence with Møller he criticised the new scattering theory, objecting that its formulae for stopping of ultrafast electrons disagreed with measurements of the cosmic rays. Referring to Møller's stopping formula, he wrote: "I would like to point out that it would no doubt signify a sharp contradiction between theory and experiment and show that the whole method of calculation with retarding fields is no longer admissible. It must be considered that



Noch eine kurze Frage: Sie schrieben mir die Formel:

$$dQ(\theta) = \frac{e^4}{m^2 v^4} \frac{\sin 2\theta \, 2d\theta \, d\varphi \; 4\cdot(\gamma+1)[\gamma+3+(\gamma-1)\cos\theta^*]}{\gamma^4 \, [\underline{\underline{\gamma+1}} - (\gamma-1)\cos\theta^*][(\gamma+3)^2-(\gamma-1)^2\cos^2\theta^*]} \cdot F$$

An der unterstrichenen Stelle muss es aber doch wohl 3 statt 1 heissen, da sonst im bewegten System keine Invarianz gegen $\theta^* \to \theta^* + \pi$ besteht.

Am 6.1. komme ich vielleicht nach Kopenhagen.

Mit vielen Grüssen

Ihr W. Heisenberg.

Fig. 7. Part of letter of 17 December 1931 in which Heisenberg points out an error in Møller's formula. See also Roqué (1992), p. 222.
Source: Møller Papers, Niels Bohr Archive.

γ could be at least 1000 for height-radiation [*Höhenstrahlung*, i.e. cosmic rays] electrons."[39] Møller admitted that his theory "would imply an unbelievable increase in the ionisation of cosmic rays." Although his formulae might not be valid for the extreme energies found in cosmic rays, still "in the domain of fast β-rays the applied method ought to be reasonable."[40]

When Heisenberg's paper appeared in *Annalen der Physik*, it included references to Møller's 1931 scattering theory and to his improved formulae in the as yet unpublished 1932 theory: "Møller has treated the collision between two free electrons according to Born's method and by taking into regard the retardation, and (after having corrected an error of calculation in the cited paper for which communication by letter I am Møller much obliged) he obtains exactly the classical result."[41]

It seems that Møller had not originally given much thought to which areas of physics his scattering theory might be applied and tested. However, at the end of the 1931 paper he commented that comparison of his formula for ultrafast electrons with laboratory data needed artificially accelerated electrons of an energy of the order 100 MeV, which at the time was unrealistic. On the other hand, he suggested that "it could be realised in the electrons produced by the northern light, and also in the corpuscular rays that accompany the height-radiation [*Höhenstrahlung*]. My formula would thus apply to the calculation of the stopping and scattering of this radiation, to which I hope to return later."[42] But he did not. While Møller was thinking of the mysterious cosmic rays in 1931, they had disappeared from his more elaborate theory of the following year, presumably as a result of Heisenberg's objections. Nor did he refer in print to his idea of high-energetic electrons associated with the aurora borealis.

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39. Heisenberg to Møller, 10 December 1931 (CMP). The symbol γ denotes the Lorentz factor.

40. Møller to Heisenberg, 4 December and 15 December 1931 (CMP). See also other correspondence between Heisenberg and Møller from November 1931 to February 1932 as listed in Roqué (1992).

41. Heisenberg (1932a), published 4 May 1932.

42. Møller (1931), p. 795.

Møller now restricted his focus to the high-energy parts of the beta spectrum from radioactive substances.

The existence of penetrating charged particles in cosmic rays was controversial in the early 1930s, when Robert Millikan and his students still defended the view that primary cosmic rays consisted of high-energy photons. According to Millikan, the photons arose from building-up processes in outer space of atomic nuclei from electrons and protons (which he confusingly insisted on calling positive electrons). Moreover, he postulated that at the same time electrons and protons were regenerated by photons emitted by the stars, thereby keeping the universe eternally in a steady state.[43] Only at about 1934 did the theory favoured by Millikan and his Californian collaborators disappear from the scene of physics.

Heisenberg was not the only physicist who at the time considered Møller's theory in relation to the puzzle of the ultrahigh energies of particles in the cosmic rays. So did Oppenheimer and his collaborator John Franklin Carlson, who in papers of 1931-1932 developed a version of Møller interaction to calculate the energy loss caused by collisions between charged particles and what they called 'magnetic neutrons'. These were not neutrons in the later sense of the word, but the hypothetical neutrinos recently introduced by Pauli as possible constituents of the atomic nucleus.[44] Whereas the two Americans initially thought that the penetrating particles might be Pauli's neutrons (that is, neutrinos), in their detailed analysis of 1932 they concluded that "there is no experimental evidence for the existence of a particle like the magnetic neutron." Their paper in *Physical Review* included a comprehensive account of Møller's 1931 theory including a derivation of his scattering formula which in this way became better known to American physicists. Oppenheimer and Carlson clearly valued the work of their Danish colleague:

> Quite recently Møller has given a beautiful method of treating the relativistic Impact of two electrons. This method is based upon a refinement

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43. De Maria and Russo (1989).

44. After Chadwick's discovery of the heavy or real neutron, Enrico Fermi proposed to name Pauli's magnetic neutron a 'neutrino'. See Brown (1978).

> of the correspondence principle; it neglects the higher powers of the interaction energy, and the effect of radiative forces; but within these limits it is strict and unambiguous, and enables one to account, not only of the relativistic variation of mass with the velocity of the electrons, but of the retardation of the forces between them, of the spin forces, of interchange and the exclusion principle. The method is applicable not only to the impacts between two free electrons, but ... to the impact of a neutron [neutrino] with an electron or proton.[45]

At about 1933 it became increasingly clear that for energies higher than about 150 times the rest mass of the electron, or about 80 MeV, the predictions of quantum electrodynamics disagreed markedly with measurements of high-energy cosmic rays. The problem added to a feeling of crisis in parts of the community of quantum theorists that persisted for several years.[46]

2.4. Experimental tests

Still at the end of September 1931, Møller believed that a direct test of his scattering theory belonged to the future. Or perhaps he just did not care very much about an experimental test. Then, on 14 October he received to his surprise an unsigned letter indicating that he might have been too pessimistic. Writing in the jocular jargon often used by Copenhagen insiders, he asked the young German physicist Max Delbrück to help him. Delbrück, who would later turn to biophysics and in 1969 receive a Nobel Prize in medicine or physiology, had written his doctoral thesis under Walter Heitler in Göttingen and in February 1931 come to Copenhagen to do post-doctoral work. At the time Møller addressed him, he had moved to Zurich to work with Pauli. Møller explained:

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45. Carlson and Oppenheimer (1931), dated 9 October. Quotation from Carlson and Oppenheimer (1932), received 18 July and published 15 September, on p. 765. Their knowledge of Møller's theory was based on his 1931 paper alone. The 1932 *Annalen* paper had not yet appeared in print.
46. See Cassidy (1981).

> The empirical facts are the following: On Oct. 14, 1931, about 11 a.m. I received a letter written on a typewriter (probably a Remington). According to the content of the letter, the sender is occupied with investigating the scattering of β-particles by means of Wilson photographs. He asks me for information about my formula for the scattering of fast electrons, [but] since he has 'forgotten' to sign his letter this is difficult, of course. If you cannot help me, I will have to address the Scotland Yard. I would believe that Blackett is the guilty one, what do you think? ... Lauritzen and I have immediately gone through the entire criminalistic library at the institute in order to find existing methods that may be used for such problems. But Edgar Wallace seems not yet to have attacked such serious problems.[47]

It turned out, without the assistance of the Scotland Yard, that Patrick Blackett was not the guilty one. The letter was written by one of his students at the Cavendish Laboratory, 23-year-old Frank Clive Champion, who was preparing his doctoral dissertation on the scattering of beta particles from radium E (Bi-210) by electrons.[48] A couple of further letters clarified the matter. Originally unaware of Møller's work, Champion studied close collisions of fast beta particles with electrons by analysing pictures of the collisions taking place in an automatic expansion cloud chamber. The method, which was developed by Blackett and his group in Cambridge, had the advantage of separating nuclear scattering clearly from the electron scattering.

By May 1931, Champion had at his disposal about 400 photographs with a total of 3,000 electron-electron tracks, but at the time he had no clear idea of the theoretical predictions he aimed to test. In a letter to Møller of 2 November – this time signed – he wrote

47. Møller to Delbrück, 14 October 1931 (CMP). Charles C. Lauritsen was a Danish-born American pioneer in experimental nuclear physics, who on some occasions visited the institute in Copenhagen. Edgar Wallace was a famous British writer of crime and detective novels.

48. Champion (1907-1976) left Cambridge in the autumn of 1932 to join the University College at Nottingham, where he was appointed professor in 1959 and served in this position until retirement eleven years later.

that now he intended his work to be "an experimental test of your formula for the scattering."[49] Realising that experiments like those in Cambridge could not easily be made in Copenhagen, Møller was happy to cooperate, and over the next few months the two physicists exchanged ideas in a series of letters. Whereas Champion informed his colleague in Copenhagen about his experimental data and their analysis, Møller wrote about his most recent theoretical results and how they related to what went on at the Cavendish Laboratory. The collaboration was beneficial to both physicists, who made good use of it in their almost simultaneous doctoral dissertations from 1932.

Champion's data analysis reported in *Proceedings of the Royal Society* in 1932 was based on 250 observed collisions with an angle of scattering ϑ varying between 10° and about 30°. The initial values of the beta electrons lied in the range from $v = 0.82c$ to $v = 0.92c$. Champion measured the number of particles scattered into various angular ranges and compared them to theoretical values derived from, respectively, Møller's theory, Mott's nonrelativistic theory of 1930, and Bohr's classical theory of 1915. Using Møller's formula he calculated 273 collisions, whereas the number of collisions based on Mott's theory was 783 and according to Bohr's theory 966. Clearly, Champion's data provided solid support for Møller's scattering theory which he used in the version that had not yet appeared in *Annalen*. "Examining the distribution of the scattering with varying ϑ we observe good agreement with Möller's formula", he stated "It is concluded that Möller's formula gives the best account of the scattering of electrons by electrons."[50]

While Champion thus dealt with Møller's theory, in his article from 1932 Møller did not refer to Champion's experiments or, for that matter, to other experiments. The article was theoretical through and through. That was the end of it, at least for the time being. Champion's experiments remained unique and were rarely questioned during the 1930s. As we shall see in the following section,

49. Champion to Møller, 2 November 1931. For the Møller-Champion correspondence and details about Champion's experiments, see Kragh (1992) and Roqué (1992).
50. Champion (1932), pp. 694-695. Received 25 June and published 1 September, two weeks after Møller's paper in *Annalen*.

new and more precise experimental testing of the Møller formula only appeared after the outbreak of World War II.

2.5. Reception and further developments

"Møller was the first man who had a really good field theoretical treatment of the two-electron collision problem. I remember being very much impressed by that paper."[51] This is how Mott, some thirty years later, recalled Møller's work on electron-electron scattering. The formulae that Møller derived were soon generally accepted and they played a considerable role in the growing literature on stopping theory related to the high energies of charged particles in the cosmic rays in particular. With the discovery of the positron it was understood that the new particle – whether identified with Dirac's antielectron or not – had to be taken into account. Carl Anderson only announced the discovery of the positron in March 1933, long after Møller had completed his theory. On the other hand, Bethe, Heitler, and other physicists soon developed elaborate stopping theories which included calculations of the electron pairs ($\bar{e} = e^+$) created by the collisions of heavy charged particles. These theories were much discussed in connection with the problem of applying quantum electrodynamics to the cosmic rays.[52]

During the period when Møller worked out his theory, Bohr took a deep interest in collision problems, a topic which occupied him almost as much as the more philosophical discussions about complementarity, correspondence, and the quantum world. In 1932 he prepared a comprehensive article on collision theory in order to obtain a better understanding of the relationship between the classical treatment and the quantum-mechanically theories based on Born's approximation method.[53] Conceptual clarification, rather than the establishment of improved stopping formulae, was Bohr's

51. Kuhn (1963).

52. Cassidy (1981). Galison (1987), pp. 96-110.

53. Unpublished manuscript on 'Atomic Collision Problems and the Recent Discoveries Regarding Nuclear Disintegrations'. Reprinted in part in Bohr (1987), pp. 278-286.

Fig. 8. Weizsäcker and Bhabha in the canteen during a lunch break in the 1936 Copenhagen conference. Credit: Niels Bohr Archive, Photo Collection, Copenhagen.

primary interest at the time. "Rosenfeld and I", he wrote in a letter to Klein, "have ... had long discussions with Møller, who understood quite well that it is possible to elaborate considerably on the question of the interrelationship between the classical and the quantum mechanical treatments."[54] After World War II, Bohr returned to collision and stopping theory. He obviously had studied Møller's work from 1932 carefully, such as shown by a comprehensive book-length study of the penetration of particles in matter he published in 1948.[55]

In a paper of 1936, the Indian physicist Homi Jehangir Bhabha calculated what became known as Bhabha scattering, which is the

54. Bohr to Klein, 28 June 1932 (BSC). Bohr (1987), pp. 712-713.
55. Bohr (1948) and Bohr (1987), pp. 425-568.

scattering of positrons by electrons. Since 1927 Bhabha, five years younger than Møller, had studied at Cambridge University and in the early 1930s he worked with Pauli in Zurich, with Fermi in Rome, and with Kramers in Utrecht. Later he spent a short period at Bohr's institute in Copenhagen, where he participated in a conference on nuclear physics held 17-20 June 1936. Responding to Bhabha's request of visiting the institute, Bohr wrote: "You shall be very welcome indeed to work here for a time. ... I may possibly be away from Copenhagen for some days, ... but Møller and Rosenfeld will be here all the time."[56] Bhabha's work relied on Møller's earlier theory of which it can be considered an extension or generalisation. Whereas Møller scattering is $e^-e^- \rightarrow e^-e^-$, Bhabha scattering is $e^-e^+ \rightarrow e^-e^+$. In the introduction to his paper, Bhabha wrote: "Møller ... developed relativistically invariant expressions for the collision of two charged particles with spin, and it may be seen directly from Møller's general formula for the collision cross-section that ... the effect of exchange does not in general vanish even when the two colliding particles initially have their spins pointing in opposite directions."[57]

As we shall see in Section 3.3., Møller spent the summer of 1935 in Cambridge on a research fellowship. It was on this occasion that he first met Bhabha:

> Homi Bhabha was actually the one I had most contact with during the summer, and we talked about the scattering, my paper on the scattering of fast electrons. I was from the beginning of course aware that it would also give the scattering of positrons. But I thought it was not so interesting because I thought this will never be possible to do any experiments with it. Positrons were a field very new at that time. But Bhabha sat down and calculated it, using this same method as I had

56. Bohr to Bhabha, 14 March 1936 (BSC). Bhabha to Møller, 28 October 1936 (CMP). See also Singh (2009).

57. Bhabha (1936), p. 195, submitted to the Royal Society in late October 1935. Bhabha to Møller, 13 October 1935 (CMP). On Bhabha and his career in science, see Chowdhury and Dasgupta (2010).

used, I mean the correspondence method, and that was during this summer.[58]

While in Cambridge, Bhabha gave a talk to the so-called Kapitsa Club on positron-electron scattering, and Møller gave another talk on his new work dealing with radiative electron-electron collisions, which he had just submitted to the proceedings of the Royal Society.[59] Dirac was also in Cambridge at the time, but Møller did not see him. As he said in the interview with Weiner, Dirac was "a lonely wolf."

Although Champion's experiments proved that Møller's stopping formula was superior to other alternatives, it was realised that the Cambridge experiments were too inaccurate to provide absolute confirmation. As noted by two American physicists at the University of North Carolina, George Hornbeck and Irl Howell, in a paper of 1941, a careful analysis of existing data seemed to reveal discrepancies from Møller's predictions. They consequently made a series of improved cloud chamber experiments with electron energies up to 2.6 MeV from which they concluded that the predictions were after all essentially correct. "[Our] result is reassuring since it shows that the Möller formula ... cannot be far wrong for either electrons or mesons at very high values of primary energy."[60] A decade later Ernesto Corinaldesi, a young Italian physicist working at Manchester University, studied theoretically the scattering of μ mesons (muons) on protons on the assumption that both particles have an extended charge distribution. For this purpose, he developed a modified Møller scattering formula.[61]

58. Weiner (1971b). With the phrase "from the beginning" Møller presumably meant from about 1933, when the positron entered physics. More on Møller's stay in Cambridge in sections 3.2 and 3.3.

59. Møller (1935a). The Kapitsa Club was an informal but very important physics discussion club named after the Russian physicist Peter Kapitsa, who started it in 1922. Among the many distinguished Kapitsa Club lecturers were Bohr and Heisenberg, who both gave talks in 1925. Dirac was an active member of the club and contributed with several talks.

60. Hornbeck and Howell (1941), p. 34. By 'mesons' they presumably referred to what was later called muons.

61. Corinaldesi (1951).

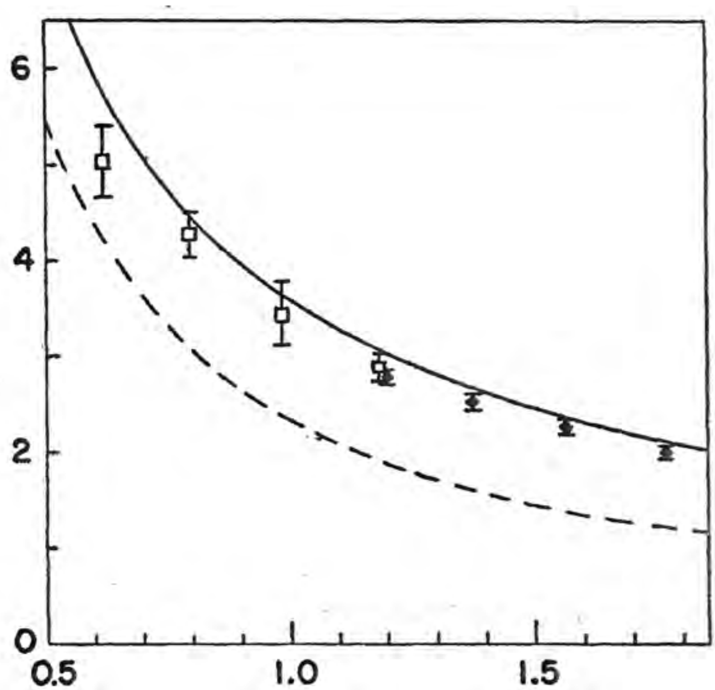

Fig. 9. Comparison of Møller's theory with the measured electron-electron scattering cross section plotted against the incident electron energy in the range between 0.5 and 1.8 MeV. Redrawn from figure in Ashkin, Page, and Woodward (1954).

After World War II, experiments to test Møller's formula continued, now using techniques more advanced than the traditional cloud chamber method. This series of experiments, almost all of them American, culminated in 1954 when a team of physicists at Cornell University applied a specially designed coincidence counter method to study large-angle scattering of beta electrons in the energy range from 0.6 to 1.7 MeV. "The results verify the Møller formula within 7 percent experimental error", they concluded.[62] They observed a

62. Ashkin, Page, and Woodward (1954), p. 357. The team also verified the Bhabha formula for electron-positron collisions, in this case with an experimental error of 10 per cent.

discrepancy between theory and experiment at energy 0.61 MeV, but wrote it off as probably due to multiple scattering.

Møller's formula for electron-electron scattering proved useful in 1957 after the sensational claim by Tsung Dao Lee and Chen Ning Yang at the University of Chicago that parity may not be conserved in weak interactions. The Swiss-American physicist Hans Frauenfelder and his group at the University of Illinois, Urbana, were able to use Møller scattering to measure the polarisation of electrons emitted in beta decay and in this way to confirm the hypothesis of parity non-conservation.[63] To test Møller's formula at much higher energies than those of beta decay, where the maximum energy is just a few MeV, required beams of accelerated electrons. Such beams were produced in SLAC, Stanford University's big-science linear accelerator which was commenced in 1962 and ready for operation four years later. In one of the first SLAC experiments, a Stanford-Princeton team using spark chambers as detectors found the Møller formula modified by a radiative correction term to be excellently confirmed up to a centre-of-mass energy of about 1 GeV.[64]

By then Møller was immersed in problems of general relativity and had long ago abandoned research in scattering theory. Nonetheless, he was aware of the development and took pride that his more than 30-year old theory had been confirmed at very high energies. In a speech of 1970, he concluded that "the scattering formula has been experimentally verified in the interval from a few MeV, as in the case of Champion, up to about 1200 GeV [MeV] in the Stanford experiments."[65]

Many years after Møller's death his scattering theory attracted new attention in the form of the large-scale 'MOLLER experiment' conducted at the Jefferson Laboratory in the 2010s with a collab-

63. Frauenfelder et al. (1957) with a reference to Møller (1932). See also Franklin (1986), pp. 20-21, and the correspondence between Pauli and Frauenfelder in Pauli (2005).

64. Barber et al. (1966).

65. Møller (1970), pp. 59-60, untitled speech on the occasion of the reception of the Ørsted medal.

oration of more than 100 scientists from 30 different institutions. The aim of this big-science experiment was to measure the parity-violating asymmetry in Møller electron-electron scattering with unprecedented accuracy. In this way the researchers obtained a precise value of the weak mixing angle (the Weinberg angle θ_W) of the standard electroweak theory. The experiments were sensitive enough to detect signals beyond the standard model. For this purpose, the physicists at the Jefferson Laboratory used electrons accelerated up to 11 GeV energy.[66] One may assume that many of the physicists engaged in the MOLLER experiment had no idea of the person to which the name of the experiment alluded – but then, officially the name was actually an acronym for *M*easurement *O*f *L*epton *L*epton *E*lectroweak *R*eaction.

To return to the 1930s, Møller's correspondence approach was sometimes regarded a provisional alternative to the complicated and troublesome Heisenberg-Pauli quantum electrodynamics. The theory from Copenhagen was admittedly less general and ambitious, and yet it gave unambiguous answers to definite physical problems. However, it soon turned out that the results from Møller's theory could be derived from quantum electrodynamics, which to most physicists meant that the more special correspondence approach was unnecessary. Based on Fermi's simpler and more manageable formulation of quantum electrodynamics dating from 1929, in 1932 Bethe co-authored with Fermi a paper in which they proved that Møller's interaction energy followed from standard quantum electrodynamics.[67] Bethe recalled:

> By 1932, using Møller's theory to calculate the interaction of relativistic charged particles, I had calculated the stopping power of charged particles of relativistic velocity in matter. Fermi was somewhat interested in my result ... [but] his main interest was in the Møller interaction ... so he proposed that we write a paper on various expressions for the interaction of relativistic charged particles: the full result of QED,

66. Kumar et al. (2014).

67. Bethe and Fermi (1932), published 2 August. Schweber (2012), pp. 215-221.

> the Møller approximation, and Breit's interaction, which was valid to order v^2/c^2.[68]

Møller was not actively involved in the discussions about quantum electrodynamics in 1932 – he may have been too busy with his doctoral dissertation and completing his large *Annalen* paper. Only in 1935, when spending his sabbatical in Rome and Cambridge, did he return to electron-electron scattering and the correspondence method used in his earlier work. The result – his last contribution to electron scattering theory – was a generalised theory which took into account also the photons emitted in collisions between two charged particles. This was a problem of great concern to physicists trying to understand high-energy particles in the cosmic rays. Oppenheimer and his collaborators Carson and Wendell Furry were heavily involved in such calculations, and at one stage Oppenheimer suggested to Carlson and Furry to take up the problem. Furry recalled:

> We never got it to the point where we could calculate. The equations were just too heavy. We did get up to a point where -– which was about where Moller got to, a year or two later, in a paper that he published on it in the Royal Society. He published a paper in which the calculation wasn't finished but he'd just gotten it to a fairly neat point in the formulation. We had got about the same point, but had felt that we ought to go ahead and get the answer.[69]

In his paper submitted to the *Proceedings of the Royal Society* while staying in Cambridge, Møller compared his correspondence-like approach to the methods of quantum electrodynamics. Noting that the problem of the radiative collision of two particles could be treated with the quantum electrodynamics of either Dirac or Heisenberg-Pauli, he wrote in the introduction:

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68. Bethe and Bethe (2002), p. 29.

69. AIP interview with Furry by Charles Weiner, 9 August 1971. https://www.aip.org/history-programs/niels-bohr-library/oral-histories/24324

> These theories, however, involve the well-known difficulties of the infinite self-energy of the particles, and to get physical results from the theory one has afterwards to subtract infinite terms. We therefore propose another method which allows us in an elementary way to treat the radiative collision independently of quantum electrodynamics. The adopted method is a correspondence method which forms an immediate generalization of the method previously used in the treatment of the non- radiative collision of fast particles.[70]

Having treated the problem with his favoured correspondence method, Møller did the same on the basis of quantum electrodynamics, using the formulation which had recently been developed by the Russian physicist Vladimir Fock. He demonstrated that the results based on quantum electrodynamics agreed with those derived by the correspondence method. At the end of the paper: "This work was begun in Rome and finished in Cambridge. I should like to thank Professor E. Fermi and Professor R. H. Fowler for the very pleasant time spent at the institutes in Rome and Cambridge respectively. Further I should like to thank Dr. Hulme and Dr. Bhabha for many discussions about problems connected with this paper." Henry R. Hulme, a former research student of Dirac, also benefitted from the discussions. In a paper from 1936 he considered the more general two-particle interaction case in which one of the particles is in a bound state. Hulme, who acknowledged "Dr. C. Møller for his assistance, and for many pleasant and valuable discussions", showed that also in this case the calculations of quantum electrodynamics yielded the same results as when using Møller's correspondence approach.[71]

Thirty-six years later, Møller recalled that back in 1935, "I was still interested in the possibilities of describing electromagnetic phenomena by a correspondence method. In contrast to the complete quantum electrodynamics, which ... contained the difficulties of the

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70. Møller (1935a), p. 482, received on 15 June. See also Møller to Bohr, 19 June 1935 (BSC).
71. Hulme (1936).

divergences." And about the later renormalised theory principally due to Schwinger, Feynman, and Tomonaga:

> I still don't regard this as a completely satisfactory theory, It's a very good theory for calculating things, astonishingly good, but in principle I don't regard it as completely satisfactory theory, because — well, it's just I said, one has to learn how to live with these things, but still it's not very beautiful. If you do this correspondence method, then you never get these difficulties.[72]

Møller was not the only physicist who felt this way about the new and, from an operationalist point of view, very successful theory of quantum electrodynamics. Thus, Dirac's attitude was strikingly similar to Møller's, only did Dirac express it in public and more forcefully. He found quantum electrodynamics to be complicated, ugly, and logically objectionable. "I am very dissatisfied with the situation", he stated in 1975, "because this so-called 'good theory' does involve neglecting infinities which appear in its equations, neglecting them in an arbitrary way."[73] However, Dirac's persistent search for a better and more beautiful theory was unsuccessful.

Julian Schwinger, one of the chief architects of renormalised quantum electrodynamics, was a prodigy who at the age of 16 mastered the advanced theories of electron scattering. At a symposium on the history of particle physics held at Fermilab in May 1980, he said: "Several years before [1934], the Danish physicist Christian Møller had proposed a relativistic interaction between two electrons, produced through the retarded intervention of the electromagnetic field. ... I asked how things would be [in quantum electrodynamics] when the retarded interaction of Møller was introduced." At a later occasion Schwinger confirmed that Møller's work of 1931 inspired him to write his juvenile paper on the interaction of several electrons. "I thought to myself that relativistic

72. Weiner (1971b).

73. Quoted in Kragh (1990), p. 184.

interactions are not local; they are functions of momenta and so on as in Møller interaction."[74]

Although Schwinger showed his manuscript to no one and did not submit it to a journal, this first and still unpublished paper proved important for his later work. When Richard Feynman in 1949 developed his novel space-time version of quantum electrodynamics, Møller scattering entered as an important example of how the theory could be used to calculate higher-order corrections for processes involving two virtual quanta. Feynman obtained Møller's scattering formula directly from what he called his fundamental equation for quantum electrodynamics.[75]

Given Møller's reservations with regard to mainstream renormalised quantum electrodynamics, it is ironic that it was only with this theory that Møller scattering came to be seen as an important part of fundamental physics. Modern textbooks devoted to quantum electrodynamics inevitably include a section on the scattering formula that Møller presented in his work from the early 1930s. But of course they do not refer any longer to Møller's original method.

74. Schwinger (1983), p. 356. Interview with Schwinger by Jagdish Mehra of March 1988. See Mehra and Milton (2000), pp. 14-15 where the main content of 'On the Interaction of Several Electrons' is excerpted.

75. Feynman (1949). The importance of Møller scattering is highlighted in Valente (2008).

CHAPTER 3

Radioactivity, and a sleeping beauty

After having received his doctoral degree in December 1932 and appointed a lecturer at Bohr's institute, Møller spent much of his time teaching courses in theoretical physics and making lecture notes for them. "For a very very long time we had no lectures in quantum mechanics", he recalled.

> So finally, the students made a kind of, what we could call now, *ungdoms-oprør* [youth revolt]. ... What we have now. Yes, revolt. And they came to me and said, "We want to have some lectures on quantum mechanics." So I started to give them some lectures on quantum mechanics, and this became then part of the regular courses.[1]

Fortunately for Møller's research career and despite his heavy teaching load, during the 1930s he managed to travel abroad on several occasions, in this way receiving new stimuli and being able to focus on his own research. His travels in the decade brought him to Italy, England, Russia, and Poland.

While quantum mechanics and its many implications, scientific as well as philosophical, were still the main business of the Copenhagen institute during the first half of the 1930s, during the second half of the decade Bohr consolidated a scientific reorientation towards nuclear physics. The reorientation also brought with it research on radioactive isotopes and their uses in biology, an area successfully cultivated by the Hungarian radiochemist George von Hevesy in particular. Hevesy, whom Bohr had first met in Manchester in 1912, worked at the institute 1920-1926 and again 1935-1943. While in Sweden as a Jewish refugee, he was awarded the 1943 chemistry Nobel Prize for his development of the radioactive tracer technique and its use in biological processes. Bohr had been interested in the atomic nucleus since about 1930, but it took several years until nu-

1. Weiner (1971b).

clear physics and nuclear reactions in particular became the primary research area of his institute.[2]

Møller, more concerned with mathematically formulated fundamental theories, mainly followed the development from the sideline. Although from about 1938 to 1943 he would investigate in detail the nature of the nuclear forces, he never was, and never considered himself to be, a nuclear physicist in the traditional sense of the term. On the other hand, he got very interested in Fermi's theory of beta decay, the beginning of what became weak interaction physics, which he developed in several papers. In addition, he contributed to subjects as diverse as ferromagnetism, many-electron systems, and the quantum behaviour of white dwarf stars.

3.1. Møller-Plesset perturbation theory

Although by 1930 solid-state physics did not yet exist as an identifiable scientific discipline, many physicists investigated the solid state of matter by means of the new and powerful methods of quantum mechanics. Pauli, who was one of the first to do so, was somewhat ambivalent in his attitude to the field, considering it to be less 'pure' than the fundamental physics of quantum field theory. In a letter of 1931 to Rudolf Peierls, who had just calculated the residual resistance in metals, he wrote: "I consider it harmful when younger physicists become accustomed to order-of-magnitude physics. The residual resistance is a dirt effect and one shouldn't wallow in dirt."[3] Dirty or not, the quantum theory of the solid state attracted much attention among the young physicists engaged in applied quantum mechanics. The field would eventually grow into the broader discipline of condensed matter physics, a name which became common only in the 1970s. Heisenberg was an early contributor to the research area and so was his student Felix Bloch, later a Nobel Prize laureate.

2. The reorientation of Bohr's institute during the 1930s is described in Aaserud (1990) and Stuewer (2018).

3. Quoted in Hoddeson et al. (1992), p. 181. In his interview in Weiner (1971b), Møller referred to the Czech physicist George Placzek calling solid-state physics "ein schmutziger Gebiet" (a dirty subject).

Bloch, one year younger than Møller, wrote in 1930 an important dissertation on the quantum mechanics of electrons in metals and two years later a no less important work on ferromagnetism.

Although solid-state physics did not attract much interest in Copenhagen, Møller was aware of the works in this area done by Heisenberg, Bloch, Bethe, Slater, and others. He may have received inspiration from Bloch, who visited the institute in 1931-1932 and again briefly in 1933. In any case, in March 1933 Møller submitted to *Zeitschrift für Physik* his first and only paper on solid-state physics, a theoretical investigation of ferromagnetism. Incidentally, this was one of his last papers written in German, which indirectly was a result of Germany's transformation this year to the Third Reich. As Møller said in an interview: "After 1933 we didn't want to write in German. So we always tried to publish in English."[4] And yet, in the second half of the 1930s Møller was the author of two papers in German language if not in German periodicals. One was a paper published in *Physikalische Zeitschrift der Sowjetunion* and the other was published in the proceedings of the Royal Danish Academy of Sciences.[5]

Møller's work was essentially a generalisation of Bloch's theory of ferromagnetism to atoms with more than one valence electron. From the extended theory he calculated the ferromagnetism of a nonconducting cubic crystal at low temperature, obtaining formulae for the temperature dependence of the spontaneous magnetisation that differed somewhat from those derived by Heisenberg and Bloch. Although of no particular scientific importance, Møller's paper was well known and cited by specialists in the quantum theory of magnetism. If nothing else, it illustrates his versatility and the ease with which he mastered a new branch of physics. For example, he had read and fully comprehended *The Theory of Electric and Magnetic Susceptibilities*, a pioneering monograph from 1932 written by the

4. Møller (1933). Weiner (1971c).

5. Møller (1937a) and Arley and Møller (1938). For unknown reasons, the two Danish authors of the 1938 memoir wrote in German rather than English. Altogether, of Møller's 77 research publications eight were in German (1929-1938) and 69 in English. Almost all of his 35 non-research publications were in Danish.

American physicist John H. Van Vleck who much later was awarded a Nobel Prize for his work on magnetism.

Another foreign physicist whom Møller met in 1933 was the 26-year-old Sicilian-born Ettore Majorana, who after a short time with Heisenberg in Leipzig visited Bohr's institute from 5 March to 12 April. His first impression of the Danish capital and its inhabitants was this: "Copenhagen is an enormous city with good architecture. The population, equally intelligent and civil from the highest to the lowest strata, is cut from the same template. … Coming from Germany, one has the impression of leaving Europe and entering a colony of Eskimos. The sense of social distinction is entirely absent."[6] In another letter to his mother, young Majorana wrote about Bohr and the institute for theoretical physics: "Bohr is good-natured and likes the fact that I speak German worse than him, and is very concerned about finding me a guesthouse near the institute. I am on good terms with Møller and Weisskopf."[7] Although shy by nature, in Copenhagen he also met and made friends with Rosenfeld and Placzek (whom he knew from Rome). In one of his letters, Majorana referred to the splendid Carlsberg Mansion, the residence of honour to which the Bohr family had moved the previous year:

> In Copenhagen he [Bohr] is quite popular. The owner of a large brewery built, and offered him use of, a charming cottage that one access by passing through mountains of beer barrels. It is notoriously difficult to find for those who go there the first time. I went there once for tea. Bohr himself guided my steps, as I was fortunate enough to meet him as he was taking a leisurely bicycle ride around the area.[8]

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6. Letter of 7 March 1933 to his mother Dorina Majorana, quoted in Recami (2020), p. 210. And in a letter of 29 March: "Residing here for about a month has confirmed me that there is not much to discover about the Danish soul. They are an extraordinarily peaceful, almost passionless people." Same source, p. 219.

7. Letter of 7 March 1933, in Recami (2020), pp. 211-212.

8. Letter of 18 March 1933, in Recami (2020), p. 217. For the Carlsberg Mansion, see Section 8.2.

Møller recalled, if not very precisely, his meeting with the Italian physicist:

> I think I had met him in 1932, I think, here in Copenhagen. He was a very quiet chap, very kind, but rather closed and he was always sitting in the library there brooding about his problems. ... He stayed for a few months. And that was of course at the time of the exchange forces, Majorana forces, you see, among others, but then he — you know that he disappeared.[9]

After having returned to Rome, Majorana did very important work in theoretical physics, in particular on neutrino theory and what became known as Majorana particles, which are neutral fermions identical to their own antiparticles. Thus the Majorana antineutrino is the same as a neutrino, $\bar{\nu} = \nu$, and similarly for the antineutron, $\bar{n} = n$. In the spring of 1938 he suddenly disappeared, leaving behind him a mystery which is unsolved to this day.

As solid-state physics was not a focus area in Copenhagen, so was it the case with quantum mechanics applied to chemical problems. Quantum chemistry in its modern meaning took off with a seminal work by Walter Heitler and Fritz London, who in 1927 proved that the covalent bond in molecules could be explained purely in terms of spin quantum mechanics. The breakthrough was quickly followed up by other physicists using tools of calculation which in many cases were the same as those used in solid-state physics. By 1932 quantum chemistry was established as a flourishing sub-discipline which in its early phase appealed more to physicists than to chemists. Several of the pioneers had visited Bohr's institute, among them Heitler, Friedrich Hund, John Slater, Erich Hückel, and Linus Pauling. Nonetheless, quantum chemistry and chemical physics were almost completely ignored in Copenhagen.[10]

An exception was the calculation based on the Schrödinger equation of the bond strength in the H_2^+ ion made in late 1926 by

9. Weiner (1971b).

10. Nielsen and Kragh (1997). On the birth and early development of quantum chemistry, see Gavroglu and Simões (2012).

Øyvind Burrau, a physicist and geodesist who at the time worked at Bohr's institute. "Hund has ceded the H_2^+ to Mr. Burrau and the latter has now really straightened out the problem finally", Heisenberg reported to Pauli in November.[11] However, Burrau left Bohr's institute for a position at the Danish Geodetic Institute already in 1928. His calculation inspired Hund to his important work on what became known as the molecular orbital method. Ironically, when a major contribution to the new field was made by two physicists at the institute, it was not recognised as such until decades later. Nor did the two physicists realise that their work belonged to quantum chemistry or might be of interest to chemists. They were just contributing to the approximation methods used in standard quantum mechanics, or so they thought.

The young Caltech physicist Milton Spinoza Plesset had in 1933 published a paper with Oppenheimer in which they used Dirac's relativistic quantum theory to analyse the production of an electron-positron pair when a gamma ray enters the Coulomb field of a heavy nucleus. The two physicists concluded that quantum electrodynamics was largely applicable to pair production at high energy, but that it failed when applied to radiation with a wavelength shorter than the value e^2/mc^2 corresponding to the classical electron radius.[12] Later the same year Plesset went to Copenhagen on a National Research Council fellowship, where he collaborated with another and even younger American visitor, 22-year-old John Wheeler, on other aspects of pair creation. "I envy Plesset very much his year in Copenhagen", wrote Oppenheimer to Bohr in a letter which enclosed a copy of the Oppenheimer-Plesset paper.[13]

Shortly after his arrival, Plesset attended one of the informal institute symposia with participation of, among others, Dirac, Heisenberg, Møller, Casimir, Bhabha, and Rosenfeld. Also Paul Ehrenfest participated, but for the last time. He was at the time seriously

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11. Mehra and Rechenberg (1987), pp. 852-855. Burrau (1927).

12. Oppenheimer and Plesset (1933).

13. Oppenheimer to Bohr, 14 June 1933, reproduced in Smith and Weiner (1980), pp. 161-162. See Wheeler (1998), pp. 131-139, for his recollections about Plesset and his first visit to Bohr's institute.

Fig. 10. Milton Plesset (left) with N. Bohr, F. Kalckar, E. Teller, and O. Frisch at the roof of the Copenhagen institute in 1934. Credit: Niels Bohr Archive, Photo Collection, Copenhagen.

depressed and after his return to the Netherlands he took his own life.[14] Plesset recalled about the Copenhagen institute:

> People were working as individuals there. I guess the man with whom I was most closely associated was an Englishman, or a Welshman—[Evan James] Williams. He and I had a lot of discussions with each other. He was a very bright fellow. He and I spent a lot of time discussing physics. The other people who were there were these expatriates, or refugees. They had known each other before, and so they stayed pretty much with each other. Generally, it was a free and easy atmosphere.[15]

14. On Ehrenfest and the tragic end of his life on 25 September 1933, see Delft (2014).
15. Interview with Plesset by Carol Bugé of 8 December 1981, online as https://oral-histories.library.caltech.edu/127/ which is also the source of the following quotation. Williams (1903-1945) worked in 1933 at Bohr's institute, where he did important work on collision theory and cosmic-ray physics.

Fig. 11. The Copenhagen conference in September 1933. On the first row: N. Bohr, P. Dirac, W. Heisenberg, P. Ehrenfest, M. Delbrück, L. Meitner. Milton Plesset is on the second row, number four from the right. To the left of Plesset: R. Frisch, W. Heitler, J. H. D. Jensen, E. Teller, C. F. von Weizsäcker, and Boris Podolsky. Christian Møller is seated behind Plesset, number three from the right on the third row. On the fourth row to the left is Harald Bohr and on the fifth row to the right are O. Klein, H. Bhabha, and L. Rosenfeld. Credit: Niels Bohr Archive, Photo Collection, Copenhagen.

In a letter to Nishina of early 1935, Bohr wrote: "Of theoreticians are at present besides Møller, who is now Klein's successor as Lektor, Rosenfeld, Teller, Placzek, Plesset and Weizsäcker working here. We are at the moment especially interested in the problem of the limitation of the correspondence methods of quantum theory."[16] At the end of Plesset's fellowship, Bohr wanted him to stay on, but "then his budget got very badly out of joint because of all the refugees that were coming through … and so he said, well, was there any other way that I might be able to stay by getting a stipend as

16. Bohr to Nishina, 26 January 1934 (BSC), reprinted in Nishina (1984), pp. 31-33.

an American. And so I did. And I stayed on there for almost a whole year." In the middle of May 1934, Plesset, together with Rosenfeld and Williams, accompanied Bohr and his wife on a visit to the Soviet Union with lectures and conferences in Leningrad, Moscow and Kharkov. On this visit, he met leading Russian physicists such as Landau, Ivanenko, Fock, and Tamm.

Møller was interested in the discussions between Plesset, Wheeler, and Williams about pair creation processes and probably took part in them. However, the outcome of his own discussions with Plesset was something quite different, namely a brief paper in *Physical Review* in which they developed a new perturbation theory for the calculation of many-electron systems.[17] Bohr valued Plesset as a promising physicist, such as he wrote to an American colleague, Leigh Page at Yale University:

> Surely he is one of the best of young American theoretical physicists and especially he has as you know a great insight in the relativistic quantum theory of the electron... He hopes soon to publish an account of some of his work together with [John] Wheeler, and you may perhaps has seen a recent paper in the Physical Review on the many-electron problem, which he published a few months ago together with Møller.[18]

The standard method for many-electron calculations was the so-called Hartree-Fock approximation, which relied on works done by Douglas Rayner Hartree in 1928 and by Vladimir Fock and Dirac in 1929-1930. The Cambridge physicist Hartree spent the period August-December 1928 at Bohr's institute, where he mostly worked on X-ray scattering in collaboration with the Swedish physicist Ivar Waller. At the time Hartree had recently developed the so-called self-consistent field approximation method for calculations of many-electron atoms. The general idea of this method was that the effect of an electron on other electrons could be represented by a

17. Møller and Plesset (1934). Received 14 July and published 1 October.
18. Bohr to Page, 23 December 1934 (BSC).

central non-Coulomb field of force.[19] In this way an approximate solution to the Schrödinger equation for complicated atomic systems could be obtained. Fock improved in 1930 the method by taking into consideration that the indistinguishability of electrons gives rise to exchange forces.

In calculations based on the Hartree-Fock method, the interaction between the electrons was taken into account only by means of an average interaction. To remedy for this deficiency various so-called electron correlation methods were developed, the first and arguably most important of which was the Møller-Plesset perturbation theory. As the two authors stated in their abstract, "A perturbation theory is developed for treating a system of *n* electrons in which the Hartree-Fock solution appears as the zero-order approximation." And later in the paper: "Thus, the perturbation method shows that the theory of the self-consistent field is accurate in the determination of energy to the second approximation." In other words, Møller and Plesset used the Hartree-Fock theory as a starting point but added a small perturbation given by the deviation of the Hartree-Fock Hamiltonian from the exact Hamiltonian. The perturbation term of the second order corresponded to the electron-electron interaction energy neglected in the Hartree-Fock theory.

Neither of the two methods were much used until the advent of computational chemistry in the 1970s. As far as the Møller-Plesset method is concerned, for a long period of time it was largely ignored. The paper by the two Copenhagen physicists was predominantly mathematical, with no indication at all of the areas of physics and chemistry to which their theory might be applied. Words such as 'atom' and 'molecule' did not appear in the paper, which also did not refer to 'chemistry' or related terms.

Møller was plainly uninterested in chemistry and may have shared Dirac's reductionist view that with appropriate approximation methods chemistry would turn out to be nothing but applied quantum physics. This is what Dirac stated in a paper dealing with the quantum mechanics of many-electron atoms: "The general the-

19. See Gavroglu and Simões (2012), pp. 138-143. Møller may have met Hartree in Copenhagen, but there is no indication of contact between the two.

ory of quantum mechanics is now almost complete … 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."[20] Dirac's somewhat provocative statement seems to have left an impression on Møller, who late in life quoted it approvingly, although sensibly adding that chemistry could be reduced to quantum physics only in principle and not in practice.[21]

Møller and Plesset met at a few later occasions. One of them was the 1951 Copenhagen meeting on problems of quantum physics, and another was the 1963 memorial conference for Bohr, where a large number of Bohr's former colleagues and friends gathered in Copenhagen. Neither of the two Copenhagen authors considered their work to be important. Thus, when Plesset was interviewed in 1981, he did not refer to Møller or the work he did with him. And yet, in a certain sense, namely as given by its later impact, the 1934 paper turned out to be far the most important of all of Møller's many publications. But how could he have known? As a leading quantum chemist stated in a review paper of 2011:

> In 1934, Møller and Plesset described in a short note of just five pages how the Hartree–Fock (HF) method can be corrected for electron pair correlation by using second-order perturbation theory. This approach is known today as Møller–Plesset perturbation theory, [which] … although in the beginning largely ignored, had a strong impact on the development of quantum chemical *ab initio* methods in the past 40 years.[22]

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20. Dirac (1929), p. 714, emphasis added. In a letter to Heitler from 1935, London expressed his and some other theoretical physicists' lack of respect for traditional chemistry: "The chemist is made out of hard wood and he needs to have rules even if they are incomprehensible." Quoted in Gavroglu and Simões (2012), p. 100.

21. Møller (1977b), p. 13.

22. Cremer (2011), p. 510. See also Kragh (2022). Møller, who had no interest whatsoever in chemistry, would have been surprised had he known about the Wikipedia article on him, where he is described as "a Danish chemist and physicist." https://en.wikipedia.org/wiki/Christian_M%C3%B8ller

The number of citations to the Møller-Plesset paper clearly shows that its impact only became noticeable in the modern era of computational chemistry some years after Møller's death. At the mid-1970s the British-American theoretical chemist and later Nobel laureate John Pople developed new versions the Møller-Plesset theory, which made the old paper of 1934 much better known. Pople concluded that the original method carried to second and third order had advantages over other methods and for small atoms and molecules agreed satisfactorily with experimental data. When Pople in 1998 gave his Nobel lecture in Stockholm, he praised the Møller-Plesset theory as a major step in the history of computational chemistry.[23]

From 1934 to 1962 the Møller-Plesset paper received just 22 citations, which means that it was nearly invisible in the scientific literature. Then, in the following 18-year period the number of citations increased drastically to 1,370 and by mid-2022 the total number had exploded to about 16,700. By comparison, the total number of citations to Møller's two papers on scattering and collision theory is 212 for the 1931 paper and 732 for the 1932 paper.[24] By far most of the citations to the 1934 paper were in journals devoted to chemical physics and quantum chemistry. The paper by Møller and Plesset is a prime example of what is known as a 'sleeping beauty', meaning a scientific paper whose relevance has not been recognised for a long time and then, more or less suddenly, becomes highly influential and much cited.[25] Such sleeping beauties are of obvious interest from a historical and sociological point of view. Why were they initially ignored? Why did a sleeping beauty wake up at a particular, much later date?

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23. Pople (1998).

24. These numbers are from Google Scholar as of July 2022. For unknown reasons, the numbers from Web of Science are a little less, namely 13,800 (1934, Møller-Plesset), 172 (1931, Møller), and 520 (1932, Møller).

25. See Ke et al. (2015), who refer to the Møller-Plesset paper as one with a very high "beauty coefficient", an expression of the number of citations a paper has received and how long after publication it gained them. According to the authors, the beauty coefficient of the 1934 paper is a little higher than that of the much better known Einstein-Podolsky-Rosen or EPR paper of 1935 dealing with the completeness of quantum mechanics.

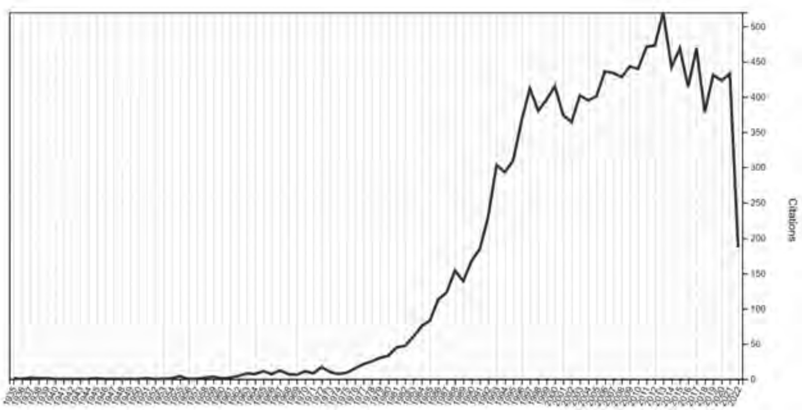

Fig. 12. Number of citations to the 1934 paper by Møller and Plesset from 1935 to the summer of 2022. None of them are self-citations. Data from Web of Science.

About thirty years after the Møller-Plesset paper, the Austrian-born American physicist Walter Kohn created another highly successful approach to the many-particle problem in quantum mechanics, the so-called density functional theory. Much like Møller and Plesset, Kohn was a theoretical physicist whose work unintendedly came to play a crucial role in quantum and computational chemistry. Indeed, in 1998 he was awarded the Nobel Prize – not in physics but in chemistry.[26] Of relevance to the present context, in early 1951 27-year-old Kohn arrived on a fellowship to Copenhagen to work at Bohr's institute with Møller as his supervisor. During his stay in Copenhagen he collaborated with another visitor, the Swiss mathematical physicist Res Jost, with whom he wrote a couple of papers on scattering theory. Kohn recalled in his Nobel autobiography:

26. Kohn, who shared the prize with Pople, was far from the first physicist to receive a chemistry Nobel Prize. A partial list of chemistry prizes awarded to physicists 1908-1977 is given in Kragh (1999), p. 432. For Kohn's life and route to the density functional theory, see Zangwill (2014).

> Originally I had planned to revert to nuclear physics there, in particular the structure of the deuteron. But in the meantime I had become a solid state physicist. Unfortunately no one in Copenhagen, including Niels Bohr, had even heard the expression 'Solid State Physics'. ... Very exciting work was going on in Copenhagen, which eventually led to the great 'Collective Model of the Nucleus' of A. Bohr and B. Mottelson, both of whom had become close friends. Furthermore my family and I had fallen in love with Denmark and the Danish people. A letter from Niels Bohr to my department chair at Carnegie quickly resulted in the extension of my leave of absence till the fall of 1952.[27]

Kohn participated in the large Copenhagen conference on problems in quantum physics held in July 1951 and attended also by Møller and Plesset, not to mention Heisenberg, Bethe, Pauli, and a host of other quantum luminaries (Section 5.4). Although he was in close contact with Møller, there is no record of any professional or social interaction between the two. When Kohn later developed his density functional theory, which was widely considered an alternative to the Møller-Plesset perturbation theory, Møller showed no interest.

3.2. Chandrasekhar versus Eddington

As we have seen, Møller only once dealt with solid-state physics and also only once with quantum-chemical calculations. Likewise, apart from his late work on black holes, which will be surveyed in Section 7.3, he got involved in astrophysics only at a single occasion. The young Indian astrophysicist Subrahmanyan Chandrasekhar studied for his PhD degree in Cambridge under Ralph Fowler and Dirac. On their recommendation he spent most of a year in Copenhagen, arriving in August 1932 and returning in May 1933.[28] Like many other young visitors – he was only 21 years old – Chandrasekhar

27. Walter Kohn, Nobel autobiography, online as https://www.nobelprize.org/prizes/chemistry/1998/kohn/biographical/. Kohn worked in Copenhagen from January 1951 to September 1952. Møller and Bohr to P. R. Wallace, 5 March 1953 (CMP).
28. Fowler to Bohr, 13 May 1933 (BSC).

looked forward to see and learn from Bohr, the fabled Danish physicist. "It could be said *only* of Bohr", he wrote to his father, "that he is not only a great mind but one whose influence on the contemporary geniuses ... has been colossal. In fact, in the whole range of mathematical and physical history, it would be difficult to find Bohr's equal."[29]

At the institute in Copenhagen, Chandrasekhar found a congenial, informal and international atmosphere, something he had sorely missed in Cambridge. He was welcomed by Strömgren, whom he already knew by correspondence, and he also met and interacted with Rosenfeld, Placzek, Delbrück, and other physicists. He came to know Rosenfeld particularly well. In early March 1933 Rosenfeld arranged some lectures for him in Liège, Belgium, and the two physicists travelled together from Copenhagen to Liège. Although astrophysics was of little concern to Bohr, he valued Chandrasekhar's research and considered his stay to have been beneficial not only for the young Indian but also for the institute. "He has been successfully engaged in the theoretical treatment of a number of important astrophysical problems", Bohr wrote in a report of 18 April 1933, "as well in the choice of these problems as in the methods used for their solution, he has shown great ingenuity and ability. In my opinion he may be regarded as one of the most competent among the younger astrophysicists, as to whose future scientific activity great expectations are justified."[30]

Chandrasekhar was on the whole pleased with his stay at Bohr's institute. On the other hand, he could not help feeling that he was an outsider and that his own work in astrophysics was not much appreciated among the quantum physicists. "I didn't belong to the scientific community any more in Copenhagen than in Cambridge", he said in an interview. "Most often I was so outside the main

29. Letter of 15 June 1932, quoted in Wali (1991), p. 99.
30. BSC, Supplement. Bonolis (2017), pp. 345-346. On Bohr and astrophysics, see also Kragh (2017). In 1983, Chandrasekhar was awarded the Nobel Prize in physics for "his theoretical studies of the physical processes of importance to the structure and evolution of the stars."

stream of things, I was never part of the scene."[31] Chandrasekhar undoubtedly also met Møller in Copenhagen, but there is no record of any professional or social contact between them at the time. That only came later, namely during Møller's stay in Cambridge in the spring and summer of 1935.

Since early 1935 Chandrasekhar had been involved in an unpleasant and now-famous controversy with Sir Arthur Eddington concerning the physics of white dwarf stars.[32] In a nutshell, while the famous Eddington was convinced that the white-dwarf state was the stable end station for all stars, the unknown Chandrasekhar argued that this was not the case for very massive stars. He demonstrated from fundamental physics that there must exist a critical mass above which stars continue to contract and radiate away energy. According to Chandrasekhar, the critical mass could be expressed in terms of the solar mass $M_\odot$ as

$$M_{\text{crit}} = 5.84 M_\odot / \mu_e^2$$

where μ_e denotes the average molecular weight per electron, a quantity which depends on the chemical composition of the star. The controversy was not so much about astronomy as about the proper use of relativity theory and quantum mechanics in regions of extreme matter density. Eddington bluntly attacked Chandrasekhar's conclusions for being based on wrong physics. Not only had the 24-year-old Indian used Pauli's exclusion principle incorrectly – so Eddington claimed – he also had applied the concept of relativistic quantum degeneracy to areas where it was allegedly invalid.

In a state of despair, Chandrasekhar asked his friend Rosenfeld for help. "Could Eddington be right? I should very much like to know Bohr's opinion." And in another letter: "I should be awfully glad if Bohr could be persuaded to interest himself in the matter. If somebody like Bohr can authoritatively make a pronouncement

31. AIP interview with Spencer Weart, 17 May 1977. https://www.aip.org/history-programs/niels-bohr-library/oral-histories/4551-1

32. For the white dwarf problem and the Chandrasekhar-Eddington controversy, see Bonolis (2017), Wali (1991), pp. 124-146, and Miller (2005), pp. 104-119.

Fig. 13. Subrahmanyan Chandrasekhar, portrait photograph of 1938.
Credit: Niels Bohr Archive, Photo Collection, Copenhagen.

in the matter, it will be of the greatest value in the matter."[33] It was all about authority, Bohr's in quantum theory versus Eddington's in astrophysics. Rosenfeld assured his friend that Eddington was completely wrong and that Bohr as well as quantum experts like Dirac and Pauli very much agreed. However, none of them wanted to stand up against Eddington by entering publicly in the controversy. This is where Møller entered.

> Chandrasekhar was rather unhappy, because Arthur Eddington had got an idea which nobody believed in, that the Dirac equation could not be applied to the electrons in the stars, and he wanted to change the equation, and he got some other formula for the equation of state, for an electron gas in the stars. And so he [Chandrasekhar] came to me and told me about it, and finally we wrote a paper which was meant against Eddington's view.[34]

Chandrasekhar only responded in print to Eddington's attack in his and Møller's joint paper submitted to the *Monthly Notices of the Royal Astronomical Society* on 7 June 1935. In this brief paper, the two authors used Dirac's relativistic quantum mechanics to derive the basic results of Chandrasekhar's theory, including the upper limit for the mass of a stable white star presently known as the Chandrasekhar limit (which is about 1.4 times the mass of the Sun). Technically they objected to Eddington's use of the energy-stress tensor $T_{\mu\nu}$, which he defined in a way that disagreed with the one accepted in quantum mechanics such as given by Pauli in his authoritative work of 1933 titled *Die allgemeinen Prinzipien der Wellenmechanik*. From this discrepancy and Eddington's heterodox interpretation of the exclusion principle, "it is now possible to see why Eddington obtains a result different from the usual treatment of a degenerate gas."[35] At the end of their paper, they diplomatically stated that "we do not intend this note as a reply in any sense to Eddington's papers."

33. Chandrasekhar to Rosenfeld, 12 and 26 January 1935, quoted in Wali (1991), pp. 129-131.
34. Weiner (1971b).
35. Møller and Chandrasekhar (1935), p. 674. See also Miller (2005), p. 117.

But of course, this is what it was, and Eddington knew. He flatly denied the implicit accusations and in a subsequent note he defended his view:

> In recent papers I have contended that the 'relativistic' degeneracy formula is erroneous. This has led Møller and Chandrasekhar to publish a note defending it. They give a derivation of the formula which is doubtless more up to date than those which I criticized. It therefore seems desirable that I should amplify my attack on the formula by showing why I am unable to accept Møller and Chandrasekhar's proof.[36]

Eddington's note only appeared in *Monthly Notices* after Møller had returned to Copenhagen. In late November, shortly before Chandrasekhar left England for a visit to the Harvard College Observatory, he wrote to Møller: "I am afraid that you will be rather annoyed by what I am enclosing! I daresay, you never wanted to get mixed up in this, but at least Eddington gets out surely the worse for this. Dirac thinks that Eddington is mad – so do all of us!'[37] Indeed, the community of theoretical physicists almost unanimously sided with Chandrasekhar against Eddington.

Among the Copenhageners, also Rosenfeld published a note with Chandrasekhar which implicitly – it did not mention Eddington by name – was a contribution to the controversy.[38] On the top of that, on Chandrasekhar's instigation Peierls entered the debate. Citing the Møller-Chandrasekhar paper and noting that "some controversy has arisen as to whether there is an equation of state in the usual sense of the word", Peierls proved from standard quantum mechanics that this was indeed the case. Many years later, he recalled that he sent the paper to *Monthly Notices* "because the

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36. Eddington (1935).

37. Chandrasekhar to Møller, 21 November 1935 (CMP).

38. Chandrasekhar and Rosenfeld (1935). In a letter to Chandrasekhar of 5 July 1935, Rosenfeld joked: "The story of Eddington's degeneracy (if I may use such an ambiguous expression) takes the shape of the *Iliad*, with the various gods and heroes coming in." Quoted in Miller (2005), p. 117.

point would be obvious to the physicists, but not necessarily to astronomers."[39]

Eddington had since 1929 concentrated on developing an ambitious theory of fundamental physics that unified quantum mechanics and relativistic cosmology. For example, he deduced that the fine structure constant $\alpha = 2\pi e^2/hc$ was related in a simple way to atomic and cosmological constants. With R denoting the radius of the static Einstein universe and N the number of electrons in the universe (which he took to be a fixed number equal to 3.14×10^{79}), he derived the equation

$$\alpha = \frac{mcR}{h\sqrt{N}} = 137$$

As another remarkable result of his theory, Eddington found from purely theoretical considerations an exact value for the mass ratio between the proton and the electron, namely $M/m = 1847.6$. He was much attached to this grand project of a theory of everything, which was one reason why he was so opposed to Chandrasekhar's white-dwarf theory.[40] As Eddington saw it, if this theory was correct, it would undermine his dream of a truly fundamental unification of all forces of nature. Practically all physicists and astronomers either rejected or ignored Eddington's bottom-up reconstruction of physics and in particular his unconventional version of relativistic quantum mechanics. The only significant exception was Schrödinger, who for a while strongly supported Eddington's project, the philosophical grandeur of which appealed to him. He opined that "for a long time to come, the most important research in physical theory will follow closely the lines of thought inaugurated by Sir Arthur Eddington."[41] But Schrödinger was wrong, seriously wrong, and after a couple of years his enthusiasm cooled.

39. Peierls (1936). Peierls to Wali, 5 May 1983, quoted in Wali (1991), p. 135.
40. Miller (2005), p. 109. Eddington (1936), where he elaborated on pp. 235, 253-254 on his objections to the Møller-Chandrasekhar-Peierls concept of relativistic degeneracy for a Fermi-Dirac gas. See Kragh (2017b) for Eddington's theory and its fate.
41. Schrödinger (1937), p. 744.

In early June 1938 a conference on 'New Theories of Physics' was held in Warsaw and Cracow. The conference was organised by the International Institute of Intellectual Cooperation, a commission established 1922 under the League of Nations, and it was cosponsored by ICSU, the International Council of Scientific Unions. Møller and Rosenfeld were not initially invited, but on Bohr's instigation they were, such as we learn from a letter he wrote to Kramers:

> I thought that, if possible, it would be a splendid idea if also Møller and Rosenfeld were invited to the congress in Warsaw. As you know, Møller is among those with the greatest insight in the problems of elementary particles and just in these days he has found some hitherto neglected difficulties regarding the theory of the semi-heavy nuclear particles [mesons]. I am sure that that these would be of great importance in the discussions concerning such questions. As to Rosenfeld, I don't have to tell you how great an expert he is in the problems of quantum electrodynamics. I also want to point out that he is the only real representative of modern theoretical physics in Belgium, just as Møller represents in such an excellent way the younger generation this field in Denmark.[42]

In Cracow, Eddington gave a lecture in front of the peers of orthodox quantum mechanics, including Bohr, Rosenfeld, Klein, Kramers, Gamow, John von Neumann, and Eugene Wigner. Also Charles G. Darwin, Samuel Goudsmit, Paul Langevin, and Léon Brillouin participated. So did Møller, who travelled to Warsaw together with Rosenfeld, all the way discussing problems of physics such as the new meson particle and how to understand it theoretically (Section 5.2). As Bohr brought his wife Margrethe with him to Poland, so Møller was accompanied by his wife Kirsten. Alluding to the political situation in Europe, in a letter shortly after his return to Copenhagen, Møller wrote: "We just came back from the journey ... The trip to Poland was very nice – we met many lovely people – all nations were represented with the exception of the German, Italian

42. Bohr to Kramers, 23 April 1938 (BSC; in Danish).

Fig. 14. The Warsaw-Cracow meeting, photo of 4 June 1938. Eddington sits alone on the front row. On the middle of the second row, from the left, G. Gamow, E. Hylleraas, and L. Rosenfeld; third row from left, J. Destouches, N. Bohr, M. Bohr, E. Wigner, and O. Klein; fourth row from left, R. Smoluchowski, C.G. Darwin, and M. Establier. Credit: Niels Bohr Archive, Photo Collection, Copenhagen.

and Soviet – quite typical."[43] Heisenberg was not permitted by the German authorities to join the conference, but he participated *in absentia* with a paper read by Kramers. At the end of the Warsaw part of the conference, the physicists attended a luncheon party in the Castle of Warsaw hosted by Ignacy Mościcki, president of Poland and a former professor of electrochemistry.

Eddington presented a controversial paper on 'Cosmological Applications of the Theory of Quanta', in which he argued that quantum mechanics, as he understood it, only made sense if linked to cosmology. However, none of the physicists in the Warsaw-Cra-

43. Møller to Charlotte Houtermans, June 1938, reproduced in Shifman (2017), p. 219. See also Section 3.3. Germany and Italy were not members of the League of Nations in 1938. The Soviet Union had become a member in 1935 only to be expelled four years later because of its aggression against Finland.

cow conference agreed and after his talk several of them expressed their utter disbelief in his ideas. Concerning the clash between Eddington and the quantum physicists, Møller recalled: "I remember that Eddington was attacked by most of the other scientists, because nobody could understand what he was doing. The only one who really tried, and that was just like him to try to get some connection with Eddington, was Hendrik Kramers."[44]

The proceedings of the Polish conference clearly illustrate how Eddington on the one hand, and Bohr and his allies on the other, failed to communicate. "[Bohr] thought that the whole manner of approaching the problem which Professor Eddington has taken was very different from the quantum point of view." Eddington, on his side, stated that "he could not understand the attitude of Prof. Bohr."[45] Dirac was not at the Warsaw-Cracow conference, but he fully agreed that Eddington's critique of the standards in relativistic quantum mechanics was illegitimate. Four years later, in a paper co-authored by Rudolf Peierls and Maurice Pryce, he wrote: "The issue is a little confused because Eddington's system of mechanics is in many important respects completely different from quantum mechanics, and although Eddington's objection is to an alleged illogical practice in quantum mechanics he occasionally makes use of concepts which have no place there."[46] Exit Eddington.

Shortly after the end of the conference, Klein wrote to Møller: "Thanks to you and your wife for the cheerful time we had together in Poland and on the way back. It was really a most pleasant journey in spite of (or because of) the troubles. I was also very happy with the scientific discussions I had with you; I shall now try to develop the five-dimensional program a little further."[47] As seen in retrospect, the highlight of the Polish conference was not Eddington's uncon-

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44. Weiner (1971b).

45. *New Theories of Physics* (Warsaw: Scientific Collection, 1939), p. 204. See also Darwin (1938) and Schweber (1994), pp. 95-96. Schrödinger, one of Eddington's very few sympathisers, did not attend the conference.

46. Dirac, Peierls, and Pryce (1942), p. 193. Eddington did not reply.

47. Klein to Møller, 9 June 1938 (CMP). Klein to Bohr, 16 July 1938 (BSC, Supplement).

ventional contribution but the one of Klein titled 'On the Field Theory of Charged Particles'. The theory Klein presented was entirely different from Eddington's and yet it was no less ambitious, as his goal was to construct a unified five-dimensional theory of gravity, electromagnetism, and the quantum forces. The much-discussed Yukawa particle or meson – which Klein called a 'mesoton' – was part of his theory too. As expressed by David Gross, a Nobel Prize laureate of 2004 for his work on so-called asymptotic freedom, what Klein presented in Warsaw was "perhaps the first respectable attempt to construct a *theory of everything.*"[48]

While the other talks in Poland were followed by several remarks in the discussion sessions, Møller was the only one who commented on Klein's talk and seemed to have appreciated it. However, he raised the problem that there was experimental evidence for a heavy neutral Yukawa meson, anachronistically π^0, which particle did not appear in Klein's theory. The Swedish physicist answered that by changing the Lagrangian for the unified field he could easily make it to accommodate a neutral meson as part of the nuclear force. Unfortunately, in this case the strong force between two protons would be repulsive rather than attractive, which obviously posed a problem. Much later Klein's theory came to be seen as a visionary anticipation of intermediate bosons and modern GUT (grand unified theory), but at the time it made no impact at all on the development of physics.

3.3. Fruitful travels abroad

Having obtained his doctoral degree and settled as a lecturer at the Blegdamsvej institute, Møller was keen to proceed with postdoctoral studies in the larger world outside Copenhagen. At some time in early 1933, Bohr suggested that he might apply for a Rockefeller International Education Board scholarship and offered to recommend him. "Placzek will probably go to Rome between the end of April and beginning of May", Majorana wrote from Copenhagen. "Møller is also planning a long stay in Rome, subject to the decision

48. Gross (1995), p. 102. Klein's lecture is reprinted in Ekspong (2014), pp. 87-104.

of the Rockefeller Foundation."[49] It took a while until the plan became a reality. In January 1934, Bohr addressed Fermi on the matter:

> I thank you very much for your kindness in sending me the English translation of your recent beautiful paper on β–ray disintegration. … You will remember that I told you in Brussels that Dr. Chr. Møller very much wishes to come to Rome for a time to work with you, and that you kindly said that he should be welcome in Rome. Now Møller applies for a Rockefeller stipend … He intends to start his work in Rome from the beginning of next autumn semester, if it suits you, and then go to Cambridge sometime in the spring of 1935.[50]

With Bohr's help the fellowship was granted. As mentioned in the letter to Fermi, Møller decided to split his time abroad between Rome and Cambridge. While Cambridge University had a long and glorious tradition in physics, it was only recently that Rome had become an attractive centre of excellence for young physicists. This was due mainly to the dynamic figure of Enrico Fermi, who at the age of 25 was appointed professor of theoretical physics at the Sapienza University in Rome. In the early 1930s, Fermi organised a strong group of young physicists at his institute on Via Panisperna, which included Edoardo Amaldi, Franco Rasetti, Bruno Pontecorvo, and Emilio Segré.

Fermi and his group changed the landscape of Italian physics, and indeed of world physics.[51] In October 1931, he organised an important international conference on nuclear physics in Rome, one of the first on the subject, and two years later he participated as a key figure in the seventh Solvay congress devoted to the structure and properties of atomic nuclei. In both cases Bohr and also Rosenfeld were present, whereas Møller did not participate in either of the

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49. Majorana to his mother, 12 March 1933, in Recami (2020), p. 215.
50. Bohr to Fermi, 31 January 1934 (BSC). Bohr's reference to Brussels was to the seventh Solvay congress 22-29 October 1933. Fermi confirmed the invitation in a letter to Bohr of 7 February 1934 (BSC).
51. Stuewer (2018), pp. 284-302. Segré (1980), pp. 200-209. More details and contexts are given in Guerra and Robotti (2009).

conferences. His long-time association with the Solvay institution dates from a later period.

By the spring of 1934, Møller was familiar with Fermi's version of quantum electrodynamics and the Bethe-Fermi theory of electron-electron interaction, both dating from 1932. He had also studied with great interest a new paper in which Fermi offered a quantum-mechanical explanation of beta decay based on Heisenberg's proton-neutron model of the atomic nucleus and Pauli's idea of the neutrino. According to Fermi, the fundamental process was a transformation of a neutron into a proton, with the creation of an electron and a neutrino: $n \rightarrow p^+ + e^- + \nu$ or in later notation $n \rightarrow p^+ + e^- + \bar{\nu}$.[52] Remarkably, Fermi had first submitted an English version of his seminal paper to *Nature*, which however turned it down. As his collaborator Rasetti recalled, "the manuscript was rejected by the Editor of that journal as containing abstract speculations too remote from physical reality to be of interest to the readers."[53]

As a consequence of *Nature*'s unfortunate decision, the first version of Fermi's theory of beta decay appeared in Italian in December 1933, published in the not widely read journal *Ricerca Scientifica*. It was only when the theory was published in an extended form in *Zeitschrift für Physik* that the neutrino became broadly known and accepted by most physicists. By comparing his theoretical expression for the beta decay spectrum with experimental data near the upper energy limit, Fermi suggested that the neutrino mass was probably zero.

As shown by Fermi's collaborator Gian Carlo Wick a little later, the new beta decay theory could also explain the inverse process as it occurred in the recently discovered artificial radioactivity, namely by $p^+ \rightarrow n + e^+ + \nu$. He interpreted the process in terms of Dirac's hole picture: "If the absorbed electron is an electron with negative kinetic energy, then there is emission of a positron. It is natural to identify this phenomenon with that observed by Joliot and Curie."

52. Fermi (1934a) published 19 March. Fermi and most other physicists in the 1930s did not distinguish between neutrinos and antineutrinos. When they did, they adopted a convention opposite to that used today. Bonolis (2005).
53. Rasetti (1962), p. 540.

Moreover, Wick hypothesised that "instead of a negative-energy electron, it can happen that what is destroyed is an electron in the K, L, M, … orbitals, which form the external structure of the radioactive nucleus."[54] The elementary capture process predicted by Wick can be written as $p^+ + e^- \rightarrow n + \nu$. Slightly later Bethe and Peierls considered the capture of neutrinos by inverse beta decay:

$$\nu + (A, Z) \rightarrow (A, Z \pm 1) + e^{\mp}$$

where the elementary absorption processes are $\nu + p^+ \rightarrow n + e^+$ and $\nu + n \rightarrow p^+ + e^-$. However, they concluded (wrongly as it turned out) that the cross section was much too low to allow neutrinos to be detected in this way.[55] Bethe and Peierls also briefly mentioned the possibility that "a nucleus catches one of its orbital electrons, decreases by one its atomic number, and emits a neutrino."

As Møller later told, his primary reason for choosing Rome as the first destination for his fellowship was his interest in Fermi's exciting theory. As a secondary reason he mentioned Fermi's work with Bethe on the interaction of two electrons, a subject which still interested the Danish physicist. He was at the time only vaguely aware, if aware at all, of the series of experiments that Fermi had initiated on neutron-induced nuclear reactions and which in June 1934 resulted in a sensational but premature suggestion of having produced two transuranic elements. Wisely, in 1934 Fermi did not claim to have actually discovered new elements with atomic numbers 93 and 94, and he did not suggest names for them.[56] However, four years later and less wisely, he did. When Fermi received the Nobel Prize in 1938, it was in part for his "demonstration of new radioactive elements." He was by then less cautious, now referring

54. Wick's paper was in Italian. I quote from the partial translation given in Guerra and Robotti (2009).
55. Bethe and Peierls (1934). Later experiments dating from about 1960 proved that the existence of solar and other neutrinos were of the kind considered by Bethe and Peierls. The experiments relied on the transformation of Cl-37 into the radioactive Ar-37 isotope, which decays by electron capture.
56. Fermi (1934b).

in his Nobel lecture to the elements 'ausonium' and 'hesperium' produced as beta decay products of uranium-239.[57]

Together with his wife Kirsten, in early October 1934 Møller arrived in Rome, where he quickly became acquainted with other visitors and members of Fermi's group:

> There was Emilio Segré and Gian Carlo Wick, and Wick actually was the one I had most contact with when I was there, because he was the only one who was working actively at that moment in theoretical physics, and we shared a room together at the Institute, in the old Institute on Via Panisperna before the University City was built, outside the walls. So it was very cozy. I mean, not very spacious but very nice, a very nice atmosphere.[58]

Just at the time when Møller came to Rome, Fermi attended a conference on solid-state and nuclear physics taking place in London and Cambridge. Among the young and at the time unknown participants was 23-year-old John Wheeler, who had come to Copenhagen in September, shortly before Møller departed for Rome.[59] Upon Fermi's return from England, he and his group started a systematic investigation of neutron-induced reactions which led to the unexpected and hugely important discovery that slow neutrons are much more effective than fast ones in producing certain nuclear reactions. Møller thus arrived at a time when Fermi and most of his group were intensely occupied with neutron experiments and had little time for theoretical work in quantum mechanics. Not very interested in the experiments, Møller was a bit disappointed. "I must say Fermi at that time had no time for theory", he recalled. "That was the time when he changed over really to experimental

57. For the chemical symbols of the two elements he proposed Ao and Hs. Today, the latter symbol refers to the superheavy element hassium with atomic number $Z = 108$.
58. Weiner (1971b).
59. For the London-Cambridge conference, see Stuewer (2018), pp. 311-317. In grief over the tragic death of his oldest son Christian, who had drowned in a sailing accident, Bohr did not attend the conference.

physics. … Well, on the other hand, I could understand that Fermi was more interested in these exciting experiments."[60]

Møller had a few discussions with Fermi, but his contact with him was limited. According to Rasetti, Fermi "always held a slight grudge against Bohr and the Copenhagen school … Oh, he was friendly with Bohr and the other people there. Moller, for instance, came here to Rome and they were very good friends."[61] On Fermi's request Møller gave a colloquium on an important work by Pauli and Victor Weisskopf in which they quantised the Klein-Gordon equation and thereby proved that Dirac's controversial idea of negative-energy particles was unnecessary. Møller may have agreed with Pauli's and others' sceptical or even hostile attitude to Dirac's 'hole' interpretation. In a Danish paper on the positron theory, he wrote: "From a physical point of view it is not very satisfying that in this theory real positrons are treated as 'holes' in a distribution of fictitious negative-energy states."[62] Nonetheless, in his scientific papers he frequently made use of Dirac's imagery of holes.

Møller mostly interacted with Wick, but he also discussed problems in theoretical physics with Bloch, with whom he shortly later published two papers on beta decay. Yet another theorist he met was Giulio Racah, who knew Møller's scattering theory and made use of it in a work on electron pair creation.[63] When Kirsten fell ill in early 1935, Møller travelled with her to Copenhagen, helping her to get a place at a sanatorium. He stayed in Copenhagen for only one or two weeks, after which he returned alone to Rome. During his brief stay, he reported in an institute colloquium about Fermi's discovery of the remarkably high cross-section of slow neutrons. Møller recalled that Bohr became very interested in the phenomenon, which stimulated him to think deeper about the structure of

60. Weiner (1971b).

61. Interview with Rasetti by Thomas Kuhn of 8 April 1963, see https://www.aip.org/history-programs/niels-bohr-library/oral-histories/4995

62. Pauli and Weisskopf (1934). Møller (1935b), p. 187. For Bohr's and Pauli's dislike of Dirac's hole theory, see Kragh (1990), pp. 112-113.

63. Racah (1935), who used a modified version of "Møller's primitive electron interaction" to obtain the cross-section for pair creation.

the atomic nucleus. John Wheeler remembered the colloquium in more vivid terms:

> The news hit me at a Copenhagen seminar, set up on short notice to hear what Christian Møller had found out during his Eastertime 1935 visit to Rome and Fermi's group. The enormous cross sections Møller reported for the interception of slow neutrons stood at complete variance to the concept of the nucleus then generally accepted. ... Møller had only got about half an hour into his seminar account and had only barely outlined the Rome findings when Bohr rushed forward to take the floor from him.[64]

In a later interview, Wheeler repeated that the seminar was in the spring of 1935: "Bohr immediately became terribly concerned, interrupted, walked up and down, talked and talked, and as he talked, one could see the liquid drop model of the nucleus taking shape right there before one's eyes."[65] However, Wheeler's memory failed him in some respects. The seminar did not take place in April, but two or three months earlier, and Møller did not remember any major intervention by Bohr during his talk. Frisch's recollection of the event is vivid too and no more accurate as he dated it to occur in late 1935:

> I remember the colloquium ... Bohr kept interrupting, and I wondered, a bit impatiently, why he didn't let the speaker finish. Then, in the middle of a sentence, Bohr suddenly stopped and sat down, his face suddenly dead; we feared he had been taken unwell. But after only a few seconds he got up again and, with his apologetic smile, he said: "Now I understand it."[66]

Although Møller diligently concentrated on his studies in physics, he could not help noticing that life in Mussolini's Italy was deeply

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64. Wheeler (1979), p. 253.
65. Bičak (2009), p. 684.
66. Frisch (1967), p. 141. See also Bohr (1986), pp. 16-17, where Peierls confronts the statements of Wheeler and Frisch with a personal communication from Møller.

Fig. 15. Portrait photography of Christian Møller, 1936. Credit: Niels Bohr Archive, Photo Collection, Copenhagen.

affected by the atmosphere of fascism. Whether he liked it or not, Fermi was part of Italy's political machinery and his institute in Via Panisperna considered something like a showcase for the fascist state. At the time of Møller's stay, Italy was relatively independent of the Third Reich and there were not as yet any racial laws of the kind known in Hitler's Germany. More than thirty years later, Møller recalled:

> It was, of course a time when from our point of view it was not very nice in Italy. Benedetto [*sic*] Mussolini was at his height, and the Ethiopian War was threatening. I remember this was discussed very much in this small circle. And actually it started then, didn't it, in the summer of 1935. Yes. But at the Institute one didn't feel any of that. I mean, it was only — there were police everywhere. I was nearly arrested one day when I was taking photographs in Via Nazionale. I didn't know that Mussolini was to pass there half an hour later. So a man in ordinary clothes came up and asked me to follow him … they brought me to the restritiere, the office where they registered all the foreigners. I told them who I was and so on … and after some time they let me loose again.[67]

Having completed his stay in Rome in April 1935, Møller did not proceed directly to Cambridge but first returned to Copenhagen to see his wife. Since Kirsten was still ill and spent her time at a sanatorium after an operation, Christian went alone to Cambridge in May and was finally back in Copenhagen in September to take up his duties at the institute. At about the same time, Kirsten and Christian moved to a rented apartment in Ordrup north of Copenhagen.

While in Cambridge, Møller collaborated with Chandrasekhar on the physics of very massive stars such as recounted in Section 3.2. He also met the Polish-born Russian physicist Aleksandr Leipunski, who worked under Rutherford on a two-year stipend making experiments on the neutrino recoil in beta decay. As director of the new Ukrainian Physico-Technical Institute in Kharkov (now Kharkiv), Leipunski invited Møller to visit him, which he eventually

67. Weiner (1971b). Italian forces attacked Ethiopia on 3 August 1935.

did a year later.[68] Bohr kept in touch with Møller during his stay in Cambridge, writing in one of his letters:

> Thanks for your kind letter with the beautiful treatise, which Rosenfeld and I have read with great interest. ... Rosenfeld was very interested in your work on the quantisation of Born's theory; we shall both be happy to know how the question has evolved, in particular with respect to Schrödinger's new formulation of the theory. ... I have recently been very busy with writing a small paper to Physical Review as a reply to Einstein's latest article on physical reality. I attach a copy of the paper and will be pleased to know what you think about it.[69]

The reference to the works by Born and Schrödinger was most likely to the Born-Infeld theory of classical electrodynamics and Schrödinger's development of it.[70] Møller's attempt to find a quantised version of the Born-Infeld equations was apparently unsuccessful as it did not result in a publication.

In the third week of June 1936, Møller participated in a large informal conference at Bohr's institute focusing on nuclear physics and attended by many of the pioneers of quantum mechanics such as Heisenberg, Pauli, Jordan, and Born. Among other participants were Heitler, London, Delbrück, Kramers, Rosenfeld, Weizsäcker, Bhabha, Wick, and Plesset. Immediately after the nuclear physics conference followed another conference in Copenhagen of a very different kind. The Second International Unity of Science Congress, following one in Paris in September 1935, was held 21-26 June 1936 with Bohr giving the opening lecture on his favourite topic 'Causality and Complementarity'.[71] This conference was dominated by philosophers associated with the school of logical positivism,

68. Leipunski to Møller, 30 May 1936 (CMP). On Leipunski and the Kharkov institute, see Kojevnikov (2004), pp. 92-98.

69. Bohr to Møller, 3 July 1935 (CMP), wrongly dated 3 June. The "beautiful treatise" was Møller (1935a) and Bohr's "small paper" received by *Physical Review* on 13 July was his reply to the famous EPR (Einstein-Podolsky-Rosen) paper.

70. Moore (1989), pp. 382-385.

71. Werkmeister (1936). Jacobsen (2012), pp. 129-132.

including Philipp Frank, Otto Neurath, Karl Popper, and Jørgen Jørgensen, a prominent Danish philosopher, but it was also attended by a few physicists such as Jordan and Delbrück. Møller could have participated, but apparently he did not. Although he was not foreign to the viewpoint of logical positivism, the philosophical problems concerning causality in quantum physics and biology did not belong to his fields of interest.

In late August, about two months after the Copenhagen conferences, Møller went to Helsinki and from there to Moscow over Leningrad (the former and later St. Petersburg). The reason for his stay in the Finnish capital was something else, namely to attend the nineteenth Scandinavian Meeting of Natural Scientists, an organisation founded in 1839 as a forum for Scandinavian scientists and medical doctors. On some occasions famous non-Scandinavian scientists were invited to give talks at the meetings, such as Einstein did at the 1923 meeting in Gothenburg, when he spoke on his general theory of relativity. The Scandinavian meetings were important during the nineteenth century but by 1920 they had become scientifically obsolete and largely degenerated to social events. They ended with the one in Helsinki. Despite their lack of scientific justification, Bohr took the meetings seriously as a way of propagating science to a broader audience. For example, when the eighteenth meeting was held in Copenhagen in 1929, he gave an important address on complementarity. Together with Møller and several other Danish physicists from the institute he also participated in the Helsinki meeting, where he gave a general lecture on the properties of atomic nuclei.[72] Møller delivered a talk on the emission of positrons from radioactive bodies in which he suggested that the positron was accompanied by two ordinary electrons and a neutrino (see Section 3.4).

Having arrived in Moscow in early September, Møller spent a few days in the city. At the train station, waiting for the train to

72. Aaserud (1990), p. 237. Apart from Bohr and Møller, the other physicists from the Copenhagen institute were N. Arley, F. Kalckar, T. Bjerge, and E. Rasmussen, who all spoke on subjects related to nuclear physics.

Kharkov, to his surprise he ran into Landau, whom he knew well from Copenhagen:

> He had come back from a holiday in the Mountains, and then he asked me, "Where are you going?" "I'm going to Kharkov." "Which train?" And I told him which train. "Oh, I'm going too." So we went down. We had a sleeper. "Which car?" I said the number. "Oh, that's the same car." "Which bunk?" "It's the same compartment we're in." It turned out I was in the bunk above him. It was a very nice coincidence.[73]

In Kharkov, Møller was impressed by the fine library and the physics laboratory with its ongoing construction of a huge electrostatic Van de Graaff accelerator. He stayed for about a month, giving two colloquia. One, which was on Landau's request, was about Bohr's recent work on nuclear structure and nuclear reactions, a subject that the Russians were eager to know more about. The other colloquium dealt with the new phenomenon of K-capture, a radioactive process where an orbital electron in a K state enters the nucleus and reacts with a proton to form a neutron. Møller's work on this subject resulted in a paper in the Russian but international journal *Physikalische Zeitschrift der Sowjetunion* which was published between 1932 and 1938 and of which Leipunski was the editor (Section 3.4).

Among the people Møller met in Kharkov, apart from Leipunski and Landau, were Ilya Lifshitz, Friedrich (Fritz) Houtermans and Alexander Weissberg. Lifshitz, who later became a leading theoretical physicist, was at the time a 19-year-old student of Landau. Houtermans and Weissberg were both Austrian physicists and dedicated communists who had migrated to Soviet Russia. However, their communist convictions did not prevent them from being arrested and put in jail. Houtermans, who about 1930 collaborated with Gamow and did important work in nuclear astrophysics, stayed in Kharkov from 1935 to October 1937, when he was arrested by

73. The following quotations are from the Weiner (1971b) interview. On Landau and Møller in Kharkov, see Gorelik (1995).

Stalin's secret police. He spent more than two years in a Soviet prison before he was extradited to Germany in April 1940.[74]

In early October, Møller returned "in a very shaky Soviet plane" from Kharkov to Moscow. While in the Russian capital, "I used the opportunity to visit the museums – I remember in particular, the Museum of Modern Art which was very beautiful. They have one of the biggest collections of Impressionists." Before taking the train to Leningrad, he visited two of Moscow's prominent theoretical physicists, Igor Tamm and Yuri Rumer, both of whom had worked extensively in Western Europe. Tamm, a close friend of Dirac and a future Nobel Prize laureate of 1958 (for his work on Cherenkov radiation), was at the time head of the theory department of the Lebedev Physical Institute. Rumer, a Russian Jew and close friend of Landau, had worked with Born and Heitler in Germany until he returned to Moscow in 1932. As Møller had sensed the ominous political atmosphere in Rome, so he sensed it in Kharkov and Moscow: "One had the feeling that the situation became tense, I remember. ... Certainly when I left, the big purge started – I mean, the completely crazy purge started. ... All the physicists at the [Kharkov] Institute were arrested shortly, a few months after."[75] On this occasion Landau avoided being arrested, but in late December 1936 he was dismissed from his professorship. Shortly later he began working in Moscow.

In November 1937 Landau completed a manuscript on stellar energy which he sent to Bohr, asking him to comment on it and submit it to *Nature*. Landau's new theory differed radically from the mainstream view of nuclear fusion processes being the source powering the stars. According to the Russian physicist, the mechanism was the formation of a neutronic core of density ca. 10^{14} g/cm^3

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74. For Houtemans' adventurous life and work, see Amaldi (2013).
75. In what is known as the Great Terror under the Stalin regime 1935-1941, millions of Soviet citizens were killed. Historians have estimated that about one-fifth of all Soviet physicists were arrested or disappeared. A disproportionally large fraction of them were Jews. Several of them died in prison or were executed, among them Matvei Bronstein, Lev Rosenkevich, and Lev Shubnikov. See Kragh (1999), pp. 243-244 and Kojevnikov (2004).

and the subsequent capture of gas particles into it.[76] He described the neutronic core as a state "where all the nuclei and electrons have combined to form nuclei." Bohr liked the idea very much, but after having discussed it with his colleagues in Copenhagen he had second thoughts and suggested that Landau revised the paper. Apparently he asked Møller to formulate how to improve the paper and how to incorporate recent astrophysical works by Friedrich Hund and Bengt Strömgren. However, Landau disagreed and wrote back to Bohr:

> I had the letter from Møller and have again looked at the passages mentioned. Strömgren's assertions are, alas, based on wild Eddingtonian pathology, which is known to be wrong not only on one point but on all points. It is quite impossible to expose all this in a note to my letter in *Nature*. That would take up more space and arguments than the whole paper.[77]

A slightly revised version of the paper, ignoring Strömgren's work but mentioning Hund's, appeared in *Nature* on 19 February 1938. About two months later, Landau was arrested by Stalin's secret police, accused of being a spy of Nazi Germany. He was only released on 29 April 1939, to a large extent due to the intervention of Kapitsa. Still unaware of the unhappy situation, Bohr informed Landau about the discussions in Copenhagen and the forthcoming annual institute conference:

> At the moment, we are in this Institute all very occupied with the new prospects about the origin of the nuclear forces opened by the discovery of the heavy electron and especially has Møller pointed out that real solutions of the Proca equations would provide the most natural way to represent the neutral field necessary to account for the forces between

76. Landau (1938).

77. Landau to Bohr, 1 February 1938 (BSC; in German), reproduced together with other letters in Khalatnikov (1989), pp. 312-317. For the episode, see Miller (2005), pp. 159-160 and Bonolis (2017). See also the interview with Strömgren by Karl Hufbauer of 24 April 1978: https://www.aip.org/history-programs/niels-bohr-library/oral-histories/4907.

> like particles. It would surely be most pleasant and instructive to all of us to discuss these various prospects with you and we hope very much indeed that you this year will be able once again to take part in our annual conference … which is planned to take place in the first week of October.[78]

After Houtermans' earlier arrest, his wife, the physics-trained Charlotte, managed to escape to Riga with her two small children and with the help of Bohr to get on a boat directly to Copenhagen (and not via a German port). Upon her arrival on 25 December 1937, she was stopped in Copenhagen harbour by the immigration authorities, but then "Bohr's assistant, Dr. Møller, appeared with some messages and documents from Niels Bohr."[79] With Møller's assistance, Charlotte and her children were allowed to enter the country and installed at a hotel. She recalled:

> Møller stood by, ordered everything, directed everything. The children were exhausted, they shrieked and cried, they would neither wash, nor drink, nor eat, least of all sleep. … Then Møller calmly announced: "Now you and I shall eat and talk." … Every morning, Møller came like the confidential attaché of a great ambassador … I never saw any money, Møller paid for everything, and the money came from Bohr.

Charlotte Houtermans spent a happy month in Copenhagen, meeting not only Bohr and Møller, but also Margrethe Bohr, Rosenfeld, Placzek, and other physicists at the institute. In late January she took the ferry boat from Esbjerg in Western Denmark to Harwich in England and eventually she ended up in the United States to become a physics teacher at Wellesley College. Charlotte exchanged

78. Bohr to Landau, 5 July 1938 (BSC). Møller (1938a) argued that the Yukawa mesons could be described by the equations that Alexandru Proca had proposed two years earlier (Section 5.2).
79. Shifman (2017), pp. 189-191. Charlotte Houtermans was a remarkable woman with a life no less adventurous than that of her husband. She befriended and corresponded with many of the great physicists of the period, including Pauli, Gamow, Oppenheimer, Franck, Einstein, Born, Blackett, and Rosenfeld.

several letters with Møller, most of them concerned with the efforts to get her husband released but also touching on other matters. In one of the letters, Møller reported: "Frisch sends his regards. Recently, he has performed some wonderful experiments with scattering of neutrons off uranium. In theoretical nuclear physics, there are lots of exciting things, too. I have worked together with Rosenfeld quite a lot and we got some very nice results."[80]

It is hard to tell from Møller's published works if he had any views on the political-ideological struggle during the turbulent 1930s, when the democratic system was challenged by totalitarian systems from the right as well as from the left. Although he may appear to have been a typical ivory-tower scientist, he did have such views and was to a limited extent involved in the struggle if in a not very visible way. Like many other scientists and intellectuals at the time, his sympathies were strongly leftist and anti-fascist. This is what he indicated in one of the 1971 interviews:

> We thought that actually what Communism stood for was good, and that was the onıy way to get out of these constant crises ... Then, of course, also under the influence of our friends coming from Hitler Germany and telling us what was happening there, and we had the feeling that the only real opponents to the Hitler barbarism was Communism, and we believed in that ... I also took part in some of the organization to help the refugees, and many of them were Communists, not only the physicists here. Most of them also were Communists in heart, although — well, I can say for my own, it was of course a little theoretical. We couldn't quite see how it could work out. But on the other hand, I must say, when I took part in the elections, I never voted Communist, because I always felt that Danish Communists, they're no good.[81]

80. Møller to C. Houtermans, 13 February 1939, quoted in Shifman (2017), p. 245. The reference is to Otto Frisch's work on the fission of uranium conducted at Bohr's institute (Section 4.1). For the Møller-Rosenfeld collaboration on meson theory, see Section 5.2.
81. Weiner (1971c).

Following the fateful year of 1933 several Danish initiatives were created with the aim of helping refugees from the Third Reich. By 1937 German migrants numbered more than 1500, among them many scientists and intellectuals. The most important of the initiatives was the government-sanctioned Danish Committee for Support to Refugee Intellectuals (*Den Danske Komité til Støtte for Landflygtige Aandsarbejdere*) of which Bohr and his brother, the mathematician Harald Bohr, were both board members.[82] There was a number of other, smaller and more short-lived help committees, some of them established on communist initiative or dominated by communists and their sympathisers. Together with individuals from Denmark's leftist cultural-political scene, Møller served as a board member of one of these groups, the Central Committee for German Immigrants (*Centralkommissionen for Tyske Emigranter*) founded in 1937.[83] It is unknown what kind of work he did for the group or what relations he had to other board members.

There is some further indication of Møller's political stance at the time in a letter he wrote to Fritz Kalckar, Bohr's young collaborator in the area of nuclear structure and reactions. As usual, Møller dealt mostly with physics, but in between he also briefly related to other matters:

> I have recently begun a general investigation on which of the relativistic wave equations result, like the Klein-Gordon equation according to Pauli-Weisskopf, in positive energies. Before Easter I was able to establish some general criteria and I now start examining the various possibilities. Of course, the goal is to find a theory for electrons and positrons in which there are no negative energies, so that one does not need to fill up [the negative-energy levels]. Perhaps this is not impossible. ... *La situation européen est très grave* – it is astonishing how fascism can still roll on with impunity (so far). ... But I must end, Ole [his small son] demands that I play with him.[84]

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82. Pais (1991), pp. 381-383.
83. Dähnhardt and Nielsen (1986), p. 37.
84. Møller to Kalckar, 1 April 1937 (CMP), italics added. Kalckar was at the time travelling in the United States. Møller's worries about the march of fascism may have been a reference to the Spanish civil war or the Italian invasion of Ethiopia.

In the year of 1938, Møller travelled abroad twice. As mentioned in the previous chapter, in June he attended the international meeting on new theories in physics in Warsaw and Cracow, where he met Eddington and other people. Shortly later he participated in the 106th meeting of the venerable British Association for the Advancement of Science founded in 1831, which took place in Cambridge in the second week of August.[85] The meeting included a symposium on nuclear physics with Bohr giving an introduction and another symposium led by Blackett was on 'High-Altitude Cosmic Radiation'. Among the lecturers were, among others, Casimir, Cockcroft, Peierls, Williams, and Van Vleck. Møller, who did not himself give a talk, may also have listened to 82-year-old J. J. Thomson speaking on his unorthodox, non-quantum ideas of what he called electronic waves. It was the last time that Bohr met the great physicist with whom his glorious scientific career had started back in 1911.

3.4. Works on beta radioactivity

Fermi derived in his theory of 1934 an expression for the long-mysterious continuous spectrum of the beta electrons. For the strength of the decay he introduced a new constant, the Fermi constant, which he calculated to be approximately $g_{\mathrm{F}} = 4 \times 10^{-50}$ cm^3 erg or 4×10^{-37} m^3 J (the present value is 1.435×10^{-36} m^3 J). The expression for the spectral distribution involved the unknown mass of the neutrino and the observed value of the upper energy limit of the beta spectrum. As Fermi proved, the two quantities were related, and from available experimental data he suggested that the neutrino was probably massless, hence moving at the speed of light. From a conceptual point of view, his theory relied crucially on the assumption that the shadowy neutrinos proposed by Pauli were real particles. The assumption was accepted by many but not all physicists. In fact, throughout the 1930s the reality of the neutrino was considered controversial in some quarters of the physics community. Thus, while Dirac had initially been sympathetic to Fermi's theory and

85. *British Association for the Advancement of Science, Report of 1938* (London: Office of the British Association, 1938).

its associated neutrino, in 1936 he surprisingly changed his mind. He now argued, if not for long, that the neutrino was nothing but a phantom postulated with the sole reason of maintaining energy conservation in beta decay.[86]

For a while Bohr belonged to the sceptics, such as he made it clear in a letter to Bloch of February 1934: "We are of course ... very interested in Fermi's new paper which no doubt will be very stimulating for the work on electric nuclear problems, although I must confess that I don't yet feel fully confident of the physical existence of the neutrino."[87] Less than a month later, Gamow wrote to Goudsmit: "Bohr, on the other hand, well you know that he absolutely does not like this chargeless little thing, thinks that continuous beta structure is compensated by the emission of gravitational waves which play the role of neutrino but are much more physical things."[88] A week later, now in a letter to Pauli, Bohr more or less retracted his speculation that the neutrino might be a quantised gravitational wave, a kind of graviton:

> The idea was that a neutrino, for which one assumes a rest mass 0, certainly can be nothing else than a gravitational wave with suitable quantisation. I have convinced myself, however, that the gravitational constant is much too small to justify such an opinion, and I am therefore fully prepared to accept that here we really have a new atomic trait before us, which could be tantamount to the real existence of the neutrino.[89]

Møller seems not to have shared Bohr's reservations and he ignored Dirac's later return to energy non-conservation. Like most of his colleagues, he found Fermi's theory to be convincing and believed that the neutrino was a real spin-½ particle with zero mass. He entered the field of beta radioactivity with two notes in *Nature* of

86. Kragh (1990), pp. 169-174. The history of the neutrino and early alternatives to it is covered in Franklin (2001).

87. Bohr to Bloch, 17 February 1934, in Bohr (1986), p. 541.

88. Gamow to Goudsmit, 8 March 1934, quoted in Jensen (2000), p. 176.

89. Bohr to Pauli, 15 March 1934, in Pauli (1985), p. 308. Bohr (1936a) unequivocally accepted the Pauli-Fermi neutrino.

December 1935, both of them written jointly with Bloch, who in October was in Copenhagen for a meeting celebrating Bohr's fiftieth anniversary.[90]

At the time Fermi's theory had been questioned by the young American physicist Emil Konopinski, who in a work with his thesis advisor George Uhlenbeck pointed out that a detailed examination of beta energy spectra in the low energy region disagreed with the original Fermi theory. Consequently, Konopinski and Uhlenbeck came up with an alternative theory of beta decay, a modification of Fermi's, which agreed better with available data. During the period from 1935 to about 1938 the Konopinski-Uhlenbeck (K-U) theory attracted much attention and was generally accepted as superior to Fermi's. However, By the early 1940s it turned out that Fermi's theory was after all better than the K-U theory, with the result that the latter theory soon disappeared from the physics literature.[91]

Møller and Bloch investigated in their first note the recoil of a light nucleus when emitting a beta electron and a neutrino. They predicted an angular correlation between the direction of emission of the electron and that of the recoiling nucleus, performing their calculations of the expected recoil distribution on both the Fermi interaction theory and the K-U alternative. Since the results were different, they suggested that "Experiments on the β-recoil might thus enable a decision to be made between the alternatives." No such experiments existed at the time, but at the Cavendish Laboratory Leipunski was preparing this kind of delicate experiment in an attempt to measure the distribution of the B-11 nuclei produced in the decay process

$$^{11}_{6}\mathrm{C} \rightarrow {}^{11}_{5}\mathrm{B} + e^{+} + \nu$$

However, the accuracy of the experiment was insufficient to provide a real test of the kind envisaged by Bloch and Møller. According to Leipunski, "the only conclusion that may be drawn is that these

90. Bloch and Møller (1935a) and (1935b). Both papers dated 26 October.
91. Franklin (2001), pp. 90-97. Franklin (2005).

results are in favour of the emission of neutrinos during β decay."[92] As mentioned in Section 3.3, Møller had met Leipunski in Cambridge and would later meet him again in Kharkov. As Møller recalled, "He made already in Cambridge some experiments on the neutrino recoil in beta decay, and — well, this renewed my interest also in beta decay theory, and then Felix Bloch came, did a work together there."[93]

In their second note to *Nature*, Møller and Bloch considered the possibility that a proton might be transformed into a neutron by bombarding the first particle with high-energy electrons. The idea of electron-induced nuclear transmutations was not new, as the Japanese physicist Hantaro Nagaoka as early as 1925 had bombarded the mercury isotope Hg-199 with electrons and sensationally but erroneously claimed to have transmuted some of the mercury atoms into Au-199.[94] However, Nagaoka's gold-making experiment was based on the old proton-electron model of the nucleus which by 1935 was long abandoned. In the case considered by Møller and Bloch, the net result would be the same, namely that the atomic number of the target element reduced by one unit, (A, Z) and $(A, Z-1)$. As Møller phrased it in a letter from Cambridge to Bohr:

> Together with Bloch we [Møller and Hulme] calculated the probability for the transformation of protons into neutrons by bombardment with very fast electrons according to Fermi's theory. We take the necessary neutrinos from the filled-up negative [energy] states. If Fermi's theory is correct, such a process should be possible although its cross section is likely to be so small that presently the effect cannot be detected by means of experiments.[95]

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92. Bloch and Møller (1935a). Leipunski (1936), p. 303. See also Franklin (2001), pp. 84-85.
93. Weiner (1971b).
94. Nagaoka (1925). Improved experiments of the same kind were made in 1928 by the American physical chemist William Harkins, who found no trace of gold.
95. Møller to Bohr, 26 August 1935 (BSC).

In their version in the published paper Møller and Bloch similarly noted that the hypothetical neutron-formation process $e^- + \nu + p^+ \rightarrow n$, if possible at all, required a neutrino source. "Such a source, however, is not necessary", they commented, "if it be admitted that in empty space all negative neutrino states are occupied in the same way as the negative energy states of the electron in Dirac's theory of the positron." Without using the name, they referred to the antineutrino previously considered by Wick and others.

Møller and Bloch further considered the process $\nu + p^+ \rightarrow n + e^+$ $(Z \rightarrow Z - 1)$ but found it to be ruled out by mass determinations, since it required $m_n - m_p < m_e$. At the time the best values in atomic mass units were $m_n = 1.0080$ and $m_p = 1.0073$, and with $m_e = 0.00054$ the inequality is clearly violated. This process and also the corresponding process $\nu + n \rightarrow p^+ + e^-$ $(Z \rightarrow Z + 1)$ had previously been considered by Bethe and Peierls, who likewise declared it "absolutely impossible."[96] Finally, Møller and Bloch calculated on the basis of both the Fermi theory and the K-U theory "the rate of transition of a hydrogen atom into a neutron", that is, the capture of an orbital electron according to

$$e^- + p^+ \rightarrow n + \nu$$

Finding for this hypothetical process a cross section of only 1.7×10^{-44} cm^2 they concluded that it was improbable and beyond experimental detection. Hence, "a transformation of protons into neutrons could only occur by bombardment with electrons of high energy."[97]

In yet another communication to *Nature*, this time submitted in early 1936, Møller discussed the probability of a process which can be written as

$$n \rightarrow p^+ + e^- + (e^- + e^+) + \nu$$

96. Bethe and Peierls (1934).

97. Bloch and Møller (1935b). During the 1930s there was a great deal of uncertainty with respect to the precise mass of the neutron and whether or not it would be unstable in a free state. It took more than a decade until free neutrons were proved to be radioactive with a life-time of approximately 15 minutes. See Amaldi (1984).

He again appealed to the imagery of Dirac's hole theory: "It may happen that an electron in a negative energy state during the creation of the β-particle makes a transition to a state of positive energy, so that we have a process in which a neutron is transformed into a proton by simultaneous creation of two electrons, a positron and a neutrino."[98] Møller calculated the probability to be proportional to $(g_F e^2)^2$ or $(g_F \alpha)^2$, where g_F is the universal Fermi constant and $\alpha = 2\pi e^2/hc = 1/137$ is the fine structure constant. With N^+/N^- denoting the number of emitted positrons relative to the number of negative beta electrons and k being a function increasing with the maximum energy of the beta spectrum, he expressed the relationship as

$$N^+/N^- = k\alpha^2$$

From this he derived a value of ca. 10^{-4} for the N^+/N^-ratio. Moreover, he found that the upper limit of the β^+ spectrum must be smaller than the corresponding limit of the β^- spectrum by an amount of $2m_e c^2$ or approximately 1 MeV. Comparing his predictions with recent measurements reported by Russian physicists, Møller judged that they were satisfactorily confirmed.

Møller referred to experiments made at the new Positron Laboratory established at the Leningrad Institute for Physics and Technology in 1934. The head of the laboratory was Armenian-born Abram Alichanow who with his collaborators specialised in measurements of the energy spectrum of electrons and positrons by means of an advanced magnetic spectrometer.[99] Alichanow and his group found on the basis of the K-U theory that the neutrino must have a finite mass, which they estimated to 0.3 – 0.8 m_e depending on the nature of the beta emitter. As Møller was interested in Alichanow's experimental results, so the Russian physicist was interested in Møller's

98. Møller (1936a), dated 10 January and published 22 February. Møller's communication to the 1936 Helsinki meeting of Scandinavian scientists, Møller (1936b), was essentially a summary of his *Nature* note.

99. Franklin (1980), pp. 17-18. Alichanow (1904-1970) was an important figure in Soviet physics. Later in his career he worked in the Soviet nuclear weapons program and also in elementary particle physics. See Abov (2004).

theoretical work. In a letter to Møller of early 1936, Alichanow wrote:

> A comprehensive account of our experiments will appear in Jour. de Physique ... and I can send you a copy of our article when it is published. Your communication on the theoretical treatment of the 'inner conversion of the energy of β rays' was of great interest to us; in fact, for some time ago Dr Hulme wrote me that he could not imagine how this effect could possibly be theoretically justified. ... With this letter I enclose the curves representing the energy spectra of the positrons emitted by Th and RaC.[100]

So-called internal conversion – a process in which the gamma ray energy of an excited nucleus is transmitted to the atomic electrons through the action of the nuclear field – had been studied since the late 1920s. In 1933 Oppenheimer and his student Leo Nedelsky were the first to point out the possibility of 'internal pair creation', namely that the gamma ray energy if larger than 1.02 MeV may be converted to an electron-positron pair.[101] The novelty of Møller's paper was that he considered the internal conversion of beta ray energy to e^-e^+ instead of or in addition to the conversion of gamma ray energy.

Møller's optimism with regard to the agreement between his theory and experiments did not last long. A more exact calculation made jointly with the young Danish physicist Niels Arley resulted in a disconfirmation rather than a confirmation. In a paper published in the proceedings of the Royal Danish Academy, the two physicists took into account not only the Fermi interaction but also the electron-electron and electron-proton interactions. After long and complicated calculations they arrived at the disappointing result $N^+/N^- = \text{ca.}\, 10^{-7}$, which clearly disagreed with measurements. The Møller-Arley theory also predicted a sharp rise of the positron-electron fraction with the maximum beta energy, another feature dis-

100. Alichanow to Møller, 16 January 1936 (CMP; in German). The forthcoming paper referred to in the letter was Alichanow, Alichanian, and Kosodew (1936).
101. Oppenheimer and Nedelsky (1933).

Fig. 16. Copenhagen conference, September 1937. First row: N. Bohr, W. Heisenberg, W. Pauli, O. Stern, L. Meitner, R. Ladenburg, J. C. G. Jacobsen. Second row: V. Weisskopf, C. Møller, H. Euler, R. Peierls, F. Hund, M. Goldhaber, W. Heitler, E. Segré. Also present: G. Placzek, C. F. von Weizsäcker, A. Mercier, J. H. D. Jensen (standing with L. Rosenfeld and G. Wick), N. Arley, O. Frisch, E. Rasmussen, F. Kalckar, and H. Levi. Credit: Niels Bohr Archive, Photo Collection, Copenhagen.

agreeing with the experiments of Alichanow and his co-workers. "The positrons detected [in these experiments] have presumably a quite different origin", they confessed.[102]

The possibility of an outer electron being captured by the nucleus, what is known as electron- or K-capture, was the main content of two of Møller's publications on beta radioactivity. The net result of this kind of a K-capture process is the same as in positron emission, a change of the nucleus from (A, Z) to its isobaric state $(A, Z - 1)$. However, in the capture process an electron from the

102. Arley and Møller (1938), p. 26. Møller's co-author Niels Arley (1911-1971) graduated in 1935 and worked for a period as Bohr's scientific secretary. At about 1950 he turned to geophysics and oceanography, albeit without much success, and eventually he lost contact with the Copenhagen physics institute.

next shell (L, $n = 2$) is likely to fall into the hole (K, $n = 1$) and as a result the daughter nucleus $Z - 1$ will emit a characteristic X-ray line of the type called K_α in the classification scheme originally devised by Henry Moseley.

Shortly after having returned from Kharkov, Møller submitted to the *Physikalische Zeitschrift der Sowjetunion* a detailed examination of "another process, in which a nucleus of charge Z transmutes into a nucleus of charge $Z - 1$, namely when an atomic electron is absorbed."[103] His calculations indicated that for heavy elements, this new kind of radioactivity would be much more probable than positron emission. Making calculations on the basis of both the Fermi and the K-U theory, Møller obtained different cross-sections, which made him suggest that "this may offer a new possibility of distinguishing experimentally between the two approaches." Realising that the Soviet journal might not be well known in the Western countries, a month later he sent a summary version of his K-capture theory to *Physical Review*. This version appeared in January 1937, before the larger article in the Soviet journal. For the ratio between the probabilities of the capture process and the positron emission process Møller found $\lambda_K/\lambda_+ = 9$ for the Fermi theory and $\lambda_K/\lambda_+ = 47$ for the K-U theory. He concluded:

> From a theoretical point of view it would therefore be of great value if this ratio could be determined experimentally. Since the capture of a K electron will always be followed by the emission of a quantum belonging to the characteristic x-ray spectrum of the element formed by the process, the ratio λ_K/λ_+ is equal to the ratio between the number of x-rays and the number of positrons emitted in a given time interval.[104]

According to Møller, his theory of K-capture explained some anomalous results found by Ernest Lawrence and his assistant James Cork, who in cyclotron experiments of 1936 had produced the radioactive isotope Pt-193 and studied its emittance of positrons ($^{193}\text{Pt} \rightarrow {}^{193}\text{Ir} + e^+$). However, the measured number of positrons was

103. Møller (1937a), p. 11.
104. Møller (1937b), p. 85.

too low to fit the data and so Møller suggested that the experiments could only be understood if the capture process $^{193}Pt + e^{-} \rightarrow {}^{193}Ir$ was taken into account. The evidence for the process would be a distinct K_{α} line.

Back in Copenhagen after his journey to Soviet Russia, Møller reported to his peers about his work on electron capture. "The paper on the capture of K electrons, that was done when I was in Russia in 1936. When I came back, and I talked about it, Jacobsen started to make some experiments on this. … Of course, it was a difficult thing at that time to do experimentally, but he had some indications that this process took place, and soon after in America they did a number of experiments which were in rather good accordance with the theory."[105] André Mercier, a 24-year-old Swiss physicist, was interested in the same problem that Møller worked on. In March 1937 he visited Bohr's institute in Copenhagen, where he discussed the problem with Møller and wrote a theoretical paper on the two forms of beta radioactivity. In September the same year he participated with Møller and other physicists, including Meitner, Peierls, and Heisenberg, in the annual Copenhagen conference. Mercier's calculations, which he also reported in a paper to the French Academy of Sciences, extended Møller's theory to a broader range of energies and atomic numbers.[106] Møller would later meet Mercier on several occasions, but then in the latter's capacity as a key figure in the renaissance of general relativity theory (see Section 6.1).

Although the possibility of K-capture radioactivity had been hypothesised earlier, first by Wick in the spring of 1934 and slightly later by Bethe and Peierls, Møller's theory went much further by its detailed calculations.[107] Apparently Møller came to believe that

105. Weiner (1971c). The experiments of J. C. Jacobsen (1937) were inconclusive and failed to yield convincing evidence for K-capture. On the other hand, Hoyle (1937) argued that Jacobsen's result agreed with a modified form of beta decay theory.

106. Mercier (1937a) dated 31 March and Mercier (1937b) presented to the French Academy on 12 April.

107. Electron capture by nuclei was first suggested in Millikan (1926) as an explanation of the origin of cosmic rays.

it was he who had predicted the phenomenon, such as he told Weiner many years later: "I wrote a paper on the capture of K electrons ... it was a new phenomenon I think which nobody had thought about before, and it was published in a Russian journal and therefore not very well known in America. So it was rediscovered in America later."[108] Møller was indeed one of the first to analyse K-capture theoretically and calculate its probability, but he was not the first and not its discoverer. In an important but at the time not much noticed paper of 1935, Hideki Yukawa and Shoichi Sakata in Japan developed a theory no less detailed than the one later offered by Møller, who at the time was not aware of the paper published in the *Proceedings of the Physico-Mathematical Society of Japan*.[109]

Not only Yukawa and Sakata, but also and independently the Swiss physicist Ernst Stueckelberg predicted electron capture radioactivity prior to Møller. Sakata later suggested that his and Yukawa's paper had undeservedly been ignored: "Although this publication was a significant test of Fermi's theory of β–radioactivity, and although we announced our calculations about it in November 1935, it was ignored by the world's physicists for more than a year. Experimentalists showed an interest only after Møller rediscovered our results in early 1937, and the effect was finally demonstrated by Alvarez."[110] However, Sakata's complaint is to some extent contradicted by the early citations to the 1935 paper by American physicists in particular. On the other hand, Møller either ignored the paper or more likely he was unaware of it until 1938. This is a bit surprising, given that Yukawa and Sakata called attention to their electron capture calculations in the issue of *Physical Review* of 15 April 1937.

108. Weiner (1971c). "Later I learned that the Japanese had already thought about this problem of the capture."

109. Yukawa and Sakata (1935). Yukawa and Sakata (1937) published 15 April, where they referred to the "similar calculations" of Møller. See also Darrigol (1988) and Rechenberg and Brown (1990).

110. Recollection from 1935 quoted in Rechenberg and Brown (1990), p. 222.

Still at the time when Møller wrote about K-capture, it was a hypothetical process, if not for long. The process was first identified in experiments made in 1937-1938 by 26-year-old Luis Alvarez at the University of California, Berkeley, who provided definite proof for the capture process

$$^{67}_{31}\mathrm{Ga} + e^- \rightarrow {}^{67}_{30}\mathrm{Zn} \text{ or } ^{67}_{31}\mathrm{Ga} + e^- \rightarrow {}^{67}_{30}\mathrm{Zn} + \nu$$

if the neutrino is included.[111] Møller had no contact with Alvarez, who in his comprehensive discovery paper of 1938 not only cited the Yukawa-Sakata prediction, but also Møller's two theoretical papers of 1937 and Jacobsen's experimental work of the same year. It soon turned out that electron capture is a common decay mode for proton-rich nuclei and for some, where positron emission is forbidden, is the only mode.

Weizsäcker was another physicist who, following Møller's paper, calculated the probability that a nucleus disintegrates spontaneously through the absorption of an orbital K electron. Interested in the relative abundance of the elements he paid particular attention to the comparatively rare potassium isotope K-40 which had been identified a few years earlier and was known to decay by ordinary beta decay according to $^{40}\mathrm{K} \rightarrow {}^{40}\mathrm{Ca} + e^-$. Weizsäcker suggested as a second mode of decay the capture process

$$^{40}_{19}\mathrm{K} + e^- \rightarrow {}^{40}_{18}\mathrm{Ar} + \nu$$

which might explain the anomalously high abundance (0.93%) of argon in the atmosphere. He further suggested that measurements of Ar-40 occluded in minerals or rocks might be used as a geochronological dating method. This kind of geological relevance was not considered by Møller, whose work on electron capture was purely theoretical. Weizsäcker's brief paper deserves attention because it laid the foundation of the later very important K-Ar dating method

111. Alvarez (1938). Segré (1987). Thirty years later, Alvarez was awarded the Nobel Prize for his development of the hydrogen bubble chamber and contributions to elementary particle physics.

used in geochronology and archaeology, a method only developed in the 1950s.[112]

Among those who investigated beta radioactivity theoretically in the late 1930s was a young British research student, Fred Hoyle, who worked in Cambridge with Peierls as his supervisor. In a paper of 1937, Hoyle argued that Jacobsen's measurements agreed with a modified form of beta decay theory according to which beta decay took place through intermediate states in the daughter nucleus.[113] The observed spectrum would thus represent a superposition of different Fermi spectra. In this and a following paper in the *Proceedings of the Royal Society* Hoyle suggested that the original Fermi theory was superior to the K-U modification. Yet another paper co-authored by Bethe and Peierls resulted in what apparently was good agreement with experimental data and predicted the circumstances under which gamma rays from the excited nuclear states should be observed.[114]

When Møller and Stefan Rozental in Copenhagen studied the Bethe-Hoyle-Peierls paper, they were critical and set out to produce a better explanation. By taking into account recent measurements of the positron spectrum due to the Japanese physicist Seishi Kikuchi, the two Copenhageners concluded that the Bethe-Hoyle-Peierls theory was wrong. In a letter to Bohr, who at the time stayed in Princeton working on uranium fission, Møller wrote:

> It is *not* possible to maintain the view of B. H. P. We were much interested in deciding the question because the meson theory opens up the possibility of other distributions than the Fermi distribution. We now investigate if the new theory can be brought into agreement with

112. Weizsäcker (1937). For the early history of the K-Ar method see Houtermans (1966).

113. Hoyle (1937). On Hoyle's brief and troubled career as a particle theorist, see Kragh (1996), pp. 162-164, and Mitton (2005), pp. 52-59. After Peierls left Cambridge for Birmingham, Hoyle was for a period supervised by Dirac.

114. Bethe, Hoyle, and Peierls (1939). According to Mitton (2005), p. 54, the paper led to "an irreversible breakdown in the relationship between student [Hoyle] and supervisor [Peierls]." See also Lee (2007), especially Peierls to Bethe, 5 November and 10 December 1938 (pp. 254-259).

> experiments. We thought of sending this note to Nature, but want to know your opinion about it. We wrote to Peierls about the same question, but as yet he has not answered.[115]

In a slightly later letter including a revised draft manuscript of the note intended for *Nature*, Møller elaborated on his and Rozental's critique:

> It is not possible to explain the positon spectrum of ^{13}N in the way proposed by Bethe, Peierls and Hoyle. That will only be possible if the ratio between the number of 'hard' γ-quanta to the number of 'soft' γ-quanta is about 45 and *not* 2.5 as found by Richardson. This seems to be quite impossible. Richardson believes that the error can possibly allow a value of 3.75 instead of 2.5, but not greater.[116]

After this, nothing more happened. For unknown reasons, the note remained unpublished. As to Hoyle, he decided to switch from fundamental quantum physics to astronomy and astrophysics, which he did abruptly and successfully. He soon became famous as well as controversial for his work in cosmology on a universe with no beginning and no end (Section 7.3).

Although focusing on his research on beta decay and other areas of theoretical physics, during the 1930s Møller also wrote more popular accounts intended for a Danish audience. *Fysisk Tidsskrift* (Physical Journal) was established in 1902 with its primary readership being Danish physicists and high school teachers of physics. Most members of the country's small community of professional physicists contributed with articles, such as Bohr did on several occasions. Like most of his colleagues, Møller "felt a little that it was one's duty to ... popularize a little what was going on in physics."[117] In 1933 he wrote a general paper on the new positron theory

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115. Møller to Bohr, 24 March 1939, and also Møller to Bohr, 13 March 1939 (BSC).
116. Møller to Bohr, 29 March 1939 (BSC). Richardson (1938). "Positon" is not a misprint (Section 5.1). John Reginald Richardson (1912-1997) was a Canadian-American physicist who later specialised in the development of cyclotron physics.
117. Weiner (1971c).

and later in his career he published in *Fysisk Tidsskrift* on a variety of other subjects. The Danish community of physicists was small indeed. As Guido Beck, who worked on beta decay theory and visited the institute in the 1930s, exaggerated: "At that time Denmark had three or four physicists. One was Bohr; one was Moller; Aage [Bohr] was too little. Then there were Kalckar and Jacobsen. They were all at the Institute."[118]

In 1937, *Fysisk Tidsskrift* contained a paper on alternative derivations of the famous $E = mc^2$ formula based on electromagnetic theory rather than the theory of relativity. The author, a 62-year-old construction engineer by the name Herluf Forchhammer, referred in particular to a non-relativistic derivation given by the German physicist Philipp Lenard, a Nobel laureate notorious for his critique of Einstein and the 'Jewish' theory of relativity. Møller's attempt in the same journal to set the matter straight and correct Forchhammer's many misunderstandings resulted in a minor controversy when the engineer and amateur physicist went on criticising the physics expert. After a couple of rounds Forchhammer admitted some of Møller's scientific objections although maintaining that Lenard's derivation was adequate and Einstein's relativity theory therefore not necessary for the $E = mc^2$ relation. Møller generally disliked controversies and when he nonetheless entered one on this occasion, it was because he associated Forchhammer's views with Lenard's anti-Semitism and reactionary views in general. Møller dismissed Lenard's textbook *Deutsche Physik* in strong words as poor science and ideological nonsense.[119] Twenty-four years later, Møller remembered that "I had a foolish discussion with a man who could not understand about the inertia of all energy, and I wrote a popular article on it."[120]

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118. Interview by John Heilbron, 22 April 1967, American Institute of Physics. https://www.aip.org/history-programs/niels-bohr-library/oral-histories. Beck participated in the 1932 institute conference.

119. Møller (1937c) followed by comments in *Fysisk Tidsskrift* **35** (1937), pp. 71-76 and 124-125. Forchammer's article was in the same issue, pp. 48-59. For Lenard's book, see Kragh (1999), p. 236.

120. Weiner (1971c).

More important than the fruitless debate over relativity theory, in 1938 Møller published with his colleague Ebbe Rasmussen a remarkable popular book on atomic and nuclear physics. I return to this book in Section 8.1, which also refers to other of Møller's popular works. By that time, Møller was slowly losing interest in beta radioactivity and had begun focusing on the meson particle predicted by Yukawa and its role in the poorly understood nuclear force. In modern parlance, he switched from weak to strong interactions, although at the time the distinction between the two forms of interaction was far from clearly recognised. This new line of work dealing with meson theory would occupy him for more than a decade.

CHAPTER 4

Nuclear fission and what followed

The sensational discovery in the end of 1938 that the uranium nucleus can be split into two lighter elements is a watershed not only in the history of modern physics but also in military and political history. Although Møller was not directly involved in the dramatic events that unfolded in Copenhagen in January 1939, he witnessed the events at close hand and contributed to the discussions of how to understand the new phenomenon. In popular contexts he wrote on the possibility of nuclear energy – or what was (and still is) often called atomic energy – even before the discovery of fission. Without publishing his insight, he was the first or one of the first to realise that free neutrons released in the fission process might lead to a chain reaction in a lump of uranium.

With the German occupation of Denmark in April 1940 Bohr's institute became involved in and seriously affected by the war. The situation worsened in the autumn of 1943 when Bohr and some of his collaborators of Jewish background were forced to flee to Sweden. After Bohr's escape it was left to Møller and his close colleague J. C. Jacobsen to run the institute. During this difficult period Heisenberg visited Copenhagen at a few occasions, where he had conversations with Møller and others. When the institute was occupied by German police soldiers in December 1943, Møller arranged that Heisenberg came to Copenhagen and helped him in negotiating an end of the occupation. His contacts with Heisenberg during the war years were not only diplomatic but also scientific, as they inspired Møller to take up a profound study of Heisenberg's new theory of the so-called *S*-matrix formalism (Section 5.3). Parts of this story concerning the fate of Bohr's institute during the years 1940-1945 are well known, but by looking at it through the eyes of Møller new details and a new dimension are added. One of the details, and not an unimportant one, is a meeting at the institute between Møller and the exiled German playwright Bertolt Brecht, who at the time was preparing his play *The Life of Galileo*.

4.1. Atomic energy

During the second half of the 1930s Bohr and his institute focused increasingly on nuclear physics. It was in this period that Bohr, assisted by young Fritz Kalckar and other collaborators, developed his important liquid-drop model of the atomic nucleus originally suggested by Gamow.[1] Kalckar was an important and most promising member of the Copenhagen institute, but he died tragically by a cerebral haemorrhage not yet 28 years old. Charlotte Houtermans recalled: "The next morning (it was January 6) before 10, I met Møller coming up the stairs, terribly pale, almost trembling: 'Kalckar is dead', he said, 'he died during the night'."[2] To Oppenheimer, with whom Kalckar had collaborated during a stay in Berkeley, Bohr wrote: "You will be very sorry to learn that Kalckar suddenly, without any previous illness, died from heart failure yesterday night. He was found unconscious by his mother with whom he lived and all efforts to bring him back to life again were fruitless."[3]

The semi-classical 'compound nucleus' model developed by Bohr and his colleagues was highly successful in explaining nuclear reactions and remained the favoured model until about 1950, when it was challenged by the shell or independent-particle model. Møller did not actively participate in this line of research, which he only followed from the side-line. Still, he was interested in it, such as shown by the popular book of 1938 that he wrote jointly with Ebbe Rasmussen. *Atomer og Andre Smaating* (Atoms and Other Small Things) included informative up-to-date sections on nuclear reactions and also discussed the question of atomic energy and its possible use. However, in the first edition the two authors dealt only with fusion processes in which "energy is produced by the building-up of nuclei with high binding energy from nuclei of a lower binding energy." They concluded that "the probability that

1. Aaserud (1990). Stuewer (2018), pp. 119-123, 335-340.

2. Shifman (2017), p. 192. Møller (1938b) is a memorial article about Kalckar, his personality, and works in physics.

3. Bohr to Oppenheimer, 7 January 1938 (BSC).

the energy of atomic nuclei can be technically useful is so small that it borders to the impossible."[4] Bohr agreed. Two years earlier, he referred to "the much-discussed problem of releasing the nuclear energy for practical purposes", stating that "the more our knowledge of nuclear reactions advances the remoter this goal seems to become."[5]

Møller and Rasmussen did not consider uranium as a possible energy source. And no wonder, given that the fission of the uranium nucleus was only proposed in the last days of 1938, when Lise Meitner and her nephew Otto Frisch interpreted recent experiments made in Berlin by Otto Hahn and Friedrich Strassmann in terms of a neutron-induced fission process liberating an energy of approximately 200 MeV. The kind of process Meitner and Frisch had in mind can somewhat anachronistically be illustrated by a typical uranium fission caused by slow neutrons such as

$$^{235}_{92}\mathrm{U} + ^{1}_{0}n \rightarrow ^{141}_{56}\mathrm{Ba} + ^{92}_{36}\mathrm{Kr} + 3^{1}_{0}n + 215\ \mathrm{MeV}$$

However, in the early days of 1939 the secondary neutrons were unknown and so was the special behaviour of the rare uranium-235 isotope making up only 0.7 per cent of the element in its natural state.

Meitner stayed in Stockholm, where she worked in Manne Siegbahn's laboratory, whereas Frisch at the time worked at the Copenhagen institute. They spent the Christmas holidays in Kungälv, just north of Gothenburg, and it was on this occasion that the fission hypothesis was born. When Frisch returned to the institute from Sweden on 3 January, he discussed the matter with Bohr, who immediately accepted the hypothesis and began working on it. Experiments made by Frisch resulted in the first physical confirmation of

4. Møller and Rasmussen (1938), p. 161 and p. 164. More about this book in Section 8.1.

5. Bohr (1936b), p. 348. As late as February 1943, Bohr wrote to Chadwick: "I have to the best of my judgment convinced myself, that in spite of all future prospects any immediate use of the latest marvellous discoveries of atomic physics is impracticable." Bohr (2005), p. 228.

the fission hypothesis, which was reported in a famous paper sent to *Nature* on 16 January 1939 but only published on 18 February. Bohr, who together with Rosenfeld was preparing for a trip to the United States – he left Copenhagen on 7 January and arrived in New York nine days later – brought the news of fission with him to the American physics community.[6]

Unknown to or unappreciated by Bohr and his associates in Copenhagen, as early as 1934 the German chemist Ida Noddack had suggested that in Fermi's experiments with neutron bombardment of very heavy nuclei (Section 3.3), these nuclei might have broken up into large fragments of isotopes of known elements instead of resulting in transuranic elements. Noddack's suggestion came to be seen as an anticipation of nuclear fission, but at the time it made no impact at all and seems to have been unknown to, or at least considered irrelevant by, the physicists later investigating the uranium puzzle. Hevesy knew Noddack well and presumably also her paper in *Zeitschrift für angewandte Chemie*. One might imagine that either Hevesy or Meitner made Bohr aware of it, but there is no indication that they did or recognised the significance of Noddack's paper.[7]

At the time Bohr and Rosenfeld arrived in America, they were still unaware of the experimental confirmation in Copenhagen and had not yet read the Hahn-Strassmann paper published in *Naturwissenschaften* on 6 February. When Møller in a letter casually referred to "Frisch's amusing experiments here concerning the splitting of the very heavy nuclei", Rosenfeld – who did not know of the experiments – got upset. The situation with regard to priority worried Rosenfeld, who did not find the late report to be amusing at all. In a sarcastic and unusually strongly worded reply, he urged Møller immediately to send telegraphic information of the state of affairs at

6. See Rhodes (1986), pp. 233-275 and also Badash, Hodes, and Tiddens (1986) for how fission was discovered and received.

7. There is no mention of Noddack in the thirteen volumes of *Niels Bohr: Collected Works*. The Noddack case has generated considerable debate among chemists, physicists, and historians of science, but in the present context this debate is of no relevance.

the institute. He complained about and apparently blamed Møller for not realising the "importance and difficulty of maintaining the connections between us and the Institute [which] do not appear to have been sufficiently appreciated in the Olympian clouds above Blegdamsvej."[8] He and Bohr were afraid that the Americans might come first with publishing the confirmation and thus obtain priority over Frisch and the Copenhagen institute. In his letter of response, Møller begged his colleague to calm down. Contrary to Rosenfeld's emotional and agitated letter, Møller's was constrained and matter-of-factly:

> Of course we were not ignorant of the fact that there were also physicists in America and that they might perhaps get the same idea as Frisch, but this is a risk which unfortunately will always be there in science. … Here at 'Olympus' it has always been customary to send notes to *Nature* and not to *Ekstrabladet* and that is also really what happened in this case. Naturally I realise that it would have been best if Frisch had sent a telegram immediately. However, as things are, I can't really see it in any other way than that the Americans have made the same discovery independently.[9]

Perhaps it was not by accident that Møller used the term 'splitting' rather than 'fission' in his first letter to Rosenfeld. The latter term was known to him but had not been sanctioned by Bohr, and the Frisch-Meitner paper (which included 'fission' in inverted commas) had not yet been published. The term was coined by an American biophysicist, William Archibald Arnold, who worked at Bohr's institute and was acquainted with it from the fission of bacteria. Frisch found the term appropriate and in a letter of 22 January he asked for Bohr's blessing to use it. 'Fission' became an instant success in the small but rapidly growing community of nuclear physicists.[10]

8. Undated letter, reproduced in Jacobsen (2012), pp. 146-148. Møller to Rosenfeld, 1 February and 26 February 1939 (RP).

9. Møller to Rosenfeld, 26 February 1939 quoted in Jacobsen (2012), p. 149. *Ekstrabladet* was and still is a Danish tabloid newspaper.

10. See Kragh (2014) for the origin and dissemination of the word 'fission'.

Although Møller was mostly a bystander to the Copenhagen fission discussions in early 1939, he participated in them and may have been the first to come up with the important suggestion that neutrons released in the fission of uranium may cause further fission processes and thus result in a chain reaction. The Meitner-Frisch paper mentioned neither neutron reproduction nor a possible chain reaction. According to the memoirs of Frisch:

> In all this excitement we had missed the most important point: the chain reaction. It was Christian Møller, a Danish colleague, who first suggested to me that the fission fragments (the two freshly formed nuclei) might contain enough surplus energy each to eject a neutron or two; each of these might cause another fission and generate more neutrons. By such a 'chain reaction' the neutrons would multiply in uranium like rabbits in a meadow! ... So from Møller's remark the exciting vision arose that by assembling enough pure uranium (with appropriate care!) one might start a controlled chain reaction and liberate nuclear energy on a scale that really mattered.[11]

Møller may have suggested the nuclear chain reaction in late January 1939 or thereabout, but only informally and without paying much attention to it himself. The possibility of free neutrons in fission – obviously a precondition for a chain reaction – had been considered early on, and Møller was not the only one who thought that the secondary neutrons might generate new fission processes. Thus, the American physicist John Dunning came to the same insight and possibly before Møller. On 25 January 1939 Dunning wrote in his laboratory notebook: "Believe we have observed new phenomenon of far-reaching consequences. ... Here is real atomic energy! ... *Secondary neutrons are highly important*! If emitted would give possibility of a self-perpetuating neutron reaction which I have considered since 1932-33 as a main hope of 'burning' materials with slow neutrons and release of atomic energy."[12]

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11. Frisch (1979), p. 118. See also Frisch (1954) for an earlier reference to Møller's informal suggestion.
12. Quoted in Badash, Hodes, and Tiddens (1986), p. 210.

A nuclear chain reaction caused by excess neutrons had been suggested as early as 1934 by the Hungarian-American physicist Leo Szilard, but at the time without having uranium in mind. Only after having talked with Wigner and Fermi in January 1939 did Szilard realise the possibility of a chain reaction in uranium. Contrary to other workers in the field he made the connection to a future atomic bomb, such as he did in a prescient letter to Frédéric Joliot-Curie of 2 February 1939: "Obviously, if more than more neutron were liberated, a sort of chain reaction would be possible. In certain circumstances this might then lead to the construction of bombs which would be extremely dangerous in general and particularly in the hands of certain governments."[13] Only in March did teams of American and French physicists independently establish that uranium fission gives rise to approximately 2.5 neutrons per split uranium nucleus and that the liberated neutrons can produce further fission.

As Møller recalled, it was in a radio program that he made the suggestion in public. "There was a round table talk in the Danish radio where Frisch and Rasmussen and I took part, and … during this discussion, I happened to say, 'Well, if we get more neutrons out than we have put in, then we have that possibility of having a chain reaction'."[14] To be more precise, on 27 February 1939 Paul Bergsøe, a well-known chemical engineer, author, and broadcaster, gave a radio presentation on nuclear physics in which he discussed at some length the fission of uranium. He interviewed four physicists from or associated with Bohr's institute, namely, Møller, Frisch, Jacobsen, and Bjerge (but not Rasmussen, as Møller mistakenly thought). While Frisch told about his experimental work with uranium and thorium fission, and Bjerge about the unsuccessful attempts to split lead by neutrons, Bergsøe invited Møller to comment on the possible use of atomic energy based on the fission process: "Tell me,

13. Weart and Szilard (1978), p. 69.

14. Weiner (1971c). Bergsøe (1872-1963) pioneered broadcasts on science and technology in the state-owned Danish radio. He was an acquaintance of Bohr and in 1959 he received the prestigious Ørsted medal, the same recognition that was awarded Bohr in 1924 and which Møller would receive in 1970.

Dr. Møller, what have you to say about the American statements concerning *the possibility of a practical extraction of energy*? If atomic energy could be used it would be a world revolution."

Møller was convinced that utilisation of atomic energy was still 'free fantasy' and that fission belonged to theoretical nuclear physics, not to applied or technical physics. He elaborated:

> To use the energy for practical purposes, the process must propagate to other uranium atoms when it has first started in one atom ... The only possibility that uranium fission might propagate by itself would be that the process also resulted in a larger number of neutrons, and that one of them was fortunate enough to hit a new uranium nucleus. However, neutrons of this kind have not been detected. Indeed, if this were really the case one would expect that it already had occurred naturally in those lumps of almost pure uranium that exist in the crust of the Earth and which are continually irradiated by cosmic rays.[15]

At a time when the greater fission cross section of uranium-235 was not yet known, Møller anticipated what is known as prehistoric or natural nuclear reactors. His speculation was turned into a prediction in the 1950s, when it was realised that ancient rocks would have been richer in U-235 because this isotope decays more rapidly than U-238. The half-lives in years are 7×10^8 and 4.5×10^9, respectively. In 1972 a team led by the French physicist Francis Perrin confirmed the prediction by studying the composition of rocks in uranium ores in Oklo, Gabon, in West Africa.[16] The rocks analysed by Perrin and his team showed an anomalously low amount of U-235 relative to U-238, namely 0.717 instead of 0.720, and the discrepancy was explained as the result of a 'natural fission reactor' in the geological past, in this case about 1.7 billion years ago.

While in 1939 Møller considered speculations of large-scale atomic energy to be nothing but free fantasy, a few years later he was not so sure. In a popular article in the trade journal of the Danish industrial company Danfoss, he envisioned in 1943 a sustained

15. Bergsøe (1940), pp. 72-73.

16. On the so-called Oklo phenomenon, see Barrow (2002), pp. 231-239.

Fig. 17. The radio interview on fission and nuclear energy. From the left, T. Bjerge, O. R. Frisch, P. Bergsøe, J. C. Jacobsen, C. Møller. Source: *Berlingske Tidende*, 28 February 1939.

chain reaction in natural uranium with heavy water to moderate the energy of the neutrons and cadmium to control the reaction rate. Should the technology become a reality, he wrote, Denmark's need of energy in a whole year would be covered by just half a ton of pure uranium. He considered a large-scale separation of the two uranium isotopes U-238 and U-238 to belong to an unforeseeable future.[17] In all likelihood Møller was unaware that Fermi and his team in Chicago had already produced the first ever sustainable nuclear chain reaction. The American 'pile' used graphite and not heavy water as moderator.

Bergsøe ended his broadcast: "As we are aware, Professor Bohr is presently in America listening at his radio receiver to this broadcast ... [and] we send him our greetings." Indeed, Bohr had been informed about the interview in a telegram dispatched from his institute on 25 February. He responded that it was a 'happy idea' and that he looked forward listening to it. In a letter to Bergsøe sent after the broadcast he expressed his "great admiration for your

17. Møller (1943a). As mentioned in Section 1.1, Møller was an old friend of the engineer and industrialist Mads Clausen, who founded Danfoss in 1933.

power of exposition and your deep familiarisation with all scientific research."[18] From Princeton he also wrote to Ebbe Rasmussen:

> You can believe that I was pleased to get this morning so many good letters from the Institute ... I also had a letter from engineer Bergsøe with the text of the radio interview, which came over excellently, and to which I listened with great pleasure over here. I am really more than happy on behalf of the Institute with the great work that Frisch and all the others have done. ... Rosenfeld also sends his thanks to Møller for his long and interesting letters, and he agrees with their contents (apart from the question of the possibility of meson emission in fission) and he hopes to be able to reply at length by the next post.

In a footnote to the letter, Bohr added:

> Neither Rosenfeld nor I believe that there is any probability at all that mesons could be emitted in the nuclear fission, because the energy with which one is concerned here is hardly available for a nuclear reaction at any given time but is released gradually under the mutual electrostatic repulsion of the nuclear fragments during their motion away from each other. ... I do not think, as mentioned above, that anything can come out of searching for mesons [in Jacobsen's experiments in Copenhagen].[19]

Apparently, the suggestion of meson production in the fission process was due to Jacobsen, who in February informed Bohr about the ongoing experiments in Copenhagen: "During the last days we have tried another experiment which may well lead to nothing but is worth trying. It concerns the emission of mesotrons from uranium + deuterons. In this nuclear process there appears an energy of about 100 M. V. [MeV], which is sufficient not only to create a me-

18. Institute staff to Bohr, telegram, 25 February 1939 (BSC, Supplement). Bohr to Rasmussen, telegram, 26 February 1939 (BSC, Supplement). Bohr to Bergsøe, undated draft of 1939 (Bohr, Private Correspondence).
19. Bohr to Rasmussen, 10 March 1939, in Bohr (1986), pp. 635-637.

sotron but also to provide it with a considerable kinetic energy."[20] The following month Niels Arley wrote to Bohr about the same issue: "Has Dr. Jacobsen told you about the beautiful idea he got, namely that the 200 M. V. in the uranium process could be used to create a mesotron? It would be wonderful if one could really make 'laboratory mesotrons'!" However, calculations made by Arley in collaboration with Heitler indicated that the probability of meson creation was close to zero. "I do not believe it is possible to create mesotrons in this way. Perhaps it is just nonsense?"[21]

Møller's belief in early 1939 that the exploitation of atomic energy was "free fantasy" was shared by Bohr and most other physicists. At the end of the year – after World War II had become a reality – Bohr gave an address on recent progress in nuclear physics to the Society for the Dissemination of Natural Science in Copenhagen with Møller in the audience. Referring to reactions with slow neutrons and natural uranium, he said that "it is clear beforehand that with this approach there can never be a question of explosions which would suddenly release a substantial part of the atomic energy." As regards the possibility of enriching uranium with the uranium-235 isotope he was no less pessimistic: "With present technical means it is however impossible to purify the rare uranium isotope in sufficient quantities to realise the chain reaction discussed above."[22] As Bohr and also Møller saw it, practical use of nuclear energy for either military or peaceful purposes belonged to the far future.

The radio interview of 27 February 1939 produced by Bergsøe was widely reported and discussed in the leading Danish newspapers *Politiken* and *Berlingske Tidende*. Four days earlier *Berlingske Tidende* had informed its readers about the work done at Bohr's institute in an article with the headline 'Two Hundred Million Volts Observed by Splitting of Atomic Nucleus'. In a comprehensive

20. Jacobsen to Bohr, 25 February 1939 (BSC, Supplement). A month later: "The mesotron experiment will soon give a result, albeit probably a negative one." Jacobsen to Bohr, 20 March 1939 (BSC, Supplement).

21. Arley to Bohr, 10 March 1939 (BSC, Supplement). The suggestion that mesons might accompany the fission process remained unpublished.

22. Bohr (1986), p. 466, lecture of 6 December 1939.

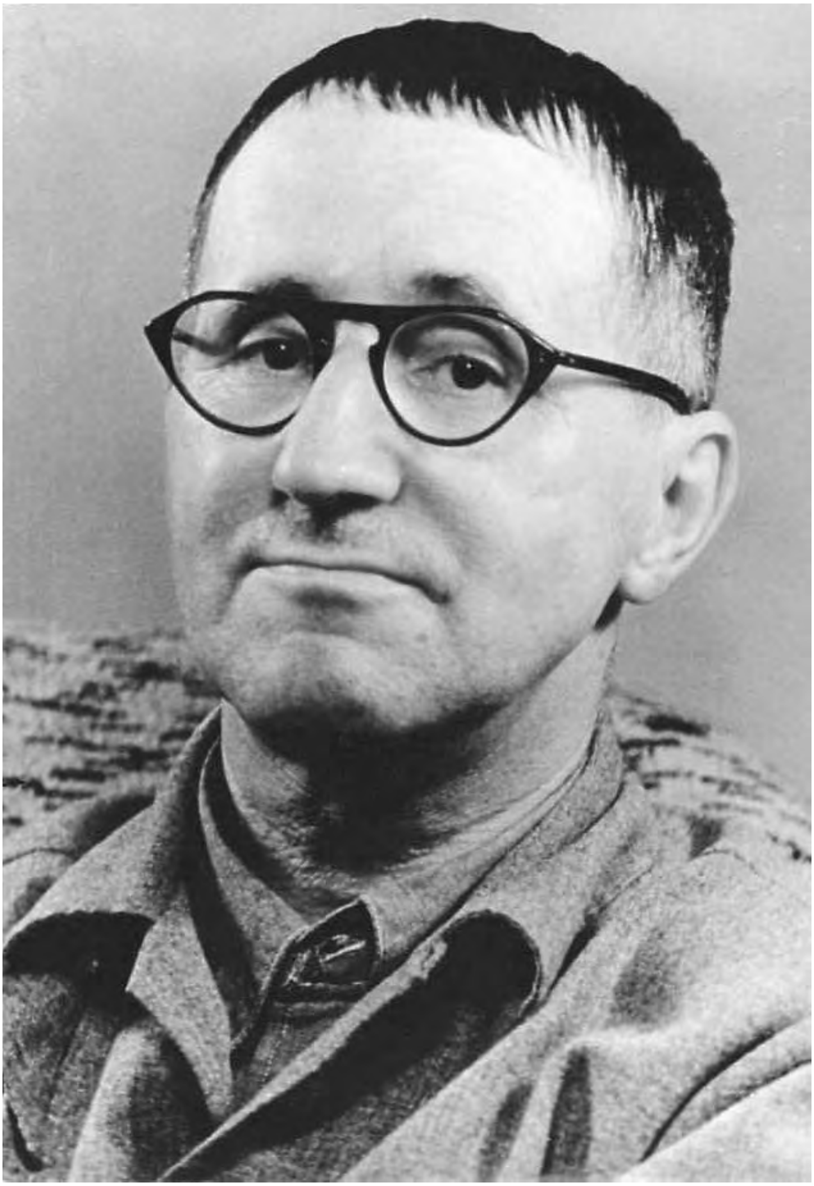

Fig. 18. Bertolt Brecht (1898-1956). https://commons.wikimedia.org/wiki/File:Bertolt-Brecht.jpg

review of the radio broadcast, *Politiken* focused on the possibility of future atomic energy and cited Møller for his pessimistic view. Among those who listened to the broadcast and carefully read the article in *Politiken* was the famous German socialist playwright and poet Bertolt Brecht, possibly the most prominent of the country's many intellectual refugees. Brecht had come to Denmark in 1933 and stayed in the country until April 1939, after which he moved on to Sweden and from there to Finland, eventually to end up, after periods in the United States and Switzerland, in East Germany in 1949. Møller's sceptical attitude regarding the practical use of atomic energy seems to have left an impression on him.[23] In fact, he had met Møller about a year earlier.

Brecht completed his main work 'The Life of Galileo' (*Leben des Galilei*) in November 1938 and later said that one of Bohr's assistants "who was working on the problem of splitting the atom" helped him with understanding the Ptolemaic world system to which Galileo was opposed.[24] The assistant was Møller, with whom Brecht and possibly also his close friend and mistress Ruth Berlau, an adventurous Danish actress and writer, had a conversation at Bohr's institute in the spring or summer of 1938. According to one source, Brecht's contact to Møller was mediated by the chemist Stig Veibel, who later became professor of organic chemistry at the Polytechnic College.[25] Veibel shared the views of the extreme Danish left and chaired one of the communist organisations for help to German political immigrants called the Liberation Committee for the Victims of Hitler Fascism (*Befrielseskomiteen for Hitler-Fascismens Ofre*), which was active 1935-1938.

According to Berlau's recollections, she (who like Veibel was a member of the Communist Party) arranged a meeting between Brecht and Bohr, whom she knew casually since she lived close to Bohr's summer cottage in Tisvilde:

23. According to Nørregaard (1986), p. 452 and Schumacher (1965), p. 114.

24. Brecht (1965), p. 115. Some writers have claimed that Brecht wrote his work on Galileo under the impact of Bohr's work on uranium fission, which is obviously wrong given the chronology.

25. Nørregaard (1986), p. 447.

> Niels Bohr was interested in everything. He even knew whom Brecht was. Of course, he did not himself guide Brecht around in his institute, as he had assistants to do that. Brecht had not studied atomic physics but had read much about it. He therefore wanted to be introduced to the physical problems on the very scene in a simple and comprehensible manner.[26]

However, it is uncertain and even unlikely that Bohr and Brecht met in person. At least, there is no documentary evidence which supports Berlau's story.

In a taped interview of 1974, Møller told that he was a bit surprised when Brecht turned up and wanted to speak with him. After all, he was not a specialist in nuclear physics and definitely not in ancient astronomy. In any case, during their conversation they came to discuss the scientist's social role and responsibility, a subject on which the two disagreed. In the 1974 interview, Møller stated his view as follows:

> I am sceptical with regard to the general claim of the relatedness of society and research. If one always has in mind that one's scientific work must be useful and serve some societal aim, then one comes nowhere. It is only when one gets liberated from this viewpoint and exclusively seeks the truth – quite independent of how useful it may be – that one can hope to obtain the grand picture of what really happens. Only then will it appear, almost by itself, which things in progress can be useful for society. Fundamental research really should be independent of views concerning usefulness. To put it simply, if it shall be useful, one should not think about it.[27]

Møller's recollection from 1974 is substantiated by an earlier correspondence he had with the East German Brecht scholar Ernst Schumacher and in which he dated the conversation to probably the spring of 1938. He had the impression that "Brecht was interested

26. Bunge (1987), p. 63. Berlau gave no date for the supposed meeting with Bohr.
27. Nørregaard (1986), p. 448. I have not been able to locate the tape or any transcript of it.

in Galileo because he saw an analogy between the Inquisition and National Socialism in Germany." Brecht argued that when Galileo was forced to reject the Copernican system in 1633 it was a serious defeat with grave consequences for science. However, Møller disagreed: "I didn't really understand this point of view, and today when I read *Leben des Galilei* I still do not understand it. Of course, this doesn't prevent me from regarding the play to be very impressive and stimulating."[28]

Brecht sent copies of his Galileo manuscript to a few writers and intellectuals. Interestingly, one of the recipients was Einstein, who responded most positively: "Your 'Galilei' has caused me much joy. ... You have understood how to create a dramatic framework which is uncommonly captivating and which must especially interest us too on account of the strong links to the problems of the present time."[29] One might expect that Brecht also provided Bohr or Møller with a copy, but he did not. The first version of *Life of Galileo* completed in Denmark – originally with the title *Die Erde bewegt sich* (The Earth Moves) – was only performed in Zurich in 1943. It was followed by two other versions, one dating from 1944, when Brecht stayed in California, and a third one from 1953 created during his years in East Berlin. Shortly after Brecht's death in August 1956, his widow Helene Weigel referred in a letter to Bohr to her late husband's meeting with Møller. She wrongly stated that the writing of *Galileo* was brought about by the discovery of uranium fission:

> In 1938, when we – Bertolt Brecht, I, and our children – lived in Denmark, Brecht began writing his play 'Leben des Galilei'. The immediate occasion for it was the account that Otto Hahn and his assistants gave of the splitting of the uranium atom and also Hitler's imminent war, which at the time was clearly in the air. During the work on this play

28. Møller to Schumacher, 3 January and 19 February 1959, as cited in Schumacher (1965), pp. 112-113.
29. Einstein to Brecht, 4 May 1939, quoted in Parker (2014), p. 392. The letter is preserved at the Albert Einstein Archive in Jerusalem. It can be found online as http://web.mit.edu/21f.404/www/Einstein-Brecht.pdf.

> Brecht had a conversation with one of your assistants who also supplied him with additional materials.[30]

Many years after his conversation with Brecht, Møller was asked by a journalist if he somehow felt responsible for the atomic bomb and the threat of a nuclear Armageddon. He did not. Møller said that scientists could not be blamed for how their discoveries were transformed into technologies and eventually used for military or other purposes. Besides, he was not much worried about the nuclear threat, for "in the end, isn't it because of this terrible atomic bomb that we have lived in peace for the last 25 years?" Møller elaborated: "I don't agree with those who wail over the development of society in this century, such as do many young people. ... It is common to blame technology, but one should use it and not be tyrannised by it."[31]

A couple of months before the discovery of the fission process, Bohr arranged on 25-29 October a conference in Copenhagen on cosmic rays. "We have planned to have a little conference on the cosmic-ray problems and the new particles", Bohr informed Fermi. "As you know [Bruno] Rossi is here already and I have just heard from [Pierre] Auger and [Patrick] Blackett that they will be able to join our discussions, so I am sure we shall all have a very instructive time."[32] Other participants in the Copenhagen meeting were Møller and Heisenberg, and Fermi came as well. The two Italians, Rossi and Fermi, were both on their way to escape from fascist Italy after the implementation of racial laws. Rossi recalled: "I spent long hours in the library bringing myself up to date on the recent developments in physics, talking with the people I happened to meet, thus, gradually rekindling my enthusiasm for science."[33] As to

30. Weigel-Brecht to Bohr, 3 October 1956 (NBA, Bohr Private Correspondence). Bohr responded through his secretary: Sophie Hellmann to Weigel-Brecht, 22 October 1956 (NBA, Bohr Private Correspondence).

31. *Jydske Tidende*, 15 March 1970. On Møller's views concerning science, technology, and society, see also Section 8.5.

32. Bohr to Fermi, 19 October 1938 (BSC, Supplement).

33. Rossi (1990), p. 41. See Bonolis (2011) for details about the consequences of the racial laws for Italian physics.

Fermi, Bohr confidentially informed him that he would be awarded the Nobel Prize, which was made official on 10 November. After having participated in the Stockholm ceremonies, Fermi and his Jewish wife Laura spent a few days in Copenhagen before they went to England and from there to New York. Rossi and his wife Nora also proceeded westwards, but in their case only to Manchester, where Rossi came to work in Blackett's laboratory. At the time of their departure from the Continent neither Fermi nor Rossi were aware of the fission interpretation of the uranium experiments made in Berlin.

4.2. Physics in occupied Denmark

World War II severely affected physics, where research in pure fields of no military or social significance declined drastically in the period from 1939 to 1945.[34] Delays in transfer of scientific communications and generally the difficulties or impossibility in maintaining international cooperation were only some of the problems. While the effect of the war was most serious in the belligerent countries, it also had a great and negative impact on physics in the European countries occupied by Nazi Germany. Among them was Denmark, which was occupied by German forces on 9 April 1940. The occupation forced most foreign visitors to leave Bohr's institute and generally implied that for five years it became a national rather than international institution. A comparison of the institute conferences in 1937 and 1941 provides telling evidence for the development (Figure 16 and Figure 19). "At the moment we feel very much cut off from the world", Bohr wrote in May 1940.[35] Foreign visitors were largely restricted to Swedes and Germans. Until the summer of 1943, it was

34. The decrease in physics publications and the slow recovery after the peace is illustrated by the number of pages in the leading physics journals, see Bullard (1975) and Kragh (1999), pp. 273-275.

35. Pais (1991), p. 480. Life at the institute during the years of occupation is described in Crowther (1949), pp. 105-122 and Dahl (1948), pp. 199-214. See Schwarz (2011) for the general background and Danish-German scientific and cultural relations during the period.

Fig. 19. The 1941 Copenhagen conference, a local rather than an international event. On the first row: N. Bohr, T. Gustafson, G. Hevesy, and Jørgen Koch. On the second row from the left: S. Rozental, C. Møller, B. Eriksen, and from the right S. Werner and J. C. G. Jacobsen. E. Rasmussen and B. Strömgren are sitting on the third row and young Aa. Bohr on the fifth row. H. Levi, Hevesy's assistant, is to the right of Strömgren. Credit: Niels Bohr Archive, Photo Collection, Copenhagen.

still possible to go to Sweden, such as Møller did at a few occasions. For example, in late March 1942 he went to Lund, where he had been invited to give a talk, and from there he went on to Stockholm.[36]

Naturally, the scientific productivity decreased during the war, not only in Copenhagen but also internationally. The sharp decline in physics publications was a general phenomenon, such as illustrated by bibliometric data. Thus, whereas the total number of pages in *Physical Review* was 2965 in the year 1938, the 1943 volume comprised only 428 pages. The development was the same for the *Proceedings of the Royal Society*, section A, where the number of pages declined from about 2900 in 1938 to 380 in 1943.[37] Also research

36. Bohr to Klein, 11 March 1942 (BSC).

37. Kragh (1999), pp. 272-275.

publications from the institute for theoretical physics in Copenhagen declined, if not quite as drastically and only from the beginning of the occupation in April 1940.[38] Still in 1939 there appeared from authors at the institute 22 papers in American and British journals (*Physical Review*, *Nature*), a number which in the five-year period 1940-1944 declined to a total of 23 and with no publications at all in the years 1942-1944. Many works were published in Danish or other Scandinavian journals, of which the most important was the proceeding series of the Royal Danish Academy. While previously several of the papers from the institute were written in German, not a single paper was published in that language through the 1940s. Møller's research output 1941-1944 was limited to three papers, one of them co-authored by Rosenfeld.

Despite the troubled situation, the work in Copenhagen continued as well as it could. When Charles Weiner in an interview of 1971 asked the former secretary Betty Schultz about life at the institute until late 1943, she said:

> It was as usual. People worked here as usual. A few times – no, only one or two – two [German] officers came and asked for Moller. They were engineers or scientists and wanted to see the Institute. ... Then Moller said to them, "Yes, you are welcome, but not in that uniform", in German. Nobody will show you the Institute when you – "No, they will come in their civil dress." And they did so, and they looked at the Institute.[39]

During this period Møller continued to give his courses in theoretical physics to his small classes of Danish students. But there were few of them. "There are no new physics students for this semester", Jacobsen reported laconically to Bohr in early 1939.[40] Møller recalled: "We kept up — certainly — a colloquium for the students.

38. See the list of publications given in Schwarz (2011), p. 416.
39. Interview of 26 March 1971, American Institute of Physics. https://www.aip.org/history-programs/niels-bohr-library/oral-histories/4867-2. Betty Schultz (1898-1980) worked as a secretary for Bohr and his institute from 1920 to 1970.
40. Jacobsen to Bohr, 26 January 1939 (BSC, Supplement).

That was a part of their education. This we did, and well, when [Hans] Jensen came or Heisenberg came, we had small colloquia. But it's true, the activity was very reduced, that's true. But if you loved to sit at a desk and think, it was not such a bad time, strangely enough."[41]

Not such a bad time, but definitely not a happy one either. In March 1941 Weizsäcker gave a series of lectures in occupied Copenhagen including one at Bohr's institute on 'The Relationship Between Quantum Mechanics and Kantian Philosophy'. The well-attended lectures were followed up later the same year when Weizsäcker visited Denmark once again, now in company with Heisenberg and the two German astrophysicists Ludwig Biermann and Hans Kienle. The occasion was an 'astrophysical week' 18-24 September arranged by the newly founded German Cultural Insitute (GCI, *Das Deutsche Wissenschaftliche Institut*) headed by the theologian Otto Scheel.[42]

Heisenberg and Weizsäcker visited the father-and-son astronomers Elis and Bengt Strömgren at the University Observatory and also, on more than one occasion, Bohr's institute, where they spoke with Møller and a few of the other physicists. "Weizsäcker brought the director of the GCI to the Institute of Theoretical Physics, pushed him without an appointment past Bohr's secretary, and forced Bohr into a confrontation he had taken pains to avoid."[43] In a later draft document, Bohr summarised:

> During those days, however, Heisenberg and Weizsäcker visited this Institute and had conversations with Chr. Møller as well as with Bohr. ... During conversations with Møller, Heisenberg and Weizsäcker sought

41. Weiner (1971c). The German nuclear physicist J. Hans D. Jensen did important work on nuclear structure and was in 1963 awarded a shared Nobel Prize for the nuclear shell model. Møller was not the only one who remembered the war years as "not such a bad time." According to his colleague Rozental (1967), p. 157, "the war was a relatively good period from the point of view of work."

42. In the summer of 1943 Scheel was replaced by Otto Höfler, an Austrian Nazi scholar of Germanic studies. Walker (1992). Weizsäcker to Bohr, 15 August 1941 (BSC).

43. Walker (1992), p. 366. Crowther (1949), p. 107.

> to explain that the attitude of the Danish people towards Germany, and that of the Danish physicists in particular, was unreasonable and indefensible since a German victory was already guaranteed and that any resistance against cooperation could only bring disaster to Denmark. In a conversation with Møller, Weizsäcker further stated how fortunate it was that Heisenberg's work would mean so much for the war since it would mean that, after the expected great victory, the Nazis would adopt a more understanding attitude towards German scientific efforts.[44]

In the 1971 interview with Weiner, Møller gave his version of the September 1941 event, which he described as follows:

> We of course were invited to go and hear the [public] talk of Heisenberg, and we of course did not go, and always told him — not that we didn't want to go, but that we were not able to come. ... So we didn't go to his lecture, but he came here and gave a small colloquium for us, for ... only I think six or seven people, and he was talking about the *S* matrix theory, and I was very much interested in that. I had just read his paper on that. That was the origin of that I started to work on the *S* matrix theory.[45]

According to Møller, Bohr was uneasy about the visits Jensen paid to the institute because he thought that he might be an agent provocateur for the German authorities. Jensen was indeed a member of the Nazi Party NSDAP since 1937, but only because he had to if he wanted to continue his scientific career. Møller did not share Bohr's suspicion:

> I knew Jensen very well from the old days and I said no, I knew that he had been a Communist before Hitler and I visited him in Hamburg

44. Handwritten document by Margrethe Bohr, reproduced in Dörries (2005), pp. 130-133. For the Heisenberg-Møller conversation, see also the recollection of Rozental cited in Pais (1991), p. 483.
45. Weiner (1971c). Møller mixed up Heisenberg's visit in September 1941 and the later one in April 1944. Heisenberg's first paper on *S*-matrix theory appeared in the 25 March 1943 issue of *Zeitschrift für Physik* (Section 5.3).

> in 1937 on the way to Paris, where he told me about how difficult it was for them to manage. ... So I was convinced that he cannot in these few years have changed so completely and become a German spy. This I cannot believe. So I told Bohr this and he finally also became convinced that he was innocent.[46]

In late March 1942 Møller went to Stockholm where he gave two talks on meson theory with Lise Meitner and Oskar Klein in the audience. As Meitner told her friend Max von Laue in Germany, she much appreciated the talks:

> Dr. Møller was here for a few days giving 2 very nice lectures on his theory ... of nuclear forces and its connection to the theory of β-decay. From a physical point of view his theory implies that in addition to mesons with spin zero there also exist mesons with spin 1, although these must have a lifetime 100 times smaller and therefore can only be found in the upper strata of the atmosphere. With these assumptions he can account for the nuclear forces (the same forces between proton-neutron, proton-proton and neutron-neutron) and for the β-decay he gets Fermi's theory. This I consider as a great progress. His lectures were also excellent in the formal sense, very well organised and very clearly exposed.[47]

Møller's conversations with Meitner were not only about physical theories. He used the occasion to update her on work in Copenhagen and to tell her about Bohr's meeting with Heisenberg.

> One evening Dr. M. stayed with me, which was very pleasant. He told me a lot about Niels and the institute ... His account of the visit of Werner [Heisenberg] and Carl Friedrich [von Weizsäcker] was in part amusing and in part sad. In addition to other peculiar things, C. F.

46. Weiner (1971c). Powers (1993), pp. 158-160.

47. Meitner to Laue, 20 April 1942, in Lemmerich (1998), p. 181. Laue responded in a letter of 26 April 1942: "As regards what you tell about Dr. Møller's lecture, unfortunately I am unable to evaluate it as I have not worked with these very modern theories." Lemmerich (1998), p. 184.

> seems to have curious thought processes: he appears to believe in certain 'constellations', but I beg you to keep this confidential. I was quite sad to hear all this, as at one time I thought highly of both of them. It was a mistake.[48]

After Møller had returned to Copenhagen, Meitner wrote him: "Once again I want to thank you warmly for your beautiful lecture. In these times one is doubly receptive to valuable scientific stimuli and interesting discussions. And the nice chatter evening with you was an extra bonus."[49]

Shortly after the end of the war, Meitner referred to her conversation in a letter to the physicist Paul Scherrer in Zurich. She confided to Scherrer that, as Møller had reported, Heisenberg had come to Copenhagen

> ... to stage a German physics congress, and was absolutely unable to understand that this was not fair. He was entirely imbued with the wish-dream [*Wunschtraum*] of German victory and had developed a theory of higher-level people and serf-people to be ruled by Germany; in this connection he considered the occupation of Denmark and Norway to be 'regrettable' [presumably because Danes and Norwegians could be classified as 'Aryans'].[50]

Of more importance, Heisenberg also had a much-discussed private conversation with Bohr concerning among other things the possibility of military application of nuclear energy based on uranium fission. The meeting with Bohr went all wrong and caused the once so warm relationship between the two physicists to cool close to the freezing point. Much later the Heisenberg-Bohr dialogue was immortalised by the English playwright and novelist Michael Frayn in his successful *Copenhagen* first staged in 1998. In the beginning of

48. Meitner to Laue, 20 April 1942, in Lemmerich (1998), p. 181. Sime (1996), p. 301. The belief in constellations is a reference to astrology.

49. Meitner to Møller, 14 April 1942 (CMP; in German). Powers (1993).

50. Meitner to Scherrer, 26 June 1945, quoted in Sime (1996), p. 301. The square brackets are due to Sime.

Fig. 20. German soldier on guard in front of Bohr's institute in late 1943 or early 1944. Credit: Niels Bohr Archive, Photo Collection, Copenhagen.

this play Heisenberg contemplates of how to face Bohr and speak with him:

> First of all, there's an official visit to Bohr's workplace, the Institute for Theoretical Physics, with an awkward lunch in the old familiar canteen. No chance to talk to Bohr, of course. Is he even present? There's Rozental ... [Aage?] Petersen, I think ... Christian Møller, almost certainly. ... It's like being in a dream. You can never quite focus the precise details of the scene around you. At the head of the table – is that Bohr? I turn to look, and it's Bohr, it's Rozental, it's Møller, it's whoever I appoint to be there.[51]

Due to increased popular resistance and sabotage activity, in late August 1943 the German authorities declared martial law. Shortly later, Hitler ordered all Danish Jews to be deported to Germany. The arrest of Bohr as an enemy of the Nazi regime was imminent and leaked to him. On the night of 29 September, with the help of the resistance movement Bohr, his wife, and his brother Harald escaped to Sweden. Bohr's sons followed separately and after a brief stay in Sweden Bohr and his son Aage went on to England on 6 October. As substitutes for its founder and director, Møller and Jacobsen functioned as heads for the physics institute until Bohr returned on 25 August 1945. At first the arrangement was informal, but on 5 February 1944 the Ministry of Education appointed Møller and Jacobsen as the new board of the institute supplied with the physicist Jørgen Bøggild as secretary.[52]

The running of the institute seemed secured, but as Møller explained, "we were not innocent from the point of view of the Gestapo ... the students made already illegal newspapers and distrib-

51. Frayn (2003), p. 7. Aage Petersen was Bohr's assistant from 1952 to 1962 but much too young to have been on the scene in 1941. Historians and physicists have discussed what went on at the meeting, more precisely. See Dörries (2005).

52. *Aarbog for Københavns Universitet 1943-1944*, p. 46. Also Rozental and his wife Hanna succeeded to escape to Sweden, which they did a few days before Bohr. So did the secretary Sophie Hellmann and the physical chemist Hilde Levi, who had come to Copenhagen in 1935.

uted them during lectures. We knew all about that, and as a matter of fact when the Institute was occupied, there was a whole stack of illegal papers which Hans Seuss [Suess] took away."[53] Indeed, on the morning of 6 December 1943 Bohr's institute was occupied by German police soldiers. Bøggild and Holger Olsen, head of the machine shop, who both lived at the institute premises, were imprisoned. The secretary Betty Schultz also lived there but being a woman with no knowledge of the scientific work she went free. According to the recollections of Rozental:

> Only one person tried to get into the occupied building. Christian Møller approached the commanding officer for permission to go to his desk to get his cigars, one of the most sought-after luxury items at that time. We teased him with this for a long time after. Later he told me that the real reason for this request was much more serious. He was afraid he had left, in one of his desk drawers, the address of a contact person that Jensen had given him on his last visit to Copenhagen in case he needed to get in touch with him in Hamburg.[54]

In his interview with Weiner, Møller largely verified the story told by Rozental, but added that what he was really looking for was a code address given to him by Jensen. However, he was unable to find it. Møller recalled that one of the police soldiers found in his desk a collection of stamps and asked him if he could have one. "Then I said, 'Well, look here, now you have occupied the country, you have occupied the Institute, and now you ask for permission to take a stamp? Go ahead.' I talked very openly with them."[55] In June 1946 the prominent British science journalist James Gerald Crowther visited Copenhagen, where he had conversations with Møller and other Danish scientists. Møller told him about the

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53. Weiner (1971c).
54. Rozental (1998), p. 67, who was in Sweden at the time and was undoubtedly told about the cigar story by Møller.
55. Weiner (1971c). In a postcard of 17 December 1946 the Norwegian physicist Egil Hylleraas thanked Møller for "your personal courage, which nearly brought you into a Danish prison" (CMP).

stamp story and also about another incident during the period of occupation:

> One day Møller came into his room and found a German officer sitting there. This Nazi said he was building a cyclotron near the Hungarian frontier and he wanted their advice. Møller told him about the limitations of the cyclotron through the relativistic increase in mass which comes with very high particle speeds. "But the theory of relativity is due to Einstein, isn't it?" said the officer, who followed this by asking "What does the outside world think about the theory of relativity?" Møller told him that they believed in it. The officer said that that was very interesting.[56]

At one stage the occupation authorities wanted to appoint a German director of the institute, and they proposed Weizsäcker for the position. However, Weizsäcker firmly declined, such as he told Heisenberg in a letter of mid-January: "Although it practically goes without saying, I wish to give you definite assurance that I would be decidedly unhappy to take on that kind of post. If this plan is still intended, I would be very grateful to you if you could use your influence to change it."[57]

Olsen was released in mid December. In early January Møller happened to meet with the Austrian physical chemist Hans Eduard Suess who participated together with Heisenberg and others in the *Uranverein*, the German atomic energy project. Suess had been in Norway to examine the facilities producing heavy water and on his way back to Germany he stopped over in Copenhagen, where he was informed about the latest developments concerning the institute on Blegdamsvej. After having telephoned Møller, who knew him, the two arranged to meet and Møller urged him, when he returned to Berlin, to inform Heisenberg about the situation in the institute he

56. Crowther (1949), p. 108. Crowther to Bohr, 4 May 1946 (BSC); Bohr to Crowther, 21 May 1946 (BSC). Crowther had first visited the Copenhagen institute in April 1932, when he reported to the *Manchester Guardian* about the Easter conference.
57. Weizsäcker to Heisenberg, 18 January 1944, quoted in Schwarz (2021), p. 54.

admired and knew so well.[58] The following day Suess got in contact with Heisenberg and told him: "Yesterday I talked with Christian Møller, and from what I heard, it seems that the Danes expect you to come to Copenhagen and to help avoid the looting of the Danish Institute. I am not supposed to say anything to this effect, but it was my impression, that they greatly hope that you will help them."[59]

Suess' account of what happened is confirmed in a report written by Møller and Jacobsen: "After Süss' [Suess'] departure for Norway, M. suggested to inform Heisenberg, an old friend of the institute and a pupil of Bohr, of what had happened at the institute in the hope that he might do something about it."[60] Møller also informed Rozental in Stockholm about the situation, and Rozental passed the information on to Bohr: "Möller did not address Heisenberg with a request for help but only told an acquaintance [Suess], who was passing through, that the institute was occupied."[61] For a while nothing happened. As Møller told Rosenfeld in early January, the situation caused great difficulties for the people at the institute, but "As a theoretician I guess I am not the worst off since my fountain pen is still intact."[62]

In Berlin, Suess and Jensen met with Heisenberg and persuaded him to be part of a scientific commission formed to investigate whether secret war work took place at the institute.[63] Another member was Kurt Diebner, head of the German uranium project. As a member of the commission, Heisenberg arrived in Copenhagen on 24 January 1944 and the same evening he met with Jacobsen and Møller in the latter's home. After Heisenberg and the other commis-

58. The section is in part based on Schwarz (2011) and Schwarz (2021).

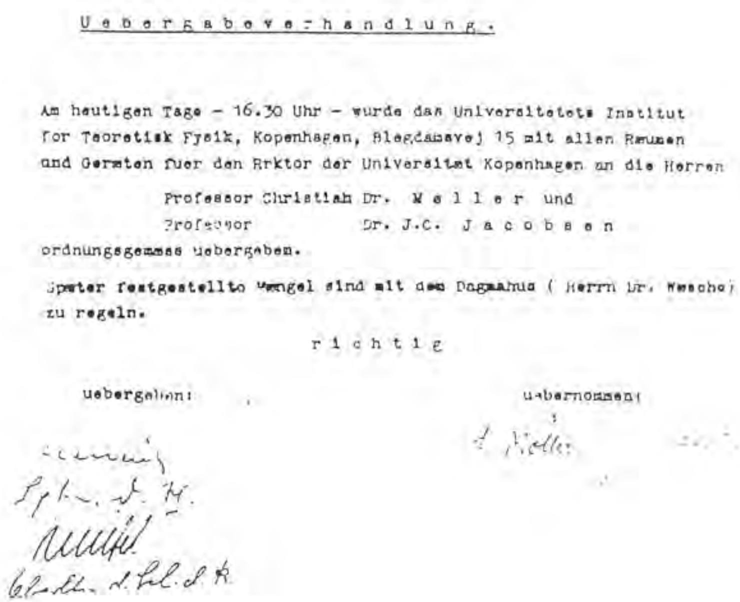
59. Powers (1993), p. 330, who quotes from an unpublished memoir by Suess.

60. Undated report on 'The Events During the Occupation of the University's Institute for Theoretical Physics from 6 December 1943 to 3 February 1944', 15 pp. (BSC, Supplement).

61. Rozental to Bohr, 20 April 1944 (BSC, Supplement).

62. Møller to Rosenfeld, 4 January 1944, quoted in Jacobsen (2012), p. 175.

63. In letters to Møller after the war, Jensen and Suess referred to the episode with the occupation of the institute and their role in involving Heisenberg in the solution of the problem. Jensen to Møller, 14 April 1946 (misdated 1942), and Suess to Møller, 12 August 1946 (CMP).



Uebergabeverhandlung.

Am heutigen Tage – 16.30 Uhr – wurde das Universitetets Institut for Teoretisk Fysik, Kopenhagen, Blegdamsvej 15 mit allen Raeumen und Geraeten fuer den Rektor der Universitaet Kopenhagen an die Herren

Professor Christian Dr. M ø l l e r und
Professor Dr. J.C. J a c o b s e n

ordnungsgemaess uebergeben.

Spaeter festgestellte Maengel sind mit dem Dagmarhus (Herrn Dr. Wescho) zu regeln.

r i c h t i g

uebergeben: uebernommen:

Fig. 21. The end of the German occupation of Bohr's institute on 3 February 1944. The document is signed (scarcely legible) by Møller and Jacobsen. Credit: Niels Bohr Archive, Photo Collection, Copenhagen.

sion members had been on an inspection tour to the institute, it was decided that no war work was done and that the occupation should end without any conditions. Bøggild, an expert in cloud-chamber measurements, was released from the prison on the 26th, and "the same day — it was before Heisenberg left — we had a small party in our home to celebrate Bøggild's coming out from prison, and Heisenberg was there also."[64] Heisenberg left after having spent three days in Copenhagen. Møller recalled: "First they wanted to place some Germans here in the Institute, and I told them at once, 'Don't do that. This will not work. Nobody of the Danes will work together with the Germans.' So finally … our only obligation was to

64. Weiner (1971c). Bøggild was one of the few Copenhagen physicists working with cosmic rays, which was the subject of his doctoral dissertation of 1937.

publish everything we found out during the war."[65] The institute was officially returned to Copenhagen University on 3 February 1944.

In a letter to Møller after the end of the war, Jensen confirmed his role in persuading Heisenberg to go to Copenhagen in January 1944. "I did not write you since my last visit, for one reason because I would not trouble you with letters from Germany", Jensen said. But there was another and more important reason for his silence: "After he tried by means of informing to bring Harteck into the hands of the Gestapo, the Hamburg Nazi physicist P. P. Koch also had denounced me to the Gestapo, claiming that I was an enemy of the state because of my good connections to Copenhagen."[66] The Austrian physical chemist Paul Harteck, known as a co-discoverer of tritium (H-3) while working with Rutherford in 1935 and twice nominated for a Nobel Prize in chemistry, was involved in the German uranium project where he cooperated with Suess and Jensen. In 1947 he was brought before a denazification tribunal but acquitted after Heisenberg had witnessed in his favour.

Heisenberg came to Copenhagen once more during the war. On the request of the German Cultural Institute, he spent four days, 18-22 April 1944, as the guest of its new director Otto Hölfer. On the 19th he gave a general talk on 'The Smallest Building Blocks of Matter' and he also visited the institute on Blegdamsvej, where he lectured in Danish on 'The Theory of Elementary Particles'.[67] On the evening the following day Heisenberg met with Danish physicists and their wives as the guest of Møller. It was almost like in the good old days – but only almost.

The only contact Møller had with Bohr during the last years of the war was by means of secret messages communicated by an officer at the British Secret Service. Møller recalled:

65. Weiner (1971c).
66. Jensen to Møller, 14 April 1946 (CMP). Peter Paul Koch (1879-1945) was professor of experimental physics in Hamburg and an ardent Nazi.
67. Dörries (2005), p. 96. Schwarz (2021), p. 58. For the topic of Heisenberg's lecture of 19 April, see Rechenberg (1989), p. 575.

> When Bohr had come to England, I received in the fall of 1944 a secret letter from him. A courier had brought it to Sweden and from there across Øresund [the Sound]. Hidden within a cigarette case there was a microfilm which asked for information about how far the German scientists had come in their nuclear research. The letter could be a forgery staged by the Germans. However, I consulted Bohr's old friend professor Ole Chiewitz, who attested that the content of the film was genuine.[68]

In the unsigned secret microfilm letter, Bohr wrote:

> This letter, which must be destroyed immediately, ... concerns the question of whether there are efforts underway on the part of Germany to utilize nuclear energy as a weapon of war. As you know, I have been worried about this ever since Heisenberg's visit to Copenhagen in the autumn of 1941. ... It is of particular interest to know whether there is any information at all in Copenhagen about German physicists, which could possibly give us a hint about where they are working and what they are doing. ... Naturally, it is not of least interest to know whether there is complete certainty about Jensen's attitude.[69]

Møller replied on 29 September 1944:

> It is still my conviction, that Je. and S. are completely reliable and I would think that their description of the prospects for an application of the methods in question on the whole is correct. H. and W. do not seem to have worked with the question during the last year, and I know for certain that they have been preoccupied with completely different matters. ...It was reported in the spring that B. was still in H. [Heidelberg], where he also had such facilities available to him, as you

68. Interview in *Jydske Tidende*, 15 March 1970. At the time Bohr was actually in the United States, not in England. Ole Chievitz (1883-1946), a professor of medicine, was not only a close friend of Bohr but also a leading member of the Danish resistance movement.
69. Bohr (2005), p. 244.

> mentioned in your letter. ... We much look forward to seeing you again and expect that now it will not be so long.[70]

Less than a year later, World War II ended after the Americans had dropped atomic bombs over the Japanese cities Hiroshima and Nagasaki. When the reality of the new weapon became public, it attracted massive media attention. Bohr was still in the United States and so Danish journalists approached Møller as the acting director of the 'atomic institute' on Blegdamsvej. On 11 August 1945, the same day that Nagasaki was destroyed by a plutonium bomb, Møller was interviewed about the atomic bomb and the potentials of nuclear energy. He told the reporter: "The Americans have worked so forcefully with the atomic problem that they almost certainly have solved it. ... A bomb must be constructed in such a manner that only the heaviest atoms are involved. For example, although a house hit by a bomb will be destroyed, the atoms in the bricks will not themselves take part in the explosion." Møller evidently had no idea of how the secret weapon was constructed. "An atomic bomb must undoubtedly be very large, as it must contain at least one ton of uranium and this substance must be mixed with other substances," he said.[71]

Although a member of the Nazi Party since 1933, during the war Pascual Jordan was not engaged in the German uranium project or other forms of military research. After the war had ended, he wrote a letter to Bohr in which he expressed his gratitude to Møller for having made his scientific life a little less isolated: "If Mr. Møller is still at Kjøbenhavn [Copenhagen] I should like to send him many kind regards and many thanks for having helped me during the war not to lose every contact with the scientific development, by sending me copies of his papers and those of his friends. I was very sorry that during the war I had no possibility to thank him."[72]

70. Bohr (2005), pp. 264-265. Je. = Jensen; S. = Suess; H. = Heisenberg; W. = Weizsäcker; B. = Bothe.

71. *Kalundborg Avis*, 11 August 1945. Nicknamed 'Little Boy', the total weight of the Hiroshima bomb was about four tons, but it contained only 64 kg of enriched uranium.

72. Jordan to Bohr, 24 October 1945 (BSC; in English).

In the spring of 1946 Jordan addressed Møller, whom he informed about his recent works in general relativity and cosmology:

> I have been occupied with the question of how to make a reasonable extension of the theory of relativity so that it conforms with Dirac's idea that the gravitational constant should be seen as a truly varying quantity. ... At the moment I work on the calculation of cosmological models according to the new field equations; it turns out that everything is in full harmony with the cosmological ideas that I previously developed on different grounds. I have completed a manuscript on this problem, but I have doubts with regard to the prospects of getting it published.[73]

As far as I can tell, Møller did not respond to the letter from his German colleague.

73. Jordan to Møller, 30 April 1946 (CMP; in German). For Jordan's cosmological theories, see Section 7.3 and Kragh (2016).

CHAPTER 5

The enigmatic nuclear force

Still when Møller completed his doctoral dissertation in the summer of 1932, the atomic nucleus was conceived as a tightly packed bag of protons and electrons. A year or two later, the neutrons had replaced the now forbidden nuclear electrons. As regards Møller's work on electron-electron scattering the radical change in the conception of the nucleus was of no relevance, but otherwise it was of monumental importance. For one thing, it highlighted the question of how the protons were kept together in the nucleus and thus stimulated new research on the short-range nuclear force that Rutherford had postulated in his seminal work of 1911 in which he introduced the atomic nucleus. With some delay Yukawa's prediction of the meson, a new elementary particle with mass between the electron and the proton, aroused much attention among Western theorists. By the late 1930s Møller was deeply immersed in developing a meson theory of nuclear forces, a line of work which resulted in the so-called Møller-Rosenfeld mixed field theory. The theory was much discussed at the time, but after a decade or so it was abandoned.

During the late war years, while still working on meson theory Møller became intensely occupied with a new fundamental theory of quantum mechanics proposed by Heisenberg. The ambitious *S*-matrix theory, as it was called, appealed to Møller because of its foundational nature and mathematical challenges. But also in this case, after much hard work he was forced to conclude that his efforts had been largely in vain. Although the *S*-matrix theory continued to fascinate him and many other physicists, after 1946 he no longer believed in it as a viable framework of quantum theory.

After peace had been restored in the summer of 1945, Møller and many other physicists engaged in a hectic series of travels and conferences, many of them dealing with the confusing number of mesons revealed in experimental studies of the cosmic rays. Møller, collaborating in part with Rosenfeld and Pais, proposed an ambitious unified theory of all elementary particles which however was short-lived as it turned out to be of limited scientific value. It was also in this period that Møller, for the first time, visited the United States and generally became internationally recognised as an important figure in theoretical particle physics. As one indication of his status, he was invited to participate in the prestigious 1948 Solvay congress.

5.1. The rise of particle physics

When Møller wrote his first research paper in 1929, it was universally believed that all matter consists of only two elementary particles, the negative electron and the positive proton. Just a few years later, the 'two-particle paradigm' broke down. The discovery of a number of new particles heralded the beginning of a new speciality in physics, elementary particle physics, which after World War II developed into a flourishing scientific discipline.[1] Chadwick's discovery of the neutron in 1932, and the slightly later recognition that it was a true elementary particle (and not a proton-electron composite), destroyed the harmonious two-particle consensus which had served physics so well as a paradigm for nearly two decades.

The neutron was arguably the most dramatic of the actors in what is often referred to as the *annus mirabilis* of nuclear and particle physics, but what more appropriately might be called the *anni mirabiles* 1931-1933. It was not the only actor, though, nor was it the first. The neutrino had been suggested a little earlier, and the same was the case with Dirac's antielectron which by 1934 had become the positron discovered not only in cosmic rays but also in the new phenomenon of artificial radioactivity. In his important paper of 1931, Dirac not only hypothesised the existence of positive electrons and negative protons, he also predicted that magnetic monopoles – magnetic charges quantised in multiples of the constant $\hbar c/e$ – existed and might be detected experimentally. However, this did not happen. To this day the magnetic monopole remains a well-known undiscovered particle.

1. The origin and early development of particle physics is summarised in Kragh (1999). For more details, see Brown and Hoddeson (1983).

There were other novelties. In late December 1931, the American physical chemist Harold Urey and collaborators announced the discovery of a mass-2 isotope of hydrogen, which they called deuterium. While Urey initially thought of the deuteron – the nucleus of the deuterium atom – as made up of two protons and one electron, in his discovery paper of 1932 the electron had disappeared and the deuteron conceived as a proton-neutron composite in accordance with the new view of nuclear constitution. Although not a true elementary particle, the deuteron proved to be as important for nuclear physics in the 1930s and 1940s as the alpha particle had been in the Rutherford era. In 1934, deuteron experiments at the Cavendish Laboratory revealed the existence of a still heavier hydrogen nucleus, the artificially produced 'triton' consisting of one proton and two neutrons belonging to the unstable tritium isotope ^{3}H.

The poorly understood penetrating component of the cosmic rays was an essential and most fertile hunting ground for the early generation of particle physicists. By the mid-1930s it was realised that the highly penetrating particles were charged and apparently, for some unknown reason, mostly positively charged. Some of the cloud chamber tracks were anomalous, suggesting that they were due to either very light protons or very heavy electrons. Both entities were considered to be absurdities. The nature of the unknown signals from the heavens caused much discussion and confusion. For a while there seemed to be two alternatives, either that quantum electrodynamics broke down at high energies or that a new elementary particle, intermediate in mass between the proton and the electron, was in play. Almost all physicists chose the first alternative, but the solution of the enigma turned out to lie with the second.

In the spring of 1937, the Caltech physicists Carl Anderson and Seth Neddermeyer concluded that the particles were in fact a strange and unanticipated kind of heavy electrons. In their discovery paper published in *Physical Review* on 15 May they did not propose a name for the new particle, which they only did more than a year later in a letter to *Nature* dated 30 September 1938. The two physicists noted that "several names have already been suggested, namely, dynatron, penetron, barytron, heavy electron, Yukon, and *x*-particles", and for this reason they wanted to consider a more appropriate name. "We should like to suggest therefore the word 'mesotron' (intermediate particle) as a name for the new particles."[2] At the same time the director of their institute, the Nobel Prize laureate Robert Millikan, advocated the name to Bohr: "I am writing to you to express the hope that the name for this particle which Anderson has suggested and which seems to me to be the most appropriate, namely 'Mesotron' ... will be generally adopted. Does it not seem to you that this is the most appropriate designation which has yet been suggested?"[3] Bohr replied:

> I take pleasure in telling you that every one at a small conference on cosmic-ray problems, including Auger, Blackett, Fermi, Heisenberg, and Rossi, which we have just held in Copenhagen, was in complete agreement with Anderson's proposal of the name 'mesotron' for the penetrating cosmic-ray particles. ... At the moment I do not know whether one shall admire most the ingenuity and foresight of Yukawa or the tenaciousness with which the group in your institute kept on in tracing the indications of the new effects.[4]

It is worth noting that 'mesotron' was not the original choice of Anderson and Neddermeyer, but due to Millikan. Anderson recalled that his and Neddermeyer's note to *Nature* referred to 'mesoton' without *r*, but then their boss intervened:

> He immediately reacted unfavourably and said the name should be *mesotron*. He said to consider the terms *electron* and *neutron*. I said to consider the term *proton*. Neddermeyer and I sent off the *r* in a cable to *Nature*. Fortunately or not, the *r* arrived in time, and the article appeared containing the word *mesotron*. Neither Neddermeyer nor I liked the word, nor did anyone else that I know of.[5]

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2. Anderson and Neddermeyer (1938).

3. Millikan to Bohr, 28 September 1938 (BSC).

4. Bohr to Millikan, unknown date but probably November 1938, quoted in Millikan (1947), pp. 509-510. In a letter to *Physical Review* dated 7 December 1938, Millikan (1939) cited Bohr in support of the name 'mesotron'. See also Monaldi (2008) and Brown and Rechenberg (1996), p. 187.

5. Anderson and Anderson (1983), p. 148.

Nonetheless, it was the word adopted at a conference on cosmic rays held in Chicago in late June 1939. As Arthur Compton explained in a foreword to the proceedings: "In the original papers and discussion no less than six different names were used. A vote indicated about equal choice between *meson* and *mesotron* with no considerable support for *mesoton*, *barytron*, *yukon* or *heavy electron*. Except where the authors have indicated a distinct preference to the contrary, we have chosen to use the term *mesotron*."[6]

Although the Caltech name 'mesotron' was widely used for several years, eventually it lost out to the abbreviated form 'meson' first suggested by Homi Bhabha in early 1939. Bhabha pointed out that the 'tr' in mesotron was redundant, "since it does not belong to the Greek root 'meso' for middle; the 'tr' in neutron and electron belong, of course, to the roots 'neutr' and 'electra'." Bhabha consequently found it "better to follow the suggestion of Bohr and to use electron to denote particles of electronic mass independently of their charge, and negaton and positon to differentiate between the sign of the charge. It would therefore be more logical and also shorter to call the new particle a meson instead of a mesotron." In a letter to Bohr sent the same day as he submitted his note to *Nature*, Bhabha asked Bohr to confirm that he was happy with meson as an alternative to mesotron: "I have allowed myself to call the new particle a meson, ... [but] I am prepared to turn it into 'mesotron' though I had the impression in London that you yourself favoured meson."[7]

The name 'negatron' for the ordinary electron was proposed by Anderson in 1933, but it never caught on. Neither did the names 'negaton'and 'positon', although they were in use for a period of time. Møller evidently liked the names negaton and positon, which he not only used in his research publications but also in his teach-

6. *Reviews of Modern Physics* **11** (1939), p. 1. The Chicago meeting 27-30 June 1939 was attended by about 300 physicists.

7. Bhabha (1939, dated 17 December 1938). Bhabha to Bohr, 17 December 1938 (BSC). See also Singh (2009) and Darwin (1939). Bohr first used the negaton-positon terminology in an article completed in January 1938, see Bohr (2007), pp. 49-64.

ing.[8] Bhabha's paper was published in the 18 February issue of *Nature*, but 'meson' actually appeared for the first time in print a week earlier, namely in an article by Møller and Rosenfeld who in a footnote referred to Bhabha's forthcoming paper.[9] Bhabha instantly addressed Møller on the terminological issue:

> I notice with pleasure that you have called the new particles mesons. ... As a result of conversations with Blackett in Manchester, & in absence of a reply from Bohr or you about the name 'meson', I had changed the name in the title of my note back to mesotron. The footnote to that effect – that 'meson' is a better name still remains, & in the text I still call the particle a meson. If I had received your letter, I should not have changed the title to 'mesotron'. I should like to know if you did write a letter which has got lost in the post.[10]

Millikan tried to persuade Bohr and Bhabha to use 'mesotron' instead of 'meson', but in both cases unsuccessfully. At a time when Bhabha had moved from Cambridge to work at the Bangalore Institute of Science in India, Millikan wrote him:

> I estimate that there are about five times as many of us who are preferring the original name mesotron to those who have changed over. Would it not be desirable for us to get together on the name mesotron, which is most generally used and which the majority of us think in view of the fact that we already have electrons and positrons is in every particular the logical one to use for another particle? I think that Eddington, Yukawa, [William] Swann, Compton, and all the groups around them,

8. In his lectures on quantum mechanics given to students in Copenhagen from about 1958 to 1970, he consistently wrote on positons rather than positrons. Møller, *Forelæsninger Over Kvantemekanik* (Lectures on Quantum Mechanics), Copenhagen, 1958-1967.

9. Møller and Rosenfeld (1939a), dated 6 January.

10. Bhabha to Møller, 11 February 1939 (CMP). When Bhabha's paper appeared in print, its title referred to "the Theory of the Mesotron (Meson)."

> are in agreement with our usage here at the Institute. I suspect that if you and Bethe would join us the other term would rapidly disappear.[11]

But 'meson' did not disappear and Bhabha continued to use it in his papers such as did a growing number of other physicists. By 1950 'mesotron' was no longer part of the vocabulary of physicists.

Terminology apart, the discovery of the new heavy electron was experimental and not guided in any way by theoretical predictions. Its mass was at first estimated to be about 130 m_e, which caused further confusion. The reason was Yukawa's earlier prediction of a nuclear field quantum of mass approximately 200 m_e, which only attracted wide interest in the wake of the discovery of the heavy electron. The 'U-field' or heavy quantum introduced by Yukawa in 1935 referred to both of the known nuclear interactions, not only to the proton-neutron binding but also to beta decay. The two interactions were distinguished by different coupling constants. He suggested the nuclear or proton-neutron field to be of finite range $1/\lambda$ and stated the potential to be given by

$$V(r) = \frac{g^2}{r} e^{-\lambda r}$$

where g is a new universal constant which expresses the strength of the nuclear force. For the λ constant he adopted the value 5×10^{12} cm^{-1} or $1/\lambda = 2 \times 10^{-13}$ cm. Moreover, he showed that the potential satisfies the wave equation

$$\left(\nabla^2 - \frac{1}{c^2}\frac{\partial^2}{\partial t^2} - \lambda^2\right) V = 0$$

In the electromagnetic interaction given by the Coulomb potential e/r the exchange particles are massless photons and the strength given by the dimensionless fine structure constant $2\pi e^2/hc$. The theory proposed by Yukawa involved particles of finite rest mass μ and the strength of the nuclear interaction was given by the much

11. Bhabha to Millikan, 3 January 1941, excerpted in Millikan to Bohr, 9 January 1941 (BSC).

larger constant $2\pi g^2/hc$. Yukawa found a fundamental length-mass relation which appealed to many of the physicists who took up his theory. With μ denoting the mass of the hypothetical nuclear quantum, Yukawa's relation was

$$1/\lambda = h/2\pi\mu c$$

From this relation he found μ to be of the order 200 m_e.

After Yukawa's theory had become known, a question naturally arose: could the Anderson-Neddermeyer mesotron be the same as the Yukawa particle? By the late 1930s the lifetime of the nuclear meson was estimated to be 10^{-7} s and that of the cosmic ray meson to be about ten times higher. Nonetheless, most physicists convinced themselves that the discrepancy could be ignored and that the discovery in 1937 justified Yukawa's prediction three years earlier.[12] This was also Bohr's opinion, such as he indicated in a letter to Klein of early 1938:

> It looks as if the existence of the 'Yukon' is not only supported by various observations but also fits most conveniently with the theoretical treatment of nuclear problems. β-rays and the Yukon phenomena really seem to offer what is of course still a very incomplete experimental foundation for correspondence-like considerations which would give the nuclear forces an unambiguous character similar to the chemical valence forces.[13]

When Yukawa became aware of the experimental results of Anderson and Neddermeyer, he thought that his own and at the time not well-known theory might provide an explanation. On 18 January 1937 he sent a letter to *Nature* in which he suggested that "it is not altogether impossible that the anomalous tracks discovered by Anderson and Neddermeyer, which are likely to belong to unknown rays with *e/m* larger than that of the proton, are really due to such [nuclear] quanta, as the range-curvature relation of these tracks are

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12. Monaldi (2005).

13. Bohr to Klein, 13 January 1938 (BSC). The name 'yukon' was occasionally and mostly informally used for the hypothetical particle introduced by Yukawa.

not in contradiction to this hypothesis." Alas, his letter suffered the same fate as Fermi's earlier letter on beta decay: the editor rejected it.[14] It took several years until it became clear that there is no such thing as a Yukawa-Anderson-Neddermeyer particle.

In the absence of a satisfactory theory of mesons, a few physicists came up with tentative speculations rather than theories based on established quantum physics. For example, in 1941 Patrick Blackett suggested that "the mean life of the meson at rest probably depended on the gravitational constant, and so, through general relativity theory, on the total mass of the universe."[15] His Eddington-inspired suggestion for the lifetime of the meson (or mesotron) was

$$\tau_0 = \frac{e^3}{\mu^2 c^3 \sqrt{G}}$$

where μ denotes the mass of the Anderson-Neddermeyer meson. Although Blackett's relation was qualitatively correct, it was widely considered a numerological speculation and for this reason disregarded by most specialists in meson theory. The same was the case with Eddington's considerations based on his heterodox fundamental theory, according to which "mesotrons have no connexion with the so-called Yukawa particle." In this he happened to be correct, if only by chance. For the mass of the Anderson-Neddermeyer mesotron Eddington derived the value 174.44 m_e and "presumably there can exist also 'heavy mesotrons' which change into protons (or negatrons)."[16] For these hypothetical particles he found a mass of 2.36 m_p or 4354 m_e.

A small group of theoretical physicists, among them Møller, Rosenfeld, Sakata, Heisenberg, Kemmer, and Bhabha, endeavoured

14. Brown and Rechenberg (1996), p. 123. Together with S. Sakata and M. Taketani, Yukawa sent a similar letter to *Physical Review* on 4 October 1937, but this too was rejected. The two unpublished letters are reproduced in Kawabe (1988).

15. Blackett (1941), p. 213. According to Einstein's cosmological model of 1917, the mass of the closed universe varied with the gravitational constant raised to the power of – 3/2.

16. Eddington (1940), p. 48.

to develop Yukawa's meson theory into a divergence-free theory of the short-range nuclear force. Some of them aimed at a more comprehensive and unified theory covering all known nuclear particles and interactions. Much ink was spilled on the ambitious project, but after years of hard work it was recognised that the mathematically abstruse theories were of little physical significance. Somehow the meson played a central role in the nuclear force, but by the early 1950s there still was no satisfactory meson theory accounting for the force. According to Klein, not only was the nuclear field mediated by mesons, the nucleons themselves were made up of stable compounds of mesons. This is what he speculated in 1948 "in an attempt to regard the nuclear field force theory as a generalization of the relativistic field theory of electromagnetism and gravitation."[17]

Only after the war was it understood that there existed at least two very different kinds of mesons, with different masses, lifetimes, and spins, and that the weakly interacting particle discovered in 1937 arose from the decay of the strongly interacting Yukawa meson ($\pi^{\pm} \rightarrow \mu^{\pm} \rightarrow e^{\pm}$). Now the first particle became known as the π meson and the latter as the μ meson. Later again the names were changed to the presently used words pion and muon, a terminology which goes back to the early 1950s. With the recognition of two different kinds of mesons, the dream of a unified meson theory of nuclear forces had to be abandoned. Since the strongly interacting π mesons quickly decayed into μ mesons, they could not be responsible for the weak beta decay.

The important two-meson theory dating from 1947 was primarily based on observations made by Cecil Powell and his research group in Bristol, and theoretically it was argued by Bethe and Robert Marshak, a physicist at the University of Rochester. The π mesons were not only found in cosmic rays but also in accelerator experiments, first in 1948. Two years later, experiments at the Berkeley synchrocyclotron resulted in the discovery of a neutral π meson with puzzling properties. It turned out to have a mass slightly smaller than the charged pion ($\Delta m \sim 10 m_e$) and to decay very differently and with a lifetime as short as 10^{-16} s. The neutral π meson was

17. Klein (1948).

identified by the conversion of gamma rays into electron pairs: $\pi^0 \to 2\gamma$ followed by $2\gamma \to e^+ + e^-$.

Shortly after the discovery of the pion, evidence was reported for the existence of new, unexpected particles. In 1947 George Rochester and Clifford Butler from Manchester produced cloud chamber photographs of what were then called '*V*-particles', recognised in the following year to be heavy mesons. More strange particles followed, none of which had been anticipated by the theorists or only vaguely so. With the diversity of new particles, some of them with lifetimes of the order 10^{-10} s, the scene of particle physics seemed at the same time confusing and challenging. At a meeting in Bagnère de Bigorre in France in July 1953, the International Congress on Cosmic Radiation suggested dividing the strange particles (as they were called) into two groups, the *K* or heavy mesons with mass smaller than the proton, and the 'hyperons' or *H* particles with mass larger than the proton but smaller than the deuteron. The *L* or light mesons included π and μ mesons with masses up to 283 m_e. At the same meeting a new and more rational nomenclature was suggested for the new particles.[18]

In the early 1950s particle physics was established as a flourishing and exciting field of research which was to a large extent dominated by American physicists. New post-war technologies, especially high-energy accelerators and sensitive detectors, helped to transform the field and make it attractive to both experimentalists and theorists. Whereas in the period from about 1930 to 1950 the most important source of new particles had been the cosmic rays, in the 1950s high-energy accelerators took over. Likewise, experimental detectors changed: in the early period cloud chambers and Geiger-Müller counters were the favoured detectors, later to be followed by the photographic emulsion which proved particularly useful in cosmic ray studies. In 1953 the invention of the bubble chamber initiated a new chapter in experimental high-energy physics and eventually made older detection devices obsolete in accelerator experiments.

18. Cronin (2011).

5.2. Mesons and nuclear forces

In the fall of 1939, Pauli wrote a letter addressed to the "Dear authors of numerous notes in Nature." The addressees were Møller and Rosenfeld, who in a series of notes to *Nature* had introduced a new meson theory of nuclear forces which Pauli followed "with a certain interest – however, also with scepticism."[19] As mentioned in Section 3.2, since June 1938 Møller and Rosenfeld had become seriously interested in Yukawa's meson theory, which they first discussed during their travel to Warsaw. In a letter to Klein, Bohr informed him about the ongoing work:

> It will interest you to know that during the last couple of weeks Møller has worked with the fascinating idea of introducing a neutral field corresponding to the real solutions of the Proca equations. ... Such neutral fields are likely to constitute a considerable part of all nuclear forces, not only between identical particles but also between different ones, and Møller contemplates to submit within a few days a little note to Nature about it. Moreover, Møller and Rosenfeld have tried to improve the present representation of the theory of nuclear forces by means of a more rational isolation of the static interactions ... [but] despite a very promising start they have not been able to overcome all the difficulties.[20]

Over the next five years the two physicists published, either jointly or individually, several articles on what at the time was known as the Møller-Rosenfeld theory. Pauli referred to it as the 'patent mixture theory' because it cured some of the divergence problems of the existing theory by making use of a mixture of two different meson fields. The ambitious theory attracted much attention in the early discussions about mesons and nuclear forces. As one indication of the status of Møller and Rosenfeld in the particle physics community, they were invited to serve as scientific secretaries in the eighth

19. Pauli to Møller and Rosenfeld, 25 October 1939, in Pauli (1993), p. 822.
20. Bohr to Klein, 5 July 1938 (BSC). Alexandru Proca's equations of 1936 were relativistic wave equations patterned on the Maxwell equations but in such a way that they described a massive spin-1 particle.

Fig. 22. Léon Rosenfeld (right) in the canteen of Bohr's institute with Walter Heitler. Photograph of 1937, shortly before Rosenfeld started collaborating with Møller on the meson theory of nuclear forces. Credit: Niels Bohr Archive, Photo Collection, Copenhagen.

Solvay congress planned to take place in the third week of October 1939.[21] However, the conference was cancelled because of the war and became a reality only nine years later.

Despite the initial interest in the Møller-Rosenfeld theory, at the end of the war it was largely abandoned, recognised to be a blind alley if far from a complete mistake. The collaboration between the two physicists initially took place either in Copenhagen or in Liège, Belgium, where Rosenfeld worked and spent most of his time. For example, on 8 December 1938 Møller travelled to Liège, where he spent twelve days with his colleague and gave two lectures, one on 'The Energy-Mass Relation' and the other on 'The Theory of β-Decay and Allied Phenomena'.[22] In May 1940 Rosenfeld was appointed

21. Brown and Rechenberg (1996), pp. 239-240.

22. Møller to Rosenfeld, 26 November 1938 (RP). Møller to C. Houtermans, late November 1938, in Shifman (2017), p. 238.

professor of theoretical physics at the University of Utrecht, and about the same time the war made travels between Denmark and the Netherlands nearly impossible. Nonetheless, the collaboration continued, now by means of letters and with the help of assistants and other collaborators. These included Abraham Pais in Utrecht, Jean Serpe in Liège, Lamek Hulthén in Lund, Sweden, and in Copenhagen Stefan Rozental and Ib Nørlund.

Among Møller's early collaborators in meson theory was also the young Chinese physicist Tsung Sui Chang, who after studies at the University of Cambridge under Ralph Fowler came to Copenhagen in the autumn of 1938 to do postdoctoral research at Bohr's institute. Shortly after having left Copenhagen in 1939 Chang completed a work on pseudoscalar mesons according to the Møller-Rosenfeld theory in which he derived an expression for the decay constant of such mesons in terms of the known masses of the electron and the μ meson. In this paper, which was only published in 1942 in a version edited by Møller and Rozental, he thanked "most heartily Dr. C. Møller for his continued interst in the investigation and for many helpful discussions."[23] After further studies in China and the United States, in 1949 Chang was appointed professor of physics in Beijing. However, under Mao Zedong's infamous Cultural Revolution he was faced with great difficulties and in June 1969, only 54 years old, he committed suicide.[24]

In a memorial speech given to the Royal Danish Academy in the autumn of 1974, Møller said about his collaboration with Rosenfeld:

> At the end of the 1930s I was fortunate enough to collaborate with Léon on the further development of Yukawa's theory of nuclear forces. When Rosenfeld was forced to leave Copenhagen because of the outbreak of the war, we continued over many years the collaboration by means

23. Chang (1942). Bohr to Chang, 23 September 1941(BSC): "It has been necessary to revise your manuscript somewhat, a task which has been kindly undertaken by Møller and Rozental."

24. On Chang's life and contributions to theoretical physics, see Yin, Zhu, and Salisbury (2013).

> of correspondence. Today, these early attempts to describe quantitatively the nuclear forces in terms of intermediary meson fields must be regarded as obsolete, as it turned out that the problem is much more complicated than thought at the time. Nonetheless, the main idea of our works, namely that it is necessary to assume the existence of several meson fields with different symmetry, seems to have survived.[25]

Møller seems to have initiated the work that led to his and Rosenfeld's theory of nuclear forces, which he did in a paper in *Nature* dated 9 July 1938 and published 13 August. Klein had been told about Møller's new theory and was eager to know more about it. Since Møller was away on summer vacation in Jutland, he addressed Bohr:

> I did not quite understand if he [Møller] thinks that the same equations which describe the charged Yukons also provide the solutions to the neutral particles, or that the latter are described by other equations of the Proca type. ... I would much appreciate to see a proof or manuscript of Møller's letter to Nature, but at the present you probably do not see him. However, I am not quite convinced about the necessity of neutral particles in order to understand the nuclear forces.[26]

In the autumn of 1938 Møller gave a talk to *Fysisk Forening* in Copenhagen in which he stressed the close similarity between Yukawa's meson theory and Fermi's theory of beta decay.[27] He further argued that in all likelihood mesons appeared in all three states of charge including the neutral state. According to Møller, Yukawa's exchange particles or quanta could best be described by the Proca equations which would also account for the neutral quanta responsible for the forces between two protons and between two neutrons. "The quantization of these equations can be performed exactly as

25. Møller (1975a), p. 66.
26. Klein to Bohr, 16 July 1938 (BSC, Supplement).
27. Møller and Eriksen (1939). The paper was co-authored by his student Bodil Eriksen, who after graduation worked as a physics teacher at Zahle's Gymnasium in Copenhagen. See also Figure 19.

in quantum electrodynamics", he wrote, "As regards energy and momentum these quanta will behave like material particles with rest mass $m = \hbar\kappa/c$."[28]

Moreover, in his talk to *Fysisk Forening* Møller considered the hypothetical endothermic process $\Delta E + n \rightarrow p^+ + \pi^-$which would however, given that the mass of the Yukawa meson was approximately 200 m_e, require $\Delta E > 100$ MeV. "A neutron must have a large energy for this process to occur … at least 100 M. e. V", he said. "Such energies are not available in ordinary atomic nuclei … [but only] in the cosmic rays, which are energetic enough to create mesons in this kind of process."[29] As mentioned in Section 4.1, some months later, after the discovery of uranium fission, Møller and Jacobsen thought for a while that the 200 MeV liberated in the fission process might be responsible for the creation of mesons in the laboratory.

Shortly after the appearance of the *Nature* note, Peierls drew Bethe's attention to what he called Møller's nice [*hübsches*] argument for avoiding some of the problems in the existing theory. Bethe answered that "Møller's argument about the retardation is certainly very nice [and] I think this is a reasonable way of introducing the approximation."[30] Without claiming priority to the idea, Møller suggested to extend Yukawa's theory by taking into account also the massive quanta relating to proton-proton and neutron-neutron interaction. By December Bethe had become sceptical with regard to Møller's ideas:

> Do you really believe, like Møller, that the neutral particles possess real proper functions, i.e., that $\psi^*\mathrm{grad}\psi$ is the electric current and not the particle current? That violates my deepest convictions – and yet it

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28. Møller (1938a). The symbol κ denotes the inverse range of the nuclear forces. While Møller (1938a) just referred to Yukawa's particles as 'quanta', in Møller and Eriksen (1939) he called them 'mesons'.
29. Møller and Eriksen (1939), p. 185.
30. Peierls to Bethe, 26 August 1938. Bethe to Peierls, 6 September 1938. See Lee (2007), p. 242 and p. 252.

> may be right. Speaking of Møller: How should the retardation cause a reduced divergence in the barytron theory?[31]

Peierls too had second thoughts with regard to Møller's theory, such as he pointed out in another letter to Bethe: "As to Möller: In the meantime, I have become convinced that it is very unlikely that retardation of the spin-spin interaction between neutrons and protons might help the mesotron theory in any way. Of course, for small distances in particular Möller is right that retardation is essential."[32]

In their first note to *Nature*, Møller and Rosenfeld found it necessary "to introduce besides the four-vector wave-function a further pseudoscalar wave-function for the meson field. ... The consideration of such a pseudoscalar meson field would also seem to be useful from the point of view of the theory of β–decay."[33] In the Møller-Rosenfeld theory the vector meson was thought to be responsible for beta decay and have a lifetime of approximately 10^{-8} s. Moreover, they assumed the two mesons to have the same mass. As to this assumption, Rosenfeld later stated: "Møller and I adopted the same mass value for our two mesons only because there was no reason to make a more complicated assumption."[34] The two physicists had no confidence in Bethe's preferred theory based on neutral mesons alone. As they argued, this theory was methodologically inferior because it amounted "to giving up the remarkable connexions suggested by the symmetrical theory between the problem of nuclear forces and those of cosmic-ray phenomena, beta decay and especially the magnetic moments of proton and neutron."[35]

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31. Bethe to Peierls, 12 December 1938, in Lee (2007), p. 262. 'Barytron' was one of several names used for mesons. It was coined by Bethe in April 1938.
32. Peierls to Bethe, 25 December 1938, in Lee (2007), p. 271.
33. Møller and Rosenfeld (1939a), dated 6 January and published 11 February. A pseudoscalar quantity changes sign when the spatial coordinates are inverted, or when the sign of time is reversed. Whereas the vector field corresponds to a spin-one particle, the pseudoscalar field corresponds to a spin-zero particle with negative parity.
34. Letter from Rosenfeld to V. Mukherji quoted in Mukherji (1972), p. 148.
35. Møller and Rosenfeld (1939b), p. 476.

In yet another note, this time co-authored by Rozental, Møller and Rosenfeld considered a problem which the German-American physicist Lothar Nordheim had called attention to with regard to Yukawa's theory of beta decay. The problem was that it led to a much shorter lifetime for light radioactive isotopes than observed. Møller and his two co-authors suggested that "the discrepancy in question can be removed as soon as a mixture of independent meson fields is introduced." Admitting that a mixture of meson fields with different lifetimes for spin zero and spin one would result in a theory of beta decay involving "a certain amount of arbitrariness", they expected that "it will enable us not only to avoid the discrepancy pointed out by Nordheim, but also to account for such considerable variations of the form of the beta-spectrum and the value of the beta-decay constant from element to element, as are already indicated by the present experimental data."[36] Shoichi Sakata in Japan found the Møller-Rosenfeld-Rozental theory attractive because it explained the rapid beta decay, but he objected that "Yukawa's beautiful relation between the meson decay and the beta-decay is lost in their theory."[37]

In their notes through 1939 the Copenhagen group referred to detailed calculations in a forthcoming memoir from the Royal Danish Academy which appeared in 1940. In this 72-page *tour de force* memoir entitled 'On the Field Theory of Nuclear Forces', Møller and Rosenfeld discussed in mathematical details their mixed field theory including both charged and neutral mesons, and they applied it to calculate the stationary states of the deuteron, the simplest proton-neutron system.[38] It is worth noting that in the late 1930s there was some experimental evidence for neutral mesons in the cosmic rays, but these hypothetical (and non-existing) particles were generally thought to be neutral mesotrons of the Anderson-Neddermeyer kind, that is, light muons symbolised as μ^0. Møller and

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36. Møller, Rosenfeld, and Rozental (1939). Monaldi (2005), pp. 434-435.

37. Sakata (1941), p. 285.

38. Møller and Rosenfeld (1940). Their part-time collaborator, the Swedish physicist Lamek Hulthén (1909-1995), investigated the nucleon-nucleon interaction within the framework of the Møller-Rosenfeld theory.

Rosenfeld were aware of the possible existence of what Arley and Heitler called 'neutrettos', but since they were concerned only with the role of Yukawa mesons in nuclear forces, they chose to ignore them.[39] For a short while the neutretto hypothesis attracted interest, but then it disappeared from the physics journals.

When Pauli got acquainted with the works from Copenhagen, he was more than sceptical. "I am not very convinced of the entire Yukawa theory of nuclear forces", he wrote in a letter to Klein in Stockholm. "With regard to Møller and based on his last publication, I believe he is completely crazy [*übergeschnappt*] and that Bohr should look much better after him." A few months later he addressed Rosenfeld, telling him that "I do not at all agree with the memoir by you and Møller concerning the meson theory." Pauli objected that the Møller-Rosenfeld 'patent mixture' did not really solve the divergence problems and also that spin-1 mesons disagreed with measurements of the cosmic rays. "Many people now believe that mesons have spin ½, but I prefer spin-0, for otherwise it causes troubles for the spontaneous decay of mesons."[40] Nor did Bethe accept the new theory from Copenhagen. According to Bethe, one of the key players in meson theory, nuclear particles interacted only through the exchange of neutral mesons, a view quite different from the one of Møller and Rosenfeld. In an influential paper in *Physical Review* he criticised the mixed field theory, which "is not considered satisfactory because of its intrinsic complication." Much of Bethe's critique was of a methodological nature:

> I believe that the solution of the problem of nuclear forces ought to be fundamentally simple, and this cannot be said of the Møller-Rosenfeld proposition. It seems that one type of mesons, either represented by a vector or by a field, is entirely sufficient to account qualitatively for all properties of nuclei … I believe that the solution of the problem of

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39. Møller and Rosenfeld (1940), p. 3. Møller to Rosenfeld, 3 August 1938 (RP). Arley and Heitler (1938). Hamilton, Heitler, and Peng (1943) referred to the neutral mesons appearing in the Møller-Rosenfeld theory as neutrettos.

40. Pauli to Klein, 28 January 1941, and Pauli to Rosenfeld, 17 April 1941, in Pauli (1993), p. 72 and p. 95.

> nuclear forces ought to be fundamentally simple, and this cannot be said of the Møller-Rosenfeld proposition.[41]

Rosenfeld came to agree with at least some of Bethe's objections. In a paper of 1945, he wrote that "The mixed theory can … admittedly be blamed for a wealth of adjustable parameters affording it an unfair amount of self-protection."[42] All the same, he preferred his and Møller's theory over Bethe's.

Pauli's initial dislike of the Møller-Rosenfeld theory did not remain. In the autumn of 1943 he wrote to Hulthén from Princeton: "Please say my regards both to Rosenfeld and Møller if you write them and tell Rosenfeld that his earlier complain that one does not take the Rosenfeld-Møller theory sufficiently into consideration in U.S.A. is not true anymore (since two years ago at least)." And to Bohr, congratulating him with having safely arrived in England: "My papers are dealing mostly with the theory of the meson-field and the last one which is under press is very closely connected with the work of Rosenfeld and Møller."[43] The mixed field theory of Møller and Rosenfeld was very popular in Japan during the war years. It was particularly important to Sakata, who used a modification of it to develop his own two-meson theory.[44]

A main reason why the theory of Møller and Rosenfeld gained some momentum in the United States, such as noticed by Pauli, was that it was taken up and modified by Julian Schwinger. At the December 1941 meeting of the American Physical Society, he presented a brief paper in which he assumed, contrary to Møller and Rosenfeld, the vector field meson to be heavier than the pseudoscalar meson. Schwinger argued that, if "in addition [to a pseudoscalar meson] a vector mesotron field is postulated which possesses the

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41. Bethe (1940), p. 412, who had received from Møller and Rosenfeld a manuscript version of their forthcoming essay in the proceedings of the Royal Danish Academy. See also Mukherji (1974), p. 52.

42. Rosenfeld (1945), p. 14.

43. Pauli to Hulthén, 18 October 1943, and Pauli to Bohr, 3 November 1943, in Pauli (1993), p. 203 and p. 204.

44. For the influence of the Møller-Rosenfeld theory on Sakata and his group, see Takabayasi (1983).

same nuclear coupling constant as a pseudoscalar field, but whose particles differ in mass from the pseudoscalar mesotrons observed in cosmic rays, the inadmissible singularities are removed."[45] He found that the vector meson might decay so rapidly into a pseudoscalar meson and a gamma photon that it would be unobserved in the cosmic rays. Although Schwinger did not develop his idea into a proper scientific paper, it was well known and influential. Pauli, for one, considered it an improvement over the original Møller-Rosenfeld theory.

Møller was not quite satisfied with the formal structure of his and Rosenfeld's theory. As he expressed it in another memoir in the proceedings of the Royal Danish Academy:

> The occurrence of two independent types of fields and four universal constants in the theory is an unsatisfactory feature which arouses the suspicion that the formalism developed in M. R. [Møller-Rosenfeld] is only part of a more comprehensive formalism in which the vector and pseudoscalar meson fields are more intimately connected and, consequently, the number of independent constants is reduced.[46]

To obtain a more unified and satisfactory formulation Møller investigated the equations as meson field equations in a de Sitter space and not in the ordinary flat four-dimensional Minkowski space. In his 1917 cosmological model based on Einstein's new general theory of relativity, the Dutch astronomer Willem de Sitter had introduced the kind of space named after him. This was originally conceived as a static and closed empty space with a radius given by

$$R = c\sqrt{3/\Lambda}$$

where Λ is Einstein's cosmological constant. However, in 1928 the American relativist Howard P. Robertson at Princeton University

45. Schwinger (1942). Mehra and Milton (2000), pp. 94-95. As mentioned in section 2.5, about ten years earlier, Møller's electron-electron scattering theory had served as inspiration for Schwinger's debut in physics.

46. Møller (1941a), p. 4. In Møller (1940) he presented a summary version of his forthcoming work.

reformulated the de Sitter space by introducing a non-static line element in Einstein's cosmological field equations.[47] In this way he was able to predict a galactic redshift in approximate accordance with the linear law established observationally by Edwin Hubble a year later. While Robertson's theory was mathematically equivalent to de Sitter's, its physical interpretation was quite different. Møller knew of Robertson's old and half-forgotten paper, which he thought might be relevant to his own work on a generalised meson theory. Taking over the line element from Lemaître and Robertson, he wrote it as

$$ds^2 = e^{2ct/R}(dx^2 + dy^2 + dz^2) - c^2dt^2$$

The idea of formulating fundamental physics in de Sitter space was not new, as Dirac in a mathematical paper of 1935 had investigated what his wave equation of the electron looked like in a space of this kind.[48] Møller was inspired by Dirac's work and knew from it and also from Robertson's paper that a de Sitter space can be interpreted as the surface of a four-dimensional sphere embedded in a five-dimensional space. The fifth dimension enters as the spherical surface having a very small but finite thickness instead of being infinitely thin. The radius of the sphere can be expressed by five coordinates satisfying $\Sigma x_\mu x_\mu = R^2$ with μ = 1-5 and one of the coordinates being imaginary.

Another inspiration came from Klein, such as documented by Møller's letter of 9 June 1938 to the Swedish theorist (Section 3.2). As Møller knew very well, in his early works Klein had formulated quantum mechanics in five dimensions and had since then continued to develop his theory in various directions. Møller mentioned in a footnote that the new formulation of meson theory, if interpreted somewhat differently, might promise a connection to Klein's five-dimensional unified theory comprising both electromagnetic,

47. Robertson (1928). Unknown to Robertson, the possibility of a non-static de Sitter world was discussed three years earlier by Georges Lemaître in Belgium. Møller (1941a) referred to Lemaître's little-known paper published in *Journal of Mathematical Physics*.

48. Dirac (1935). Kragh (1990), p. 168.

gravitational, and quantum forces. However, he did not go further along this speculative road.

In his correspondence with Rosenfeld, Møller told about "some speculations concerning the meson theory" from which followed "a complete change in our conception of elementary particles."[49] He had presented these speculations at an institute colloquium on 8 April 1940, noting in his letter that this was the day before the 'shock' of the German occupation of Denmark. According to Møller's preliminary theory, one should expect a small component of 'heavy electrons' in addition to the ordinary electrons in the beta spectrum and therefore also a change in the Fermi distribution of beta decay. He estimated that the decay of radium E (Bi-210) into Po-210 involved 10 per cent of these heavy electrons with mass greater than $2m_e$. What Møller was referring to as heavy electrons were hypothetical particles entirely different from the particles in the cosmic rays for which Anderson and Neddermeyer had originally used the same term. Another remarkable and scarcely believable consequence of Møller's admittedly speculative theory was that the neutrino should have a mass comparable to that of the electron. Incidentally, this is what Pauli suggested when he informally announced the hypothesis of nuclear neutrinos in December 1930.

When Møller presented his ideas at the colloquium in Copenhagen, Lise Meitner was in the audience and she objected that the neutrino could not possibly be that heavy. Perhaps as a result of Meitner's objections, when Møller's memoir appeared in print in late 1941, there was no trace of heavy electrons and neutrinos. "I have now written on the 5-dimensional theory and hope very soon to send you a copy of the manuscript", he wrote to Rosenfeld in October 1940. "The interesting consequences regarding the β-theory etc., which followed from the original formulation, have now disappeared and all the results of the theory are as in our joint work with the one difference that now ... the statements of the theory become more precise." A few months later: "The more I have occupied myself with the de Sitter space (in Robertson's formula-

49. Møller to Rosenfeld, 14 April 1940 (RP) with an attached manuscript of his Copenhagen colloquium talk.

tion), the more attractive it appears to me. It really gives a natural generalisation of Minkowski's space, which is valid in the special theory of relativity, and at the same time it explains in a free and easy manner Hubble's law."[50]

Møller's work of 1941 was predominantly mathematical and with no definite physical results except that "the theory implies the existence of particles with different values of the rest mass which perhaps opens the possibility for a unified theory of all known elementary particles with the same spin."[51] Indeed, Møller predicted a whole spectrum of hypothetical mesons heavier than the Yukawa meson. For their masses he found

$$m = m_\pi \sqrt{1 + n^2 C}$$

where C is a constant, n is an integer, and m_π is the rest mass of Yukawa's quantum. Many years later and with the benefit of hindsight, Møller said: "I made a jump into a five dimensional formulation which seemed quite natural at that time, which was actually a failure, didn't lead to anything. Only it became natural to assume that we didn't have one type of mesons, but quite a number of them."[52]

Although a failure, Møller was followed in his excursion into the five-dimensional world by a few other physicists. One of them was young Abraham Pais, who with Rosenfeld as his supervisor completed his doctoral dissertation in the summer of 1941 on a related topic, namely the Møller-Rosenfeld meson theory in terms of five-dimensional projective relativity. In this formulation of relativity theory proposed by the American mathematician Oswald Veblen and others, space-time is based on so-called projective geometry. The title of Pais' thesis was 'Projective Theory of Meson Fields and Electromagnetic Properties of Atomic Nuclei'. However, Pais objected to Møller's de Sitter space interpretation that "it seems not to give rise to any significant physical consequences" and consequently he found it more rewarding to examine the theory within the frame-

50. Møller to Rosenfeld, 9 October 1940 and 22 January 1941 (RP).

51. Møller (1941a), p. 39.

52. Weiner (1971c).

work of projective relativity.[53] Having read Pais' dissertation, Møller discussed with Rosenfeld the merits of his own de Sitter interpretation versus Pais' projective interpretation. If a pseudoscalar theory involving only nucleon interactions was impossible also according to the Pais' theory, he thought it might "convince Pauli that the 'mixture theory' is preferable to a pure pseudoscalar theory."[54] In Copenhagen, Rozental made use of Møller's five-dimensional theory in an investigation of the lifetimes of different kinds of mesons.[55]

Another Copenhagener contributing to the five-dimensional meson theory was 24-year-old Ib Nørlund, a nephew of Margrethe Bohr who for a time worked as an assistant to Møller. In a treatise published by the Royal Danish Academy in 1942, Nørlund examined the symmetrical Møller-Rosenfeld theory in five dimensions by making use of the 'undor' representation introduced by the Dutch theorist Frederik Belinfante, a student of Kramers.[56] Belinfante introduced the undor concept, a kind of spinor, in his 1939 dissertation *Theory of Heavy Quanta*. As he explained, the Dirac wave function was an undor of the first rank. After the war Belinfante emigrated to Canada and subsequently to the United States, where he settled as a professor at Purdue University. After many mathematical manipulations, Nørlund concluded that among current meson theories Møller's recent five-dimensional formulation was the only one which could be expressed by compact undor equations, which he considered a strong argument for the Møller-Rosenfeld theory.

At the time when his treatise appeared, Nørlund was no longer at the Copenhagen institute, the reason being that he, as a prominent member of the Communist Party, had been arrested by the Danish police in January 1942 and interned in a prisoner camp. While interned he continued his studies of generalised meson theory, but

53. Pais (1942), p. 268. Pais (1997), pp. 38-40. Jacobsen (2012), pp. 167-168.

54. Møller to Rosenfeld, 25 August 1941 (CMP). He also discussed the matter with Klein, arguing that the 'vector mesons' would have a much shorter lifetime than the 'pseudoscalar mesons' and that the latter mesons therefore made up the major component of the cosmic rays. Møller to Klein, 1 September 1941 (CMP).

55. Rozental (1941).

56. Nørlund (1942), who acknowledged "Dr. C. Møller for his constant guidance and for many valuable discussions." Belinfante (1939).

after half a year he escaped and went into hiding in Copenhagen.[57] In January 1945 Nørlund was arrested by the Gestapo and after torture transferred to another prisoner camp, where he stayed until the liberation in May. Nørlund devoted his later life to politics and the communist cause, not to physics. From 1945 to 1947 and again from 1973 to 1979 he represented the Communist Party as a member of the Danish Parliament.[58]

A lengthy memoir of 1943 was the last major fruit of the Møller-Rosenfeld collaboration.[59] In what the two authors considered a sequel to their work of 1940, they investigated in great mathematical detail the electromagnetic properties of nuclear particles on the basis of the Møller-Rosenfeld mixed field meson theory. As they noted, the publication in the Royal Danish Academy "[had been] much delayed, partly due to fortuitous circumstances" – possibly a euphemistic reference to the worsened German occupation. At the time that the memoir appeared, Møller had become interested in other aspects of fundamental quantum physics, *S*-matrix theory in particular, and was also slowly warming up to work on problems in general relativity. He published the same year his first work on general relativity theory, an important study of the so-called clock paradox which will be considered in Section 6.2. With a few exceptions, Møller did not return to the meson theory of nuclear forces to which he had devoted so much of his research over a five-year period. He did not give up meson theory instantly, though, and until the early 1950s he continued to write on it and give talks on the subject.

Although the highly abstract and formalistic Møller-Rosenfeld theory proved to be wrong, it was more than just a short-lived failure. As mentioned, during the war years it was well known and stimulated further works in meson theory such as Schwinger's and

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57. Nørlund to Møller, 2 March 1942 (CMP), asking Møller for literature and discussing the theories of Pais, Rosenfeld, Klein, and others. Nørlund to Bohr, 10 February 1942, and Bohr to Nørlund, 24 February 1942 (NBA, Bohr Private Correspondence).
58. Nørlund (1991). See Jacobsen (2012), pp. 166-168, 269 for his views on physics and politics.
59. Møller and Rosenfeld (1943).

Sakata's. The physics literature in the immediate post-war period provides evidence that aspects of the theory were still regarded as appealing in the community of particle physicists. After all, it seemed to offer a quantitatively consistent explanation of the decay-time discrepancy and of the unobserved mesons with a very short lifetime.

According to Bhabha, in a review paper written in 1944 but only published two years later, "the best account of nuclear forces is given by the theory of Møller and Rosenfeld ... [in which] theory positively and negatively charged mesons of spin 0 and 1 play a part, as well as neutral mesons."[60] At about the same time, still before the discovery of the π meson, the Manchester physicist John G. Wilson concluded in a review article on cosmic ray mesons that "the general picture of meson theories to-day is unsatisfactory." This was indeed the consensus view, but as the least unsatisfactory theory Wilson singled out and briefly described "the most effective detailed theoretical development, that of Møller and Rosenfeld."[61] Finally, here is how the Hungarian physicist Lajos Jánossy judged the theory in a monograph on cosmic rays from 1950: "Møller and Rosenfeld (1940) were the first to consider the possibility that β-decay might after all not be connected with the comparatively long-lived μ-meson. ... The theory of Møller and Rosenfeld (1940) represented for some time the most satisfactory formulation of the meson theory."[62] However, as Jánossy pointed out, by 1950 the situation had changed considerably and the theory could no longer be considered adequate. It belonged to the past.

Whereas Møller's extensive work on meson theory is today largely forgotten, one indirect result of it has stood the test of time. Rather than speaking of neutrons and protons separately, physicists have for long adopted the common name 'nucleon', a neologism

60. Bhabha (1944), p. 257. For other evaluations of the theory, see Mukherji (1974).
61. Wilson (1947), p. 60. The same year Bethe (1947, p. 96) summarised the state of affairs: "Much effort has been spent to treat the strong coupling problem in meson theory, but so far no results have been obtained which throw light on the problem of nuclear forces."
62. Jánossy (1950), p. 119 and p. 122.

introduced by Møller in 1941. In his 1939 dissertation, Belinfante had suggested the name 'nuclon', which he used throughout his work and in a series of papers published 1939-1940. According to a paper co-authored by Pauli, "the particle that is a proton in its charged state and a neutron in its neutral state, we have called a *nuclon*."[63] Møller found it an appropriate term and made it known to a larger public in his 1940 paper in *Physical Review*. He mistakenly assumed that Belinfante's 'nuclon' was correct from a philological point of view, but Rosenfeld disagreed. "I will ask you as quickly as possible to send me your reasons for the *e*", Møller wrote him. "Is 'nucle' really the root, meaning that 'us' and 'on' are endings? Is the word Greek or Latin?" Admitting his deplorable lack of classical education, Møller asked for Rosenfeld's assistance, assuring him that "I really do not want to be responsible for the introduction of monsters in the language of physics."[64] His learned friend in Liège answered promptly and pedantically:

> Physicists have used the innocent *Greek* ending '-on' in the sense of 'elementary particle' because the first isolated elementary particles happened to have the purely Greek names 'electron' and 'proton'. ... Now the word 'neutr-on' has been formed with this Greek ending and the *Latin* root 'neutr-'. Moreover, 'posit-on' and 'negat-on' which really should be understood as abbreviations of the more correct words 'positiv-on' and 'negativ-on'. Again, 'meson' and 'deuteron' are purely Greek words. For 'nuclear particle' one has the choice between the Greek word 'karyon' (which is not very attractive) and the word 'nucle-on' formed like 'neutron' with the Latin root 'nucle-'. You will see that the root is 'nucle-' and not 'nucl-' from e.g. the English adjective 'nuclear' (not 'nuclar'!).[65]

In a brief letter to *Physical Review*, Møller suggested to replace 'nuclon' with 'nucleon' as the common name for neutrons and protons:

63. Pauli and Belinfante (1940), p. 179. The paper was essentially written by Belinfante but approved by Pauli. See Enz (2002), p. 334.
64. Møller to Rosenfeld, 26 November 1940 (RP; in Danish).
65. Rosenfeld to Møller, 7 December 1940 (CMP; in Danish).

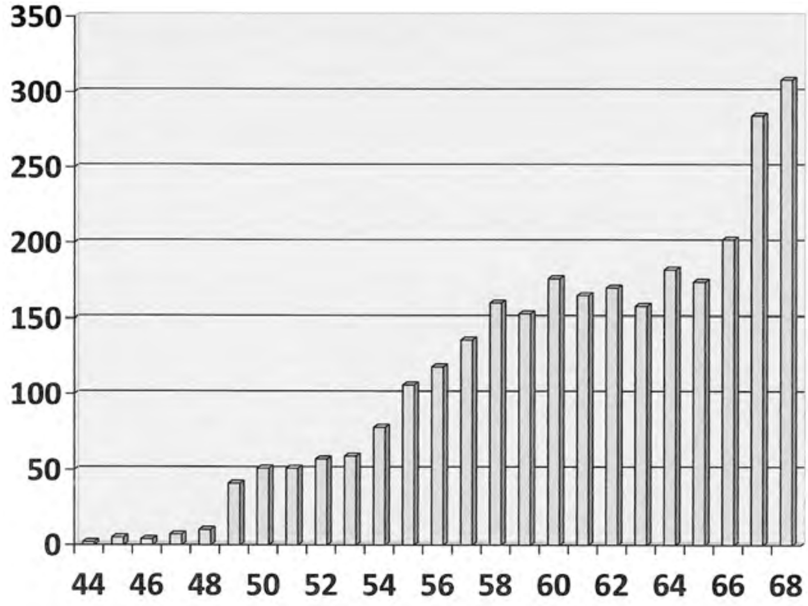


Fig. 23. Number of articles in nuclear and particle physics 1944-1968 with "nucleon" or "nucleons" in the title. Source: Web of Science.

"It has been pointed out to me that, since the root of the word nucleus is 'nucle', the notation 'nucleon' would from a philological point of view be more appropriate for this purpose, and I am therefore glad to have the opportunity to call the attention of interested physicists to this point."[66] Although he did not mention Rosenfeld by name, as far as the parenthood to 'nucleon' is concerned, perhaps it should be shared between Møller and Rosenfeld. Belinfante's name 'nuclon' was used only by a handful of physicists and then replaced by 'nucleon'. The neologism quickly caught on and within a few years it had entered the physicists' standard vocabulary, in most cases without reference to Møller's original suggestion. Thus, in a paper of 1942 on meson theory Pauli and his American co-au-

66. Møller (1941b), dated 10 December 1940, with the same remark in a footnote in Møller (1941a). Møller (1940). See also Møller to Samuel Glasstone, 1 September 1949 (CMP).

thor Sidney Dancoff used the term, casually noting in a footnote that "nucleon is equivalent to 'proton-neutron'."[67]

The first paper with nucleon in its title appeared in 1944 and twenty years later the number of such papers had increased to about two hundred (Figure 23). The word received official recognition at a conference held in Cracow in October 1947, where the Cosmic Ray Commission under the International Union of Pure and Applied Physics recommended its usage.[68] When interviewed by Charles Weiner thirty years later Møller did not recall his naming of 'nucleon' as important, but he did refer to the issue of particle terminology as discussed at the time:

> *Møller*: We had to find a name for these particles, and Fredrik [*sic*] Belinfante had introduced a word, what was it? *Weiner*: Mesotron. *Møller*: Oh, mesotron that is one thing, yes. It wasn't — particularly the Americans wanted to have the word mesotron. I think it was Enrico Fermi who said, "Mesotron, it sounds better in Italian than meson." But from the linguistic point of view, it was not too good. The 'tron' has no meaning there. Also, the nucleons — I think I was the one who used the word nucleon for the first time. ... But it was a reaction to a term that was used by Belinfante. What was it he called it? Well, this one could find out. Anyway, it contained also some letters that were superfluous [*sic*].[69]

Aware of the linguistic suggestions made by his two colleagues, Bohr entered the fray over particle nomenclature in his correspondence with Millikan. "As far as I understand", he wrote in a letter of April 1941, "the choice of the words neutron, proton and deuteron is in this [philological] respect most satisfactory, and it equally appears to me that the recently proposed word nucleon is very fitted indeed for the short comprehension of neutrons and protons." He then turned to the names advocated by Millikan:

67. Pauli and Dancoff (1942), p. 85.
68. 'The Cracow cosmic ray conference.' *Science* **107** (1948): 60-61.
69. Weiner (1971b).

> The criticism by filological [*sic*] authorities of the words positron and mesotron rests entirely on the last syllable, which is felt as unduly stressing the relationship to the word electron. It is thus suggested that the word electron may be reserved for the comprehension of the fundamental electric particles and to use the words negaton and positon in the description in which it is essential to emphasize the special charge of the particles concerned. Likewise the word meson is intended as a comprehension of the intermediate particles without discrimination whether they are positive, negative and neutral. ... The nomenclature problem might indeed be one of those which should be discussed at some international conference as soon as times again allow scientists from all countries to meet.[70]

It is well known that Bohr was fascinated by the concept of language in its relation to physics and science generally. He suggested that the essence of scientific knowledge, as well as any other kind of genuine knowledge, is that it allows unambiguous communication in terms of words. "We are suspended in language", as he phrased it in a conversation with his assistant Aage Petersen.[71] It is much less known that Bohr also took a deep interest in scientific nomenclature and terminology, an area which has not received any attention by Bohr scholars. As mentioned earlier (Section 5.1), Bohr's preference for 'negaton' and 'positon' was followed by Møller and other Copenhagen physicists. For example, in a 1945 paper on cosmic rays Arley repeated Bohr's argument that 'electron' was a generic term for the two charge states. "The terms positrons and negatrons often used are incorrect", he wrote, "as the *r* belongs to the Greek word for amber and not to the ending –on."[72] However, the term 'positon' never became popular in English-language papers and books and 'negaton' even less so.

To stay in the linguistic track, when participating in a large international conference in Cambridge in the summer of 1946, Møller and Pais suggested another successful neologism. Møller gave a talk

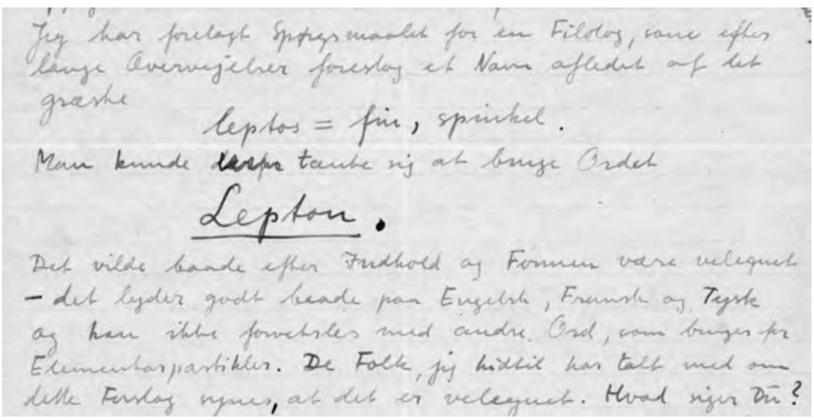
70. Bohr to Millikan, 18 April 1941 (BSC)
71. Petersen (1963), p. 11. Favrholdt (1993).
72. Arley (1945), p. 3.

on the mass spectra of fundamental particles, and in this context he referred to "some considerations made by Dr. A. Pais and myself" based on Møller's memoir of 1941. The considerations led to the suggestion of "three types of fundamental particles: the nucleons, the mesons, and the 'light' particles, neutrinos and electrons." Whatever their type, Møller found that the particles might exist in different states with rest masses

$$M = \sqrt{M_0^2 + n^2 a^2}\,, \qquad n = 0, \pm 1, \pm 2, \ldots$$

The rest mass of the lowest energy state M_0 differs from one type to another, but the constant a is the same for all three types. Then, in a footnote: "For the 'light' particles we propose the name 'leptons'."[73]

The name may first have been more widely circulated in Rosenfeld's impressive monograph *Nuclear Forces* of 1948, where he remarked in a footnote: "Following a suggestion of Prof. C. Møller, I



Jeg har forelagt Spørgsmaalet for en Filolog, som efter lange Overvejelser foreslog et Navn afledt af det græske

leptos = fin, spinkel.

Man kunde ~~derfor~~ tænke sig at bruge Ordet

Lepton.

Det vilde baade efter Indhold og Formen være velegnet – det lyder godt baade paa Engelsk, Fransk og Tysk og kan ikke forveksles med andre Ord, som bruges for Elementarpartikler. De Folk, jeg hidtil har talt med om dette Forslag synes, at det er velegnet. Hvad siger Du?

Fig. 24. Part of the letter from Møller to Rosenfeld, 12 October 1946, in which he proposes the name *lepton*. Source: Rosenfeld Papers, Niels Bohr Archive.

73. Møller (1947a), p. 184. See also Pais (1989), p. 349. For more about the Cambridge conference, see Section 5.4. The term 'lepton' can be found in the science literature as early as 1921, but in a sense very different from the one suggested by Møller and Pais. See *Oxford English Dictionary*.

adopt—as a pendant to 'nucleon'—the denomination 'lepton' (from λεπτός, small, thin, delicate) to denote a particle of small mass, irrespective of its charge; i.e., a lepton would be susceptible to two kinds of states, in which it appears as an electron and a neutrino, respectively." Moreover, and in agreement with Bohr's view: "The word 'electron' retains its original meaning of a particle of small mass with an elementary charge of either sign. When it is necessary to indicate the sign of the charge, the words 'positon' and 'negaton' may be used."[74] Rosenfeld also promoted the name 'lepton' in his talk on nuclear forces given to the 1948 Solvay conference, where he used it repeatedly as a common name for electrons and neutrinos. Notice that originally the μ mesons were not classified as leptons.

There is little doubt that the word 'lepton' was due to Møller. In the autumn of 1946 he addressed Rosenfeld with a philological question, as he had done previously with regard to 'nucleon'. Looking for a common name for electrons and neutrinos he had consulted an unnamed Danish philologist, who proposed a name derived from the Greek 'leptos'. Møller liked 'lepton' for other reasons because "it sounds good in both English, French and German, and it cannot be confused with other names for elementary particles." He wanted Rosenfeld's blessing for proposing it and asked him: "Would you consider to use it in your book instead of corpuscles, which strikes me as too general as it is almost the same as particles."[75] Møller did not suggest a corresponding name for strongly interacting particles. Such a name, namely *hadron*, was coined by the Russian physicist Lev Okun in a paper given to the 11th International Conference on High Energy Physics in July 1962.

Although Møller and Pais soon abandoned their theory of elementary particles, for a short while, when the experimental situation was still unsettled, they took it seriously. "In the last days", Møller reported to Cecil Powell in September 1947, "I have looked through the old calculations of Pais and myself on the decay constants of heavy mesons according to the formalism I told you about

74. Rosenfeld (1948), p. xvii.

75. Møller to Rosenfeld, 12 October 1946 (RP; in Danish).

in Dublin."[76] Based upon his and Pais' theory he suggested that the mass of Powell's primary particle (pion) was 320 m_e and that of the secondary particle (muon) 248 m_e. The mass ratio would thus be $m_\pi/m_\mu = 1.3$. "I remember that you thought this ratio should be larger than 1.4. My first question is whether a mass ratio of 1.3 is absolutely excluded?" Møller also considered the lifetime of the new π meson in relation to the value 2×10^{-6} s for "ordinary mesons." He stated that the Møller-Pais theory "allow us to estimate the lifetime of your π-mesons and we find a value between 1.3×10^{-8} sec and 1.4×10^{-9} sec, a value which is quite reasonable in contrast to the value of 10^{-18} sec following from Schwinger's theory."[77]

As he further explained in the letter, the Møller-Pais theory predicted a large number of heavy mesons and also heavy leptons of the electron type. One of those was an electron-like particle of mass 496 m_e, which might conceivably be detected in experiments. "Would you be able to see such a particle in the plate?" Møller asked Powell. No such monster electron turned up in Powell's photographic plates exposed to cosmic rays or in other high-energy meson experiments.[78] On the other hand, in 1947 three Soviet physicists reported that they had found meson-like particles in the cosmic rays with masses up to 2000 m_e.[79] However, their results were generally dismissed as unreliable by Powell and other experts. In a letter to Guido Beck from the same period, Møller wrote:

> Life here in Copenhagen is now getting rather normal again, at least as far as possible in this abnormal world. We have been very much

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76. Møller to Powell, 3 September 1947 (CMP), referring to a meeting in Dublin in July 1947 (Section 5.4). Powell to Møller, 19 November 1947 (CMP).

77. Modern data referring to charged pions are $m_\pi = 273.2\, m_e$, $m_\mu = 206.8\, m_e$ $m_\pi/m_\mu = 1.32$, $t_\pi = 2.6 \times 10^{-8}$ s , and $t_\mu = 2.2 \times 10^{-6}$ s . The lifetime of π^0 is 8.5×10^{-17} s.

78. With the discovery in the mid-1970s of the superheavy tau lepton with mass almost twice that of a proton ($m_\tau = 3484 m_e$), a kind of monster electron did turn up. From a linguistic point of view, the tau is not a lepton, but otherwise it is.

79. Alichanian, Alichanow, and Weissenberg (1947), who referred to the particles as 'varytrons'. Møller knew Alichanow (Section 3.4) but ignored his and his colleagues' discovery claim.

> discussing the implications of Powell's beautiful discoveries of mesons with different masses, but at the moment it is very difficult to obtain a simple synthesis of all experimental results. We have to wait for further experiments.[80]

A few months later, when the $\pi^{\pm} \rightarrow \mu^{\pm} \rightarrow e^{\pm}$ decay chain had been firmly established, the Møller-Pais theory was relegated to the scrapyard of wrong theories.

The Møller-Pais mass formula, or Møller's earlier but more restricted formula of 1941, was not the only failed attempt in the period to reduce all known elementary particles to manifestations of a single field or mass state. Thus, as early as 1939 Proca and his co-author Samuel Goudsmit speculated that the mass of the 'mesoton' (no *r*) could be expressed in terms of the masses of the proton, the neutron, and the electron, namely as

$$m_\pi = \sqrt{a\left(m_n^2 - m_p^2\right) + b m_e^2}$$

where $a = 5/3$ and $b = 8/3$. They commented: "This theory thus reduces the diversity of known elementary particles to a single fundamental mass ... with a value between that of the neutron and the proton."[81] The unification philosophy of Proca and Goudsmit was similar to the one which inspired Møller for a period of time.

5.3. The lure of the *S*-matrix

From about 1943 to 1946 Møller was intensely occupied with a new fundamental theory proposed by Heisenberg, the so-called *S*-matrix theory, where *S* stands for *Streuung* or scattering in English. When he discussed this theory with Heisenberg during the latter's visit to Bohr's institute in April 1944 (Section 4.2), he was well prepared. After having studied Heisenberg's difficult paper of 1943, Møller

80. Møller to Beck, 27 November 1947 (CMP).

81. Proca and Goudsmit (1939), who admitted that the neutrino did not fit into their theory.

wrote him a 14-page long letter with comments and suggestions which Heisenberg brought with him to Copenhagen.[82]

Møller immediately began developing the theory, the main results of his work being two extensive memoirs published in the proceedings of the Royal Danish Academy. However, by late 1946 and after much work he – as well as Heisenberg – came to the conclusion that the ambitious *S*-matrix program was after all a blind alley, a phenomenological description that could not be developed into a satisfactory quantum theory of elementary particles.[83] On the other hand, Møller did not completely abandon *S*-matrix theory and there may have been other reasons why he stopped exploring it. He mentioned one of them to Pauli: "During the last months, I have been exclusively occupied by writing a book on oldfashioned relativity for the Clarendon Press; thus I have left the *S*-Matrix for the moment." In the same letter Møller reported that "Professor Bohr is expected home from America about 23rd of November."[84]

As Heisenberg saw it, many of the notorious divergence problems of field quantum electrodynamics could be traced back to the theory's inability to incorporate into its framework the notion of a fundamental or smallest length. In 1938 he had introduced such a length as given by $h/m_\pi c = \text{ca.}\, 10^{-15}$ m (where m_π is the mass of the Yukawa meson), and he now made it a crucial ingredient of the new *S*-matrix theory. In a paper published during the height of the war in *Zeitschrift für Physik* on 25 March 1943, he wrote: "The known divergence problems in the theory of elementary particles indicate that the future theory will contain in its foundation a universal constant of the dimension of a length, which in the existing form of the theory cannot be built in any obvious manner without contradiction."[85] He also speculated, as physicists had done previ-

82. Rechenberg (1989), p. 560.

83. Grythe (1982), partly based on conversations with Møller, who with the benefit of hindsight told him that Heisenberg's program was "a great disappointment."

84. Møller to Pauli, 13 November 1946, in Pauli (1993), p. 398. Møller to Rosenfeld, 12 October 1946 (RP): "For the moment I work on the book on relativity theory, which I have promised Clarendon Press." For Møller's book, see Section 6.2.

85. Heisenberg (1943), p. 513, which was the first of three articles sharing the same title. The two next parts appeared in 1943 and 1944. The history of the *S*-matrix

ously, about a corresponding smallest time interval, a unit sometimes called a 'chronon'. This unit was typically conceived as the time it takes light to pass a classical electron of diameter $2e^2/mc^2$, that is, $\Delta t = 2e^2/mc^3$. In Heisenberg's theory, the chronon was of the order $\Delta t = h/m_\pi c^2 = 10^{-24}$ s.

Heisenberg's program of 1943 was consciously modelled on his original quantum mechanics of 1925 and, like this theory, based wholly in terms of observable quantities. As such quantities he chose those that do not depend on the existence of a minimal length. The central feature of the new theory was a certain scattering or *S*-matrix representing the transition of a two-particle collision process from an initial state Ψ to a final state Φ. The formal connection is

$$\Phi = S\Psi$$

where the square of the elements in the 2×2 scattering matrix S gives the transition probabilities. Although Heisenberg could not derive or specify the elements of the *S*-matrix, by appealing to conservation laws and symmetry principles he could restrict its arbitrariness. He proved that S must be unitary, which means that

$$S^\dagger S = SS^\dagger = 1$$

with $S^\dagger$ denoting the conjugate of S and 1 the unit matrix. From this property he derived the relation

$$S = \exp i\eta$$

where the quantity η is a Hermitian matrix ($\eta^\dagger = \eta$) containing only observable elements. While in ordinary quantum mechanics atomic systems are defined by a Hamiltonian H and a Schrödinger equation in terms of the wave function ψ, in Heisenberg's theory these quantities were taken over by the *S*-matrix.

The rationale behind Heisenberg's early *S*-matrix program was to ignore what causes the interaction between particles and also what takes place inside the unobservable region of interaction given by the fundamental length. He focused only on the observable states

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theory is described in Rechenberg (1989), Cushing (1986), and Cushing (1990). The problem of an absolute or smallest length is examined in Hagar (2014).

before and after the interaction where the particles are free and therefore with constant and well defined momenta. These momenta are the only variables that enter the theory, whereas, due to the uncertainty principle, the space coordinates are unknown. In this picture, time does not appear as a significant variable except that it distinguishes between the initial and final states. Heisenberg conceived the *S*-matrix as the fundamental object of study in particle physics. As he argued, all observable quantities, such as the scattering cross sections, the energies of bound states, and decay lifetimes, could in principle be derived from it. But – and that remained a nagging question – was it in principle only?

A few months after having discussed the *S*-matrix theory with Heisenberg in Copenhagen, Møller wrote to Rosenfeld:

> As mentioned, for some time ago we had a visit by Heisenberg and interesting discussions with him about the η-matrix etc. Since also Heisenberg suggested that I should publish my considerations about the η-matrix, I now contemplate to write it down together with some remarks of how to treat bound states by means of complex eigenvalues of the η-matrix following Kramers' idea. It appears that the theory of analytical functions of several complex variables will now play a greater role in physics than earlier, which I remember that Dirac predicted many years ago.[86]

In quantum mechanics, the state of a quantum system is usually represented by a function of real variables, the domains of which are the eigenvalues of certain observables. In a paper of 1937, which Møller obviously knew about, Dirac suggested to treat the dynamical variables as complex quantities. Although they could then no longer be associated with physical observables in the usual sense, Dirac argued that the loss of physical understanding was more than compensated by the theory's "beautiful mathematical features"

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86. Møller to Rosenfeld, 1 June 1944 (RP; in Danish). In October 1943 Heisenberg discussed his *S*-matrix program with Kramers, who suggested that the *S*-matrix should be considered as an analytic function of its variables. His critical interest caused Heisenberg to propose a joint publication, but Kramers declined to enter as co-author. Rechenberg (1989).

Nr. 1 21

$$(\boldsymbol{x}_1 \boldsymbol{x}_2 \,|\, \boldsymbol{k}_1' \boldsymbol{k}_2') = (2\pi)^{-3}\, e^{i(\boldsymbol{k}_1'\boldsymbol{x}_1 + \boldsymbol{k}_2'\boldsymbol{x}_2)}\, e^{i\gamma(\boldsymbol{k}_1'\boldsymbol{k}_2')}, \tag{80}$$

where $\gamma' = \gamma(\boldsymbol{k}_1'\boldsymbol{k}_2')$ is a real function of $\boldsymbol{k}_1'$ and $\boldsymbol{k}_2'$ depending on the phases in the chosen $\boldsymbol{k}$-representation.[8)] γ' may also be a function of the time. For the incident waves we get, by (8) and (9),

$$(\boldsymbol{x}_1 \boldsymbol{x}_2) = (2\pi)^{-3}\, e^{i(\boldsymbol{k}_1^0\boldsymbol{x}_1 + \boldsymbol{k}_2^0\boldsymbol{x}_2)}\, e^{i\gamma^0} = (2\pi)^{-3} e^{i\boldsymbol{K}^0\boldsymbol{x}_1} e^{i\boldsymbol{k}_2^0(\boldsymbol{x}_2-\boldsymbol{x}_1)}\, e^{i\gamma^0}, \tag{81}$$

while the scattered waves on account of (8), (15), and (16), are given by

$$\left.\begin{aligned} \mathfrak{c}_1 \boldsymbol{x}_2) &= (2\pi)^{-3} \int e^{i(\boldsymbol{k}_1'\boldsymbol{x}_1 + \boldsymbol{k}_2'\boldsymbol{x}_2)}\, e^{i\gamma'}\, \delta(\boldsymbol{K}' - \boldsymbol{K}^0) \\ &\qquad \delta_+ (W_1' + W_2' - W^0)\, (\boldsymbol{k}_1'\boldsymbol{k}_2' \,|\, U_{\boldsymbol{K}^0} \,|\, \boldsymbol{k}_1^0 \boldsymbol{k}_2^0)\, d\boldsymbol{k}_1'\, d\boldsymbol{k}_2' = \\ 2\pi)^{-3} &e^{i\boldsymbol{K}^0\boldsymbol{x}_1} \int e^{i\boldsymbol{k}_2'(\boldsymbol{x}_2 - \boldsymbol{x}_1)}\, e^{i\gamma(\boldsymbol{K}^0 - \boldsymbol{k}_2', \boldsymbol{k}_2')} \\ \sqrt{\kappa^2 + |\boldsymbol{K}^0 - \boldsymbol{k}_2'|^2} &+ \sqrt{\kappa^2 + k_2'^2} - W^0\big)\, (\boldsymbol{K}^0 - \boldsymbol{k}_2', \boldsymbol{k}_2' \,|\, U_{\boldsymbol{K}^0} \,|\, \boldsymbol{k}_1^0 \boldsymbol{k}_2^0)\, d\boldsymbol{k}_2'. \end{aligned}\right\} \tag{82}$$

Now we are only interested in the asymptotic values of T for $r = |\boldsymbol{x}_2 - \boldsymbol{x}_1| \to \infty$. Introducing polar coordinates in the $\boldsymbol{k}_2'$-space with the direction $\boldsymbol{e}_2' = \dfrac{\boldsymbol{x}_2 - \boldsymbol{x}_1}{|\boldsymbol{x}_2 - \boldsymbol{x}_1|}$ as polar axis, the integration with respect to the angles in the integral in (82) leads to the expression[9)]

$$\left.\begin{aligned} \mathfrak{c}_1 \boldsymbol{x}_2)_{r\to\infty} &= (2\pi)^{-3} e^{i\boldsymbol{K}^0\boldsymbol{x}_1} \frac{2\pi}{ir} \Big\{ \int_0^\infty e^{ik_2'r} (\boldsymbol{K}^0 - k_2'\boldsymbol{e}_2', k_2'\boldsymbol{e}_2' \,|\, U_{\boldsymbol{K}^0} \,|\, \boldsymbol{k}_1^0, \boldsymbol{k}_2^0) \\ &e^{i\gamma(\boldsymbol{K}^0 - k_2'\boldsymbol{e}_2', k_2'\boldsymbol{e}_2')}\, \delta_+ \big(\sqrt{\kappa^2 + |\boldsymbol{K}^0 - k_2'\boldsymbol{e}_2'|^2} + \sqrt{\kappa^2 + k_2'^2} - W^0\big)\, k_2'\, dk_2' - \\ {}^{k_2'r} (\boldsymbol{K}^0 &+ k_2'\boldsymbol{e}_2', -k_2'\boldsymbol{e}_2' \,|\, U_{\boldsymbol{K}^0} \,|\, \boldsymbol{k}_1^0, \boldsymbol{k}_2^0)\, e^{i\gamma(\boldsymbol{K}^0 + k_2'\boldsymbol{e}_2', -k_2'\boldsymbol{e}_2')} \\ &\qquad \delta_+ \big(\sqrt{\kappa^2 + |\boldsymbol{K}^0 + k_2'\boldsymbol{e}_2'|^2} + \sqrt{\kappa^2 + k_2'^2} - W^0\big)\, k_2'\, dk_2', \Big\} \end{aligned}\right\} \tag{83}$$

where only terms of the order $\frac{1}{r}$ have been retained. When the argument of the δ_+-function is denoted by $A(k_2')$, and when $f(k_2')$ is any function of k_2', we have[9)], neglecting terms of the order $\frac{1}{r^2}$,

Fig. 25. A page from Møller's first paper on *S*-matrix theory illustrating the mathematical character of his work.

which would eventually result in new knowledge about nature on the fundamental level.[87]

The result of Møller's contemplations was a long and difficult memoir on the 'General Properties of the Characteristic Matrix in the Theory of Elementary Particles' which he submitted to the Royal Danish Academy on 1 December 1944. However, the treatise was only printed after the end of the war, on 16 July 1945. In this work he introduced and studied what became known as 'Møller operators', quantities which describe the scattering process without comprising the time-dependent details of the event. In the later literature on generalised scattering theory, these operators play an important role. A second and equally lengthy memoir followed on 14 October 1946 and at about the same time he also published a short non-technical version in *Nature* addressed to a more general physics audience.

Eager to know more about "the fine work of Møller", Heisenberg addressed Bohr in a letter of October 1945 in which he inquired if Møller's *S*-matrix paper had appeared in print. "My correspondence with Møller unfortunately came to a standstill last fall because of the external catastrophes. I ask you, however, to greet Møller and to tell him that I hope soon to resume the correspondence."[88] Only in June 1946, now back in Göttingen as director of the new Max Planck Institute for Physics, did he write to Møller. He told him that the Göttingen group had debated "much about the problems of the η–matrix and the elementary particles, and we have also discussed in detail the problems of your previous letter."[89] In his letter of reply, Møller stated that "I have written two papers on the subject – as soon as possible I shall send you reprints (at the moment one is not allowed to send reprints)." He further reported that British physicists were much interested in Heisenberg's theory and that "at

87. Dirac (1937). Kragh (1990), pp. 282-283.

88. Heisenberg to Bohr (BSC). The letter is undated, but as it includes congratulations to Bohr with his sixtieth birthday it was undoubtedly written in early October. At the time of writing, Heisenberg and several of his German fellow physicists were still interned at Farm Hall, a mansion near Cambridge.

89. Heisenberg to Møller, 1 June 1946, in Rechenberg (1989), p. 563.

the conference in Cambridge at the end of July the problem of how to determine the *S*-matrix will be one of the points of discussion."[90]

After the Cambridge conference, Pauli reported in a letter: "Møller was also in Cambridge and gave me proofs of Part II of his paper on the *S*-matrix. It discusses essentially the method of analytical continuation to obtain the discrete states." Also the Norwegian physicist Harald Wergeland, in a letter to Heisenberg, referred to Møller's talk. He thought it was impressive: "Chr. Møller, whom I met in Cambridge, gave there a brilliant account of your theory which he advocated in an almost exceptionally clever way."[91]

In his first paper on the general properties of the *S*-matrix or 'characteristic matrix', Møller extended and deepened many of Heisenberg's results, in the sense that he provided them with a more rigorous mathematical foundation. Apart from proving the Lorentz invariance of the characteristic matrix, he introduced what he called 'constants of collision', by which he meant quantities with the same value before and after a collision. These quantities, he wrote, "will probably in the future theory play a similar important role as the constants of motion in ordinary quantum mechanics."[92] In the second of his papers he focused on two-body systems and showed, among other things, that the lifetime of radioactive decay could be determined by the *S*-matrix alone. For the decay constant λ he derived an expression in agreement with the results obtained in the late 1920s by Gamow, Condon, and Gurney, and, in the relativistic regime, by Møller himself (Section 1.3). His conclusion was optimistic:

> It thus seems that all experimental results may be described by means of Heisenberg's characteristic matrix without making use of the wave functions of ordinary quantum mechanics, and the way is open for a relativistic description of atomic phenomena which does not involve

90. Møller to Heisenberg, 7 July 1946, in Rechenberg (1989), p. 563. It took another year before Heisenberg received the reprints. For the Cambridge conference, see Section 5.4.

91. Pauli to Shih-Tsun Ma, 5 August 1946, and Wergeland to Heisenberg, 5 September 1946, in Pauli (1993), p. 374.

92. Møller (1945), p. 5.

> the difficulties inherent in all relativistic quantum field theories of the Hamiltonian form.[93]

As Møller observed, there is no one-to-one correspondence between the Hamiltonian *H* and the characteristic matrix *S*; for a given *S* there might exist either no Hamiltonian or many of them. Moreover, he discussed more carefully than Heisenberg and other authors what it meant that some physical phenomenon or variable is an observable. He emphasised that the question was necessarily theory-dependent (or 'theory-laden' to use the phrase of philosophers of science):

> In order to draw any conclusions about the values of atomic quantities from … direct observations, we need a theory, and if the theory allows unambiguous conclusions to be made about an atomic variable, this variable is said to be an 'observable' quantity. Strictly speaking, the question whether a definite atomic quantity (like the position of an electron) is observable or not can, therefore, only be decided after the theory has been fully developed.[94]

On the other hand, Møller admitted that some variables were so directly associated with observations that they could be safely regarded as observables in any theory. Among those variables he mentioned cross sections for atomic processes, radioactive decay constants, and the basic parameters of elementary particles such as their mass and electric charge. Having studied Møller's papers, Heisenberg found them to be most valuable: "It has made a great impression on me, how detailed and carefully you have discussed mathematically the various aspects of the *S*-matrix problem. Especially your considerations on the transformation properties of the *S*-matrix and the cross sections were, as far as details are concerned, new to me."[95]

Not all responses to the *S*-matrix research program and Møller's contributions to it were equally positive. Thus Pauli, notorious for

93. Møller (1946a), p. 5. Møller (1946b), p. 410.
94. Møller (1946b), p. 404.
95. Heisenberg to Møller, 23 June 1947, in Rechenberg (1989), p. 564.

his critical scepticism, wrote to Møller: "In Zurich I found the manuscript of Part III & IV of Heisenberg's paper on the '*S*-matrix'. ... I am still not convinced; not only the whole frame of concepts is empty (no theory is given which determines *S*), but the rather complicated formalism does not contain the classical mechanics as a limiting case. Please write me what you are now thinking about it!"[96] Peierls reacted in much the same way: "I am not at all impressed with Heisenberg's new scheme or Møller's work on the same subject. It seems to me quite an empty scheme which is completely indefinite until one formulates the laws to which his matrix *S* is subjected."[97]

Pauli was awarded the 1945 Nobel Prize for his discovery of the exclusion principle, but only the following year did he go to Stockholm to receive it and give the traditional Nobel lecture on 13 December. On his way back from the Swedish capital to Zurich he spent a couple of days in Copenhagen, and on Christmas day he wrote to Heisenberg:

> I believe that without a theory which determines $e^2/\hbar c$ [the fine structure constant] it is no longer possible to make more progress. With regard to your point of view about the 'fundamental length', I miss, however, the connection to the $e^2/\hbar c$ problem, and I also don't know whether perhaps different lengths are associated with different particles. ... The celebration days in Stockholm were strenuous but also delightful; it was nice to stay again in Copenhagen with Bohr, who has now returned to physics and will complete his old and long shelved publications. Møller no longer works with the *S*-matrix, since he does not see any possibility of getting on with it.[98]

Apart from Møller, several other mathematically inclined physicists took up Heisenberg's *S*-matrix theory and developed it in various ways. One of them was the young Swiss theoretical physicist Res Jost, who stayed at Bohr's institute from January 1946 to late

96. Pauli to Møller, 18 April 1946, in Pauli (1993), p. 351.
97. Peierls to Born, 14 June 1946, in Peierls (2009), p. 58.
98. Pauli to Heisenberg, 25 December 1946, in Pauli (1993), p. 403.

September doing postdoctoral work on scattering and *S*-matrix theory under the wings of Møller. At the institute on Blegdamsvej he collaborated with Møller and the 26-year-old Dutchman Dirk ter Haar. He also met Pais, who had arrived a little earlier and left Copenhagen at about the same time as Jost. Pauli wanted Jost to become his assistant in Zurich and corresponded with Møller on the matter. Jost later recalled: "It was impossible to reject this [Pauli's] offer, and hence I cut short my stay in Copenhagen after less than half a year, boarded an airplane for the first time and arrived in Zurich at the beginning of October."[99]

Another of the Swiss visitors was Ernst Stueckelberg – his full name was Ernst Carl Gerlach Stuckelberg de Breidenbach – who for more than a decade had worked in relative isolation on quantum electrodynamics and other areas of fundamental physics, where he was a most active if not always appreciated contributor. Møller had met Stueckelberg at the 1946 Cambridge conference, where the Swiss physicist gave a talk on the *S*-matrix. Stueckelberg inquired whether it might be possible to come to Copenhagen for a period of time and Møller subsequently arranged an invitation. As a result, Stueckelberg spent a month in the spring of 1947 at Bohr's institute. Here he discussed *S*-matrix theory with Møller and completed a paper which, however, was rejected by the editors of *Physical Review*.[100]

In Dublin, Walter Heitler had since the early 1940s collaborated with his assistant Huan-Wu Peng on a general 'damping theory' for the calculation of scattering amplitudes. When he got acquainted with Heisenberg's theory of the *S*-matrix, he realised the similarity between the two theories. In early 1946 he wrote to Møller, inviting him to visit the Dublin institute:

99. Quoted in Enz (2002), p. 408. See also Pais (2000), pp. 107-120. As mentioned in Section 3.1, Jost later returned to Copenhagen, where he collaborated with Walter Kohn on scattering theory.

100. Møller to Stueckelberg, 29 November 1946 (CMP). On Stueckelberg and his work, see Blum (2017), Schweber (1994), pp. 576-582, and Lacki, Ruegg, and Wanders (2009).

> I have heard from [Herbert] Fröhlich that you are on a visit in Bristol at present. I would like to ask whether you would consider to pay a visit also to our Institute here before you return to Copenhagen. We would all be very glad indeed to see you and to hear about our friends in Copenhagen and their recent work. In particular Peng and I are very much interested in your recent paper on Heisenberg's theory and we would like to hear more about it. We think there must be some close relation with our own recent attempts (theory of damping) in fact I think that the latter is a special case of Heisenberg's.[101]

Although the *S*-matrix program came to be seen as a glorious mistake, it was more than just a dead end. For one thing, the successful renormalized field theory of quantum electrodynamics developed principally by Tomonaga, Schwinger, and Feynman had formal elements in common with the *S*-matrix theory, such as Freeman Dyson, one of the chief architects of the new theory, pointed out with regard to Feynman's formulation. In a letter to Oppenheimer from the autumn of 1948, Dyson wrote: "I believe it to be probable that the Feynman theory will provide a complete fulfilment of Heisenberg's *S*-matrix program. The Feynman theory is essentially nothing more than a method of calculating the *S*-matrix for any physical system from the usual equations of electrodynamics."[102]

For another thing, when the *S*-matrix and related techniques were incorporated into new theories of strong interactions its fate changed considerably. Geoffrey Chew and other American theorists developed throughout the 1960s a new 'analytic *S*-matrix theory' the general idea of which was to avoid the conventional association of quantum fields with the strongly interacting particles known as hadrons. Chew's idea of what he also referred to as 'nuclear democracy' or the 'bootstrap hypothesis' was originally independent of Heisenberg's *S*-matrix and yet the two ideas were closely related in

101. Heitler to Møller, 11 February 1946 (CMP).
102. Letter of 17 October 1948, quoted in Cushing (1986), p. 122. Dyson (1949). See also Blum (2017) for the connection between *S*-matrix theory and the Feynman-Dyson quantum electrodynamics.

both a formal and conceptual sense.[103] For a decade or so, the analytic *S*-matrix or bootstrap theory attracted massive attention, but by the early 1970s it ran into troubles and was eventually abandoned. On the other hand, elements of it lived on in the early versions of string theory. It has even been suggested that "Without the *S*-matrix program, it is unlikely that quantized strings would ever have been discovered and studied."[104]

Møller was not involved in this later development, but until the mid-1950s he pursued some of the long shadows cast by the *S*-matrix theory. In a joint work with Belinfante, he published a detailed study in which the two authors critically compared Heisenberg's original *S*-matrix with other versions such as the one introduced by Dyson in 1949. As they noted, "In the numerous papers on this subject, the definition of the *S*-matrix itself has, however, not always been the same, and the connection between the different definitions has not always been quite clear."[105] After long and complex calculations, they concluded that the equality of Dyson's *S*-matrix and the one of Heisenberg, or the full equivalence between the two notions, was probably unjustified. Belinfante and Møller doubted if a general equality valid also for systems with bound states could be proved. The 1954 work with Belinfante is noteworthy because it was Møller's last research publication on quantum mechanics. He had worked on aspects of this general theory since his first paper in 1929, but after a quarter of a century he stopped. All his later research papers, from 1955 to 1979, were on problems related to the general theory of relativity, such as will be covered in chapters 6 and 7.

While the Belinfante-Møller paper attracted but little attention, another of Møller's collaborative memoirs in the proceedings of the Royal Danish Academy fared better. Together with the young

103. Cushing (1990). Kaiser (2005), pp. 306-331. For an overview of the rise and fall of the bootstrap program, see Kragh (2011), pp. 141-163.

104. Cushing (1986), p. 133. Rickles (2014), pp. 27-29.

105. Belinfante and Møller (1954), p. 3. Dyson (1949). Møller continued to be interested in the *S*-matrix. In 1958-1959 he gave a series of lectures on the subject to physicists at Nordita and the Bohr institute. Møller, *Lectures on Elementary S-Matrix Theory* (Copenhagen, 1959), mimeographed lecture notes, 126 pp.

physicist Povl Kristensen, he worked since late 1951 on a theory of nucleon-meson interaction based on the *S*-matrix method. Their collaboration resulted in two publications, a preliminary note in *Physical Review* and a much larger and detailed memoir published by the Royal Danish Academy.[106] The note to *Physical Review* was submitted on 14 January 1952 from the physics institute of the Universitá Degli Studi in Rome, where the two physicists stayed temporarily. The larger paper was submitted on 17 April and printed only on 20 November. The two authors argued that with a particular choice of the so-called form factor they obtained finite values for the particles' self-energy and vacuum polarisation. Referring to the new renormalisation method they expressed doubts whether it was applicable to the case of nucleons interacting with mesons. Moreover,

> It should be kept in mind that the method itself, in spite of its practical success, is not entirely satisfactory from a theoretical point of view, since the transformation leading to the renormalized equations is not a mathematically well defined unitary transformation, as is obvious from the fact that its purpose is to remove infinities. It would therefore be more attractive, at least in the case of nucleons in interaction with meson fields, to replace the usual field equations by slightly modified equations which, from the beginning, are free of divergences.[107]

Both during and after the preparation of the Møller-Kristensen memoir, the two authors communicated with Pauli, who was much interested in the subject. As he told Møller in April 1952: "I just read yours and Kristensen's form-factor paper and I still have the same positive impression on it which I received already when I did *not* hear your lecture about it in Copenhagen and instead had a talk with C. Bloch on this subject. ... It is an interesting enrichment of the found possibilities."[108]

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106. Kristensen and Møller (1952a) and (1952b).
107. Kristensen and Møller (1952b), p. 3. For Møller's persistent scepticism with regard to renormalisation methods, see also Section 2.5.
108. Pauli to Møller, 12 April 1952, in Pauli (1996), p. 605.

Pauli referred to a conference sponsored by the Council of Representatives of European States on meson theory and related topics held at the Bohr institute from 3 to 17 June 1952. During this conference, Møller gave a talk on his and Kristensen's 'convergent meson theory'. Also present at the conference were Bohr, Heisenberg, Jost, Mottelson, Pais, Rosenfeld, Kristensen, and the young French theorist Claude Bloch (not to be confused with Felix Bloch), who had previously spent an extended period at the institute working on non-local quantum field theories. The work of Bloch on meson theory complemented in many ways that of Møller and Kristensen, who were communicating with him.[109] Yet another participant in the 1952 conference was the American theorist Arthur Wightman who in 1951-1952 stayed as a visiting researcher at the institute and would return 1956-1957. Wightman, who worked on new mathematical formulations of quantum field theory, shared Møller's interest in foundational quantum mechanics. While in Copenhagen, he collaborated with two Swedes, the quantum theorist Gunnar Källén and the mathematician Lars Gårding.

In a later letter to Møller and Kristensen, Pauli started:

> Dear Sirs! I re[a]d with pleasure your letter of April 20 and my only objection is that you should have written it half a year earlier. Anyhow the result, that one eventually obtains correct answers from you, if one is waiting sufficiently long time, is consoling and encouraging. It also gives me the courage to disturb your plan to live the idle life of noblemen in Ordrup... .[110]

In some of his other letters Pauli jokingly addressed Møller as *Landgraf* (landgrave) and his wife Kirsten as *Landgräfin*. "Dear Landgraf!" he wrote, "This is again to thank you for your kind hospitability in Copenhagen, also in the name of my wife and to

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109. Bloch to Møller, 11 April 1952, in Pauli (1996), p. 621. Claude Bloch, who stayed in Copenhagen 1948-1951, published two of his works on non-local field theory in the proceedings of the Royal Danish Academy. After a stay at Caltech 1952-1953 he returned to France to work as a theorist for the French Atomic Energy Commission.
110. Pauli to Møller and Kristensen, 23 April 1953, in Pauli (1999), p. 129.

Mrs. Møller too."[111] Since 1935 the Møller family did indeed live in Ordrup, a wealthy suburb north of Copenhagen, but not exactly in the style of either a nobleman or a landgrave. At first they lived in an apartment on Ordrup Jagtvej 101 and from about 1945 they moved to a villa on nearby Fröhlichsvej 42a.

5.4. The world opens up

Less than two months after Bohr returned to Denmark, his friends and colleagues at the institute on Blegdamsvej celebrated his sixtieth birthday on 7 October 1945. Congratulating Pauli with the recently announced Nobel Prize, the Swedish physicist Lamek Hulthén told about the celebration, which was a major if largely local event:

> Bohr's sixtieth birthday was celebrated with grand festivities, but in the true Copenhagen spirit. In the morning there was a meeting at the institute with Rozental as conferencier. Speeches were made and gifts presented by Møller, Jacobsen and other members of the institute, Klein, Rosseland, Hylleraas and Gustavson. The Danish and Norwegian dedication publications were handed over, and, of course, a new issue of the 'Journal of Jocular Physics'. ... Some jocose films and pictures were also shown.[112]

The frivolous and humorous *Journal of Jocular Physics*, a stencilled kind of festschrift aimed at local consumption only, first appeared in 1935 on the occasion of Bohr's fiftieth birthday, and a third volume was produced twenty years later, when Bohr turned seventy.[113] With the exception of a witty contribution from Rosenfeld, the articles in the slim second volume were all written by Danish physicists in either Danish or English. One of the authors was Møller, who

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111. Pauli to Møller, 19 October 1955, in Pauli (2001), p. 371.

112. Hulthén to Pauli, 13 November 1945, in Pauli (1993), p. 327. The jocose pictures included slides with members of the staff, Møller included, as small children and dressed up like children. See Figure 27.

113. On this 'journal' and the use of humour in the Bohr circle, see Halpern (2012) and Beller (1999).

Fig. 26. Møller and Harald Bohr on their way to Niels Bohr's 60-year's birthday party. Credit: Niels Bohr Archive, Photo Collection, Copenhagen.

Fig. 27. A double photo of Møller in 1905 and in 1945, dressed up as a child. Together with other similar double photos of staff members, the photographs were presented at Bohr's sixtieth birthday. The lady in the picture of 1905 is presumably an aunt. Source: Photograph album kept at the Niels Bohr Archive.

contributed with a delightful story relating to the tobacco rationing in Denmark after the war. Møller was a devoted cigar smoker and in his piece he tells about his alter ego, a member of 'the Danish Committee for Purchase of Tobacco in Sweden'. The story is not so much about tobacco as about the relativity of time. During a visit to Stockholm, the member of the tobacco committee meets a young physics student whose girlfriend works as an attendant in an elevator moving and accelerating so fast that people in it get younger. Do they really get younger? Do the student and his girlfriend age so differently that they will never marry? The story is about the clock or twin paradox in general relativity, but presented in a Gamow-like fantasy version. For Møller and the more scientific version of the clock paradox, see Section 6.2.

The years after the end of the war was a busy period in Møller's life. Apart from his teaching and administrative duties at the Copenhagen institute, together with other members of the staff he was also responsible for the endeavours to rebuild the institute and re-establish its connections to physicists abroad. After the war, when the conditions for fundamental research had changed radically, the institute was in danger of losing its former status as the celebrated Mecca of quantum physics as it was sometimes affectively called. To many of the new generation of physicists it seemed that Bohr's magic belonged to a distant past. Bohr recognised the problem:

> We are all very busy with an extension of the Institute which will allow us to widen the field of our present experimental researches, although there is of course no question that we can extend these to the fields in which the U.S.A. with such extraordinary resources new and most promising advances are being made. We hope, however, to be able again to establish a smaller center for international cooperation in physics and we have already had a number of visitors from other countries. The reorganization of the Institute should be finished at the end of the coming year.[114]

114. Bohr to Nishina, 9 December 1948 (BSC), reprinted in Nishina (1984), pp. 63-65.

It was primarily the focus on nuclear structure which in the 1950s revitalised the institute and perhaps saved it from ending up as a museum of quantum thought. The work of Aage Bohr and his close collaborator Ben Mottelson, a young American who had earned his PhD under Schwinger and in 1951 come to Copenhagen, was of particular importance in this regard.[115] Møller too was active in the revitalisation process, but research in the detailed structure of atomic nuclei was not an area that appealed to him. He was outside the strong group of experimental and theoretical nuclear physicists which in the 1950s and 1960s made the Blegdamsvej institute an international centre of excellence in this area of research. On the other hand, Møller's recognition as an authority in meson theory and later in general relativity also attracted many foreign visitors to the institute and contributed to its scientific status.

One of Møller's first travels abroad after the occupation had ended was to Stockholm, where Klein had invited him to give a couple of lectures in September 1945. Møller chose to speak on the *S*-matrix theory and the clock paradox. In the following years from 1946 to 1954, Møller participated in a large number of international conferences, which for the first time brought him beyond Europe, as far as to the United States, India, and Japan. He gave addresses on theoretical quantum and particle physics, which areas were still his main fields of research. None of the conferences he attended in this period dealt with problems of general relativity and the same was the case with his scientific papers – but that would soon change.

Some of the conferences which Møller organised or co-organised were domestic, taking place at the institute in Copenhagen. The most important of these was perhaps the 'meson conference' of June 1952 mentioned above. Bohr had originally suggested an international conference of this kind to be held in Copenhagen in early 1947 and his proposal was supported by Pauli and Wheeler. However, the Americans were in favour of a conference taking place in the United States and targeted primarily at young American the-

115. Together with the American James Rainwater, Aage Bohr and Ben Mottelson were awarded the 1975 Nobel Prize for their development of the collective model of nuclear structure. Kragh and Nielsen (2001).

oretical physicists. Of course, the Americans got it their way and the Copenhagen conference never materialised in the more ambitious form originally conceived. Among the arguments against the Bohr-Pauli-Wheeler proposal was that "few Americans are likely to be asked to Bohr's conference", and another was that Bohr did not publish proceedings of his conferences.[116] As a kind of substitute, in September 1947 a large but as usual informal conference was held in Copenhagen, attended by Kramers, Pauli, Pais, Weisskopf, Peierls, Rosenfeld, Blackett, Wheeler, and several other physicists.

Under the title 'Recent Developments in Relativistic Quantum Theory' Møller delivered in February 1946 a series of lectures on *S*-matrix theory at the H. H. Wills Physics Laboratory, University of Bristol. Heisenberg's theory was still new or even unknown to many British physicists, who first became acquainted with it as mediated by Møller.[117] While in England, Møller used the occasion to visit also Oxford, London, Birmingham, Manchester, and Cambridge, where he met with Dirac and other old acquaintances. To Bohr he reported that "for the moment Cambridge is not quite as it was in the old days, as many of the collaborators are spread for all winds."[118] At the time Blackett was looking for a physicist to take up the position as professor of theoretical physics at Manchester University and, as he confided in a letter to Bohr, he had Møller in mind as one of the first candidates:

> The two names that occurred to me first as possibilities were Casimir and Möller. Möller's work is just in the very field which I want to see developed here in Manchester. I am writing to you personally and quite unofficially to know whether you think that Möller would possibly consider a permanent or temporary post in Manchester. ... We will, of course, I hope meet at the Cambridge International Conference in July,

116. Schweber (1994), pp. 161-162.

117. "Recent Developments in Relativistic Quantum Theory," 39 pp., lecture notes prepared by Ian N. Sneddon, a collaborator of Mott. The lectures in Bristol followed closely Møller (1945) and Møller (1946a).

118. Møller to Bohr, 13 February 1946 (BSC).

> but I do not want to wait as long as this. I met Möller while he was in London and was very much impressed by his personality.[119]

Bohr found it unlikely that Møller would accept a call for the position, to which Blackett replied: "I can quite understand that he may be fulfilling a key role in Denmark to-day and that you would not want him to leave. If you prefer me not to mention the possibility to Møller I will not do so."[120] In the end Manchester University settled for Rosenfeld, who took up his new position in February 1947.

In the summer of 1946 Møller returned to England, now to participate in the conference mentioned by Blackett, the International Cambridge Conference on Fundamental Particles and Low Temperatures which convened at the Cavendish Laboratory from 22 to 27 July.[121] As mentioned in Section 5.2, there he gave a talk jointly with Pais on the mass spectra of elementary particles, and in another talk he dealt with the *S*-matrix theory. The Cambridge conference was attended by a large number of prominent physicists most of whom Møller had previously met and interacted with. They included Born, Pauli, Dirac, Fermi, Heitler, Bhabha, Rosenfeld, and Wentzel. The opening address was given by Bohr, who at the end of his report briefly referred to Heisenberg's ideas of the *S*-matrix, "which have been developed especially by Møller and which will surely be a main topic at this meeting."[122]

Heisenberg did not attend the Cambridge meeting because his travels were still restricted by the British occupation authorities. However, Max Born informed him about the meeting, writing that "I have read a couple of your papers on the *S*-matrix and have learned further things from Møller's publications. I tried at the Cambridge Congress in July to connect these things with my own,

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119. Blackett to Bohr, 14 March 1946 (BSC). Bohr to Blackett, 1 April 1946 (BSC).
120. Blackett to Bohr, 11 April 1946 (BSC).
121. *Report of an International Conference on Fundamental Particles and Low Temperatures*. London: The Physical Society, 1947.
122. Bohr (1996), p. 222.

rather nebulous ideas."[123] These nebulous ideas Born presented in a talk on quantum mechanics and what he called the principle of reciprocity, which he later developed into an ambitious but unsuccessful unified theory of elementary particles.

Another of the talks given in Cambridge was by three British physicists who reviewed the search for negative protons, which at the time were debated but not necessarily identified with the antiprotons predicted by Dirac in the early 1930s.[124] Møller's former collaborator Niels Arley was among those who believed that negative protons made up a substantial part of the primary cosmic rays, and he was not alone. In an extensive memoir of 1945 Arley discussed the hypothesis, arguing that the negative protons were real and abundant in the cosmic rays.[125] However, the hypothetical particles remained hypothetical until 1955, when a team of Berkeley physicists headed by Emilio Segré detected the antiproton in high-energy experiments. Soon thereafter, the particle was also found naturally, in the cosmic rays.

It is worth pointing out that whereas Dirac somewhat casually introduced the antiproton in 1931, his proposal caused much less interest than the hypothesis of non-Dirac negative protons. Thus, in a series of papers from the 1930s Gamow argued that negative protons different from antiprotons might be constituents of the atomic nucleus. For example, rather than writing the Be-9 nucleus as $(4p^+, 5n)$, he suggested the structure $(5p^+, 3n, 1p^-)$. The idea was taken seriously in Copenhagen, where it was discussed by Bohr, Gamow, Williams, and others. Pauli informed Heisenberg about the discussions and Bohr's view:

> Bohr thought much about negative protons and believes to have evidence for their existence in the cosmic radiation. There are theoretical as well as experimental … reasons for assuming that the relativistic Dirac wave equation is not at all applicable to heavy particles, and Bohr believes therefore that the negative protons should *not at all be related to the*

123. Born to Heisenberg, 2 October 1946, quoted in Rechenberg (1989), p. 564.
124. Broda, Feather, and Wilkinson (1947).
125. Arley (1945).

Fig. 28. Møller (right) at the Dublin Institute for Advanced Study in 1947. To his left: J.L. Synge, P. de Brún, C.F. Powell, W. Heitler. Credit: Österreichische Zentralbibliothek für Physik, University of Vienna.

> *hole idea and hence not annihilate with the positive protons*! This might well be a possibility. Could negative protons be expelled from the nuclei?[126]

It took until the early 1950s before physicists realised that if negative protons exist, they must be antiprotons. The discovery in 1955 was expected, and yet it was rewarded with a Nobel Prize four years later to Segré and his collaborator Owen Chamberlain. Møller was undoubtedly aware of the discussions in Copenhagen in 1934 and also of Arley's work, but he seems to have been uninterested in whether negative protons existed or not. In some of his works on meson theory after the war he referred to antiprotons and also antineutrons, but only as short-lived intermediate particles.

In July 1947 Møller gave a couple of lectures at the Dublin Institute for Advanced Study, an institution created in 1940 by the Ireland's mathematically trained Prime Minister Eamon de Val-

126. Pauli to Heisenberg, 17 April 1934, in Pauli (1985), p. 316. See also Bohr's unpublished manuscript on 'The Electron and Proton' in Bohr (1986), p. 124. The strange story of the negative non-Dirac protons in the 1930s is recounted in Kragh (1989).

era. Since 1941 its School of Theoretical Physics was directed by Schrödinger, who stayed in Dublin until retiring in 1955 after which he returned to Vienna. Schrödinger's right hand in Dublin was Walter Heitler, who by 1947 had taken over the directorship. Both during and after the war the colloquia and lectures at the Dublin school attracted many eminent scientists such as Eddington, Dirac, Born, and Pauli. Bohr had agreed to come to Dublin and lecture at the institute in 1947, but in the last moment he had to cancel his participation because of ill health.

Heitler first asked Møller to come to the Dublin institute in February 1946, but at the time Møller had to decline the invitation. When he was invited again in March 1947, he was happy to accept.[127] Apart from Møller, the two other lecturers in the summer course of 1947 were the Irish mathematician John Synge and the eminent Bristol physicist Cecil Powell, who three years later would receive the Nobel Prize for his work on mesons and cosmic rays.[128] Synge, who at the time was at the Carnegie Institute of Technology in Pittsburgh, was a specialist in the mathematical methods of general relativity. "We were very happy to have Møller with us and we enjoyed his beautiful lecture course very much", Heitler reported to Bohr after the conference. Møller had told him about Bohr's "considerations concerning the capture of slow mesons", which apparently related to a meson being caught in the continuous part of the spectrum of an atom and not in its K orbit. "I have yet a difficulty of understanding your idea completely", Heitler wrote with an understatement, indicating that neither did Møller fully understand what Bohr meant.[129] During his stay in Dublin, Møller also met with Schrödinger, Born, Herbert Fröhlich, and the Hungarian specialist in cosmic rays Lajos Jánossy.

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127. Heitler to Møller, 11 February 1946 (CMP), and Møller to Heitler, 19 March 1947 (CMP).

128. *Nature* **160** (1947): 393. See also Hyland (2015) with a photograph on p. 84 of Møller and other participants at the Dublin conference.

129. Heitler to Bohr, 21 August 1947 (BSC). Bohr did not publish his idea of meson capture, but he referred briefly to it at the 1948 Solvay congress. Bohr (1987), p. 571.

Møller dealt in his Dublin lectures with the centre of gravity in arbitrary classical systems and systems governed by relativistic quantum mechanics, which subjects he also discussed, and in even greater detail, in a later article in *Annales de l'Institut Henri Poincaré*.[130] For the mass centres of any classical system he derived the result that its spatial extension r was given by

$$r \geq L/M_0 c$$

where L is the intrinsic angular momentum and M_0 the system's rest mass. It follows that real spinning bodies cannot be treated as point particles. The limit $r = L/M_0 c$ is sometimes known as the 'Møller limit'.

Without knowing of Møller's paper in the communications of the Dublin institute, Wheeler referred to a recent investigation by John Toll, one of his students, which indicated that, in the quantum regime, "the higher the angular momentum of the bound particle the smaller the region of space within which the particle can be confined." Wheeler wanted to know if Møller had published anything on the subject. "I recall the interesting two lectures you gave in Copenhagen two years ago this fall, about the problem of an angular momentum of a complex system. ... We are particularly concerned because of the recollection – which may be wrong – that the region of space in your case was *larger* the larger the angular momentum."[131] A month later Wheeler addressed Bohr on the same question:

> I hope to talk with you about the reconciliation between some results obtained from the Dirac theory of the electron and Møller's theorem about proportionality of space extension of a dynamical system and angular momentum of that system. In the case of the Dirac electron it

130. Møller (1949a). Møller (1950a). He summarised the content of the two papers in his textbook on relativity theory. See Møller (1952), pp. 166-173.
131. Wheeler to Møller, 3 August 1949 (CMP). Wheeler referred to the informal Copenhagen conference in September 1947.

> turns out that the higher the angular momentum the *smaller* the size of the region within which it can be localized.[132]

Powell lectured in Dublin on the recent discovery of two kinds of mesons (μ and π) and on whether the light meson coupled strongly with nuclei or not, a question which was still undecided. "If it should turn out that the light meson is also a nuclear force meson, then the form of meson theory proposed by Møller and Rosenfeld and modified by Schwinger … is most likely correct."[133] However, it soon turned out that the light μ meson contrary to the heavier π meson was unconnected to the strong nuclear force. Møller, who had first met Powell at the 1946 Cambridge conference, was much interested in his lecture. He recalled:

> I was in Dublin to give a few lectures on a different subject, on the notion of the center of gravity or center of mass in nuclear theory, and Powell was there simultaneously. We were together there for a whole week and he talked about his discovery. … I invited him to come here [Copenhagen], when I heard him there. I told Bohr about it and he immediately agreed that we should try to get him to come. He came now and then in these years over here.[134]

In an important paper of October 1947, Powell and his two collaborators, the Italian Giuseppe Occhialini and the Brazilian César Lattes, acknowledged discussions with Møller and other of the participants at the Dublin meeting. They noted that "Møller and Pais have also considered the possibility of genetical relationships between different types of particles of intermediate mass."[135]

Later in 1947, Møller went to Paris, not to participate in a scientific conference but to read a brief address by Bohr at a commemoration symposium on the occasion of the tenth anniversary of

132. Wheeler to Bohr, 3 September 1949, in Bohr (1986), p. 667.
133. 'Colloquium at the Dublin Institute for Advanced Studies'. *Nature* **160** (1947): 393.
134. Weiner (1971c).
135. Lattes, Occhialini, and Powell (1947). See Section 5.2 for the Møller-Pais theory.

Rutherford's death. The event was organised by the World Federation of Scientific Workers (WFSW) of which the prominent French physicist and devoted communist Frédéric Joliot-Curie, a Nobel Prize laureate of 1935, served as president. The WFSW founded in 1946 was a leftist, anti-fascist organisation widely and not without reasons suspected to be sympathetic to the communist cause. Bohr, who had been invited to the symposium by Joliot-Curie, was cautious not to have his name associated with the organisation. On the other hand, Rosenfeld was a board member of the WFSW and he tried to change Bohr's attitude, but in vain.

After Rosenfeld had addressed Møller on the matter, and Møller had talked to Bohr, the compromise result became that Bohr wrote a tribute for the Paris symposium but without participating. Excusing himself with pressure of work, he persuaded Møller to go to Paris and read the tribute to Rutherford on 7 November at a public meeting at the Sorbonne attended by more than thousand people. Among the many speakers was the British crystallographer and sociologist of science John D. Bernal, who was an outspoken Marxist and vice-president of the WFSW. Although Bohr's address read by Møller was apolitical, naturally it referred to the new situation under the shadow of the atomic bomb: "The advance of science does not only hold out the brightest prospects for the improvement of human welfare, but also may bring with it ominous menaces to world security, unless mankind can adjust itself to the exigencies of the new situation."[136]

On his trainride to Paris, Møller was joined by Hevesy and Meitner, both of whom came from Sweden, and by Cécile Morette who had worked under Møller in Copenhagen. By chance the group came to include also the brilliant French mathematician Laurent Schwartz, who shortly earlier had developed his very important theory of distributions as a mathematical generalisation of the concept of function. Schwartz had been in Denmark and Sweden to give lectures and was now on his way back to Paris. Dirac's well-known 'δ-function' introduced in 1927 was a kind of ill-defined predecessor

136. Bohr (2007), p. 277-278. Joliot-Curie to Møller, 21 August 1947 (CMP). See also Jacobsen (2012), pp. 211-213, and Oliphant (1947).

of the theory of distributions, but still at the time of his trainride twenty years later Schwartz conceived his distributions to belong to the realm of pure mathematics. He had not yet quantum mechanics in mind. In a letter to his wife, he recounted:

> Returning from Sweden, I travelled with some physicists (such as Copenhagen quantician Möller) and Louise [*sic*] Meitner (universally known German jewess, a rather tender and nice old lady). They were going to Paris for the Rutherford commemoration, and we held a little seminar in wave mechanics. They were violently interested in distributions and wanted to be able to resolve certain mathematical contradictions in w. mech. I absolutely must study that, which might be the most beautiful application of distributions.[137]

Møller again visited England in September 1948, when he participated in two conferences devoted to nuclear physics and cosmic rays, respectively. The conference on 'Problems in Nuclear Physics' in Birmingham 14-18 September organised by Peierls and Oliphant was a major event with participation of Bohr, Fermi, Oppenheimer, Teller, Dyson, Bethe, and other leading physicists. Møller proceeded immediately from Birmingham to the Bristol conference 20-24 September, where the focus was on cosmic rays and the new particles found there. Only four theoretical papers were presented in Bristol, namely by Heitler, Rosenfeld, Møller, and the Czech-born British physicist Reinhold Furth.[138] Bohr was invited, but unable to come.

Møller surveyed the theory of mesons as known at the time. With regard to the $\pi \to \mu + \mu^0$ decay he assumed from the measured mass ratio $m_\pi/m_\mu = 1.34$ that the accompanying neutral particle μ^0 was most likely a zero-mass neutrino and not a massive neutretto. From this he concluded that the μ meson must have spin ½. In agreement with his and Pais' idea of mass spectra from 1946, he found it "tempting to treat the μ-meson as a kind of lepton in a

137. Quoted in Barany, Paumier, and Lützen (2017), p. 381. Sime (1996), p. 352.
138. The Bristol conference resulted in the proceedings volume Frank and Rexworthy (1949). See also the American physicist Robert Brode's review of it in *Physics Today* **3** (3) (1950): 35.

higher mass state, the different mass states being distinguished by a new dynamical variable of the lepton, in addition to the space coordinates, the spin variables and the isotopic spin variables."[139] He thus anticipated what is known as the universality of weak interactions, an idea also considered by Klein, Bruno Pontecorvo and some other physicists in the late 1940s. With regard to the decay of the μ meson Møller wrote it as

$$\mu^{\pm} \rightarrow e^{\pm} + \nu + \nu$$

where the neutrinos might either be of the same kind ($\nu\nu$, $\bar{\nu}\bar{\nu}$) or different (ν, $\bar{\nu}$). Experimental support for the three-particle disintegration, which was originally hypothesised by Lothar Nordheim in 1941, followed a few years later. For the neutral π meson with a very short lifetime Møller considered the scheme $\pi^0 \rightarrow p + \bar{p} \rightarrow 2\gamma$.

Moreover, Møller referred to the possibility that "a μ-meson in the K-shell of an atom may now be absorbed in a nucleus by emission of a neutrino, and the probability of this happening will be proportional to Z^4." This was an early reference to μ-mesic or so-called muonic atoms, where a μ meson (muon) replaces an orbital electron in the orbit closest to the nucleus and acts in the same way as an electron in a K-capture process (Section 3.4). In this kind of muon capture process, which was later investigated in detail, a nuclear proton is transformed into a neutron according to $\mu^- + p^+ \rightarrow n + \nu_\mu$. The nucleus will then change from (A, Z) to $(A, Z - 1)$. At the time of the Bristol meeting μ meson capture was known to exist and for $Z > 10$ to be more probable than the $\mu \rightarrow e$ decay in the K orbit. However, it was not yet known that the neutrino emitted by muon capture ν_μ is different from the ordinary electron neutrino ν_e.

At the end of his report, Møller referred to the puzzle of the so-called τ meson. This short-lived particle with a mass of approximately 900 m_e had recently been identified by Powell and his Bristol group, who at the end of the year found it to decay according to $\tau^+ \rightarrow \pi^+ + \pi^+ + \pi^-$. Still unaware of this decay scheme, Møller

139. Møller (1949b), p. 144.

expected the new heavy meson to decay as $\tau^+ \to p^+ + \bar{n} \to \pi^+ + \gamma$. "It seems tempting", he said,"

> to consider these τ-mesons as heavier π-mesons, analogous to the way that μ-mesons were tentatively treated as heavy leptons. ... The easiest way to describe the existence of the heavy τ-mesons would be to introduce a new mass variable for the π-mesons. Introduction of such a new variable for an elementary particle is, however, likely to introduce new selection rules for the possible physical processes and it is conceivable that these new selection rules would forbid a transition of the type $[\tau^+ \to p^+ + \bar{n} \to \pi^+ + \gamma]$.[140]

With the benefit of hindsight one can perhaps see in Møller's remarks an anticipation of the lepton number as a conserved quantity, a concept which was first suggested in a paper by Konopinski and Hormoz Mahmoud from 1953.[141] With the discovery at about the same time of the θ meson, the famous θ-τ puzzle was recognised: although the two particles had the same mass and lifetime, they decayed differently ($\theta^+ \to \pi^+ + \pi^0$) and had different spins and states of parity. Within a few years the puzzle led to the celebrated discovery of parity non-conservation in weak interactions.[142] Among those who listened to Møller's report in Bristol was Louis Michel, a young French physicist who at the time studied with Rosenfeld in Manchester. Inspired by Møller's paper and his idea of a particle spectrum, Michel investigated theoretically the electron spectrum from the $\mu \to e$ decay in a paper clearly indebted to Møller's lecture. Forty years later, Michel recalled:

> Back in Manchester, I first did the computation suggested by Møller on the electron spectrum from μ decay. Why did I not quote him in the letter sent to *Nature* a few months later? To this day, I do not know.

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140. Møller (1949b), p. 146, who used the symbol n_{anti} for the antineutron, "a hole in the sea of negative energy neutrons." The antineutron was hypothesised by the Russian-Italian physicist Gleb Wataghin in a paper of 1935 but only detected in 1956.
141. Konopinski and Mahmoud (1953). Pais (1986), p. 530.
142. Franklin (1986), pp. 39-72.

> Probably because there was no text to quote (no preprints in those times, at least in Europe). My revered teacher Rosenfeld, who had to correct my first writings in English a great deal, did not comment on this omission, although Møller was his personal friend. ... At last, I can repair this omission today.[143]

Møller's next assignment was to participate in the eighth Solvay congress convening in Brussels from 27 September to 2 October 1948. Of the 44 physicists who participated as either invited speakers, members of the scientific committee, or 'reporters', only six were from the United States, a remarkably small number given the subject of the conference and the American dominance in high-energy physics. None were from Germany, undoubtedly an indication that the scars from the war and the Nazi past were still painfully visible. The participants included Bohr, Powell, Bhabha, Kramers, Klein, Schrödinger, Rosenfeld, Dirac, Møller, Peierls, Oppenheimer, and Robert Serber. During the banquet of the Solvay conference, the physicists burst into songs composed for the occasion. One of them, the 'Meson Song', was due to Edward Teller and with music by Otto Frisch, who was an accomplished piano player. I quote two verses from this unforgettable piece of poetry:

> There are mesons pi, there are mesons mu
> The former one serve as nuclear glue
> There are mesons tau, or so we suspect
> And many more mesons which we can't yet detect
> Can't you see them at all?
> Well, hardly at all
> For their lifetimes are short
> And their ranges are small.
>
> From mesons all manner of forces you get,
> The infinite part you simply forget,
> The divergence is large, the divergence is small,
> In the meson field quanta there is no sense at all.

143. Michel (1989), p. 379. Michel (1949). Louis Michel visited the Bohr institute in 1950-1951 and 1952-1953.

What, no sense at all?
No, no sense at all!
Or, if there is some sense
It's exceedingly small.[144]

Bohr gave the opening address in Brussels, a very general talk on causality and complementarity in which he dealt with the epistemological problems in quantum physics. He actually read from a paper which was about to be published in a special issue of the periodical *Dialectica* edited by Pauli. "It must never be forgotten", Bohr reminded his audience, "that we ourselves are both actors and spectators in the drama of existence ... [and] that our task can only be to aim at communicating experiences and views to others by means of language, in which the practical use of every word stands in a complementary relation to attempts of its strict definition."[145] Powell and Serber both gave talks on their current works on meson physics, the first dealing with mesons observed in the cosmic rays and the latter with those produced artificially in the Berkeley laboratory.

Edward Teller presented a report on element formation in the universe written jointly with his Chicago colleague Maria Goeppert Mayer (who was not invited to Brussels). According to the Teller-Mayer theory, the elements had their origin in 'polyneutrons', hypothetical primordial objects of nuclear matter with a large excess of neutrons.[146] Teller compared the polyneutron theory with the new big-bang theory of Gamow and his collaborators which was thus made known to the physicists in Brussels among whom Klein and Peierls made critical comments on the two theories of cosmic element formation. The polyneutron theory of Teller and Mayer was short-lived and after a few years it was abandoned as an alternative

144. Solvay (1950), p. 382 and Mehra (1975), pp. 262-263. Other songs and ballads were composed by Casimir and Rosenfeld.
145. Solvay (1950), p. 17.
146. Kragh (1996), pp. 123-125. The Polish-born German physicist Maria Goeppert came to the United States in 1930 and after marriage with the American chemist Joseph Mayer she changed her last name to Goeppert Mayer.

to Gamow's theory, although this theory was also generally considered to be unsatisfactory as it could not account for the formation of elements heavier than helium.

In the discussion following Oppenheimer's report on the present state of quantum electrodynamics, Dirac expressed his strong dislike for renormalisation methods and his hope of avoiding infinities by looking for solutions to the wave equations without perturbation methods. Bohr entered the discussion in support of Oppenheimer, who found the situation in electrodynamics to be satisfactory whereas "in the meson-nucleon problems, everything is wrong."[147] Møller might not have agreed, but he did not intervene in the discussion.

Instead of giving a separate lecture in Brussels, Møller contributed to the final discussion about the current state of meson theory. Oppenheimer raised questions about the μ meson decay $\mu^{\pm} \rightarrow e^{\pm} + \nu + \nu$ and also about the relation between the μ meson lifetime and that of beta decay. "We might ask Prof. Moeller to report on the calculation made by three of his Danish colleagues who have the general formulation for all values of the mass of the neutral meson for various couplings.'[148] Møller responded by elaborating on the analogy between the decay of the μ meson and the beta decay, largely repeating some of his arguments from the recent Bristol conference. Theory as well as experiment, he said, "indicates that the decay of the μ meson is due to a similar process as the β decay and that we have a kind of Fermi interaction between all particles of spin ½."[149]

Rather than going back to Copenhagen after the Solvay congress, Møller stayed in the Low Countries for a week, during which period he visited Kramers in Leiden. Afterwards, he went to Amer-

147. Solvay (1950), p. 284. Also Bhabha, Pauli, and Casimir joined the discussion, the latter with comments on the zero-point energy of free space and its connection to the so-called Casimir effect which he had introduced the same year.

148. Oppenheimer referred to Horowitz, Kofoed-Hansen, and Lindhard (1948), a recent paper by the two young Copenhagen physicists Otto Kofoed-Hansen and Jens Lindhard in collaboration with Jules Horowitz, a visitor from Paris.

149. Solvay (1950), p. 365. Mehra (1975), pp. 238-265.

ica. Purdue University in Lafayette, Illinois, had hired him as visiting professor for four months, principally to give a series of lectures on quantum field theory. He also had to give a weekly seminar, for which he chose mesons and *S*-matrix theory, and in addition he gave lectures on relativity theory. Unusually at the time, Møller crossed the Atlantic by airplane, leaving from Brussels and landing in New York on 4 October 1948. The first thing he did in the new world was to buy a car and then drive all the way to Lafayette, a ride of about 1,200 km. "On this tour", he wrote to Bohr, "I got a very good impression of the landscape and the population of America. I was surprised how well I already knew America from books and movies without ever having been here."[150] Kirsten joined him in the early days of the new year.

At Purdue, Møller met his friends Harald Wergeland, a Norwegian physicist, and Dirk ter Haar, the Dutch physicist who in 1946 had spent a year as a research fellow at Bohr's institute. He also met another Dutchman, Frederik Belinfante, who three years earlier had been appointed professor at Purdue and with whom he would soon enter a collaboration. As mentioned in Section 5.2, Møller's coinage of the word 'nucleon' in 1941 was inspired by Belinfante's earlier 'nuclon'. In his mimeographed 100-page lecture notes, Møller covered in mathematical details the formalism of quantum electrodynamics and the quantisation of the free electromagnetic field. Rather than following Dirac's original hole theory of 'positons' and 'negatons' (as Møller insisted to call the particles) he preferred an alternative and "more symmetrical" formalism proposed by Heisenberg in 1934.

Møller's careful exposition of quantum field theory relied entirely on theories developed prior to 1948 by European physicists such as Dirac, Pauli, Heisenberg, Fermi, Kramers, and Wentzel. It only briefly alluded to the new methods leading to renormalised quantum electrodynamics:

> The discovery of the Lamb-Retterford [Retherford] effect and other similar effects forced the theoretical physicists seriously to take the

150. Møller to Bohr, 25 October 1948 (BSC). Møller to Rozental, 25 October 1948 (Rozental Papers, NBA).

> question of the region of validity of quantum electrodynamics up again, since this effect was connected with the self-energy of the electron. Investigations of Bethe, Kramers, Weisskopf, Schwinger, and many others have shown that quantum electrodynamics is able to account for the Lamb-Rutherford [Retherford] effect and other similar effects and quantum electrodynamics has thus, in spite of its defects, turned out to be quite useful.[151]

The Lamb-Retherford effect mentioned by Møller was a small shift in the atomic hydrogen spectrum discovered by the American physicist Willis Lamb and his student Robert Retherford in 1947. Whereas the hydrogen states $2S_{1/2}$ and $2P_{1/2}$ should have the same energy according to Dirac's theory, the two physicists at Columbia Radiation Laboratory found that the states were separated by an energy corresponding to the wave number $1/\lambda = 0.033\ \mathrm{cm}^{-1}$. The Lamb shift, as it is generally called, was immediately recognised to be a significant guide to an improved theory of quantum electrodynamics and was fully explained by the renormalised theory which appeared shortly later. Møller was impressed, but to him it merely showed that the new theory was "quite useful." At the very end of the lecture notes Møller said that "the primitive form of quantum electrodynamics' presented in the notes "allows us to treat the same effects as the new elegant formalisms developed by Tomonaga and Schwinger." He nowhere mentioned Feynman.

In connection with his stay at Purdue University, Møller also visited the University of Wisconsin, University of Chicago, Washington University (Saint Louis), Stanford, and Berkeley, in all cases giving lectures. Yet another destination was the Institute for Advanced Study in Princeton, where Oppenheimer had invited him to spend the Christmas vacation.[152] Moreover, he was invited to give a paper to the New York meeting of the American Physical Society in late

151. C. Møller, *Elementary Quantum Field Theory*, mimeographed lecture notes prepared by E. Strick, Purdue University, 1948-1949, pp. 2-3.

152. Felix Bloch to Møller, 18 February 1949 (CMP). Oppenheimer to Møller, 1 December 1948 (CMP): "Kitty [Katherine Oppenheimer] and I hope that you remember that you promised to stay with us."

January 1949, where the plan was that he should present a paper complementing one given by Yukawa. However, he did not find it possible to join the New York meeting at Columbia University.[153] Nonetheless, Møller did meet Yukawa. As the latter wrote in a letter to Bohr, "I met Prof. Møller and talked about the possibility of visiting your institute during my stay in this country."[154]

Møller's main research interest in America was the theory of mesons and its possible extension to a more ambitious theory of all known elementary particles that might even include particles not yet detected. This was the subject he had dealt with in his 1946 Bristol lecture, and he summarised his general idea in a letter to Bohr:

> I think it will be of some heuristic value to try ordering all elementary particles in 3 families: Nucleons, mesons (comprising the π-mesons and possibly heavier states: some of the τ-mesons), and leptons (comprising the electron, neutrinos, and maybe some of the τ-mesons), and to ascribe different, more or less analogous transition processes between these groups of particles. ... Moreover, the formalism also requires the existence of higher mass states of the nucleons, and at least presently these have not been found.[155]

In February 1949 Møller accompanied by Kirsten went on a grand tour to California, visiting Pasadena, Stanford, and Berkeley. He became informed about the most recent American high-energy experiments, which confirmed his suspicion that the μ meson decayed into an electron and two neutrinos. At the time a neutrino was a neutrino, and it was not yet realised that the μ meson or muon decays into two different neutrinos, one an electron neutrino and the other a muon neutrino. The decay schemes are

$$\mu^- \rightarrow e^- + \bar{\nu}_e + \nu_\mu \text{ and } \mu^+ \rightarrow e^+ + \bar{\nu}_\mu + \nu_e$$

153. Yukawa to Møller, 16 November 1948 (CMP), with an invitation from Karl Darrow, secretary of the American Physical Society.

154. Yukawa to Bohr, 7 January 1949 (BSC, Supplement). Yukawa came to Copenhagen in mid-December 1949, where he stayed for about a week.

155. Møller to Bohr, 12 December 1948 (BSC).

As Møller reported to Bohr, the Berkeley physicists had detected a large number of photons when bombarding a target with fast protons. This he found most interesting: "[Robert] Serber assumes that the radiation is due to the decay of primary created π-mesons into two photons, a process which has been considered theoretically by [Robert J.] Finkelstein several years ago. The lifetime of this neutral π-meson should be very short, ca. 10^{-16} sec."[156] The Møller couple left Purdue University on 16 March and went to New York from where they departed by boat to Copenhagen two weeks later.

After his return to Denmark and a much-needed summer holiday, Møller continued his busy travel schedule. On 2 September 1949 he went from Copenhagen to a conference in Basel and from there to a meeting in Como 11-16 September arranged by the Italian Physical Society on the occasion of the 150th anniversary of Alessandro Volta's discovery of the electric battery. While the subject of the Basel conference was nuclear physics and quantum electrodynamics, the one in Como focused on cosmic rays. The Como conference was attended by a large number of eminent physicists, among them Fermi, Segré, Kramers, Powell, Heisenberg, Pauli, and Meitner.[157] Bohr had also been invited, but was unable to participate, and thus Møller was alone in representing the Copenhagen institute. Reporting to Belinfante on the two conferences, Møller singled out a report given by Edwin McMillan on experiments made in Berkeley on the still poorly understood neutral meson: "It seems to be rather certain now that a neutral meson is produced with nearly the same cross-section as the charged meson and that the neutral meson disintegrates into *two* photons. This shows that this neutral meson has spin 0. Its mass is about 300 electron masses, i.e. it is of the π-meson type, as one should also expect if the π-mesons are the nuclear force quanta."[158] Direct confirmation of the two-photon decay was provided the following year in experiments using the Berkeley electron synchrotron.

156. Møller to Bohr, 15 March 1949 (BSC). On the Berkeley discovery of the neutral pion and its decay $\pi^0 \rightarrow 2\gamma$, see Pais (1986), p. 480.

157. Schein (1950).

158. Møller to Belinfante, 11 November 1949 (CMP).

On Bohr's recommendation, Møller was invited to a conference on elementary particles held in Edinburgh in mid-November 1949. "For years [Møller] has been so deeply involved with the meson problems", Bohr wrote to Born. "He is at present in the U.S.A. from where he has sent me some very interesting letters about his ideas of correlating the experimental evidence, on which he entered already at the Birmingham conference last September."[159] Before the opening of the Edinburgh conference, which took place from 21 October to 11 November, Bohr delivered a series of ten Gifford Lectures on 'Causality and Complementarity'. He and his wife Margrethe also participated in the conference that followed. Other participants in Edinburgh included Born, Kramers, Powell, Proca, Rosenfeld, Michel, and Pontecorvo.[160] Born, who since 1936 had served as Professor of Natural Philosophy at the University of Edinburgh, gave a presentation on 'General Theory of Elementary Particles'. Contrary to the Solvay congress, Heisenberg was invited, but to the relief of Born and possibly also to Bohr he cancelled at the last minute: "I am still more sorry about what you told me in regard to his [Heisenberg's] attitude in the matter of the atomic bomb. I frankly confess that I was rather relieved when he informed me that he could not attend our conference."[161]

Yet another of the participants was the German-born physicist Klaus Fuchs, who had worked in the Manhattan Project and only later, in early 1950, was exposed as a communist spy and then sentenced to fourteen years in prison.[162] In fact, at the time of the Edinburgh conference he was already suspected to be a spy by the British military intelligence. Rozental, who attended Bohr on his journey to Scotland, remembered "a pleasant lunch we had with the two [Fuchs and Jánossy] and some other colleagues, where Fuchs talked a lot to me about his plans for NB's [Niels Bohr's] trip to

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159. Bohr to Born, 24 December 1948 (BSC).

160. *Nature* **164** (1949): 561. See also Michel (1989), p. 379.

161. Born to Bohr, 26 December 1949 (BSC). The first sentence refers to Bohr's meeting with Heisenberg in September 1941 (Section 4.2).

162. See Born (1978), p. 284-288 for his recollections about the Edinburgh conference and the Fuchs affair.

Harwell."[163] After having served nine years in prison, Fuchs was released and emigrated to DDR, the German Democratic Republic. Bruno Pontecorvo was another brilliant communist physicist who emigrated to the East, but in his case in 1950 to the Soviet Union and without being an atomic spy. Pontecorvo was supposed to attend the Harwell conference on nuclear physics in September 1950, where Bohr gave a talk, but at the time he had already secretly defected to Russia with the help of Soviet agents.[164]

The year of 1950 offered Møller two travels, one more exciting than the other. First he drove by car with his wife to attend an International Theoretical Physics Conference on Fundamental Particles and Nuclei which took place at the Institut Henri Poincaré in Paris 24-29 April. It was sponsored by the Centre National de la Recherche Scientifique (CNRS), the large French science agency established in 1939. Among the many physicists attending the conference were Bhabha, Belinfante, Dirac, Feynman, Rosenfeld, Casimir, Pauli, and Proca.[165] On the request of Alexandru Proca, an influential Romanian-French theorist, Møller reviewed critically the most recent works on meson theory, including those of Yukawa published in 1949-1950.[166] He also discussed the extremely short-lived π^0 meson (lifetime 10^{-16} s) and its possible role in the nucleus as given by the hypothetical reaction $\pi^0 \rightarrow p + \bar{p} \rightarrow 2\gamma$, the same decay scheme he had suggested at the Bristol conference in 1948. Møller concluded that there still was no satisfactory meson theory of nuclear forces and that the methods of renormalisation were of no use in getting rid of the infinities in meson theory, a point which also Pauli stressed in his contribution.

Among those Møller met in Paris was the 23-year-old Swedish quantum theorist Gunnar Källén, who had just begun his brilliant

163. Rozental (1998), p. 111. Lajos Jánossy was a Hungarian physicist who did very important work on cosmic rays while staying in England. His decision to return to Hungary in 1950 was in part politically motivated.

164. Bonolis (2005). Close (2015), p. 41 and p. 184.

165. Belinfante (1950). Møller to Bohr, 1 April 1950 (BSC, Supplement).

166. Møller (1953a). Proca had spent a couple of months at Bohr's institute in 1934-1935, where he met Møller.

but short career in theoretical physics. Källén's talk on new formulations of quantum electrodynamics impressed Møller, who in a memorial article many years later recalled his "brilliant appearance at international conferences, starting with the Paris Conference in the spring of 1950." Two years later, Källén came to Copenhagen, where he had close contact to Møller, first as a fellow of CERN's theoretical study group and then hired to a permanent position. He served as a professor at Nordita, the Nordic Institute for Theoretical Atomic Physics, in 1957-1958 and was subsequently appointed professor at the University of Lund. "In the light of history Gunnar Källén's appearance in the world of physics was like a shooting star", Møller wrote.[167]

During the 1930s, several Indian physicists contributed importantly to frontier research, such as did C. V. Raman (Nobel Prize 1930), S. N. Bose, H. J. Bhabha, M. Saha, and S. Chandrasekhar (Nobel Prize 1983). However, many of them worked in England or elsewhere outside India. The country won full independence only in August 1947 and at the same time the former crown jewel in the British empire was split into two, India and Pakistan. Homi Bhabha, a leading expert in meson theory and quantum field theory, worked in Cambridge but was in India when World War II was declared and decided to stay there, first at the Bangalore Institute of Science. On his instigation and with funds provided by the Dorabji Tata Trust, a conglomeration of industrial companies, in June 1945 the Tata Institute of Fundamental Research was established in Mumbai (then Bombay) with Bhabha as its director.[168]

The first international conference at the Tata Institute, co-sponsored by UNESCO, was on elementary particle physics. It took place 14-22 December 1950 with Møller as one of several invitees. Other physicists who attended the conference included Rosenfeld, Peierls, Wentzel, Blackett, and Saha. Strangely, with the possible

167. Møller's memorial article was first published in 1969 and later reprinted in Jarlskog (2014), pp. 304-309. At another occasion Møller described Källén as "a genius, really very bright." Weiner (1971c). Källén died in an airplane crash in 1968.
168. See Sreekantan (2006) for a history of the Tata Institute and its scientific activities. The founder of the trust, Sir Dorabji Tata (1859-1932), was Bhabha's uncle.

exception of Wentzel (who was German but since 1948 had been at the University of Chicago), no physicists from the United States were invited. As the negative proton had been part of the 1946 Cambridge conference, so attempts to detect the particle was discussed by the Edinburgh physicist Norman Feather at the Mumbai conference. On the last day of the conference Møller delivered an address on non-local field theories in which he examined from a mathematical point of view various candidates for describing the meson field.[169] He referred for the first time to the calculations of Povl Kristensen, with whom he had begun collaborating on what would be the Møller-Kristensen convergent meson theory.

At a social evening after the conference, Peierls and Rosenfeld composed a number of verses. Bohr had been invited to India, but declined the invitation:

This time, as many times before,
The first to speak was to be Bohr
But Bohr, though he was chosen, failed
To come, so we had Rosenfeld.

Another of the verses referred to Møller's absence from the evening event:

From Bristol we expected Powell, or
His chief collaborator Fowler.
We sadly missed Professor Møller
I think he ate too much – poor fellow.[170]

169. Møller (1951). In a non-local field theory, the Lagrangian density depends not just on the value of the field at any particular time but also on the value of the field at other points.

170. *Report of an International Conference on Elementary Particles Held at the Tata Institute of Fundamental Research* (Mumbai: Commercial Printing Press, 1951), p. 201. The poem is reprinted in Chowdhury and Dasgupta (2010), p. 126, which also includes a group photo of Møller and the other participants at the Old Yacht Club in Mumbai.

After the conference in Mumbai had ended, the participants were the guests of the Indian authorities on a tour of some of the famous sights of India, ending at Bangalore where they visited Raman's laboratory.[171]

Having returned to England, Peierls wrote to his friend Bethe, who was contemplating a visit to Birmingham:

> I am also due to go to the conference in Copenhagen, and our plan is to take the car across via Ostend and to drive up to Copenhagen. ... It looks possible that we should reach Copenhagen on the evening of the 5th. What would you think about joining us in the trip? ... Scientifically the most important result of the Bombay conference for me was that the Bristol people produced convincing evidence in favour of multiple as opposed to plural production of mesons. ... I also learnt there for the first time about Fermi's theory of meson production which is very attractive and simple as everything else that Fermi does.[172]

The Copenhagen conference mentioned by Peierls was originally scheduled to June but postponed to 6-10 July 1951. Møller invited Pauli and kept him updated on the program:

> We are trying now to make a tentative program for our Conference in July. Although it was our intention to limit the number of participants it still looks as if it will be rather large, about 100 or even more. ... As you know, the title of the conference is 'Problems of quantum physics' and it is planned to have discussions on mesons, nuclear forces, nuclear constitution, field theory, etc. ... Even if the free and informal discussions is the main purpose of the conference it was suggested to ask some people to tell us about special problems. Therefore, one day will be devoted to mesons and cosmic radiation and we have asked Powell to deliver the introductory talk. Bohr will lecture on complementarity on the afternoon of Thursday, the same on which field theory is planned to occupy the morning. As to theory of nuclear constitution and nuclear reactions, we have asked Weisskopf to speak on Monday, and we

171. Peierls (1985), pp. 262-263.

172. Peierls to Bethe, 28 February 1951, in Lee (2007), pp. 368-369.

> would like to have Wick (on Saturday) to introduce the discussion of artificially produced mesons.[173]

Bhabha was also invited, but at first he declined. "I am glad that you enjoyed your trip to India", he wrote to Møller. "I should like very much to attend a conference in Copenhagen again but it is doubtful whether I will be able to come from the 6th to the 10th July."[174] However, he did turn up at the Copenhagen conference, which because of the large number of attendees was held at the nearby Rockefeller Institute and not at Bohr's institute on Blegdamsvej. Apart from Pauli, Bhabha, and Peierls, among the numerous other participants were Dirac, Meitner, and Frisch, and also Møller's former collaborators Milton Plesset and André Mercier.

After the obligatory opening address by Bohr, there were reports on meson physics by Powell and Wick, on nuclear forces by Bethe, on nuclear structure by Aage Bohr and Nordheim, on nuclear reactions by Weisskopf, and on field theories by Møller, Rosenfeld, and Bethe. Among the participants was also Maria Goeppert Mayer, who came to Copenhagen from Chicago and at the time had started her work on the nuclear shell model which in 1963 would make her the second woman, after Marie Curie, to win a Nobel Prize in physics. The appearance of a lone woman among all the men attracted interest in Danish newspapers, which interviewed her about how she could be a housewife and at the same time a nuclear scientist.[175] In between the presentations and discussions, one day was reserved for a social tour to North Zealand. The conference ended with a general discussion about complementarity, naturally presided by Bohr.

As a member of the Solvay scientific committee, Møller participated in the ninth and tenth Solvay congresses in Brussels, both of which were devoted to problems in solid-state physics. While the subject of the 1951 conference 25-29 September was simply 'The Solid State', that of the 1954 conference 13-17 September was 'Electrons in Metals'. The field of solid-state physics or what later

173. Møller to Pauli, 7. May 1951, in Pauli (1996), p. 297.

174. Bhabha to Møller, 2 March 1951, quoted in Singh (2009).

175. *Nationaltidende*, 13 July 1951.

came to be the broader field of condensed matter physics was at the time new and still in the early phase of what turned out to be an explosive growth.[176] However, research in solid-state physics was absent from Bohr's institute in Copenhagen, where neither Bohr nor Møller, nor others of the staff, took any interest in the field. It was not considered fundamental physics. Møller listened to the lectures and discussions in Brussels without intervening or, assumedly, being much interested in them.

The highly successful Les Houches summer school of physics (École des Physique des Houches) was established in 1951 by the young French theoretical physicist Cécile Morette, who the same year married the American theorist Bryce DeWitt and changed her last name to DeWitt-Morette.[177] The school, located in a scenic area close to Chamonix and Mont Blanc, quickly attracted interest from physicists and physics students although during the early years the living conditions were quite primitive. Teachers at the first session included leading physicists such as Pauli, Heitler, and Segré, and at the second session in 1952 Rosenfeld gave a course. The third annual session of the summer school was held 6 July to 29 August 1953 for about thirty students of which half were French and most of the others from other European countries. According to Geoffrey Chew, who gave lectures on elementary particles: "Regular courses were held in the morning, six days a week, and seminars were given in the afternoon. Students attending the session worked very hard, having only Saturday afternoons and Sundays for relaxation."[178] Møller spent the summer of 1952 in England, mostly on vacation but interrupted by a lecture on meson theory which the German-British physicist Herbert Fröhlich had invited him to give at the University of Liverpool.[179]

176. Hoddeson et al. (1992).

177. See Verschueren (2019). She is also referred to as C. M. DeWitt or C. Morette-DeWitt.

178. Chew (1953). See also Pauli (1999), pp. 185-186.

179. Møller and Fröhlich to Pauli, 16 July 1952, and Pauli to Møller, 2 August 1952, in Pauli (1996), p. 669 and p. 686.

Cécile Morette wrote her 1947 doctoral thesis on the Møller-Rosenfeld mixed meson theory and related theories. From September 1947 to the spring of 1948 she continued her studies of meson theory under Møller's supervision at Bohr's institute, where she stayed on a Rask-Ørsted fellowship.[180] In early 1953 she asked Møller to give lectures on the same topic at the third session of the Les Houches school, which Møller accepted. He wrote her:

> What I have in mind for my own (possibly 6 to 10 lectures) is to talk about the pseudoscalar meson theory, starting with a historical survey of the development of meson theory from the beginning in 1934 and ending with today's work on this problem, where especially Lévy's investigations show some preference for the pseudoscalar theory. Also our work on non-local theories should be mentioned.[181]

Instead of going by train (or by airplane to Paris), Møller went all the way from Copenhagen to Les Houches by car. He liked sitting behind the wheel. Having taught his course in the early part of the session he went directly to Hamburg, where he had to be on 23 July at the latest. On his way to Hamburg he visited the University of Heidelberg, where he had been invited by Hans Jensen. Møller thus missed one of the Les Houches session's main attractions, namely Pauli's lectures on 10 August on the *H* theorem in statistical mechanics originally formulated by Ludwig Boltzmann in 1872.

For a change, Møller's reason for going to Hamburg was not a meeting on meson theory or some other topic of physics, but was rather of a political or ideological nature. The city hosted 23-26 July a congress on *Wissenschaft und Freiheit* (Science and Freedom) with participation of a large number of authors, philosophers, historians, sociologists, and scientists. The sponsoring committee included prominent physicists and chemists such as Oppenheimer, Arthur

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180. Morette to Møller, 12 March 1947 (CMP).

181. Møller to Morette, 26 February 1953, as quoted in Pauli (1999), p. 185. Møller to Belinfante, 30 June 1953 (CMP). The French physicist Maurice Lévy, who gave a course on field theory at Les Houches, established at about the same time a theoretical physics group at the École Normale de Supérieure in Paris.

Compton, Otto Hahn, and Lise Meitner. Among the scientists who attended the congress was, apart from Møller, the Ukrainian-born American geneticist Theodosius Dobzhansky, the Hungarian-British polymath Michael Polanyi, and the physicists James Franck and Hans Thirring. Yet another participant was Charlotte Houtermans, whom Møller thus met again after they had been in close contact in Copenhagen some fifteen years ago.

The Hamburg congress was one of numerous cold war initiatives of the Congress for Cultural Freedom (CCF), an anti-communist organisation founded in 1950 with the aim of defending the cultural values of Western liberal democracies against the communist propaganda. In his opening address, Franck pointed out that the freedom of science was principally threatened by the communist system: "The National Socialist system was not the only one to use distorted interpretations of scientific conclusions as a cloak for its objectives. The same methods are now being applied in countries which have fallen victim to Communism." Unfortunately, even the democracies were not entirely free of guilt, for here too there were many examples of political interference in science. With a hidden reference to the McCarthyist era Franck warned against the "undesirable consequences of the spy-scare in the United States" which included "a disastrous and stupid form of intervention" in scientists' freedom to think and work.[182] Precisely what Møller did in Hamburg and why he attended the congress (as the only participant from Denmark) is unknown.

Japanese physicists such as Yukawa, Sakata, and Tomonaga were key players in theoretical physics during the difficult period of the 1940s. Yukawa, who in 1949 became the first Japanese citizen to receive a Nobel Prize, continued to develop his meson theory in a series of papers. With the American occupation followed a decree that "all research in Japan of either a fundamental or applied nature

182. *Science and Freedom: Report on the Hamburg Congress* (London: Congress for Cultural Freedom, 1955), pp. 22-23. As it later turned out, the Congress for Cultural Freedom was to a large extend created and funded by the CIA. For the Hamburg congress, see also Scott-Smith (2002).

in the field of atomic energy should be prohibited."[183] Nonetheless and in spite of the many problems, theoretical high-energy physics and related fields flourished in a remarkable manner.

In order to strengthen the international relations to the somewhat isolated Japanese physics community, from 14 to 24 September 1953 a large International Conference of Theoretical Physics was arranged in Tokyo and Kyoto under the auspices of IUPAP (International Union of Pure and Applied Physics) and the Science Council of Japan. This meeting, the first purely scientific international conference ever held in Japan, was supported by UNESCO and the Rockefeller Foundation. According to Masao Kotani, one of the local organisers, "It may not be an undue exaggeration to say that the conference opened the door to international exchange in science, which had been closed since the beginning of the World War."[184] The American occupation had ended the previous year and Japan was beginning its impressive economic recovery.

Møller had been asked earlier in the year to come to the Vanderbilt University in Nashville as a visiting professor for the year 1953-1954 but regretfully declined the invitation, giving as reasons not only the forthcoming travel to Japan but also his obligations with the new CERN theory group in Copenhagen.[185] Starting with a single day of celebration in Tokyo, the Japanese conference was organised in three parallel sections in Kyoto, one on field theory and nuclear physics, another on statistical physics, and yet another one on solid-state physics. It was attended by about 600 Japanese physicists and students and about fifty foreign participants from thirteen different countries. One of the invited participants was Møller, who joined the conference together with, among others, Pais, Feynman, C. Bloch, Marshak, Wigner, Per-Olov Löwdin, Amaldi, Lars Onsager, and Chen Ying Yang.[186] Pais, who might have had a share in

183. Konuma (1989), p. 536.

184. Low (2005), p. 175. See Konuma (1989) on Japanese particle physics in the 1950s.

185. Møller to D. L. Hill, 8 May 1953 (CMP). See Section 8.3 for the CERN theory group.

186. Feynman, who talked on the theory of liquid helium in Kyoto, shared a hotel room with Pais. See Feynman (1986), pp. 239-244 for his charming and unconven-

Møller's coinage of 'lepton' in 1946, introduced in his talk in Kyoto another and very successful name for members of the nucleon family. He suggested to call the heavy particles 'baryons', a term that denoted both nucleons and hyperons.[187]

As the only Danish physicist attending the Tokyo-Kyoto conference, Møller stayed in Japan for nearly a month. On his return to Denmark he was interviewed by a newspaper about his impression of the exotic country, its culture, and its inhabitants. Møller mentioned the unusual Japanese gardens and the unique hospitability of the Japanese as particularly noteworthy.[188] Among the Japanese physicists Møller met in the Land of the Rising Sun were not only Yukawa and Tomonaga but also Shoichi Sakata, who much earlier had worked in the same area as Møller, namely electron-capture radioactivity (Section 3.5). Upon his return to Denmark, Møller arranged a Rask-Ørsted fellowship for Sakata, who came to Copenhagen in May 1954 and stayed for about three months.[189]

In his presentation on 18 September, Møller examined 'The Problem of Convergence in Non-Local Field Theory' with particular reference to the ideas of Yukawa, who had recently written on the same subject and compared his own non-local theory with what he called 'the Bloch-Kristensen-Møller formulation'.[190] Møller discussed and advocated his own new work with Kristensen, which he related to theories proposed by C. Bloch, Källén, Yukawa, and some other physicists. Pais, Bloch, Feynman, Peierls, Bloch, and the Japanese theorist Ryoyu Utimyama participated in the discussion following Møller's presentation. During the discussion session, Feynman remarked at one point that a certain theorem was wrong, and when asked what was wrong with the theorem, he answered in his characteristic way: "I have not got the slightest idea. I have difficulty in

tional account of his travel experiences in Japan. See also Peierls (1985), pp. 263-265.

187. Pais (1986), p. 514. 'Baryon' was proposed by Belinfante before Pais coined the word, but in the sense of an excited nucleon.

188. *Berlingske Tidende*, 4 October 1953.

189. Møller to Sakata, 27 October 1953 (CMP). Sakata to Møller, 8 February 1954 (CMP). Yukawa to Møller, 27 January 1954 (CMP), where Yukawa referred to "our brief but very pleasant stay in Copenhagen."

190. Møller (1953b). Yukawa (1953).

understanding of formal proofs, and I rarely can understand what is wrong with them. I only noticed it is wrong. I have got one idea maybe, but I am not sure."[191]

In early March 1957 Møller attended a series of invited lectures on quantum mechanics and relativity theory which the Russian theorist Vladimir Fock gave at the Bohr institute (Section 8.4), and shortly thereafter he went to Italy for some time to give lectures in Pisa. In April the following year the two German physical societies – one in the West and the other in the East – arranged a common celebration of the one hundredth anniversary of Max Planck, the founder of quantum theory. The celebration event attracted a large number of German and foreign physicists, and also several high-ranking politicians such as the DDR leader Walter Ulbricht. Møller, who was among the invited guests, gave an address on general relativity and he met in Berlin with Lise Meitner, James Franck, Max Born, Otto Hahn, Paul Dirac, and many others.[192] At the time Møller no longer worked actively in problems of quantum mechanics but instead focused on Einstein's theory of relativity of which Planck had been the earliest and most important supporter.

191. Møller (1953b), p. 23.

192. Møller (1959b). Weisskopf (1958). On the political context of the Planck centennial events in Berlin during the Cold War, see Hoffman (1999).

CHAPTER 6

The attraction of gravitation

Ever since his youth Møller had been greatly interested in Einstein's theory of relativity and in particular in the mathematically difficult general theory which explained gravitation in a radically new way. However, Bohr's institute on Blegdamsvej was not the right environment to cultivate this interest as work in the institute focused rather one-sidedly on atomic and quantum physics. Moreover, when Møller started his scientific career, general relativity attracted very little international attention. It was widely considered a fringe area of more interest to mathematicians and philosophers than to physicists. Although Møller was quickly drawn into the world of quantum mechanics – a branch of physics with almost no connections to general relativity – Einstein's fascinating theory remained in the back of his mind. His continual interest in the area is indicated by his 1941 memoir on meson field equations in five-dimensional de Sitter space. But this was an anomaly in his scientific production and the same was the case when he two years later published a research paper on the clock paradox in general relativity, a work he did not himself regard as important.

For most of the next decade Møller worked on meson theory, the *S*-matrix formulation of quantum mechanics, and aspects of elementary particle physics, apparently giving no more thought to relativity theory. But only apparently, such as indicated by a paper of 1950 he published in a Danish mathematics journal.[1] The subject was the so-called Thomas precession, the historical background of which is the following. In the early days of the electron spin hypothesis, calculations based on the new quantum mechanics gave a doublet splitting for the hydrogen spectrum which differed by a factor 2 from the one observed. The discrepancy was resolved in the early spring of 1926, when the British physicist Llewellyn H. Thomas explained the missing factor as arising from a Lorentz

1. Møller (1950b). See also Møller (1952), pp. 53-58.

transformation from the frame of reference of the spinning electron against the laboratory system. In his paper of 1950 Møller gave a more general theory of the Thomas precession, which he explained as a purely kinematical effect resulting from two successive Lorentz transformations.

More important than this advanced ecercise in special relativity theory, two years later Møller published on Oxford University Press a comprehensive textbook on relativity theory, special as well as general, which established him as an authority in the field. Nonetheless, for a few more years he continued doing research in quantum field theory and related areas.

By the mid-1950s Møller decided to switch to problems of general relativity, which he did rather abruptly. Although he continued to give lectures and seminars on quantum mechanics, both in Copenhagen and on his many visits abroad, after 1955 all his scientific works were devoted to the general theory of relativity. It thus makes sense to speak of a pre-1955 quantum Møller and a post-1955 relativity Møller. He spent twenty-four years in the world of quantum mechanics and the same number of years in the world of general relativity. While he contributed to the international research literature with 47 publications in the first area, his wrote 35 publications including one monograph in the latter area. A few other physicists switched in the post-World War II era from quantum mechanics to general relativity, witness the examples of Wheeler and Jordan to mention two noteworthy cases. But I know of no other physicist who made the change in research activity as cleanly and consistently as Møller did.

With regard to Møller's publication output it is worth noticing that in the later, relativistic period none of his works were written with co-authors. This stands in contrast to the earlier quantum period 1929-1954 during which he published 14 papers with co-authors, all of whom were or had been associated with the Copenhagen institute (namely: Plesset, Chandrasekhar, Bloch, Arley, Rosenfeld, Rozental, Kristensen, Belinfante). While he had used the *Proceedings of the Royal Danish Academy* as a vehicle of publication also in the earlier period, his preference for this periodical intensified in the later period. About one third of his publications on general relativity appeared in the *Proceedings* of the Danish Academy in the form of articles too lengthy to be published in ordinary physics journals. Another third of his works on general relativity appeared as contributions to international conference proceedings volumes.

Møller's conversion to general relativity coincided with the revival or so-called renaissance of the field, and Møller and his few collaborators in Copenhagen became important parts of the renaissance. For example, in 1957 a small group of specialists gathered in Copenhagen for the first meeting ever on quantum gravity. And this was just the beginning of Møller's engagement in general relativity theory, which was to occupy him for the rest of his life. Copenhagen was also the site of a much larger and very different meeting on gravitation and general relativity which took place in 1971 with Møller as co-organiser and a central figure. This meeting will be described in Section 8.4.

6.1. The renaissance of general relativity

During the early years of Bohr's institute, Kramers and Klein were interested in general relativity, but none of them specialised in the field or induced their students to take it up. Klein's concern was mostly with the five-dimensional extension of relativity on which he wrote important papers in 1926 and 1927. In 1921 Kramers published in the proceedings of the Royal Dutch Academy a paper on the application of Einstein's theory to a stationary gravitational field, after which he turned fully to atomic and quantum theory. Kramers was the first in Denmark to give a university course on relativity, which he first gave in 1920, before the official founding of the Institute for Theoretical Physics. From 1923 to the spring semester of 1926 he lectured on relativity theory (special and general), using chapters in the Austrian physicist Arthur E. Haas' textbook, *Einführung in die theoretische Physik* dating from 1921.[2]

As mentioned in Section 1.1, Møller was originally fascinated by

2. For Kramers' notes to his lectures on relativity theory 1920-1926, see microfilm 25, sections 8-11, in Archive for the History of Quantum Physics, cp. Kuhn et al. (1967), p. 57. Haas' textbook was widely used by non-experts in relativity theory.

Einstein's theory of general relativity but dissuaded by Bohr from making it his future field of research. The quantum theory of atoms and nuclei were far more promising research areas, Bohr assured the young student, who followed the master's advice. In fact, for a period of more than thirty years there was practically no research interest at all in general relativity at the Copenhagen centre of theoretical physics. When Rosenfeld came to the institute in the early 1930s he had recently published a long paper in *Annalen der Physik* on the quantisation of wave fields which would later be recognised as a pioneering work in quantum gravity. However, it was not considered important at the time, not even by its author.[3] Rosenfeld thus brought with him expertise in general relativity, but without it having any impact on the Copenhagen physics environment. On the other hand, in the spring semester of 1939 Møller offered lectures on the general theory of relativity to students at the institute, who appreciated the initiative. As he wrote to Bohr, who at the time stayed in the United States with Rosenfeld: "To entertain the students I give in this semester an additional lecture on general relativity theory; as judged by the number in the audience, they seem to find it enjoyable."[4]

Elsewhere the situation was different, but not markedly so. Even at Princeton, where Einstein resided, relativity theory was considered a somewhat peripheral subject closer to mathematics than to physics. When the young Polish physicist Leopold Infeld arrived in Princeton to work with Einstein, he observed to his dismay that the earlier interest in general relativity had almost completely lapsed. At an international meeting on gravitation theory held in Warsaw in 1964, Infeld recalled:

> The number of physicists working in this field in Princeton could be counted on the fingers of one hand. I remember that very few of us met in the late H. P. Robertson's room and then even those meetings ceased. We, who worked in this field, were looked upon rather askance by other

3. On Rosenfeld's 1930 theory, see Peruzzi and Rocci (2018).
4. Møller to Bohr, 13 March 1939 (BSC).

> physicists. Einstein himself often remarked to me "In Princeton they regard me as an old fool: Sie glauben ich bin ein alter Trottel." This situation remained almost unchanged up to Einstein's death. Relativity theory was not very highly estimated in the 'West' and frowned upon in the 'East'.[5]

The low reputation of fundamental gravitational physics in the sense of general relativity was in a different way experienced by Robert Dicke, eighteen years younger than Infeld:

> As a graduate student of physics 20 years ago [1941] I had been told by my professor, a well-known and outstanding physicist, that I should not trouble to learn General Relativity, Einstein's theory of gravitation. As he put it, gravitation was too weak an interaction to be important inside the atom, the site of the big mysteries. This attitude is still mirrored in our graduate training programme, for few universities have even a single graduate course on General Relativity.[6]

More than a decade later, 22-year-old Stanley Deser, who would devote much of his research career to problems in general relativity theory, faced the same attitude: "On my 1953 arrival as a fresh PhD at the Institute for Advanced Study, its Director, Robert Oppenheimer cautioned me to have nothing to do with Einstein, its most famous denizen, nor with GR [general relativity] in any form, lest I become unsaleable on the job market!"[7]

Historians and physicists agree that from about 1925 to 1955 general relativity was a decidedly unfashionable field compared with branches such as nuclear physics, elementary particles, and solid-state physics. They speak of the 'low-water mark of general

5. Quoted in Eisenstaedt (1989), p. 289.

6. Dicke (1961), p. 797. Dicke completed his graduate work at the University of Rochester.

7. Deser (2021), p. 1.

relativity'.[8] Not only was the annual output of research papers very small, of the order thirty, relativity studies were also considered intellectually inferior to studies of the quantum world. There were several reasons for this state of affairs, the most important being that studies in general relativity seemed to have almost no connection to experiment and appeared irrelevant to most branches of physics and astronomy. It was, or was thought to be, an academic ivory tower activity.

Only in a few cases did general relativity prove useful in other areas of science and then without it was fully recognised by contemporary physicists. For example, in the late 1930s Oppenheimer and collaborators (George Volkoff, Hartland Snyder, Robert Serber) studied by means of general relativity the physics of collapsing stars in works which were later considered very important and even qualifying for a Nobel Prize. At the time, these works made almost no impact at all on specialists in relativity theory and Oppenheimer quickly abandoned his flirt with Einstein's general theory. Relativity textbooks of the 1940s and early 1950s did not refer to Oppenheimer's innovative contribution to relativistic astrophysics. Møller's book of 1952 was no exception.

Cosmology was the only area in which general relativity was successful, but unfortunately cosmology was itself a small research field with a reputation of being speculative or even non-scientific. Leading theoretical cosmologists such as W. de Sitter, R. Tolman, G. Lemaître, A. Eddington, and H. P. Robertson (not to mention Einstein himself) were all experts in general relativity and convinced that only this theory could serve as the foundation for a scientific understanding of the universe in its entirety. To many but not all physicists and astronomers, the discovery of the expanding universe in about 1930 was a result of general relativity theory as well as a confirmation of it. In the words of Tolman, "general relativity pro-

8. The name and idea of a low-water mark in general relativity was introduced in Eisenstaedt (1989). Goenner (2017) questions that there was such an interregnum in research in general relativity and that it was followed by a dramatic renaissance already in the 1950s. See the discussions in Lalli (2017), pp. 7-19, and in Blum, Lalli, and Renn (2016).

vides our present best theory of gravitation – and a very good one at that – and it is my opinion that this is the appropriate theory of gravitation to use in treating the motions of the nebulae."[9] Many agreed, but not all. At about the time that interest in Einstein's theory began to increase, the new steady-state theory of the universe offered an explanation of the cosmic expansion which did not rely of general relativity. The three originators of the steady-state theory, Fred Hoyle, Hermann Bondi, and Thomas Gold, were not opposed to Einstein's theory but they denied that it was applicable to the universe as a whole or that observations of the receding galaxies amounted to crucial support of the theory.

When interest in general relativity was reawakened in the 1950s it was not because of the challenge from the steady-state theory but primarily the result of experimental advances which promised to bring the field within the domain of laboratory physics. In addition, from the late 1950s new discoveries in astronomy stimulated the application of relativity theory to astrophysical problems. During the renaissance era of general relativity, research activity increased markedly and now comprised topics such as gravitational waves, quantum gravity, quasars, and neutron stars, which previously had attracted very little attention. No less importantly, the field became established as a proper scientific discipline based on an international community of scientists with shared research interests. The earlier community consisted of separate groups formed around individuals such as Wheeler and Bergmann in the United States, Bondi in England, Infeld in Poland, Nathan Rosen in Israel, and Møller in Denmark.

One sign of the increased interest was a series of international conferences on problems in general relativity and associated areas, the first one being the 1955 Berne conference celebrating the fiftieth anniversary of Einstein's relativity theory. It was followed by the Chapel Hill conference held at the University of North Carolina (1957), a conference in Royamont, France (1959), one in Warsaw and Jablonna (1962), and several later conferences. It later became customary to designate these conferences as GR0 (Berne),

9. Tolman (1949), p. 377.

GR1 (Chapel Hill), GR2 (Royamont), and so on.[10] Contrary to the Chapel Hill conference, the one in Royamont outside Paris included scientists from both sides of the Iron Curtain. So did the GR3 Warsaw-Jablonna meeting, which was the first large gathering of physicists in Poland after the war. The latter conference, which mostly took place in the small town Jablonna some 20 km from Warsaw, was joined by 114 participants of whom 33 were from the East, with 11 of them from the Soviet Union. The corresponding numbers at GR2 were 119, 8, and 3.[11]

As a result of the Royamont conference, an International Committee on General Relativity and Gravitation (ICGRG) was established with the French scientists André Lichnerowicz and Marie-Antoinette Tonnelat as co-presidents. The sixteen members of the new committee included most of the experts in general relativity, among them Bergmann, Bondi, DeWitt, Fock, Synge, and Wheeler. Dirac was also a committee member and so were Møller and Rosenfeld. The two Copenhagen physicists proposed to limit participation in the ICGRG conferences to a fixed number of invited scientists, an elitist policy which was adopted by the new organisation but eventually turned out to be controversial.[12]

Of course, there were other important meetings on general relativity not belonging to the GRn series. One of them was the 1957 Copenhagen meeting on quantum gravity and another, of a very different kind, was the 1958 Solvay congress on gravitation and cosmology (see sections 6.4 and 7.3, respectively). In December 1963 a large and very important symposium on relativistic astrophysics took place in Dallas, Texas, the first of a series of symposia which continues to this day.[13] Contrary to the series of conferences on gen-

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10. See http://www.isgrg.org/pastconfs.php for a list of the 'GRn' conferences. Møller participated in GR0, GR2, GR3 (Warsaw-Jablonna, 1962), GR4 (London, 1965), GR5 (Tbilisi, 1968), GR6 (Copenhagen, 1971), and GR7 (Tel-Aviv 1974).

11. See Infeld (1964) and Demianski (2014) for the Warsaw-Jablonna conference.

12. Lalli (2017), pp. 59, 87 and 104.

13. Mercier reported on the Dallas symposium in *Bulletin on General Relativity and Gravitation* 5 (1964): 2-6. See also Schücking (1989). In December 2021 the 31st Texas Symposium on Relativistic Astrophysics was held – not in Texas but in Prague, the Czech Republic.

eral relativity and gravitation, where participation was by invitation only, the symposia on relativistic astrophysics were open to everyone wishing to participate. It quickly turned out that the community of relativistic astrophysicists was quite different from that of the relativists organised within the ICGRG. Tellingly, only a couple of ICGRG members participated in the first Texas symposium, Møller not being among the few. To him and many other European experts in relativity theory, the focus of the Texas symposium was too much on astrophysics and too little on the theory of general relativity. Some of the leading European relativists tended to see the Texas symposium as a kind of supplement to the conferences on general relativity arranged by ICGRG, but this was not at all how the organisers of and participants in the Dallas conference looked upon it.[14]

The key figure in ICGRG was its secretary, the Swiss physicist André Mercier, who was also the chief organiser of the earlier Berne conference. One of the first results of ICGRG was a new publication called the *Bulletin of General Relativity and Gravitation*, which was a kind of newsletter rather than a journal consisting of research papers. The first issue of *Bulletin* that appeared in 1962 included a list of 223 scientists who were actively working on, or just interested in, general relativity along with their affiliations and research interests. Møller stated his interests as "generalities, fundamental, etc.; conservation laws, energy-momentum tensor, etc.; book on GR; various problems of classical quantum theory; nuclear physics; quantum field theory, etc."[15]

Eight years later *Bulletin* was incorporated into a new scientific journal titled *General Relativity and Gravitation*. Many but far from all specialists in general relativity theory published some of their works in this journal (Møller never did). In a spirited editorial in the first issue of the journal, its editor Mercier called attention to "the extraordinary and very satisfactory combination of astrophys-

14. The tensions between the two communities of relativists, one focused on applied general relativity and the other more on pure gravitation theory, are described in Lalli (2017), pp. 61-65.

15. *Bulletin of General Relativity and Gravitation* **1** (1962): 3-38.

Fünfzig Jahre Relativitätstheorie
Cinquantenaire de la Théorie de la Relativité
Jubilee of Relativity Theory

Bern, 11.—16. Juli 1955

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HELVETICA PHYSICA ACTA
SUPPLEMENTUM IV

BIRKHÄUSER VERLAG BASEL
1956

Fig. 29. Front page of the proceedings volume of the 1955 Berne conference, an important document in the renaissance of general relativity theory.

ics and GRG [general relativity and gravitation] which has arisen throughout the years." Concerning this combination, he wrote:

> We can, for sure, consider astrophysics as one of the main fields of GRG. Cosmology, by the way, is either part of it, or astrophysics is part of cosmology, as you please. In any case, GRG has definitely saved cosmology from the 'too-hypothetical', and astrophysics combined with particle physics has made GRG theories very concrete, whereas, during one or two decades, they had remained despised by so many physicists on the pretext that they were unphysical.[16]

The Berne conference was essentially the brainchild of Mercier, who in the preparatory stage sought to engage also Einstein and Bohr in the grandly conceived project. While both declined (and Einstein passed away before the conference), Pauli wholeheartedly supported Mercier's plan and accepted to become president of the organising committee. In a letter of 1 March, Pauli requested Møller to tell Bohr that

> ... two Russians will come to the Relativity Congress in Bern: *B. A. Fock* and *A. D. Alexandrow*. I doubt whether the first is *the* famous Fock (whose initial is V.), regarding the *second* he is *not* identical with the mathematician (topologist), but I know some of his publications on quantum theory (somewhat 'philosophical' in a bad sense, but not in such a degree, as was the case with Blochinzew). ... With all good wishes to yourself and to the whole Blegdamsvej-family (including mother Hellmann).[17]

Pauli's reference to "mother Hellmann" was to Sophie Hellmann (1894-1979), a Jewish refugee who came to the institute in 1935, where she worked as Bohr's personal secretary until his death.

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16. Editorial, *General Relativity and Gravitation* **1** (1970): 1-7, p. 4.

17. Pauli to Møller, 1 March 1955, in Pauli (2005), p. 133. Aleksandr D. Aleksandrov (1912-1999) was a mathematician and physicist with an interest in relativity theory. Dmitri I. Blokhintsev (1908-1979) was a nuclear physicist and the author of texts on quantum mechanics and its interpretation according to Marxism-Leninism. See Graham (1966), pp. 81-93, 122-127.

Vladimir Fock did indeed attend the Berne conference as official delegate of the USSR Academy of Sciences (his first name starts with a 'B' in Cyrillic, which transliterates to 'V' in the Latin alphabet). This was the first time he was allowed to travel outside the Soviet dominated parts of Europe after the war. Not only did Mercier want Bohr to be a part of the conference, he was also eager to enlist Møller as a speaker. At a stage of the preparation when he still thought he might persuade Bohr, he wrote:

> Could you discuss with Møller the question of participation? Unfortunately he sais [*sic*] that it would be very difficult for him to come at the proposed date. Some colleagues have asked for a postponement of the conference, therefore it is possible that we decide to have it a couple of weeks later. I don't doubt that that would also suit you. But in case we must leave it as announced, could not you help Møller to come? Of course you will be our guest at Berne and we shall be glad to refund your travel-expenses.[18]

Møller did go to Berne, where he gave a talk on the behaviour of clocks according to the general theory of relativity. He also delivered a brief address from the absent Bohr in the latter's capacity of president of the Royal Danish Academy of Sciences and Letters.

With about 90 attendees and a large number of presentations – some in English, some in French, and others again in German – the Berne conference held 11-16 July was a success. As the leading relativist Peter Bergmann summarised, "The conference had been the common meeting ground of workers from the four corners of the earth. ... Einstein would certainly have enjoyed the scientific discussion of his principal field of work, but he would have considered equally important the fact that scientists from all countries could get together and in a spirit of common endeavor help each other with their problems."[19] In his foreword to the proceedings volume published in 1956, Mercier commented on relativity physics as compared to the hugely more popular quantum physics of atoms

18. Mercier to Bohr, 17 September 1954 (BSC, Supplement).
19. Bergmann (1956), p. 494.

and nuclei: "The theory of relativity ... is the achievement of a physics of Cartesian spirit that gives an account of the phenomena by figures and motions. ... One hears today the saying that we live in the atomic era. Should we not also speak of the relativistic era?"[20]

Several of the many speakers in Berne, among them H. P. Robertson, Fred Hoyle, and Otto Heckmann, addressed questions of cosmology whereas experimental relativity, gravitational waves, and quantum gravity received but scant attention. In his detailed review of relativistic cosmology, Robertson noted that the accepted value of the Hubble parameter, which he took to be $H_0 = 180\ \mathrm{km/s/Mpc}$corresponding to a Hubble time equal to $T_0 = 1/H_0 = 5.4 \times 10^9$ years, was still in conflict with the age of the oldest stars. This made him to reconsider a positive cosmological constant Λ, whereas generally this constant was assumed to be zero. Cosmology was also the subject of Jordan's presentation but in the version of his scalar-tensor theory with a varying gravitational constant which most experts received with scepticism or indifference.

Einstein's past collaborator Nathan Rosen presented a short communication in which he concluded that energy-transmitting gravitational waves cannot exist, but other participants disagreed. Bondi recalled that his interest in gravitational waves, a subject he would soon specialise in, was sparked by the Berne conference:

> It was a most excellent meeting. But perhaps it was particularly memorable for me because of discussions we had at that meeting on gravitational waves. The mathematical and physical complexity of Einstein's theory of gravitation is so great that there was still confusion, and a variety of opinions, about whether the theory predicted the existence of gravitational waves or not.[21]

Quantum gravity was touched upon by Oskar Klein, who spoke about his recent ideas of a five-dimensional generalisation of Einstein's theory and its possible relevance for the physics of mesons

20. Mercier and Kervaire (1956), p. 19. The volume, identical to a special issue of *Helvetica Physica Acta*, is available online through the website http://www.e-periodica.ch.
21. Bondi (1990), p. 79.

and atomic nuclei.[22] Klein had conversations with a young American attendee, Stanley Deser, who would soon become his son-in-law. The conference, Deser recalled forty years later, "was held in the Natural History Museum, access to whose auditorium required filing past cases full of stuffed primates. The front row seated almost all of the old-line GR researchers ... for example, Born, Fock, [Adriaan] Fokker, Jordan, [Cornelius] Lanczos, Pauli, von Laue and Weyl."[23]

More important from a scientific point of view, and more oriented towards recent work in general relativity, was the smaller, less formal, and highly successful American-dominated Chapel Hill Conference on The Role of Gravitation in Physics held at the University of North Carolina from 18 to 23 January 1957. This GR1 conference was chiefly organised by Bryce DeWitt and his wife Cécile DeWitt-Morette with strong support from Wheeler.[24] Participants included Wheeler, Bergmann, Bondi, Klein, Dicke, Belinfante, Rosenfeld, Feynman, and others. Rosenfeld had originally been denied a visa to the conference, undoubtedly because of his Marxist persuasion, but after Cécile complained about the decision, it was reverted. While four of the Chapel Hill sessions were devoted to various aspects of classical or 'unquantized general relativity', there were also sessions on 'quantized general relativity' which subject was discussed in particular by Bergmann, DeWitt, Deser, and Feynman. As noted by Dean Rickles, a historian and philosopher of science, "The Chapel Hill conference was a genuine break from the Berne conference, both in terms of its organization, its content, but more so its *spirit*."[25] It also differed from the Berne conference by all the presentations and discussions being in English.

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22. Bergmann (1956). Kiefer (2020).
23. Deser (1995), p. 50.
24. The official proceedings of the Chapel Hill conference were published in *Reviews of Modern Physics* **29** (1957): 351-546. For a summary account of the conference, see Bergmann (1957). DeWitt-Morette and Rickles (2011) includes the reports and discussions of the conference based on an internally circulated report.
25. DeWitt-Morette and Rickles (2011), p. 20.

6.2. The clock paradox

While immersed in the complex calculations of the meson theory of nuclear forces but before knowing of Heisenberg's work on the *S*-matrix, in 1943 Møller published his first research paper on general relativity. He dealt with a classical and already much discussed problem of relativity theory, the so-called clock paradox going back to Einstein's famous *Annalen* paper of 1905 in which he introduced the special theory of relativity. Møller commented: "I wrote a paper on general relativity, and the clock paradox, which foolishly enough takes up so much paper still in the journals. I mean, it's a rather trivial problem, but it seems that every generation has to tackle this problem anew."[26] Einstein originally imagined two identical and coincident idealised clocks A and B. If B moves with constant velocity v along a closed curve, when it returns to A after a time t as measured by the A clock, according to B the journey will have lasted only $\sqrt{1-v^2/c^2}\,t$ seconds. To A, the B clock will thus appear to run slower by the period of time given by $\left(1-\sqrt{1-v^2/c^2}\right)t$ or approximately $2(v/c)t$.

The consequences of this remarkable phenomenon, based entirely on the time dilation formula of special relativity theory, was first spelled out in qualitative terms in an address of 1911 given by the French physicist Paul Langevin to the International Congress of Philosophy held in Bologna.[27] Eleven years later, the famous mathematician and physicist Hermann Weyl replaced the A and B clocks with two twins, or twin-brothers to be precise.[28] In the later very extensive literature on the subject, the thought experiment is often referred to as the 'twin paradox' instead of the 'clock paradox', but the first name became commonly used only after about 1970

26. Weiner (1971c).
27. Langevin (1911), who based his address on Lorentz's electromagnetic theory rather than Einstein's relativity theory. Benguigui (2012) contains a discussion of Langevin's account, which was the first to illustrate the clock paradox by means of a space traveller.
28. Weyl (1922), p. 187: "Suppose we have two twin-brothers who take leave from one another at a world-point A, and suppose that one remains at home…"

and Møller never used it. As most physicists agreed and still agree, although the behaviour of the clocks, or the different ages of the twins, may appear contrary to common sense, the phenomenon is in no way paradoxical. The situation only leads to a logical contradiction if the same shortening of time applies to the A clock as viewed from the system of B. Nonetheless, it may seem to violate the very spirit of relativity. From the point of view of A, he or she is at rest and B first travels away and then returns younger than himself; but from the point of view of B, it is A who recedes and later returns – younger than B. How can B *really* be younger than A or, for that matter, how can A *really* be younger than B?

Since the clock or twin must at some point accelerate in a closed loop, it would seem natural to appeal to the general theory of relativity, such as did the American physicist Richard Tolman in his pioneering textbook of 1934 on relativity, thermodynamics and cosmology. However, Tolman's investigation was admittedly incomplete and restricted to the case of small velocities. "The treatment of the problem without approximation would involve the full apparatus of the general theory of relativity", he wrote.[29] It was such a treatment that Møller offered in his 1943 paper, which referred to the earlier works of Einstein, Langevin, Tolman, and others. The new analysis provided what he somewhat immodestly called "a complete solution to the clock paradox."[30] Indeed, by clearly distinguishing between the effects of special and general relativity and by taking full account of the latter he showed that the acceleration of a clock relative to an inertial system has no influence on its rate.

In relativistic kinematics the velocity of a uniformly accelerated body will approach the velocity of light asymptotically and the associated world line be a hyperbola. To describe such hyperbolic motion Møller derived a formula connecting the coordinates of a frame of reference with the coordinates of another frame uniformly accelerated relative to the first one. With the direction of the acceler-

29. Tolman (1934), p. 197.

30. Møller (1943b), p. 6. In Møller (1952), p. 258, he repeated the claim of a complete solution or what he elsewhere called a "final solution."

ation g taken to be along the x-axis, he found that the corresponding metric could be written as

$$ds^2 = -(1 + gx/c^2)^2 dt^2 + dx^2 + dy^2 + dz^2$$

The hyperbolic coordinates for this kind of motion are known as either Møller or Kottler-Møller coordinates, where the first name refers to the Austrian physicist Friedrich Kottler who studied the problem in the 1910s.[31]

Published in a not widely circulated periodical during the height of the war Møller's memoir was not much noticed until he summarised the main content of it in his textbook nine years later.[32] Then it attracted a good deal of attention, for example by Carlo Giannoni, an American philosopher of science, who carefully discussed the clock paradox and clarified some of the points in Møller's analysis of it.[33] By using natural clocks rather than coordinate clocks, Giannoni showed that the sudden increase in time on the return trip, as it appeared in Møller's treatment, could be avoided. Møller returned to the clock paradox in two papers of 1955 of which one was a detailed investigation dedicated to Bohr on the occasion of his seventieth birthday printed just four days before the birthday on 7 October. It appeared in a commemorative volume published by the Royal Danish Academy which also included articles by Born, Gamow, Hevesy, and others. The other and shorter paper was the address delivered to the Berne conference in July 1955 commemorating the fiftieth anniversary of the special theory of relativity.

Møller investigated in great detail the rate of a moving clock placed in a gravitational field. For an ideal standard clock he based his analysis on the general formula giving the proper time τ for a particle moving with velocity v in a gravitational potential Φ. With

31. Møller (1943b), eq. 17, see also Møller (1952), pp. 75, 255-258. For the history of coordinate transformations describing hyperbolic motion in the theory of relativity, see https://en.wikipedia.org/wiki/Rindler_coordinates.

32. Møller (1952), pp. 48-51, 258-263. Møller (1972), pp. 47-49, 292-298.

33. Giannoni (1974). Møller reviewed the paper and suggested some changes, after which he recommended it for publication in *American Journal of Physics*.

t denoting the time in the moving system the formula connecting the two time parameters was given as

$$d\tau = dt\sqrt{1 + \frac{2\Phi}{c^2} - \frac{v^2}{c^2}}$$

For $v = 0$ the formula gives Einstein's expression for the gravitational redshift,

$$d\tau = dt\sqrt{1 + 2\Phi/c^2}$$

or, if expanded to the first order of the small quantity Φ/c^2, the frequency formula

$$\Delta\nu/\nu = \Delta\Phi/c^2$$

Møller concluded that the acceleration of an ideal clock has no influence on the rate of the clock and that "no real contradictions connected with the rate of moving clocks can ever arise in this theory [of general relativity]."[34] For this reason, he preferred to speak of 'the so-called clock paradox'. Moreover, Møller found it interesting to investigate the conditions under which a real clock satisfies the above formula for an ideal clock. This was not only of 'didactical interest' but also and more importantly a question related to the newly constructed atomic clocks based on the vibrations of ammonia molecules and other molecular or atomic systems. As he put it in his Berne address:

> [The] question is also of a more practical interest in view of the fact that he construction of accurate time measuring instruments in recent years has made such progress that a direct verification of [the formula] with $v = 0$ by terrestrial experiments is in sight. The 'atomic clocks' constructed so far, in which atomic systems like ammonia molecules act as the balance of the clock, have already an accuracy of the order

34. Møller (1955a), p. 3. Møller also discussed the behaviour of a clock in a gravitational field at the 1957 quantum gravity meeting held in Copenhagen. On this occasion he and others of the participants considered how quantum theory might enter the problem, but Møller suggested that for the time being it was better to ignore quantum effects. Blum and Hartz (2017), p. 127.

> 3×10^{-10}, while the relative difference in rate, according to [the formula], of two clocks at rest at suitable places on the earth may be of the order 10^{-12}.[35]

He concluded that even the primitive atomic clocks constructed at the time were ideal to a high degree of accuracy and that more advanced atomic clocks would most likely be able to measure variations in the gravitational field of the Earth. But all this was theoretical and in 1955 Møller did not and could not refer to any real experiments from the new field of atomic clocks research.

Among the earliest and most notable laboratory experiments on general relativity was one made in 1960 by Robert Pound and Glen Rebka at Harvard University. The two physicists measured the 'Apparent Weight of Photons' (as their paper was titled), which in their case meant the gravitational redshift of 14.4 keV gamma photons emitted by a Fe-57 isotope. To measure the tiny shift in frequency caused by the variation of the gravitational field of the Earth over merely 22.5 meters – the height of the Harvard laboratory building – Pound and Rebka made sophisticated use of the new Mössbauer effect discovered two years earlier by the German physicist Rudolf Mössbauer. The Harvard experiment resulted in a frequency shift $\Delta\nu_{\text{exp}} = (1.05 \pm 0.10)\Delta\nu_{\text{theory}}$ and thus confirmed the prediction of general relativity within 10%. Improved experiments soon resulted in a much better agreement.[36]

The importance of the Pound-Rebka experiment was not only that it provided additional support for Einstein's theory, but also that it ushered in a new era of experimental relativity. "These are exciting days", wrote Alfred Schild, an American mathematical physicist. "Einstein's theory of gravitation ... is moving from the realm of mathematics to that of physics. After 40 years of sparse meagre

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35. Møller (1956), p. 54. Møller (1955a), p. 5. In his treatment of the clock paradox in Møller (1952) there was no mention of atomic clocks, an instrument technology which was then still in its infancy.

36. For the measurements and their bearing on Mössbauer's discovery, see Hentschel (1996).

astronomical checks, new terrestrial experiments are possible and are being planned."[37]

As far as Møller was concerned, there was nothing more to say about the clock thought experiment or so-called paradox, which was now fully understood. End of story, he may have thought, but if so he was mistaken. During the following years the subject gave rise to an extensive controversy primarily taking place in the pages of *Nature*, *Science*, and *American Journal of Physics*. For example, in a 1958 paper in the latter journal two American physicists, Charles B. Leffert and Thomas M. Donahue, argued that Møller's equations as given in his textbook implied a new paradox not recognised by Møller or other physicists. The new paradox allegedly turned up when one of the clocks (or twins) moved in an abruptly changed gravitational field with the consequence of an acceleration in the opposite direction of the motion. Leffert and Donahue suggested that the behaviour of the clock corresponded to the nonsensical notion of causality violation with time running backwards. In a careful response to the paper, Møller demonstrated that the alleged new paradox was an artefact due to the use of a four-dimensional form of the equations of motion. If treated by means of the three-dimensional formulation, the paradox disappeared.[38]

The central figure in the early controversy over the clock paradox was the British astrophysicist and philosopher of science Herbert Dingle, who in 1956 argued that relativity theory leads inevitably to symmetric ageing and not, as generally supposed, to asymmetric ageing. "When the observers meet again … their clocks agree", he asserted.[39] A seasoned polemicist, Dingle criticised in strong words the majority of physicists who disagreed with him. As a result, a minor flood of letters and papers on the clock or twin paradox followed. Some of them were written by Dingle, others by Hermann Bondi, Charles G. Darwin, William McCrea, and Siegfried Singer,

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37. Schild (1960), p. 778.
38. Leffert and Donahue (1958). Møller (1959a). Møller (1972), pp. 296, 383-384, summarised his solution to the Leffert-Donahue clock paradox.
39. Dingle (1956). For a full discussion of Dingle's unorthodox view, see Chang (1993).

to mention but a few. Møller, who generally disliked controversies and polemics, did not take part in the debate except that his writings on the clock paradox referred to several of the contributions. At least initially, Dingle seems to have been unaware of Møller's 'complete' solution to the clock paradox.

Møller participated in a congress held in Turin 6-11 September 1956 on the occasion of the hundredth anniversary of the death of the great chemist and natural philosopher Amedeo Avogadro, the father or rather grandfather of the constant named after him, $N_A = 6 \times 10^{23}$. (Avogadro proposed in 1811 that equal volumes of different gases contain the same number of particles, but he was utterly unaware of the number, which was only estimated at the turn of the century.) Focusing on the fundamental constants of physics, the congress involved experimental as well as theoretical problems. Among the theorists convening in Turin were Dirac and Klein, the first speaking on 'The Vacuum of Quantum Electrodynamics' and the second on 'Problems Related to the Small and Big Numbers of Physics'.

In his talk given in Turin Møller returned in a more concrete way to the problem of using atomic clocks in experimental tests of general relativity. He was now aware of the new maser technology about which he had been informed in conversations with Charles Townes, a physicist at Columbia University and the chief inventor of the maser.[40] The device – maser is an acronym for *m*icrowave *a*mplification by *s*timulated *e*mission of *r*adiation – grew out of Townes' idea of using stimulated emission in a beam of ammonia molecules as a source for strongly amplified microwaves. The first practical maser of 1955 used ammonia molecules sent through an electrical filter system in which molecules in the excited state were separated from those in the lower state. The beam of excited molecules then entered a cavity where they created an oscillating electromagnetic

40. "I am grateful to Professor Townes for stimulating discussions on problems of general relativity in connection with the maser." Møller (1957), p. 398. Townes to Møller, 11 July 1957 (CMP). Over the years 1956-1959 Townes and Møller maintained a correspondence concerning the use of maser experiments in tests of the theory of relativity.

field emitted as output waves.[41] It appeared to Møller and a few others that the new maser instrument might be useful in testing the predictions of both special and general relativity, an idea which appealed to Townes.

Møller calculated from general relativity that the ratio w_2/w_1 of the rates of two identical masers at places with different gravitational potentials Φ_2 and Φ_1 was given by

$$\frac{w_2}{w_1} = \frac{\sqrt{1 + 2\Phi_2/c^2}}{\sqrt{1 + 2\Phi_1/c^2}} \cong 1 + \frac{\Phi_2 - \Phi_1}{c^2}$$

As an example he considered one maser at sea level and the other placed at a mountain top of height 3 km. The difference in rate then comes out as

$$\Delta = \frac{w_2 - w_1}{w_1} = \frac{\Phi_2 - \Phi_1}{c^2} \cong \frac{1}{3} \times 10^{-12}$$

Concerning this small effect, Møller noted that it was "at the edge of what can be observed with the present accuracy of available instruments, and we cannot gain much by climbing higher mountains." As an exciting alternative, although one that appeared "somewhat fantastic at the moment", he investigated the possibility of placing one of the masers in an artificial satellite. After the Soviet satellite named Sputnik 1 had been launched in orbit on 4 October 1957, the possibility seemed much less fantastic. For a satellite moving in an elliptic orbit with the largest distance r_2 and the smallest r_1, Møller found

$$\Delta = 0.7 \times 10^{-7} \left(1 - \frac{3R}{r_1 + r_2}\right)$$

41. See Forman (1992) for details on the invention of the maser. Townes was awarded the 1964 Nobel Prize for his work on the maser and its subsequent development into the laser.

where R is the radius of the Earth. He recommended a highly eccentric orbit and suggested that the communication signal could be sent each time the satellite came closest to the Earth at the apogee. At about the same time as Møller, Siegfried Singer at the University of Maryland proposed satellite experiments as tests of general relativity, and he too referred to atomic clocks (but not masers) as suitable measuring instruments.[42] Singer's paper was published in August 1956 and Møller had seen his manuscript prior to publication.

In his 1956 address Møller pointed out that maser measurements might be used to discriminate between the 'absolute ether theory' and the theory of special relativity. With the first term he referred to Lorentz's electron theory in which the ether is not dragged at all by refractive substances, but stays constantly at rest in an absolute system of inertia. His idea was to use two masers with opposite directions of the molecular beams and then observe if the relative rate of the two masers varied with the rotation of the Earth. Given that the classical ether theory had long been abandoned, Møller did not find an experiment of this kind to be very interesting except that it would constitute a further test of the relativity principle. Three years later Townes and his collaborator John Cedarholm reported such an experiment and unsurprisingly with the expected result of no ether wind or, to be precise, an ether wind of velocity smaller than 30 m/s.[43] The two authors acknowledged Møller's calculations as an inspiration for their work.

Møller took up the same question, experimental tests of special relativity, at a one-day Royal Society symposium on 22 February 1962 dealing with the state of relativity theory.[44] Since the famous interferometric experiment of Albert Michelson and Edward Morley in 1887, numerous attempts had been made to sharpen the limit of a hypothetical ether drift. Although the many experiments could

42. Singer (1956).

43. Cedarholm and Townes (1959).

44. Møller (1962a). 'A Discussion on the Present State of Relativity'. *Proceedings of the Royal Society A* **270** (1962): 297-356.

not prove the ether wind to be zero, they established a still smaller upper bound in agreement with the relativity principle.[45] Møller discussed in his talk the most recent experiments based on methods similar to those of the Pound-Rebka experiment which yielded an upper limit of the ether wind of only a few metres per second. He considered the new experiments to be an accurate test of the special principle of relativity, but of course he did not believe that such tests were really needed. Møller's talk attracted public attention when it appeared in the American science writer Martin Gardner's best-selling *Relativity for the Million*, a brilliant exposition of relativity theory for lay readers:

> In February 1962, at a meeting of the Royal Society of London, Professor Christian Møller of the University of Copenhagen explained how such an experiment could easily be performed by using the Mössbauer effect as a source of electromagnetic radiation, mounting the source and receiver at opposite ends of a table that could be rotated. Such an experiment, Møller pointed out, would falsify the original contraction theory.[46]

At the London symposium Møller met with several other specialists in relativity theory, among them J. Synge, P. Dirac, H. Bondi, D. Sciama, A. Trautman, F. Hoyle, and J. Narlikar. Also David Bohm and the ever-critical Herbert Dingle were present. Hoyle and his collaborator Jayant Narlikar gave an address on Mach's principle and matter creation in steady-state cosmology, whereas Dirac spoke on his recent idea of replacing the point electron with an extended object which he likened to a bubble in the electromagnetic field. He likewise considered bubble-particles in the gravitational field. In the discussion following Dirac's talk, Møller objected that the spin of the electron could not be obtained from the extended model,

45. Haugan and Will (1987).
46. Gardner (1962), pp. 28-29. Gardner, with whom Møller corresponded, was not only a prominent popular science writer but also a sharp critic of all forms of pseudoscience, which he unmasked in an influential book of 1957 with the title *Fads and Fallacies in the Name of Science*.

to which Dirac evasively answered that he hoped to do so in some future generalisation of the model.[47]

In 1970 Møller was awarded the prestigious H. C. Ørsted medal from the Society for the Dissemination of Natural Science established in 1824. On this occasion he emphasised that new experiments had proved beyond any doubt that "the general theory of relativity is not just mathematics, as some physicists have tended to believe for half a century. It is a physical theory reflecting how nature really is, and I take this to justify that for the last fifteen years I have almost exclusively occupied myself with problems in this area."[48]

Møller was particularly impressed by the new experiments of Irwin Shapiro and collaborators using radar-echo time delays to determine the precise distances to Mercury and Venus. The idea was to send radar signals to Venus and Mercury and to measure the delay time of the echoes for different constellations of the planets. When the signal passes close to the Sun, general relativity predicts an excess delay of the echoes due to the Sun's gravitational field. The result for Mercury is $\Delta t = 1.6 \times 10^{-4}$ s and only half this value if the curvature of space caused by the Sun is neglected. Thus, by measuring the delay time it is possible to decide whether or not the Sun curves the space around it in accordance with general relativity. Møller said about Shapiro's method that it "has verified the dependence of the velocity of light on the gravitational potential and also the non-Euclidean structure of space in the vicinity of the Sun." Shapiro had predicted the time delay effect in 1964 and by 1970 data from this fourth test of general relativity, as it is often known, were in close agreement with those derived from Einstein's theory.[49] The data collected by Shapiro and his group also resulted in a tight bound on the time-variation of the gravitational constant, namely $dG/Gdt < 1.5 \times 10^{-10}$ per year, which ruled out the $G(t)$ hypotheses proposed by Dirac and Jordan according to which $G \sim t^{-1}$.

47. Dirac (1962). Half a year later Dirac defended his model of finite-size electromagnetic and gravitational particles (gravitons) at the GR3 conference in Warsaw.
48. Møller (1970), p. 66.
49. Shapiro et al. (1968). Møller (1972), pp. 501-504.

Møller continued to be concerned with the behaviour of clocks according to relativity theory. In one of his last papers on the subject, a contribution to an international meeting in 1975 on cosmology and gravitation held in Erice, Sicily, he focused on time measurements in very strong gravitational fields. We shall deal with this paper in Section 7.3.

6.3. A classic textbook

Numerous physics students (one of them being the author of this book) have learned relativity theory from Møller's textbook with the straightforward title *The Theory of Relativity*, a comprehensive work published in 1952 with a second and much enlarged edition of 1972. Although the book was not the first to treat both the special and the general theory, it was one of the first and arguably the most important for a period of about two decades.

Still during the 1940s most relativity courses were limited to the special theory of relativity, and there were only few books dealing also with the general theory. Moreover, few of these books were designed as textbooks for students and actually used as such. One of them was *Introduction to the Theory of Relativity* from 1942 written by Peter Bergmann, the German-American theoretical physicist and collaborator of Einstein. Bergmann's work came out in a fourth printing in 1948. Other choices for courses in relativity could be Tolman's *Relativity, Thermodynamics and Cosmology* from 1934, Landau and Lifshitz' *The Classical Theory of Fields* from 1951, or Weyl's *Space-Time-Matter* from 1952, a translation of a work originally published in German in 1921. Yet another early classic in German, Pauli's authoritative *Relativitätstheorie* from 1921, was translated into English only in 1958.

The Theory of Relativity was an extended version of the lecture course that Møller had given at the Copenhagen institute for theoretical physics for about two decades. He had apparently been approached by Oxford University Press in 1945 or 1946, for in November 1946 he was fully occupied with writing the book on "old-fashioned relativity", as he told Pauli (Section 5.3). Two years later he still worked hard on the project. "My book on relativity is now progressing with increasing speed", he reported to Belinfante, "and

I still believe that the manuscript will be ready by the end of the year."[50] It took a little longer, but in a paper of 1950, he announced that now the book was "in the press."[51] For some reason the publication was delayed to 1952, the preface being dated November 1951.

Møller's textbook appeared in a prestigious series published by the Oxford University Press called the International Series of Monographs of Physics which was originally established in 1930 with Ralph Fowler and Peter Kapitsa as general editors. The first and most successful volume in the new book series was Dirac's *Principles of Quantum Mechanics* (1930), which was followed by other classics such as Gamow's *Constitution of Atomic Nuclei and Radioactivity* (1931), Van Vleck's *Theory of Electric and Magnetic Susceptibilities* (1932), and Tolman's *Relativity, Thermodynamics and Cosmology* (1934). At about 1950, no book in the series had dealt with relativity theory since the one published by Tolman.[52] Admittedly, in 1948 E. Arthur Milne's *Kinematic Relativity* was published in the same series, but this work was highly unconventional and not dealing with relativity theory as ordinarily understood. Thus, Milne denied the central message of general relativity that space or space-time is a deformable entity subject to the action of gravitating mass and energy. According to him, space was nothing but an abstract system of reference and as such could have no structure, curved or not.

It is a little surprising that Oxford University Press commissioned Møller to write the book, given that at the time he was known mainly from his works on theoretical quantum physics and not on relativity theory. In fact, he had only written two research papers dealing with problems related to general relativity and none of them were well known (the 1941 paper on meson theory in de Sitter space and the 1943 paper on the clock paradox). There were several specialists in the field, such as John Synge and William McCrea, and Møller was still not recognised to be among them.

50. Møller to Belinfante, 11 November 1949 (CMP).

51. Møller (1950b), note 2.

52. In 1952, the general editors of the book series were Nevill F. Mott, Edward C. Bullard, and Denys H. Wilkinson. The book series continues to this day when it comprises about 150 monographs.

Nonetheless, it was he who was asked to write the book, a decision which Oxford University Press probably did not regret.

As Møller pointed out in the preface to his 386-page book, it was limited to classical Einsteinian relativity and included "only those developments of the theory of relativity which can be regarded as safely established."[53] He thus completely ignored alternative relativity theories such as those proposed by Milne and Alfred North Whitehead, and also the modifications based on the hypothesis of gravity varying in cosmic time as developed by Paul Dirac, Pascual Jordan, and a few others. Moreover, he disregarded unified electro-gravitational theories and attempts of merging quantum mechanics and general relativity. Møller admitted that his decision to disregard the quantum world "might be considered a serious defect of the book", but he found it justified from a didactical point of view. To this he added the argument that "at present, a complete self-consistent relativistic quantum theory does not exist." Moreover, classical relativity theory could easily stand on its own legs. After all, this theory "is one of the most fascinating and beautiful parts of theoretical physics on account of its inner consistency and the simplicity and generality of its basic assumptions." In this methodological regard Møller considered the theory of relativity to be superior even to quantum mechanics.

In the first chapter of the book Møller covered the historical development leading up to Einstein's special theory, including the classical experiments of Hippolyte Fizeau and Albert Michelson and also if only briefly the pre-relativity theories of Lorentz, Poincaré and others. He considered the historical introduction to be important, "since a real understanding of a physical theory is possible only through an intimate knowledge of its predecessors." As Møller pointed out, the many ether-drift experiments of the past had solidly confirmed Einstein's principle of relativity. On the other hand, in a footnote he referred to the American physicist Dayton Clarence Miller, who from a series of precise experiments made through the 1920s concluded that there was a non-zero ether drift contradicting

53. If not otherwise mentioned, the quotations in this section are from Møller (1952), pp. v-vii.

special relativity theory. It was in this connection that Einstein in 1921 famously commented: "Raffiniert ist der Herr Gott, aber boshaft ist er nicht" (Subtle is the Lord, but malicious He is not).[54]

By the early 1950s the anomalous results found by Miller had not yet been satisfactorily explained and yet the large majority of physicists chose to disregard them. Møller was one of them. In a letter of 1949 to Belinfante, he wrote, "About Miller's experiments I say in my book that they are probably wrong. Is this not your opinion?"[55] When the book was published three years later, it merely stated that contrary to other experiments "Miller obtained a small effect." Only in 1955 did Robert Shankland and collaborators finally establish a plausible reason for the 'Miller effect', namely that Miller had not correctly taken into account the thermal effects causing unequal expansion of the rods in his interferometer.[56]

Møller was an avid reader of the scientific literature, not only of the recent papers relating to his own fields of research but also of old and mostly forgotten papers on theoretical physics. In his unpublished gold medal essay of 1929 on the optical-mechanical analogy, he carefully studied the old papers of W. R. Hamilton and F. Klein (Section 1.2). Much later, when dealing with relativistic thermodynamics, he similarly scrutinised the works of M. Planck, F. Hasenöhrl, and A. Einstein from the first decade of the twentieth century (see Section 7.2). Møller seems to have enjoyed reading critically these works of the past and if possibly relating them to problems of modern physics. After all, he believed that "a real understanding of a physical theory is possible only through an intimate knowledge of its predecessors." Yet another example of this quasi-historical approach is provided by the sections in his textbook dealing with the momentum of light in a refractive medium and the associated energy-momentum tensor.[57]

54. Lalli (2012), p. 165.

55. Møller to Belinfante, 11 November 1949 (CMP).

56. See Lalli (2012). Although by 1972 the Miller effect had been fully explained and thus was no longer anomalous, in the second edition of his textbook Møller repeated his footnote from the 1952 edition.

57. Møller (1952), pp. 202-211.

In 1909 Max Abraham and Hermann Minkowski published their contrasting views on this question, and since then it had given rise to a long discussion in the literature with most physicists in favour of Abraham's theory based on a symmetric energy-momentum tensor. After having studied the extensive literature including the original papers of Abraham and Minkowski, Møller suggested that the latter's expression for the tensor was 'more natural' and probably preferable to that of Abraham. In about 1970 also Peierls got interested in the question, which he discussed with Møller:

> I would very much have liked your opinion [about] the old question of the momentum of a light wave in a refracting medium, of which a student of mine has done some work ... In your book on relativity the question appears to be left open whether the answer given by Minkowski or that by Abraham is right. The first would give for the ratio of momentum to energy the result $1/c'$, in other words the value n, whereas Abraham predicts c'/c^2 or $1/n$. Here, of course, c' is the velocity of light in the medium. Our conclusion is that Abraham is right.[58]

Møller answered that the problem was to some extent "a matter of taste", as it depended on how one chose to separate the energy-momentum tensor in its matter part and electromagnetic part. In the second edition of his textbook, he elaborated on his answer to Peierls:

> In our opinion many different expressions may be equally correct, for the separation of the total energy-momentum tensor into a material part and a field part is largely a question of definition. ... Which of the possible definitions of the electromagnetic tensor should be preferred in the description of the physical phenomena is largely a matter of

58. Peierls to Møller, 1 July 1970. Møller to Peierls, 25 September 1970. Both letters are reproduced in Peierls (2009), pp. 733-736. Møller's letter to Peierls refers in this source erroneously to "the electromagnetic field in a dialectic", where "dialectic" should read "dielectric."

> convenience. It can be shown that a number of experimental facts are most conveniently described by the Minkowski tensor.[59]

Møller was far from a conventionalist in the philosophical sense of the term, but in this particular question he was. As to Peierls, he continued to investigate the problem and eventually obtained an answer in broad agreement with Møller's ideas.[60]

Møller's book differed in several respects sharply from Bergmann's earlier textbook, which presented the theory of relativity as a logical necessity and paid no attention to how it had developed historically. What mattered to the German-American physicist was how the theory ought to have developed from a logical and scientific point of view. While Bergmann dealt in some detail with unified field theories and with five-dimensional Kaluza-Klein theories in particular, this was a subject left out in the book by Møller, which also, to a large extent, presented relativity in a three-dimensional formulation. Instead of writing the flat space-time metric as $ds^2 = dx_\mu dx_\mu$, he preferred the form $ds^2 = dx^2 + dy^2 + dz^2 - c^2dt^2$. Møller found it "useful to stress again the fundamental physical difference between space and time, which was somewhat concealed by the purely four-dimensional representation" in earlier expositions of the theory.

As noted by David Kaiser, an American historian of science, there was in the period up to the late 1950s a strong emphasis on the geometrical foundations of general relativity which then gradually passed over into more dynamical ones.[61] Bergmann's book was an example of the first approach, whereas Møller's was an early example of the second. The Danish author believed that it made

59. Møller (1972), pp. 120-121. Møller referred to a detailed investigation of Iver Brevik, a Norwegian research fellow at Nordita, who concluded that in most cases the tensor expressions of Minkowski and Abraham were physically equivalent but that Minkowski's expression was the most convenient. Brevik (1970).

60. Peierls (1976). As late as 2010, more than a century after the rival works of Abraham and Minkowski, two British physicists proposed a new solution to why both of the two different expressions are supported by experiments and seem natural in certain cases but less natural in other cases. See Barnett and Loudon (2010).

61. Kaiser (1998), which includes details on Bergmann's book.

it "easier for the student fully to grasp the physical content of the general theory of relativity." The Hungarian physicist Lajos Jánossy agreed. "I like that you stress so much the difference between time and space components rather than their symmetry", he wrote to Møller. "Reading your book I see that this makes the physical contents of the theory very much clearer."[62]

The Theory of Relativity was structured in twelve chapters followed by nine appendices of a predominantly mathematical character. While the first half of the book was devoted to special relativity, the other half was about general relativity. In the final chapter on experimental verifications of general relativity, Møller only dealt with the three classical astronomical tests (light bending, gravitational redshift, Mercury anomaly), admitting that these were few and might not in themselves be completely convincing evidence for the truth of the theory. To strengthen the case, he appealed to the correspondence between relativistic and classical mechanics:

> It should be remembered, however, that the general theory is not only a natural, but nearly a cogent generalization of the experimentally well-founded special theory. Further, since Einstein's gravitational theory contains Newton's theory as a first approximation, all the numerous observations which confirm the predictions of Newton's theory may therefore in a certain sense also be regarded as a support of the general theory of relativity.[63]

Like several other authors, Møller also referred to cosmology as empirical support of general relativity. He ended the book with a concise 16-page summary of static cosmological models based on Einstein's theory, mentioning only *en passant* the possibility of a non-static universe. "Our present knowledge of the actual universe, which only covers a limited region in space and time, is, however, totally insufficient and thus no unique choice between the different non-static models is possible." For more information about rela-

62. Jánossy to Møller, 10 July 1952 (CMP).
63. Møller (1952), pp. 355-356.

tivistic cosmology, he referred the readers to Tolman's somewhat dated monograph.

Although Møller decided to leave out quantum theory, parts of the book reflected his background in atomic, nuclear, and particle physics. For example, in his treatment of energy and momentum in special relativity, he made use of cases from nuclear physics such as the reaction

$$ {}^{7}_{3}\mathrm{Li} + {}^{1}_{1}\mathrm{H} \rightarrow {}^{4}_{2}\mathrm{He} + {}^{4}_{2}\mathrm{He}, $$

where the loss in mass transformed into energy by $E = mc^2$ gave 17.28 MeV and the best experimental value at the time was $E = (17.28 \pm 0.05)$ MeV. Møller also included a section on his old favourite topic of electron-electron collisions at high speed. In this section he discussed Champion's experiments from the early 1930s as a confirmation of relativistic mechanics, but curiously without referring to his own scattering theory on which the experiments were based. As yet another example, Møller included a section on Yukawa's theory of meson fields, a subject not normally included in books on relativity theory. Also worth noticing is his comment on Einstein's famous $E = mc^2$ formula: "The statement can be tested only (and therefore has a meaning only) if a process exists in which the particle is annihilated completely. After the discovery of the positive electrons, the positons, in 1932 it became clear that such annihilation processes do exist in which a positive and a negative electron (a positon and a negaton) are annihilated, in accordance with Dirac's theory of electrons."[64] Notice Møller's use of the not widely used names 'negaton' and 'positon' proposed by Bohr and Bhabha (Section 5.1).

The book was generally well received, but there were critical voices as well. Frederik Belinfante, whom Møller knew well and with whom he would write a joint paper two years later, praised the book as a valuable reference not only for students but also for many teachers of physics: "It deserves a wide circulation, and it will certainly become one of the recognized standard works on

64. Møller (1952), p. 91. The first sentence indicates support for a positivistic or perhaps Popperian philosophy of science.

classical relativity theory."[65] However, Belinfante expressed some dissatisfaction with Møller's policy of avoiding four-dimensional geometrical interpretation of the relativistic formulae wherever possible. The same complaint was repeated and reinforced in John Synge's review:

> There have been two ways of looking at relativity – the old kinematical way, with constant reference to measuring-rods and clocks, and the Minkowskian way, in which the geometry of the four-dimensional continuum dominates. Prof. Møller definitely favours the former. ... It seems to me that the book would have been better if Prof. Møller had been more sympathetic to the Minkowskian way of looking at things. When the head begins to swim with contracted rods and slowed clocks, the best antidote to confusion is a simple space-time diagram.[66]

The Irish-English mathematical astronomer William Hunter McCrea was, like Synge, a recognised expert in general relativity but at the time probably better known for his contributions to cosmology and advocacy of the controversial steady-state theory of the universe. He thought that Møller's book was too much an engineering approach to relativity theory and too little concerned with the theory's conceptual, logical, and philosophical foundation. Like some other critics, he noted that "Throughout the book he makes a special point of preserving the distinction between space and time and of not conceding everything to a four-dimensional treatment."[67] Moreover, McCrea complained that the scope of the book was not wide enough as illustrated by its missing references to contributors such as Eddington, Milne, Luther Eisenhart, Schrödinger, Whitehead, and Edmund Whittaker. But this was a deliberate choice from the side of Møller, who did not consider their works to belong to classical relativity or to have been safely established.

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65. F. J. Belinfante, *Science* **116** (1952): 641-643. Belinfante to Møller, 15 August 1952 (CMP).

66. J. L. Synge, *Nature* **117** (1953): 140-141. The mathematical physicist Clive Kilmister agreed with Synge's criticism in a review in *Science Progress* **41** (1953): 147-148.

67. W. H. McCrea, *Mathematical Gazette* **37** (1953): 152-153.

As witnessed by reprints with minor alterations from 1955, 1957, 1960, and 1962, Møller's textbook became a success, which must have secured him a considerable income in royalties. The original edition was translated into Japanese in 1959, carrying a foreword by the Nobel laureate Sin-Itiro Tomonaga. In 1972 the book appeared in a new edition with translations into Russian (1974) and German (1975). The new edition was substantially extended in size to 557 densely printed pages and it included a comprehensive bibliography. Moreover, it was updated with new topics, one of them being the mathematical theory of gravitational radiation, a subject he only briefly mentioned in the original edition: "Fluctuating matter in general gives rise to the emission of gravitational waves travelling with the velocity of light and carrying with them a certain amount of energy. As shown by Einstein [in 1918], the gravitational energy emitted in this way is, however, too small to give any measurable astronomical effect."[68] In none of the editions of the textbook did Møller reveal whether he thought the gravitational waves to be physically real or not, an issue which will be dealt with in more detail in Section 7.3.

In a discussion of the concept of simultaneity Møller pointed out, possibly as the first one, that in general simultaneity depends on the curve connecting two events in space-time. "Intuitively", he wrote, "it would seem more satisfactory if simultaneity between two events could be defined in such a way that it depended only on the frame of reference." Although this could be done for events taking place at adjacent points, "if one tries to extend this definition to events that are spatially far apart by connecting the two events by a curve ... one finds that the simultaneity obtained in this way depends on the connecting curve. Thus, in a general system of reference it is impossible to define globally standard simultaneity between any two events."[69]

68. Møller (1952), p. 321. Kennefick (2007), pp. 66-68.

69. Møller (1972), p. 378. See also Jammer (2006), p. 285, who comments on Møller's insight: "Distant simultaneity, the very same concept that in 1905 was instrumental for the creation of the theory of relativity was finally disqualified by the generalized version of the same theory as having lost its general validity."

The second edition also had more to say about the expanding universe and in the sections on cosmology Møller briefly referred to concepts such as the cosmological constant, cyclic models, the big-bang theory, and the rival steady-state theory. As McCrea noted in another review, much more positive than the earlier one, "Møller's book is more justly regarded as a book of 1972 than as the re-issue of a book of 1952." Møller maintained in the preface that even the larger edition was "a textbook for beginners in the field", which McCrea, undoubtedly correctly, considered somewhat unrealistic: "A beginner would have to be very bright indeed to work through the book on his own, but under the guidance of a knowledgeable teacher he will find it of much value as a textbook and probably even more as a work of reference."[70] Among those who used Møller's book was Gamow, who gave a course on relativity theory at the University of Colorado, Boulder. He found it to be useful, but also difficult and too mathematical to his mind. As he wrote to Møller: "I am using your book in my class on Relativity this semester, but, since it has much too much mathematics both for my students and myself, I have to curtail it quite a bit."[71]

There is no doubt that Møller's book had a major impact on studies of relativity and that it contributed significantly to the early phase of the renaissance of general relativity theory. According to WorldCat, a large international network of library catalogues, *The Theory of Relativity* and its various translations came in 93 editions or printings in the period 1952-1994. The book is presently held in 1,348 libraries worldwide.[72] However, the renewed interest in general relativity which changed the field in the 1960s required textbooks that went beyond Møller's, which after all, even in its 1972 edition, clearly belonged to the classical tradition. The same year another comprehensive but much more modern book came on the market, Steven Weinberg's *Gravitation and Cosmology* subtitled *Principles and Applications of the General Theory of Relativity*. Weinberg's 657-page textbook was on the same subject as Møller's and yet it was com-

70. W. H. McCrea, *Nature* **239** (1972): 115.

71. Gamow to Møller, 10 February 1961 (CMP).

72. http://worldcat.org/identities/lccn-n50003499/

pletely different with respect to coverage, structure, and style. To mention but one striking difference, whereas the cosmic microwave background discovered in 1965 did not appear in Møller's book, Weinberg covered it in detail. Incidentally, Møller knew Weinberg quite well from the early days when Weinberg spent a year as a student at Bohr's institute in Copenhagen (see Section 8.3).

Wheeler appreciated Møller's book, which he used in his attempt to learn general relativity theory, but he soon wanted to go further.[73] In 1968, Wheeler contemplated the idea of an entirely new kind of book and suggested to his former students Charles Misner and Kip Thorne to write one. "The classic texts on relativity theory by Christian Møller and Peter Bergmann", he pointed out, "excellent and authoritative though they may be, are decades old and badly out of date. It's time for a book that incorporates all of the recent developments, one that emphasizes the physics and not just the mathematics."[74] Wheeler soon became part of the ambitious project, the result of which was the innovative *Gravitation* published in 1973, a massive work very different from Møller's in content, style, and pedagogy.[75] The three authors originally thought of a concise book of 200 pages or so, but it ended up at no less than 1,279 pages and a weight of more than six pounds. Møller studied the Misner-Thorne-Wheeler mammoth book and sometimes referred to it. I don't know how he felt about it, but my guess is that it was not to his taste.

6.4. Quantum gravity in Copenhagen

The DeWitt couple – Bryce and Cécile – wanted very much Møller to take part in the planned Chapel Hill conference, not only as a recognised capacity in general relativity theory but also as a representative of Bohr's renowned institute in Copenhagen. A new Institute of Field Physics had been established at Chapel Hill in 1956, and in April the same year Bryce DeWitt invited Møller to

73. See Lalli (2017), p. 143.

74. Wheeler (1998), p. 305.

75. Misner, Thorne, and Wheeler (1973).

come as a visiting professor in the following academic year. With the invitation followed an offer of a salary of $9,000 for nine months plus a round trip to and from the United States.[76] The offer was attractive, but Møller felt forced to decline the invitation:

> You may know that I am involved in the work with CERN. The Theoretical Study Group will stay in Copenhagen till the fall of 1957, and I have committed myself to leading the Group until that time. Thus, no other obligation can be considered in the year to come. ... I just come to think that it might be a good idea to meet you while you are in Europe. How long will you stay in Les Houches? I am planning to participate in a Congress, starting in the beginning of September, in Torino. ... Also the [Chapel Hill] conference in February would have been of much interest to me. However, you have certainly gathered from my above plans that, to my deep regret, I shall also be unable to come on this occasion.[77]

Wheeler too hoped to see Møller in Chapel Hill. In a letter written from Leiden, where Wheeler stayed as a Lorentz Professor, he related to some recent discussions about gravitational waves that the two physicists had had in Copenhagen during one of Wheeler's visits:

> I want to thank you for your helpful discussions of the problem of gravitational radiation. Misner and Putnam and I have been working hard this past week to give specific examples of pure gravitational waves with a non-zero total energy, and we hope we will have something to report to you. ... Bryce DeWitt has just written me that he has invited you to go to the Chapel Hill for a year. I am sure that you would find him and Cecile Morette DeWitt very congenial; also I would hope to see something of you now and again.[78]

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76. DeWitt to Møller, 30 April 1956 (CMP).
77. Møller to DeWitt, 16 May 1956 (CMP).
78. Wheeler to Møller, 15 May 1956 (CMP). Peter Putnam (1927-1987) wrote his PhD under Wheeler in 1960, after which he left physics.

In the summer of 1956, after having participated in the Les Houches summer school, DeWitt made a trip to Copenhagen, where he reasserted the invitation, now for 1957-1958. However, Møller had already arranged to spend the autumn of 1957 at the Carnegie Institute of Technology in Pittsburgh and had received a Fulbright Travel Grant for the journey. He consequently once again turned down the generous invitation. In an interview to a Danish newspaper, he said:

> That is an old invitation, which I now see myself in a position to accept … The director of the Institute has so to say repeated the invitation each year, but I have so far been compelled to refuse, among other things because of my work as head of the theoretical division of the European Organization for Nuclear Research [CERN], which, however, as of next autumn will no longer be located at Niels Bohr's institute. My lectures [in Pittsburgh] will mostly deal with the general theory of relativity.[79]

The Chapel Hill conference had no Danish attendees, but it did have a Copenhagener, namely the 26-year-old Polish-born Stanley Deser, who at the time of the conference was a postdoc at the Institute for Theoretical Physics. Deser spent the years 1955-1957 in Copenhagen, most at the time associated with the CERN theoretical study group headed by Møller.[80]

Although Møller at first declined to visit Chapel Hill for a longer period, after Cécile had intervened and put pressure on him, he agreed to come as a visiting professor in early 1958 for a two-month period. The busy physicist accompanied by his wife actually stayed only for one month, beginning 25 January, during which period he worked on atomic clocks and the role of gravitation in artificial satellites.[81] After having left Chapel Hill in late February, Møller went on to Cornell University, where Bethe had invited him to give

79. 'Professor Møller til USA'. *Berlingske Aftenavis*, 20 August 1957.
80. Blum and Hartz (2017), pp. 119-120. Deser, who obtained his PhD from Harvard in 1953 under the supervision of Schwinger, married Oskar Klein's daughter Elsbeth in Copenhagen in 1956.
81. DeWitt and Rickles (2011), p. 30.

two lectures on the third and fourth of March 1958.[82] The Møller couple left New York by boat two days later. In the autumn of 1958 Møller briefly returned to the United States to give a colloquium at the University of Wisconsin. The subject of his colloquium held on 15 November was 'Time Measurement in the General Theory of Relativity and the Possibility of Terrestrial Tests of the Theory'.

In between Pittsburgh and Chapel Hill, Møller visited Princeton, where he delivered a lecture on the localisation of gravitational field energy at the Institute for Advanced Study, which at the time had Oppenheimer as its director. The Princeton institute was famous not least because of its close association with Einstein, who worked there from 1933 to his death in 1955. Founded in 1930 and opened three years later, during the 1930s it was housed within Princeton University, but the institute was not and never has been part of the university. In early 1958, when Møller lectured in Princeton, Einstein's former office was occupied by his old friend, the astronomer Bengt Strömgren, who stayed there until he returned to Denmark in 1967. Strömgren recalled "how Christian Møller presented his results in this area at a symposium at the Institute for Advanced Study before a most knowledgeable audience, who clearly appreciated his contribution very much."[83] While in Princeton, Møller also met Bohr but it is unknown if Bohr attended the symposium.

Before leaving for his visiting position at the Carnegie Institute of Technology, Møller participated in a small but retrospectively important meeting in Copenhagen, the first ever devoted to quantum gravity. The meeting, which took place between 15 June and 15 July 1957, involved six physicists of whom only three stayed for the whole period, namely the Americans Bryce DeWitt, Stanley Deser, and Charles Misner. The other three participants were Møller from Denmark and Oskar Klein and Bertel Laurent from Sweden who joined the others for the last eleven days of the meeting.[84] Of the

82. Møller to Bethe, 3 February 1958 (CMP). Møller to Rozental, 2 February 1958 (NBA, Rozental Papers).

83. Strömgren (1981), p. 103. On Strömgren and his time in Princeton, see Rebsdorf (2005), pp. 422-429.

84. The Copenhagen quantum gravity meeting is described in Blum and Hartz (2017).

six physicists all except Møller had been at the Chapel Hill conference and three (Møller, Klein, and Deser) had participated in the 1955 Berne conference. Laurent was Klein's PhD student and at the time still working on his thesis exploring a synthesis of quantum mechanics and general relativity. Neither Bohr nor Rosenfeld seems to have been involved in the meeting. Møller was its host despite being somewhat sceptical with regard to the new attempts at formulating theories of quantum gravity, attempts which he did not really believe in and did not contribute to.

In fact, Møller only dealt with the problem at a single occasion, namely in his talk to the 1959 GR2 conference in Royamont, France, on the energy-momentum complex in general relativity. Concerning the quantisation of the gravitational field, he reminded the audience that "no one has as yet succeeded in carrying through this programme consistently for the exact non-linear gravitational-field equations."[85] He had no confidence in "the notion of gravitons or the analogy of photons and mesons in the case of electromagnetic and nuclear fields." On the other hand, Møller shared the consensus view that somehow and at some time it must be possible to unify the two fundamental theories of physics or at least to bring them together into a single scheme: "We must account for the simultaneous existence of Planck's constant of action and gravitational fields in our universe", he stated. Møller's reservations with respect to current theories of quantum gravity were that they were premature, as they did not build on the full theory of general relativity. He did not reject to higher goal of unifying quantum mechanics and general relativity, but thought that it could only be realised, if at all, in an unforeseeable future.

In his report from the meeting, DeWitt, who was the prime mover behind it, commented that the sessions were held "in a very informal style on numerous mornings, afternoons, and evenings." Moreover: "The exposition itself was a lengthy process, involving many arguments at various temperature levels and frequent wandering into side issues. On the other hand, 'plenary' sessions formed only a part of the activity. Often only two or three participants

85. Møller (1962b), pp. 21-25.

were involved in a single discussion, and much of the hard work was carried out in solitude."[86] DeWitt further noted that there was no time to prepare material for publication but that hopefully two or three future papers based on the meeting would be published. This did not happen.

The topics discussed in Copenhagen included various techniques of quantising the gravitational field (such as Feynman quantisation and the canonical approach), approximation methods, topological problems, and the so-called question of measurability. The idea of using measurability as a means of investigating the consistency of a fundamental theory, such as quantum electrodynamics, goes back to a long and difficult paper published by Bohr and Rosenfeld in 1933.[87] The underlying and generally accepted presupposition of this paper was that a well-defined physical quantity must in principle be measurable. While Bohr and Rosenfeld were concerned only with the quantum theory of the electromagnetic field, in 1936 the Russian physicist Matvei Bronstein extended the Bohr-Rosenfeld analysis to the gravitational field by taking into account quantum restrictions. However, Bronstein's prescient paper published in a Russian journal was little known and effectively forgotten by the 1950s.[88] Even Møller seems to have been unaware of it.

In the Chapel Hill conference, the measurability question was discussed with regard to the gravitational field. Wigner's student Helmut Salecker gave a presentation on 'Conceptual Clock Models' in which he applied thought experiments with an ideal clock to conclude that there is an absolute limit to the measurement of

86. DeWitt (2017), p. 160, reproduced from an unpublished document dated 8 October 1957. The following quotations are from the same source.

87. An English translation of the Bohr-Rosenfeld memoir can be found in Bohr (1996), pp. 123-166. See also Darrigol (1991) and Jacobsen (2012), pp. 81-90.

88. Bronstein (2012) is an English translation with an editorial note by Stanley Deser and Alexei Starobinsky. In 1937, at the age of 32, Bronstein was arrested by the Soviet secret police, falsely accused of being a foreign spy. He ended his life before a firing squad.

the gravitational effect on very small masses.[89] For a proton he concluded that the uncertainty in the measurement of the mass was of the same order as the mass itself and that the gravitational mass of a single proton was therefore not strictly an observable quantity. Following the presentation of Salecker, Rosenfeld commented on his and Bohr's earlier analysis, and Wheeler reminded his colleagues that "the history of [quantum] electrodynamics shows that it is always a ticklish business to conclude too early that there are certain limitations on a measurement."[90] He therefore suggested to forget temporarily about the measurement problem in the case of quantum gravity and to focus on other aspects.

In Copenhagen, DeWitt and Misner seemingly subscribed to Wheeler's opinion. So did Møller, who at the end of the meeting discussed the applicability of quantum mechanics in the gravitational domain. According to DeWitt's report:

> Professor Møller quoted a statement by Professor Bohr after finishing his famous 'measurement' paper with L. Rosenfeld, viz. "During the course of our study of the quantum limitations of the measurability of the electromagnetic field we made every possible mistake. In each case, in order to extricate ourselves, we had to go back and look at the formalism!'"[91]

Møller emphasised that without a fully developed quantum formalism of the gravitational field there could be no adequate theory of measurement. Again quoting from the report, "The question of measurability was raised in Copenhagen by Professor Møller, who had some lengthy discussions on it just a few weeks previously with Professor E. P. Wigner of Princeton." However, "While Professor Møller was in general agreement with Wigner's analysis of clocks ... he was utterly unable to follow Wigner's subsequent implications in regard to the limitations on the measurement of gravitational effects

89. Salecker and Wigner (1958). M. DeWitt and Rickles (2017), pp. 171-186. For the Salecker-Wigner argument, see Hagar (2014), pp. 120-121.

90. DeWitt and Rickles (2011), p. 179.

91. DeWitt (2017), p. 161.

of small masses." Møller found it necessary, such as Wheeler did, "in the case of gravidynamics, to have a valid formalism available, before one can make any reliable statements about the measurement problem." Contrary to Wigner and Salecker, Møller believed that at least in principle and given enough time the gravitational effect of small masses can always be measured.

The 'lengthy discussions' between Møller and Wigner mentioned in the report took place by means of letters and continued after the Copenhagen meeting. In one of those letters Wigner wrote to Møller:

> Thanks a lot for your comments about the ms of Salecker and myself. The more I cogitate about the matter, the clearer it becomes to me that one should attack the four-dimensional case. As an example, the question which you raise, concerning the relevance of the time which the signal spends within the area of the clock, for measurements with the clock, cannot be answered in the two-dimensional case. Quite apart from the fact that the gravitational equations cannot be formulated in two-dimensional space-time, the basic concept of 'distant observer' loses its meaning in such a space. I am therefore planning to make a real attack on the core of the problem which seems to me the transmission of signals in real space.[92]

In the concluding section of his report, DeWitt highlighted the crucial importance of the concept of energy in general relativity, a concept which lied at the very heart of the matter but was far from fully understood. "On the one hand the invariance of the theory leads to strong conservation laws, among which one expects the law of conservation of energy. On the other hand, the very concept of energy somehow seems to dissolve. The participants agreed that this concept needs a thorough study and review." Møller definitely agreed with DeWitt's statement. Much of his work over the next decade was concerned with a critical analysis of the concept of energy in general relativity and new ways of understanding it (Section 7.1). The 1957 Copenhagen meeting stimulated some further work

92. Wigner to Møller, 22 July 1957 (CMP).

in quantum gravity and related areas, but on the whole, it was not the breakthrough that DeWitt had optimistically hoped for. It soon faded from the memories of the participants. Neither Møller nor Klein mentioned it in any of their recollections, and Deser and Misner only remembered it vaguely.[93]

93. E-mails to Alexander Blum of 2016 quoted in Blum and Hartz (2017), p. 151.

CHAPTER 7

Works on general relativity theory

Møller's earliest works in general relativity dealt with the clock paradox and terrestrial tests of the theory's prediction of how time behaves in a gravitational field. In the late 1950s he turned to another problem of a more foundational nature which would occupy him for a long time. The problem he attacked was concerned with the definition of energy density or more generally the 'energy-momentum complex' in general relativity. Møller thought that he had found an expression for this quantity that avoided some of the objectionable features in Einstein's original expression. The problem was more than just an academic exercise as it was of direct relevance to one of the hot topics discussed by relativists at the time, namely the possible existence of gravitational waves and the means to detect them. Møller presented his idea of a new energy-momentum complex and its consequences in several publications and orally at many international conferences. Further developments let him to introduce a 'tetrad theory' of gravitation which in some formal respects differed from Einstein's original theory. By the mid-1960s he had developed the tetrad modification of general relativity which he refined and defended for the rest of his life.

At about the same time Møller got interested in how to formulate thermodynamics in agreement with the theory of relativity. In 1968 he presented a detailed and comprehensive answer to the question, which had first been considered by Planck and Einstein as early as 1907. He concluded that now he had found the final answer, but not all physicists agreed. Møller only followed the development of cosmology, traditionally an important area of application of general relativity, at a distance. Nonetheless, he was aware of and mildly interested in the development that eventually led to the hot big-bang theory of the universe in the mid-1960s. What did interest him, not mildly but greatly, were the singularities which turned up either in cosmology or as black holes formed by collapsing stars. According to Møller, the singularity problem was nothing less than a catastrophe, an indication that somehow the established and much-admired theory of gravitation was in need of repair. After much work he came to the conclusion that the singularities were after all not inevitable and that they would not appear in his favoured tetrad theory of gravitation. As to the black holes, he did not believe, contrary to other experts in general relativity, that they were real objects in nature.

During his last years, Møller was occupied with what he conceived as a grave crisis in the theory of general relativity. To reject Einstein's theory was not an option, but to modify it was an acceptable solution. He communicated his tetrad theory and non-singular cosmology based upon it in papers, lectures, and letters to his colleagues in theoretical physics. Although some responded positively, most did not. Møller stayed outside the cosmological steady-state controversy that raged between 1948 and 1965. Although he never said so directly, there is little doubt that he was dissatisfied with the standard big-bang model of a universe beginning in a singular state corresponding to infinite curvature and energy density.

7.1. The energy problem in general relativity

When Einstein derived his gravitational field equations in 1915-1916, he was guided by the requirement of energy conservation, which he formulated in terms of an energy-momentum tensor ${T_i}^k$ referring to the matter field including the electromagnetic but not the gravitational field. While ${T_i}^k$ is not conserved, by adding a quantity t_i^k referring to the gravitational field he obtained a conserved total energy-momentum given by $\theta_i^k = {T_i}^k + {t_i}^k$. However, because t_i^k is not a tensor, but what is called a pseudo-tensor, the total energy-momentum θ_i^k does not transform in a coordinate-invariant manner. If a tensor equation holds in one coordinate system, it holds in all, which is not the case for equations based on pseudo-tensors. It also means that the Einstein energy-momentum cannot describe the local distribution of energy.

Associated with the question of the correct formulation of energy conservation in general relativity was the question of the localizability of the gravitational field energy. Einstein admitted that his expression for the total energy did not allow a calculation of the distribution of energy in space or of the energy content in a limited part of space. His choice of the energy-momentum tensor was criticised at an early date by the Italian mathematician Tullio Levi-Civita, who found it to be unsatisfactory for both mathematical and physical reasons. More criticism came from the Austrian physicists Erwin Schrödinger and Hans Bauer, who objected that Einstein's expression was not coordinate-independent and in some cases led to physically absurd results. While Schrödinger in 1918 showed that Einstein's energy-momentum sometimes vanished despite the presence of a gravitational field, the same year Bauer, a physics teacher at a Viennese gymnasium, objected that it does not always vanish in the absence of such a field. Nonetheless, although Einstein admitted that "almost all colleagues stand against my formulation of the energy-momentum law", he maintained his choice involving the tensor $T_i^{\ k}$ and the pseudo-tensor $t_i^{\ k}$. Over the next decades these questions continued to be discussed by the small world of experts in general relativity theory.[1] As a result of these discussions, Einstein's view became generally accepted: the localisation of energy and momentum has no meaning within the framework of general relativity.

In a series of papers 1958-1962 Møller entered the discussion by proposing a new expression for the energy of the gravitational field sometimes called 'Møller energy'. He began working on the problem while staying at the Carnegie Institute of Technology in the autumn of 1957, such as he informed Bohr:

> I have been concerned in particular with the old problem of the localisation of energy in gravitational fields. For more than 40 years ago Einstein found fully satisfactory integral expressions for the *total energy* of closed system, but … these integral expressions could not be interpreted as energy density. … This has always appeared most unsatisfactory to me and it has made the discussion of energy-carrying gravitational waves

1. For the early debate, see Cattani and De Maria (1993) with the Einstein quote from p. 83. See also Xulu (2003) for a discussion of the energy problem in general relativity including Møller's contributions to it.

> and the associated 'gravitons' very unclear. I now believe to have cleared up this old problem … I have already applied the theory to various special cases and have begun to doubt that gravitational waves radiate at all when one uses the exact non-linear gravitational field equations.[2]

The name 'graviton' for a unit quantum of gravitational energy was originally coined in 1934 by two Russian physicists, Dmitri Blokhintsev and Fëdor Galperin, in a little-known paper written in Russian in which they related the gravitational quantum to the neutrino in agreemen with Bohr's idea (Section 3.4).[3] A few days later, Møller similarly but in more technical language reported on his new work to Aage Bohr, again in a mood of excitement:

> I have now succeeded to find another 'pseudo-tensor' which also satisfies the continuity condition, but from which one can get expressions for the energy density h and energy current density S which 1) have the right transformation properties … and 2) [where] $\int hdV$
>
> has the same value for closed systems as in Einstein's expression for the total energy, but now one can ascribe a well-defined meaning also to the energy of non-closed systems. I consequently am of the opinion that it is possible to speak in a physically satisfactory manner of the distribution of the energy across space, which obviously is important in the discussion of so-called gravitational waves.[4]

Like in his letter to Bohr senior, to Bohr junior he suggested that energy-carrying gravitational waves did not exist, although cautiously adding that "it is too early to state something definite about it." Møller also informed Bethe about his discovery: "I have been working on the old problem of the 'localization of energy in gravitational fields' which has been a puzzle since Einstein gave integral expressions for the *total energy* of closed systems over 40 years ago.

2. Møller to Bohr, 13 December 1957 (BSC).
3. Gorelik and Frenkel (1994), p. 96. The name only became widely known after Dirac proposed it at a meeting in New York in early 1959, see Kragh (1990), p. 246.
4. Møller to Aage Bohr, 17 December 1957 (NBA, Aage Bohr Papers).

I think I have found the solution and that it is possible to give physically acceptable expressions for the energy density and the energy current vector in gravitational fields."[5]

After having spent a week in New York in early September 1957, Møller went on to Pittsburgh. "Contrary to my expectations and the rumours of 'the smoking city', I find Pittsburgh and its surroundings in particular to be lovely", he wrote to Aage Bohr. To get around in the American way, "I have bought a [Chrysler] de Soto 1953 and now feel that I got back my freedom of movement."[6] Among other things, he used the car to go to New York and pick up his wife Kirsten, who joined him about two months after he had arrived. In another of his letters to Aage Bohr, Møller discussed an application from a young American physicist at the Carnegie Institute who wanted to come to Copenhagen. Møller found him to be competent and "moreover, he has a wife whose exceptional beauty and sweet nature would be a considerable ornament to Blegdamsvej ... but perhaps this argument cannot be used in evaluating his application."[7] No, perhaps not, but the physicist's application was accepted.

During his time at the Carnegie Institute, Møller was invited to give lectures at several other places in the United States. In the months of November and December the invitations brought him and his wife to Notre Dame (Indiana), Madison (Wisconsin), Lafayette (Illinois), Chicago (Illinois), Ann Arbor (Michigan), and Columbus (Ohio). He and Kirsten spent the Christmas vacation in Florida. Møller continued his studies on the energy concept in general relativity while visiting Chapel Hill, where he formulated his ideas in a festschrift to the distinguished Norwegian quantum theorist Egil Hylleraas on the occasion of his sixtieth birthday.[8] The Paper was presented to the Royal Norwegian Society of Sciences

5. Møller to Bethe, 20 December 1957 (CMP).

6. Møller to Aage Bohr, 24 September 1957 (NBA, Aage Bohr Papers). Møller to Rozental, 3 October 1957 (NBA, Rozental Papers).

7. Møller to Aage Bohr, 30 October 1957 (NBA, Aage Bohr Papers). The physicist in question was Donald A. Geffen, who worked on dispersion relations in particle theory.

8. Møller (1958a), in *Festskrift til Egil Hylleraas* (Trondheim: Bruns Bokhandel, 1958). Hylleraas (1898-1965) is best known for his calculation in 1929 of the ionisation energy

and Letters by Harald Wergeland, another prominent Norwegian theoretical physicist and a friend of Møller, on 26 February 1958. Møller's paper attracted the interest of Max Born, who in a letter referred to his own work dealing with the clock paradox:

> As you may have noticed, together with my collaborator [Walter] Biem I have published a small work on the clock paradox in the proceedings of the Amsterdam Academy. I would like to know if you agree with our exposition. It is not essentially different from what you have done, only more condensed and perhaps more comprehensible. I had a most favourable discussion of it with Chandrasekhar! I see that you have written two small memoirs on the concept of mass and energy according to general relativity in the festschrift for Hylleraas. I have read them with pleasure and was quite surprised that the mass can be conceived as the self-energy of the gravitational field. As yet I have not fully understood it, but I will take a close look at it.[9]

In the festschrift paper Møller referred to the remarkable possibility that particles might have negative mass, such as Hermann Bondi had recently concluded.[10] Without suggesting that bodies with negative gravitational mass actually exist, Bondi proved that according to general relativity they are possible objects. While a positive-mass body will attract one of negative mass, the negative-mass body will repel one of positive mass! Møller commented: "Of course this does not prove that particles of negative mass really exist in Nature, it merely shows that a negative mass is not in contradiction with the principles of the present theory of gravitation. However, so far there are no experimental facts which indicate that such particles exist."[11] Nor is this the case today.

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of the helium atom, which provided convincing evidence that quantum mechanics can be applied to a system of more than one electron.

9. Born to Møller, 20 June 1958 (CMP; in German). Born and Biem (1958).

10. Bondi (1957).

11. Møller (1958a, II), p. 7. See also Brevik (2011). The hypothesis of negative mass had been discussed in cosmological contexts since the late nineteenth century. See Jammer (1997), pp. 127-132.

While Møller's presentation to the Norwegian Society was "perhaps mostly of didactical interest", in a subsequent paper in *Annals of Physics*, a new journal founded in 1957, he described in mathematical details his proposal of a new concept of energy in general relativity. As he pointed out, physicists were "led to the conclusion that in general relativity it has no well-defined physical meaning to make any statements regarding the localization of the energy in a physical system. The absence of a consistent expression for the energy density has caused a lot of controversial discussions in recent years in particular as regards the question of the existence of energy carrying gravitational waves."[12] Møller found it intolerable that current theory did not offer any answer at all to questions concerning the energy of a part of a closed system or for the localisation of energy inside it. He consequently searched for a better expression of energy and momentum, and also for a law of energy conservation in the form of a continuity equation which was independent of any particular coordinate system. As Møller knew, his view with regard to the localisation of gravitational field energy was unconventional. The conventional wisdom was the one expressed in the widely used textbook series by Landau and Lifshitz, namely, "it is meaningless to talk of whether or not there is gravitational energy at a given place."[13]

Møller's search led him to a quantity $S_i^{\,k}$ that could be added to Einstein's $T_i^{\,k}$ in such a manner that the energy-momentum complex $T_i^{\,k} + S_i^{\,k}$ transformed as a tensor. In order to retain the satisfactory features of Einstein's expression, he concluded that the new $S_i^{\,k}$ must depend on the metric tensor g_{ik} and on its first and second derivatives. Møller believed in this way to have found an energy-momentum complex that was superior to Einstein's and yet in full agreement with the principles of general relativity. He considered his theory to be an extension of Einstein's theory of gravitation and not an alternative to it. As an important illustration of his new

12. Møller (1958b), p. 348. This paper, one of Møller's most important, has been cited 573 times (Google Scholar).
13. Landau and Lifshitz (1971), third edition, p. 307.

theory, he applied it to a spatially closed universe, for which he concluded that its total energy content must be zero (Section 7.3).

In a contribution to the 1958 Max Planck festschrift celebrating the centenary of the birth of Planck, Møller restated the essence of his new theory and applied it to the case of gravitational waves. Whereas Nathan Rosen had concluded in 1955 that gravitational waves cannot carry energy, two years later Bondi argued the opposite. By 1960 most relativists agreed that gravitational waves transported energy in a manner roughly analogous to electromagnetic waves. However, contrary to what many physicists believed, Møller concluded that if these still hypothetical waves exist, they cannot carry energy and will thus be undetectable: "It is proved that [planar and cylindrical] gravitational waves must have zero total energy. This strongly supports the conjecture that there are no energy-carrying gravitational waves at all, and that this is the reason why the application of the usual quantization procedures has not led to satisfactory results."[14]

Møller thus belonged to the minority of relativists who doubted the existence of gravitational waves and found the electromagnetic analogy to be utterly misleading because it did not match the non-linear gravitation theory. He was not alone, though, for somewhat similar scepticism was for a time expressed by authorities such as Leopold Infeld, Nathan Rosen and – perhaps surprisingly – Hermann Bondi. Joshua Goldberg, an American specialist in general relativity, recalled that at the Chapel Hill conference he had "long discussions with Bondi and Gold, who took the position that gravitational radiation does not exist."[15]

The following year, in a talk given to the GR2 conference in Royamont, France, Møller offered some further considerations on the reality of gravitational waves, although as usual "without expressing any definite opinion on this question." As mentioned, at the time most experts in general relativity believed that the waves were real and detectable at least in principle, but Møller remained

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14. Møller (1959b), p. 139. See Kennefick (2007) for a history of gravitational waves.
15. Goldberg (1993), p. 91. As late as 1979, Rosen wrote a paper in *General Relativity and Gravitation* with the telling title 'Does gravitational radiation exist?'

sceptical or perhaps agnostic: "At the moment, I still regard the question of the existence of energy-carrying gravitational radiation as undecided."[16] Even after Hermann Bondi, Rainer Sachs, and others had made it likely that gravitational waves exist in nature, and Joseph Weber at the University of Maryland had announced (albeit controversial) experimental evidence for the waves, Møller was not completely reassured. In his 1972 textbook he wrote about Weber's project:

> The bold project of Weber to construct emitters and receptors for gravitational radiation is of the utmost importance in principle. His experiments show already the effects of fluctuating gravitational fields in distances of the order of the wavelength, but this is not sufficient for our purpose, since the retardation effects are vanishingly small in this region.[17]

Møller believed that if gravitational waves exist, their intensity must be so small that they would be invisible even to Weber's delicate experiments with massive resonant-bar detectors. Two years later, the American astrophysicist Joseph Taylor and his research student Russell Hulse discovered the first binary pulsar, a system of two neutron stars orbiting around each other in eccentric orbits. In 1993 Taylor and Hulse shared the Nobel Prize for their work. Physicists quickly understood that the discovery could be used to confirm general relativity to high accuracy and that it provided strong evidence for gravitational waves propagating with the speed of light. One might expect that Møller found the Taylor-Hulse discovery to be very important as it related directly to his own research interests. However, for some reason he did not. At least, he never referred to it, such as did many other relativists.

In a predominantly mathematical memoir of 1959 Møller proved that his expression for gravitational energy could be derived from

16. Møller (1962b), p. 21. Although Møller dealt extensively with gravitational waves, he rarely commented on whether the waves existed in nature or not. For other of his contributions to the subject, see Møller (1963b) and Møller (1964b).
17. Møller (1972), p. 472.

calculations based on a variational principle, which increased his confidence that he was on the right track.[18] However, two years later he realised that the expression was not as satisfactory as he had thought and that, consequently, "the question of the localizability of the energy cannot yet be regarded as finally settled."[19] Unwilling to abandon the idea of localised energy and equally unwilling to depart more than absolutely necessary from Einstein's theory, he now suggested that the components of the metric tensor g_{ik} might not be the fundamental gravitational variables. On this basis he formulated a revised but still not entirely satisfactory expression for the energy-momentum complex.

Møller expounded his theory at several occasions, in papers as well as in lectures. For example, in the summer of 1960 he gave a series of lectures on general relativity at a summer school held at Brandeis University, Massachusetts. As usual, he went together with his wife Kirsten. "Campus is wonderful", he reported to Rozental, "it consists of new buildings of a rarely seen harmonious architecture located at hills outside Waltham, a suburb of Boston. We live in a Scottish-style castle which is much older than the 12-year-old university."[20] Some months earlier Møller had been invited by André Mercier to come to the University of Berne, Switzerland, as a visiting professor, but because of his obligations in the United States he had to decline the invitation.[21]

Møller's lecture course at Brandeis included a thorough mathematical discussion of the energy problem and the advantages of adopting the Møller energy-momentum complex instead of other expressions. Møller also treated the subject, as usual in mathematical details, in the 1972 edition of his textbook on relativity theory.[22] Although his theory of gravitational energy was thus well known to

18. Møller (1959c).

19. Møller (1961a), p. 118.

20. Møller to Rozental, 6 July 1960 (NBA, Rozental Papers).

21. Møller to Mercier, 13 April 1960 (CMP).

22. C. Møller, *Selected Problems in General Relativity*, lecture notes by J. Stachel and L. Pande, 122 pp., printed by Copenhagen University, 1960. Møller (1972), pp. 453-459, 479.

the small community of relativists, it failed to attract much immediate attention. Petros Florides, a physicist at the Dublin Institute for Advanced Studies, reviewed critically but sympathetically the new energy concept, which he found to be a great improvement over Einstein's. The reason for his praise was not only that Møller avoided the absurd results that followed from Einstein's energy expression, but also that it offered for the first time a meaningful concept of localisation of energy.[23]

From the early 1960s onwards, Møller developed his theory into what he called the 'tetrad' theory of gravitation, the tetrads being mathematical quantities that replaced the metric tensor as the fundamental gravitational variables. As he explained, the tetrads were not directly observable as they were subsidiary quantities playing the same role as the potentials in electrodynamics. The space-time in Møller's tetrad theory was not the Riemannian continuum employed in the usual formulation of general relativity, but a space-time first investigated by the German mathematician Roland Weitzenböck in 1923. In such a space two vectors at distant points could be parallel independently of the curve connecting the two points, a property known as absolute or distant parallelism. By means of the tetrad formalism, Møller could define an energy-momentum complex that satisfied the basic relativistic criteria without sacrificing the localisability of energy. These criteria, he proved, could not be satisfied within the framework of Einstein's theory.

Moreover, there was the exciting possibility that the new formulation might lead to a unified theory. Because there were sixteen tetrad field variables, and thus more than the ten independent metric components, he found it "natural to assume that the tetrad field actually describes a larger domain than mere gravitation, ... the unified field of gravitation and electromagnetism."[24] However, to Møller, who was not a 'unificationist' in the style of Einstein and some other relativists, this was merely a side remark. Although he

23. Florides (1962). For later positive responses, see for example Novotny (1987) and Lessner (1996), who wrote about Møller's "famous energy-momentum complex" that it "turns out to be a powerful concept of energy and momentum in general relativity."
24. Møller (1961b), p. 5 and pp. 28-31.

Fig. 30. Group photo of participants and guests at the 1961 Varenna School of Physics. Møller is number four to the right on the first row, sitting next to Dicke on his left. On the same row, to the left, Kirsten Møller, Ms. Annie Dicke, Fred Hoyle, and Arthur Schild. Eduardo Amaldi is to Møller's right. Behind Ms. Dicke is Joseph Weber and to his right Claudio Pellegrini. Photograph in Møller Papers, Niels Bohr Archive, Copenhagen.

thought that it might be possible to find a unified theory based on the tetrad formalism, he concluded that the formalism was not yet sufficiently developed and consequently shelved the idea.

Møller was well aware that the general idea of a 16-component tetrad was not original, as Einstein had used it and also the Weitzenböck space-time in attempts from the late 1920s to formulate a unified theory. Møller's theory had more than a few mathematical features in common with Einstein's earlier idea of so-called *Fernparallelismus* (distant parallelism), but he stressed that the similarity between his and Einstein's old theory was superficial rather than essential. "Both the basic equations and their physical interpretation are different", he pointed out.[25] Whereas Einstein's unified tetrad theory was a failed attempt to unify gravitation and electro-

25. Møller (1962c), p. 263. See Sauer (2006) for a description of Einstein's theory of distant parallelism, which he developed in works from 1928 to 1931 but then abandoned.

magnetism, Møller's new tetrad theory was basically a theory of gravitation only.

In Copenhagen, Møller suggested to one of the young physicists, the Icelander Magnus Magnusson, to investigate in greater detail the uniqueness of the Møller energy expression.[26] Magnusson was one of the first fellows of Nordita, the new Nordic institute for theoretical physics of which Møller served as director (Section 8.3). Møller also discussed the new energy-momentum complex with the Italian physicist Claudio Pellegrini, another Nordita fellow, and his Polish colleague Jerzy Plebański, a student and collaborator of Infeld in Warsaw. The Pellegrini-Plebański collaboration led to a paper published in 1963 in which the authors formulated Møller's tetrad theory in terms of an action principle and investigated its use in quantum field theories describing the neutrino.[27] At about the same time Stanley Deser visited what was still the University's Institute for Theoretical Physics but a few years later would be renamed the Niels Bohr Institute. The discussions he had with Møller and others resulted in a paper in which Deser compared Møller's energy-momentum with previous non-tetrad definitions.[28]

Oskar Klein was initially sceptical with regard to Møller's project but changed his attitude after he had studied it more carefully. In a letter to Møller commenting on his contribution to an informal festschrift in honour of Klein's seventieth birthday, he wrote:

> I now see that there is an essential point in what you have achieved, namely that the energy and momentum transform as a four-vector also in the case of a transformation to an *externally* accelerated coordinate system. ... I did not realise at all that it makes special demands on the energy-momentum complex and that these demands are not satisfied by Einstein's original quantities. I find it most interesting that they can be satisfied by the bein-components [*ben-komponenter*] but not by the

26. Magnusson (1960). Gudmundsson et al. (2021), pp. 179-180.
27. Pellegrini and Plebański (1963).
28. Deser (1963).

> usual gravitational potentials. I look forward to talk with you and then understand in more depth your considerations.[29]

The 'bein' (leg) mentioned by Klein was a reference to Einstein's so-called *vierbein* (four-legs) formulation of general relativity dating from 1928. Møller's tetrads were related to yet different from Einstein's vierbein quantities, which Einstein had introduced in his search for a unified field theory in curved space.

Møller first presented the tetrad theory of gravitation in a lecture given to the Enrico Fermi Summer School in Varenna, Italy, in the summer of 1961, and half a year later he expounded it in a lengthy memoir to the Royal Danish Academy.[30] On 26 July 1962 he discussed it in a lecture to the GR3 conference in Jablonna organised by Infeld and with Plebański in the organising committee.[31] This meeting was attended by a large number of experts in general relativity among which were Fock, Rosenfeld, Chandrasekhar, Dirac, Schild, McCrea, Sciama, and Penrose. Among the participants was also Peter Higgs, who two years later would introduce the mechanism and boson particle named after him. The most memorable of the many lectures was however delivered by Feynman, who presented his unconventional program of quantising general relativity by means of Feynman diagrams and other techniques based on renormalisable quantum electrodynamics. Møller chaired the session and initiated the discussion that followed Feynman's talk:

> *Møller*: May I, as a non-expert, ask you a very simple and perhaps foolish question. Is this theory really Einstein's theory of gravitation in the sense that if you would have here many gravitons the equations would go over into the usual field equations of Einstein?

29. Klein to Møller, 11 December 1964 (CMP, in Danish). I have not been able to locate the festschrift, which was probably distributed in the form of a stencilled work and not as a published book. Møller's contribution may have been the same or almost the same as Møller (1965).
30. Møller (1962c). Møller (1961b).
31. Møller (1964a).

Feynman: Absolutely.
Møller: You are quite sure about it?'[32]

While Feynman maintained his view, he was forced to admit that some of his arguments were weak. Møller was clearly unconvinced that Feynman had offered a satisfactory quantisation of Einstein's field equations. As regards Møller's tetrad theory of gravity it attracted considerable attention at the Polish conference, where it was also discussed in lectures given by Plebański and the Russian physicist N.V. Mitzkevič. In his concluding remarks to the conference, Bergmann referred somewhat sceptically to Møller's theory of gravitational energy. As he pointed out, the tetrads could only be fixed by requiring that they satisfied certain criteria or restrictions, and to some extent these were bound to be arbitrary:

> The work by Professor Møller and by others that is now in progress may persuade us that a particular set of restrictions is to be preferred on physical grounds; that remains to be seen. ... Professor Møller, I feel certain, agrees with this analysis. In view of the fact that the search for an acceptable fixation of tetrads is in its beginnings, I shall not, as it were, uproot the new plant to see whether or not its roots are properly growing; let us rather wait and see.[33]

As we have seen in Section 5.4, physicists sometimes included in their international meetings jocular elements in the form of songs and the like. The Warsaw-Jablonna conference was no exception. At the concluding dinner a play was presented with a ballad composed by Arthur Komar.[34] Two of the verses were:

Oh you may be a colleague of John Wheeler,
Ivanenko you may think fine,
You may have studied with Bergmann or Fock,
But you're no colleague of mine.

32. Feynman (1963), p. 855.
33. Infeld (1964), p. 277.
34. Infeld (1964), p. 376.

> Oh you may be a colleague of Møller,
> Lichnerowicz you may think fine,
> You may agree with Infeld and Synge,
> But you're no colleague of mine.

In addition to the Varenna and Jablonna meetings, Møller also presented his tetrad theory of gravitation at a meeting held in Florence in 1964, which was part of the celebrations of the four-hundredth anniversary of the birth of Galileo. Møller ended his Florence lecture with a reference to the great Italian scientist to whom the conference was devoted: "The new formulation constitutes a certain rounding off of Einstein's beautiful theory of gravitation, which in many respects can be regarded as the last stage of a development started by Galileo 400 years ago." In the discussion session after the talk, Bondi objected that "until we have physically significant ideal experiments to measure energy and momentum locally, … the full potentialities of Møller's expressions can hardly be appreciated." Moreover, he stated that to his mind the general principle of relativity was "virtually empty", to which Møller responded:

> I do not agree with the statement that the general principle of relativity is empty. The requirement that all relations between measurable physical quantities (including of course the gravitational field variables) must have the same form in any system of space-time coordinates seems to me to be a meaningful and extremely useful working principle in a situation where are main difficulty is an *embarras de richesse*.[35]

While in Florence, Møller used the occasion to participate in an international conference on cosmology in nearby Padua, which was another part of the 'Galilean Days' in September 1964. The following year Yukawa invited him to an international conference on elementary particles in Kyoto, which in the week 24-30 September commemorated the thirtieth anniversary of the meson theory. Yukawa valued Møller's work, such as indicated by a telegram sent to Aage Bohr after having learned of his death: "Greatly grieved

35. Møller (1965), p. 15.

at sad news of Professor Moeller's passing. His great achievements will remain forever with science and philosophy."[36]

This was the second time that Møller visited Japan, giving him an opportunity to witness the remarkable progress the country had made since his last visit in 1953. The Kyoto conference included among its invited speakers also David Bohm, Friedrich Bopp, Yoichiro Nambu, Chen Ning Yang, Léon Rosenfeld, Robert Marshak, and Igor Tamm.[37] Møller's address on 26 September 1965 was the only one which dealt with general relativity rather than elementary particle theory, which was after all the subject of the conference. Based on his earlier writings, he discussed what he called 'particle-like systems' from the point of view of general relativity rather than the special theory of relativity. He raised objections to Einstein's and others' versions of the energy-momentum complex and argued, as he had done previously, that the gravitational field must be described by tetrad fields connected with but different from the metric tensor.

A long discussion followed Møller's talk, with Wentzel, Bopp, and others asking critical questions. Concerning the Schwarzschild radius and singularity, Møller said: "I do not like those topological models of Wheeler and others where one tried to interpret the inside [of the black hole] with complicated topology. I am too simple-minded for that. I am always thinking of the matter extending beyond the Schwarzschild radius."[38] In response to a question from Yang, he said that he did not believe that so-called Schwarzschild particles, where the mass is concentrated at the centre, exist in nature. Moreover, Møller commented on the possibility of a unified field theory, something he did not really believe in:

> In principle it would be very nice to unite the electromagnetic field and the gravitational field which are the only really classical fields in a way. ...Well, Einstein started these attempts already in the 1920s, [but] he

36. Yukawa to Aage Bohr, 21 January 1980 (NBA, Aage Bohr Papers).

37. Tanikawa (1966). Born, Heisenberg, and Dirac were also invited, but none of the three pioneers of quantum mechanics could come.

38. Møller (1966a), p. 226. Other quotations are from the same source.

> did not succeed, and, I think, nobody has succeeded. Also Einstein's attempts in his later years, I do not think, were successful. Now we have so many fields and so many particles. ... There is a large difference between the electromagnetic field and the gravitational field. They are perhaps larger than the similarities. ... While with the electromagnetic field, we know that it should be quantized, we do not know anything about the existence of gravitons. Maybe they don't exist at all. Maybe the gravitational field itself should not be quantized.

Finally, in response to Wentzel's view that the apparent non-existence of gravitons carrying energy and momentum seems to be "in deep conflict with our whole concept of quantization", Møller said: "I agree that there is a certain conflict with the usual quantum mechanics, but I don't know maybe, quantum mechanics is only approximately valid." He did not follow up on this somewhat enigmatic remark.

In the session where Møller spoke, Bohm gave a long and very general talk on his new ideas of discrete structural processes in nature. The only similarity of the two papers was that none of them were about mesons. While Møller's paper was densely packed with equations, there were none in Bohm's, which started with declaring that "Physics is in a state of flux, in which the theories that will eventually emerge may well be as different from current theories as those latter are from those of the nineteenth century." What he had in mind was what he later called an 'implicate order', which "not only refers to the whole of nature, but also to all human beings and all their activities, including, of course, that of scientific research."[39]

Bohm introduced his famous as well as controversial causal interpretation of quantum mechanics, also known as the hidden variable interpretation, in 1952. When it became widely known it was flatly rejected by Pauli, Rosenfeld, and other defenders of the more orthodox Bohr-Heisenberg view of quantum mechanics. Niels Bohr reportedly said that Bohm's theory was "very foolish", while

39. Bohm (1966), p. 252 and p. 273.

his son Aage Bohr was more sympathetic to it.[40] In 1957 and again in 1958 Bohm visited Copenhagen, where he had conversations with Niels Bohr. Bohm was on his way from a temporary position in Haifa, Israel, to a new one in Bristol, and may have decided to stop over in Copenhagen to talk with Bohr. He recalled about his meeting in the fall of 1957:

> I tried to discuss my cosmology with him, to try to understand the quantum mechanics more deeply, this dialectical cosmology, this dynamic cosmology. He did not really quite appreciate it. He said the ideas are beautiful, but they were on the wrong track. We tried to talk. He always stuck to his presentation. It was often hard to really talk seriously with him. It was not very clear, how he talked. He sort of, I think he often tried to throw the discussion off the track. He would start to smoke his pipe and light it, then he dropped a box of matches and spent a long time picking them up and get his pipe lighted again. By that time, we had sort of forgotten where we were. I think he had methods for getting the discussion off a certain track onto his track. He was very friendly.[41]

During Bohm's first meeting with Bohr, Møller was in Pittsburgh, but he possibly met Bohm when he visited the institute the following year. If he did, it left no traces in his correspondence or recollections. My guess is that Møller was simply uninterested in Bohm's ideas, which he may have found too speculative and philosophical to take seriously.

Just a month after having returned from Japan, on 2-5 November 1965 Møller participated in a symposium in East Berlin arranged by the German Academy of Sciences celebrating the fiftieth anniversary of Einstein's general theory of relativity. The main organiser was the East German theorist Hans-Jürgen Treder, who was not

40. Freire (2015), pp. 45-46. On Rosenfeld versus Bohm, see Jacobsen (2012), pp. 271-281.

41. Interview by Maurice Wilkins of 22 December 1986, American Institute of Physics. https://www.aip.org/history-programs/niels-bohr-library/oral-histories/32977-6. Bohm's visit is attested in the list of visitors from abroad kept at the Niels Bohr Archive, which however refers to the years 1958 and 1959 (see appendix II).

only a recognised authority in general relativity but as an avowed Marxist also enjoyed full confidence of the political leadership in the German Democratic Republic. Among the participants from Western countries were experts such as Bondi, Wheeler, Papapetrou, Tonnelat, and Rosenfeld. Russian participants included Fock and Ivanenko, the latter giving a talk on 'Cosmology and Elementary Particles' in which he discussed the cosmological arrow of time and the behaviour of antiparticles in an expanding universe.

Much like Bondi, Fock believed that "general covariance is not a physical idea, [but] a mathematical idea without any physical content."[42] Although Møller emphatically disagreed, this time he did not object to what he considered a serious misunderstanding of the foundation of general relativity. Møller participated in some of the other discussions, including those of Treder and Lanczos who both discussed tetrad formalisms of general relativity that differed from what he thought was the correct version. He used his own talk to survey his investigations over the last decade on the energy-momentum complex and the tetrad theory of general relativity. The two were closely connected, he said, as "the tetrad formulation has given us more confidence in the application of the energy-momentum complexes which for many years by many physicists have been regarded as not quite respectable quantities."[43] Bondi, who spoke on 'Gravitational Radiation and Gravitational Energy', acknowledged that "the question of the energy of the gravitational field, which Møller has dealt with so beautifully, is indeed very difficult", but without endorsing the tetrad formulation.[44]

At the end of his life, Møller returned to his tetrad theory of gravitation, which he considered in relation to black holes and on the basis of which he proposed a cosmological model without an initial singularity (Section 7.3). His theory attracted some but not much attention and after his death it was developed in various

42. Mercier (1966), p. 1. The proceedings volume of the Berlin symposium was published as Treder (1966).

43. Møller (1966b), p. 12. This work was approximately identical to his contribution in Treder (1966), pp. 100-118.

44. Treder (1966), p. 120.

directions. Hildegard Meyer, a German physicist who had studied under Møller at Nordita, wrote a paper dedicated to the memory of Christian Møller in which she examined some of the mathematical aspects of his tetrad theory.[45] Other physicists took up the theory in studies of gravitational waves, black holes, and the wormholes introduced by Wheeler and Misner in 1957 (but first envisaged in a 1935 paper by Einstein and Rosen).[46] The Møller tetrad theory continues to be investigated by a small number of theoretical physicists.

7.2. Relativistic thermodynamics

With its roots in the nineteenth-century theories of Hermann von Helmholtz, Rudolf Clausius, and William Thomson, thermodynamics does not belong to so-called modern physics. Yet the two fundamental laws of the new theory of heat, the first about energy conservation and the second about the continual increase in entropy, have largely survived the relativity and quantum revolutions. Einstein, a great expert in thermodynamics, admired the classical theory because of its generality and logical simplicity. In his autobiographical notes of 1946, he wrote about "the deep impression which classical thermodynamics made upon me." It is, he continued, "the only physical theory of a universal content which I am convinced that within the framework of the applicability of its basic concepts, it will never be overthrown."[47]

Although few twentieth-century physicists specialised in thermodynamics, Einstein's high appreciation of the theory was shared by almost all, as is still the case. To young Møller, thermodynamics was an eye-opener which influenced his career choice. At the age of twenty-two or so he became fascinated to see "how one could use mathematics to get the relations between the different thermodynamic quantities and how the whole thing could be formulated in these very few simple laws, the first and the second laws" (Section 1.1).

45. Meyer (1982).

46. For example, Salti and Aydogdu (2006), and Aygün and Yilmaz (2007).

47. Einstein (1949), p. 33.

Without publishing on the subject, Møller kept an interest in thermodynamics and statistical physics over the years. At some stage he even contemplated to write a book on this branch of classical physics together with Rosenfeld, such as evidenced in a letter of 1941: "In the fall semester I shall give lectures on thermodynamics and statistical mechanics, which has caused me to think again about these problems. It is with some sadness that I recall the draft to the introduction of a book on statistical mechanics which lies on my desk and which we wrote for almost five years ago. When will we be able to continue this work? And when will it be completed??"[48] However, the planned Møller-Rosenfeld book on statistical mechanics never materialised. On the other hand, Møller eventually wrote down the lectures on statistical mechanics that he gave to students in Copenhagen. In these mimeographed notes, effectively a condensed textbook, he covered classical and quantum statistical mechanics restricted to the non-relativistic domain.[49] He only took up thermodynamics and statistical mechanics as a research topic in the mid-1960s and then in connection with the relationship of these fields to the theory of relativity.

This was an old problem which had occupied a minority of physicists ever since Einstein formulated his special theory of relativity. It was first attacked by Max Planck in 1907 and the same year also by Einstein in a review paper on his new theory.[50] The task was to find Lorentz transformations for thermodynamic quantities (such as heat, entropy, pressure, and absolute temperature) which secured form-invariance of the thermodynamic laws. It turned out that such transformations exist, with the implication that thermodynamics is consistent with the requirements of special relativity. Had it not been the case, physicists would be forced to look for modifications of the laws of thermodynamics – or perhaps of relativity. Planck

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48. Møller to Rosenfeld, 2 August 1941 (RP, in Danish).

49. Møller, *Forelæsninger Over Statistisk Mekanik*, 145 pp. (Copenhagen: University of Copenhagen, 1962). Independently of Møller, also Rosenfeld dealt with foundational aspects of statistical thermodynamics. See Rosenfeld (1979), pp. 762-807.

50. For the early history of relativistic thermodynamics, see Liu (1992), Liu (1994), and Lacki, Ruegg, and Wanders (2009), pp. 101-116.

concluded that the exchanged heat ΔQ in a system moving at constant speed v relates to the same quantity in the system at rest ΔQ_0 by the simple formula

$$\Delta Q = \Delta Q_0 \sqrt{1-\beta^2}$$

where $\beta = v/c$. He found a similar formula for the temperature,

$$\Delta T = \Delta T_0 \sqrt{1-\beta^2}$$

For the entropy of a body in thermal equilibrium Planck concluded that it is Lorentz invariant, $S = S_0$ in accordance with the definition of entropy as $\Delta S = \Delta Q/T$. Using arguments different from those of Planck, Einstein arrived at the same set of formulae in his article published in Johannes Stark's *Jahrbuch der Radioaktivität und Elektronik*.

What may be called the Planck-Einstein theory of relativistic thermodynamics was generally accepted for more than half a century. Thus, in his tenth Josiah Willard Gibbs Lecture delivered on 29 December 1932, Richard Tolman obtained the same transformation formulae as proposed by Planck and Einstein. For the validity of these formulae he appealed to their "qualities of rationality and coherence", whereas "any direct test of the extension would for the present be out of the question since ... we could only expect differences of this practically undetectable order $[v^2/c^2]$ for any thermodynamic theory of moving systems that might be proposed."[51] Tolman also discussed the laws of thermodynamics in the light of general relativity, and in this case he was led to a major revision of the second law and its cosmological consequences, namely that the heat death resulting from the growth of entropy was not inevitable. In his textbook published the following year Tolman repeated and reinforced this conclusion.

In his 1952 relativity textbook Møller included brief sections on thermodynamics in which he reproduced the earlier transformation formulae. He commented, "As shown by Planck and Einstein, the usual laws of thermodynamics may be easily incorporated in the

51. Tolman (1933), p. 293.

special theory of relativity."[52] However, at around 1966 Møller became aware of a remarkable paper published a few years earlier by the German physicist Heinrich Ott, a professor at the University of Würzburg and a former assistant of Arnold Sommerfeld. Ott died on 26 November 1962, at a time when his posthumous paper had not yet been submitted to *Zeitschrift für Physik*.[53] He argued that $S = S_0$ was the only correct one of the Planck-Einstein formulae, whereas those for heat and temperature had to be replaced by

$$\Delta Q = \frac{\Delta Q_0}{\sqrt{1-\beta^2}} \quad \text{and} \quad \Delta T = \frac{\Delta T_0}{\sqrt{1-\beta^2}}$$

Thus, the temperature of the moving body is higher than that of the body at rest, contrary to the earlier Planck-Einstein result. At first Ott's new relativistic thermodynamics went unnoticed, but when the French physicist Henri Arzéliès two years later independently came to the same formulae, a large number of papers on the subject began to appear in the journals *Nuovo Cimento* and *Nature*. Some physicists supported Planck-Einstein, others Ott-Arzéliès, and others again came up with alternative proposals. For example, the German-born English physicist Peter Landsberg argued that the temperature is Lorentz invariant, $\Delta T = \Delta T_0$.[54]

Møller seems to have been dissatisfied with the confusing state of affairs and the lack of mathematical and conceptual rigor in many of the papers. He suggested that "the disagreement between the different authors was largely due to a different use of words like heat, work, energy, etc."[55] But it was not only a matter of semantics, for Møller was convinced that whereas the Ott-Arzéliès formulation was correct, the Planck-Einstein formulation was incorrect. He decided to present a detailed, comprehensive and authoritative theory of relativistic thermodynamics based on Ott's insight.

Møller found time to do this when he was invited to Leiden to spend the autumn of 1966 as a Lorentz Professor. This chair for

52. Møller (1952), p. 211.
53. Ott (1963), submitted 11 January 1963.
54. Landsberg (1966).
55. Møller (1968a), p. 203.

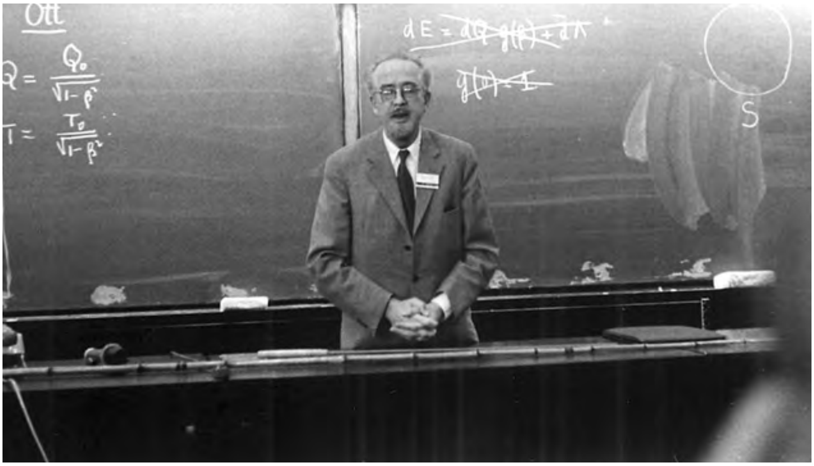

Fig. 31. Møller lecturing on relativistic thermodynamics at a Nordita conference in 1967. Credit: Niels Bohr Archive, Copenhagen.

eminent theoretical physicists was established in 1955, the centenary of Lorentz' birth, with previous holders including Wheeler, Wigner, Heitler, and Klein. The result of his work in Leiden was an extensive memoir published the following year by the Royal Danish Academy. It carried the subtitle 'A Strange Incident in the History of Physics', but as usual, Møller was not concerned with history as such but solely with technical and conceptual clarification.[56] In this memoir he formulated a generalised form of the first law and derived transformation formulae in agreement with those found by Ott. Møller was invited to a meeting on 'Relativistic Statistical Mechanics and Thermodynamics' arranged by Ilya Prirogine in Brussels 8-11 May 1968, and on this occasion he discussed his new paper with Prirogine and others of the participants.[57] He brought with him to Brussels the Norwegian physicist Iver Brevik, who worked with him as a Nordita fellow and had written a treatise on relativistic thermodynamics in which he further developed some of

56. Møller (1967). Møller reused the essay in a couple of later papers, for example Møller (1969a).

57. Møller to Prirogine, 8 February 1968 (CMP).

Møller's results.[58] Møller knew Prirogine, a Russian-born Belgian physicist, well from their common work in the Solvay institution. Nine years later Prirogine would be awarded the Nobel Prize in chemistry for his contributions to non-equilibrium thermodynamics and dissipative structures.

In a festschrift of 1968 to the Italian physicist Gilberto Bernardini, Møller went beyond special relativity and discussed for the first time the laws of thermodynamics, whether applying to reversible or irreversible processes, when gravitational fields are present.[59] This problem had previously been considered by Tolman in his 1934 book, and Møller showed that Tolman's main results could be obtained from a more general theory. As he pointed out, Ott's formulation (but not other formulations) could rather easily by accommodated within the framework of general relativity. Naturally, Møller took advantage of the new results in the 1972 edition of his textbook, which contained a revised and fuller exposition of relativistic thermodynamics.[60] Apart from correcting the formulae for heat and temperature transformations, he extended the treatment considerably. As a special case he derived in great detail the law of black-body radiation from the relativistic theory of thermodynamics. Although Møller's memoir was authoritative, it did not provide the final answer to the problem of which set of relativistic transformation formulae is the correct one. The problem continued to be debated, with few of the debaters referring to Møller's works on the subject. Still today, the problem is controversial.[61]

Instead of basing thermodynamics on energy and entropy as isolated variables, in the late nineteenth century Helmholtz, Joshua Willard Gibbs, Pierre Duhem, and a few others formulated the theory in terms of more abstract 'potentials'. In a memoir of 1969 on thermodynamic potentials from the point of view of relativity theory, Møller commented on the debate concerning the correct transformation of temperature. "The violent discussions in the lit-

58. Brevik (1967).

59. Møller (1968a).

60. Møller (1972), pp. 232-248. Tolman (1934), pp. 291-330.

61. See the review in Farias, Pinto, and Moya (2017).

erature following Ott's paper", he wrote, "have made it clear that the relativity principle alone does not lead to a unique concept of temperature relative to an arbitrary system *S*, for the transformation law for the temperature will depend on which of the classical thermodynamical relations holding in the rest system, are assumed to retain their form under Lorentz transformations."[62] In a letter to the Turkish-American physicist Asim Barut, he wrote: "The transformation properties of the temperature in a Lorentz system depends on the *definition* of temperature in a Lorentz system where the body is moving. ... Since we are completely free to choose our definitions, one can ask if Planck's definition is the most practical."[63]

Møller formulated this insight more elaborately in his textbook, where he argued that the laws governing relativistic thermodynamics are theoretically as well as empirically underdetermined. For this reason, the chosen system of laws must depend on extrascientific criteria:

> From this principle [of relativity] we may conclude only that the classical laws of thermodynamics are valid in the momentary rest system S^0 of the matter, independently of the motion of this system with respect to the fixed stars. However, there is a wide spectrum of possible ways of describing relativistic thermodynamics in any other system *S*, since the basic laws may be assumed in a rather arbitrary way to depend explicitly on the velocity of the matter relative to *S*. In this situation we must have recourse to *arguments of simplicity and convenience*.[64]

In his 1969 memoir Møller also pointed out, such as Rosenfeld did in greater detail, that the statistical theory of thermodynamics provided what he called an instructive example of complementarity in classical physics: "Energy and pressure are complementary to temperature and volume, respectively, in much the same way as

62. Møller (1969b), p. 5.

63. Møller to Barut, 12 February 1968 (CMP).

64. Møller (1972), p. 233. Emphasis added. Møller's argument in favour of extrascientific criteria was similar to the one he had proposed in the case of the energy-momentum of refracting media (Section 6.3).

momentum and position of a particle in quantum mechanics. ... In principle, and in special cases also in praxis, the recognition of this complementarity is of importance for the understanding of the properties of thermodynamical systems."[65] Møller's view echoed that of Bohr, who much earlier, in his Faraday Lecture of 1930, said:

> This situation [in statistical thermodynamics] presents a remarkable analogy with the peculiar irreversibility characteristic of the description in quantum mechanics. ... In thermodynamics as well as in quantum mechanics, the description contains an essential limitation imposed upon our control of the events which is connected with the impossibility of speaking of well-defined phenomena in the ordinary mechanical sense.[66]

The relativistic version of thermodynamic potentials was the subject of some of Møller's later papers and addresses. For example, in the autumn of 1971 he was invited to the physics department of Queen Mary College, London, where he gave a talk at the theoretical physics seminar on 'Thermodynamic Potentials in the Theory of Relativity'. During his stay in London, he participated in a meeting at the Royal Society and even found time for some non-scientific activity, such as he reported after having returned to Copenhagen:

> The last day of our stay in London, my wife and I followed the suggestion of one of your collaborators (I think it was Gupta) to go to the Mayfair Theatre and see 'The Philanthropist'. It was marvellously played and a very witty, and sometimes even profound, play. Unfortunately, I completely forgot the name of the author who is obviously a young man from whom we can expect good things in the future.[67]

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65. Møller (1969b), p. 21. Møller (1968b) was an in-depth investigation of the relativistic version of Gibbs' statistical mechanics.
66. Bohr (1932), pp. 376-377. On Bohr's view on complementarity between energy and temperature, see Lindhard (1986).
67. Møller to J. G. Valatin, 2 November 1971 (CMP). Møller's intuition was right. *The Philanthropist* was written by the 26-year-old Christopher Hampton, later a well-known playwright. Møller knew the Hungarian-born physicist Jean Valatin, who spent part of 1950-1952 in Copenhagen.

As we shall see shortly, Møller's revised textbook on relativity theory contained a much extended and updated treatment of cosmological models based on Einstein's field equations. Remarkably, in this context he did not discuss or even mention the second law of entropy increase, which played a most significant role in Tolman's work. If the second law were assumed to be valid for the universe as a whole, the consequence would be the notorious 'heat death' of the universe in the far future first discussed by Clausius in the 1850s. As entropy increases endlessly, all organisation and structures in the universe will disappear irreversibly, of course life included. However, Tolman argued that if the relativistic form of thermodynamics were applied to the universe, it would lead to results very different from those of classical thermodynamics. As he phrased it, "It would seem wisest, if we no longer dogmatically assert that the principles of thermodynamics necessarily require a universe which was created a finite time in the past and which is fated for stagnation and death in the future."[68] While the question of cosmic entropy was considered important among cosmologists and also attracted much public concern, Møller chose to ignore it.

Neither Møller nor other contributors to the debate about relativistic thermodynamics in the late 1960s had any idea that Einstein had obtained Ott's results as early as 1952. Nor could they have known, for Einstein only communicated his insight in letters to his old friend Max von Laue, who had recently published a revised version of his *Relativitätstheorie*, a classic treatise on relativity theory. Einstein read the book and on 27 January 1952 he wrote to Laue, "I cannot agree with your formula for the transformation of the absorbed heat G (and of temperature)."[69] After a brief non-mathematical argument based on a thought experiment, Einstein derived '$G/G_0 = T/T_0 = 1/\sqrt{1 - v^2/c^2}$ (but *not* $\sqrt{1 - v^2/c^2}$)' to which he added: "I have not studied your book precisely enough in order to see where the difference comes from. This consideration is so simple, that I can hardly imagine that it contains any mistake."

68. Tolman (1934), p. 444.

69. The correspondence between Einstein and Laue is reproduced in Liu (1992). Einstein's G corresponds to Q.

Apparently, Einstein did not contemplate to publish his result or correct in public his own mistake made forty-five years earlier.

In a later letter to Laue, Einstein followed up his remarks on relativistic thermodynamics, which he now saw in a new light. "I hear the voice of my conscience when I remind you of the dispute concerning the rendering of the fundamental thermodynamic concepts in the special-relativistic form. There is actually no compelling method in the sense that one view would simply be 'correct' and another 'false'. One can only try to undertake the transition as naturally as possible."[70] From this more conventionalist perspective Einstein found it justified to treat also the heat and temperature as invariants, $\Delta Q = \Delta Q_0$ and $\Delta T = \Delta T_0$, a change which would still retain the invariant nature of the entropy. This was what Landsberg proposed in public thirteen years later.

7.3. Gravitation and cosmology

Studies of the universe as a whole and its evolution in time were not a subject which attracted much interest at Bohr's institute or, after 1957, at its associated Nordita research group. Nor was cosmology cultivated as a research area by Danish astronomers and astrophysicists. In early December 1932, the British astrophysicist and cosmologist E. Arthur Milne visited Bohr's institute, where he gave a colloquium on his radically new, non-relativistic or 'kinematic' theory of the expanding universe. On the same occasion he also gave an evening lecture on solar astrophysics to the Danish Astronomical Society (*Astronomisk Selskab*) founded in 1916. While in Copenhagen, Milne met Bohr, Strömgren, Chandrasekhar, and other physicists, among them possibly also Møller.[71] None of the Copenhageners felt tempted to follow up Milne's cosmological alternative or otherwise to work on problems of cosmology.

The leading figure in post-World War II physical cosmology was George Gamow, whom Møller knew well from his earlier vis-

70. Letter of 2 March 1953, reproduced in Liu (1992).

71. See Rebsdorf (2005), pp. 194-195, and Weston Smith (2013), p. 161. Milne to Bohr, 8 February 1933 (NBA, BSC).

its in Copenhagen and whom he had met again at the June 1938 Warsaw-Cracow conference. But it was only a few years later that Gamow turned from astrophysics to cosmology and began developing the theory of the early universe which eventually came to be known as the big-bang theory. The very first indication of Gamow's big-bang program appears in a remarkable letter to Bohr of 24 October 1945 in which Gamow congratulated him with his sixtieth birthday. Gamow wrote:

> It would be really so much nicer if one could begin to work again on pure science without the heavy clouds hanging in the air! That is what I am trying to do at present studying the problem of the origin of elements at the early stages of the expanding universe. It means bringing together the relativistic formulae for expansion and the rates of thermonuclear and fission reactions. One interesting point is that the period of time during which the original fission took place (as estimated from the relativistic expansion formulae) must have been less than one millisecond.[72]

Several years later, after he and his assistants Ralph Alpher and Robert Herman had largely completed the ambitious theory, on 13 April 1951 Gamow was elected a foreign member of the Royal Danish Academy. Wanting to use the proceedings of the Academy to present his newest ideas of galaxy formation in an expanding universe, Gamow communicated with Møller on the matter. "I am missing the good old Køpenhown [Copenhagen], but I am afraid of coming so close to the Iron Curtain", he wrote. In late 1952 Møller presented Gamow's paper at an academy meeting in Copenhagen and in February the following year it was published in the proceedings.[73]

Møller was thus updated on and peripherally involved in the most recent developments in big-bang cosmology, but he merely helped Gamow without endorsing the theory or even expressing interest in it. His brief review of cosmological models in his 1952

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72. Gamow to Bohr, 24 October 1945 (BSC). Kragh (1996), pp. 106-107.

73. Gamow to Møller, 19 November 1952 (CMP). Gamow (1953), submitted 21 October and read to the Academy by Møller.

textbook did not refer to Gamow's model, which was also absent from his revised 1972 version. Møller was primarily interested in the mathematical aspects of cosmology, whereas he tended to ignore the physical aspects relating to nuclear and quantum physics. But of course, he was aware of the works of Gamow and his collaborators. For example, in late 1967 he received a letter in which Gamow announced the latest version of his big-bang model and asked Møller for assistance in solving some of the mathematical problems related to it. Møller responded that he had discussed Gamow's work with "Bengt Strömgren, who is now back from Princeton and lives in Bohr's former house at Carlsberg."[74]

When Møller came to deal with cosmology, which he did on some occasions, it was closely related to his abiding interest in the theory of general relativity. In his later years he was much concerned with the problem of gravitational collapse not only of stellar bodies but also in relation to the closed universe. But first a condensed review of some of the high points in cosmological research from about 1940 to the early 1970s.[75]

Einstein's cosmological theory of 1917 was based on a set of canonical field equations, which in a slightly modernised formulation can be written as

$$G_{ik} - \Lambda g_{ik} = \frac{8\pi G}{c^2} T_{ik}$$

The quantity to the left, the Einstein tensor G_{ik}, expresses the geometry of space-time, g_{ik} is the metric tensor, and T_{ik} the energy-momentum tensor. For the cosmological constant Λ, Einstein found that in his static model of the universe it must have a precisely fixed value, namely

$$\Lambda = \frac{4\pi G}{c^2} \rho$$

74. Gamow to Møller, 19 December 1967 (CMP), reproduced *in extenso* in appendix I. Møller to Gamow, 28 January 1968 (CMP). The paper in question was Alpher, Gamow, and Herman (1967) in which the three authors argued for a spatially hyperbolic universe of age approximately 9.3 billion years.

75. For extensive reviews, see Kragh (1996) and Peebles (2020).

where ρ is the average density of matter, a quantity which Einstein originally grossly overestimated to be approximately 10^{-22} g/cm^3.

In an important but initially ignored paper of 1927, the Belgian physicist Georges Lemaître developed from Einstein's cosmological field equations a model of the closed universe which expanded in time and agreed with the few galactic redshifts $\Delta\lambda/\lambda$ known at the time. According to Lemaître, the redshifts were solely due to the expansion of space and increased approximately linearly with the distance to the galaxies. For the constant of proportionality – what came to be known as the Hubble constant or parameter H – he obtained a value of about 600 km/s/Mpc. The expanding universe only became generally known in 1930, when it was realised that Edwin Hubble's new redshift-distance data could best be understood on the basis of Lemaître's theory or the earlier and similar one of the deceased Russian physicist Alexander Friedman. Contrary to what is often stated, Hubble did not discover the expanding universe and never claimed so. In fact, throughout his life he remained undecided of whether the universe is expanding or not. In a paper of 1931 Lemaître went further by arguing that the universe had originally come into being a finite time ago in the radioactive explosion of what he called a 'primeval atom'. This first model of the big-bang universe was generally ignored or dismissed by leading astronomers.

Another early and much more influential model formally belonging to the big-bang category was proposed by Einstein and Willem de Sitter, who in a joint paper of 1932 considered a homogeneous flat model universe with no pressure term and characterised by a critical matter density given by

$$\rho_c = 3H^2/8\pi G$$

In terms of the dimensionless density parameter $\Omega = \rho/\rho_c$, the geometry of the Einstein-de Sitter universe was later described as $\Omega = 1$. In his 1972 textbook on relativity theory, Møller formulated the Einstein-de Sitter model as follows: "It starts with a 'big bang' at $t = 0$, where $R = 0$ and $\dot{R} = \infty$. At the present time t_0, $H_0 = H(t_0)$ is the observed Hubble coefficient, and for the 'age' of the universe

t_0 we get ... $t_0 = 2/3\,H_0 \cong 2.7 \times 10^{17}$ s $\cong 9 \times 10^9$ years. ”[76] Møller's value of the Hubble parameter corresponds to $H_0 = 75$ km/Mpc/s, which by chance agrees well with the modern value of 73.3 ± 5 km/Mpc/s.

While the cosmological constant Λ introduced by Einstein in his 1917 field equations played a crucial role in Lemaître's work on cosmology (and also in Eddington's), in the Einstein-de Sitter model it was taken to be zero. For more than half a century $\Lambda = 0$ remained the consensus view among physicists and astronomers, and only by the late 1990s did the controversial constant return to mainstream cosmology, now as an indispensable and fundamental constant of nature, an expression for the energy density of empty space. Møller, who seems to have shared the $\Lambda = 0$ consensus view, commented by paraphrasing Einstein: “If the Hubble expansion had been discovered at the time of the creation of the general theory of relativity, the cosmological term would never have been introduced.”[77]

Although the idea of the big-bang theory can thus be found in the early 1930s, it was only with the works of Gamow and his collaborators Alpher and Herman in the late 1940s that big-bang cosmology was established as a quantitative and testable theory. The aim of the three physicists was to calculate from thermonuclear reactions in the very early universe the present abundance distribution of chemical elements in the universe. Although they only succeeded in the case of helium, where they found approximately 30% by mass, in a paper of 1948 Alpher and Herman derived from the theory that space must presently be filled with a blackbody-distributed cosmic microwave background of a temperature of about 5 K. The later discovery of this background radiation was a watershed in cosmology, but during the 1950s the predicted microwave background played almost no role at all. In fact, the Gamow-Alpher-Herman big-bang theory was unsuccessful and came to a halt in 1954 only to be resurrected a decade later.

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76. Møller (1972), p. 528. The symbol $\dot{R}$ denotes the time derivative of the scale factor R. The term 'big bang' was introduced by Hoyle in 1949 but only used sparingly in the scientific literature until the 1970s.

77. The quote is almost identical to Einstein (1956), p. 127.

While the Gamow theory and also the older Einstein-de Sitter theory built on the equations of general relativity, through the 1950s relativistic evolution cosmology faced stiff competition from the steady-state theory proposed in 1948 in two different versions, one by Fred Hoyle and the other by Hermann Bondi and Thomas Gold. The basic claim behind the classical steady-state theory was the so-called 'perfect cosmological principle', namely that the universe is not only spatially but also temporally homogeneous: on a very large scale it looks the same at any point and at any time. It follows that there is no beginning of the universe and no end either. To make this requirement agree with the observed cosmic expansion, the three physicists assumed that matter is continually created through space with a creation rate given by

$$dm/Vdt = 3\rho H = \text{ ca. } 10^{-43}\ \text{g/s}/\text{cm}^3$$

The theory did not predict the form of the new matter, but it was generally assumed to be hydrogen atoms or protons plus electrons.

The steady-state matter creation was *ex nihilo*, not creation out of energy ($m = E/c^2$), and for this reason it violated one of the most fundamental laws of physics, the principle of energy conservation. It was a major reason why mainstream cosmologists such as Gamow, Robertson, Tolman, and Lemaître rejected the steady-state theory without examining it seriously. Nonetheless and contrary to the diverse class of relativistic models, the theory had the methodological advantage that it led to several precise and testable predictions. One of them was a critical and constant matter density $\rho_c = 3H^2/8\pi G$ and another that cosmic space is Euclidean and expanding at a definite rate given by

$$R(t) = R_0 \exp(Ht)$$

Contrary to the situation in relativistic cosmology, the Hubble constant H appearing in the steady-state theory was a true constant which had nothing to do with the age of the universe, which of course was infinite. It follows from the $R(t)$ expression that the deceleration parameter q_0 defined as

$$q_0 \equiv -\left(\ddot{R}/RH\right)_0$$

Fig. 32. The 1958 Solvay congress on astrophysics, gravitation, and cosmology. Sitting at the table from the left: W. McCrea, J. Oort, G. Lemaître, C. Gorter, W. Pauli, W.L. Bragg, J.R. Oppenheimer, C. Møller, H. Shapley, and O. Heckmann. Standing from the left: O. Klein, W. Morgan, F. Hoyle, B.V. Kukarkin, H.C. van de Hulst, M. Fierz, A. Sandage, W. Baade, J. Wheeler, H. Bondi, T. Gold, H. Zanstra, L. Rosenfeld, L. Ledoux, A.C.B. Lovell, J. Géhéniau. https://commons.wikimedia.org/wiki/File:Solvay_conference_1958_g.jpg

is equal to –1. For the flat Einstein-de Sitter universe the same quantity is +½. The deceleration parameter can be found from galactic redshift-magnitude measurements and yields a determination of the space curvature constant, which is $k = -1$ for the steady-state universe. The method indicated which world models were ruled out by astronomical measurements and which not, but for a long time it failed to provide sharp results for q_0. To make a long story short, it was only in the early 1960s that a new method based on counts of radio sources brought the steady-state theory in serious troubles. A few years later, the discovery of the cosmic background radiation, and also measurements of the helium content of the universe and the distribution of quasars, provided convincing evidence that we live in a hot big-bang universe governed by the laws of general relativity without – or apparently without – a cosmological constant. Møller said about the steady-state theory that its lack of a beginning of the universe was an "attractive feature", but on the other hand it had the severe disadvantage of not complying with Einstein's field equations.[78]

78. Møller (1972), pp. 529-533.

The revolution in cosmology did not occur instantly, but at around 1970 it was completed, and the steady-state alternative no longer taken seriously by mainstream cosmologists. According to the Russian physicist Yakov Zeldovich and his collaborator Igor Novikov, the revolution confirmed the universality and supreme status of Einstein's theory. In a textbook originally published in Russian in 1975, they wrote:

> There are no observational data suggesting a limitation of GTR [general theory of relativity] to the scales of the Universe. Therefore, the assumption that a change in GTR is needed in applications to cosmology is unfounded. Thus, the aggregate of theoretical, experimental, and observational facts stands in favor of the applicability of the physical laws and GTR to a description of the Universe from *almost* the very beginning of the expansion. They apply from times when the matter density is much greater than the density of nuclear matter, $\rho > 10^{14}$ g/cm^3, up to the present time.[79]

Although Møller certainly shared the boundless admiration of general relativity, at about the same time he worried about some of the consequences of standard general relativity, which made him propose a new formulation with the purpose of avoiding the initial singularity in which the universe was supposedly born.

As a member of the Solvay scientific committee, Møller participated in the eleventh congress taking place in Brussels between 9 and 13 June 1958. The congress was originally scheduled for September 1957, but on the suggestion of Lawrence Bragg, who served as president of the scientific committee, it was decided to postpone it so that the participants could combine it with a visit to the large international exhibition in Brussels known as Expo 58.[80] Other committee members present in Brussels were Oppenheimer, Pauli, and the French physicist Francis Perrin, a son of the Nobel laureate Jean Perrin. Pauli was at the time at good health, but he

79. Zeldovich and Novikov (1983), p. xxi.

80. Bragg to Møller, 25 January 1957 (CMP). The Brussels World Fair was held from 17 April to 19 October 1958.

died about half a year later by pancreatic cancer. The theme of the congress, 'The Structure and the Evolution of the Universe', was innovative insofar that it was the first international conference ever devoted to cosmology, a science which at the time was in a state of transformation.[81] Among the speakers at the 1958 Solvay congress were the leading steady-state advocates Hoyle, Bondi, Gold, and McCrea, whereas Lemaître was alone in defending the big-bang theory in his own primeval-atom version.

Quite remarkably, at least as seen in hindsight, Gamow's more advanced nuclear-physical version of the big bang and the early universe played no role at all in the discussions in Brussels. It was not even mentioned. Gamow, who had very much wanted to participate, was not invited to the Solvay congress, which he interpreted as a result of his uncompromising opposition to the European steady-state cosmology.[82] There may have been other reasons why Gamow was not invited, such as euphemistically suggested in a letter from Pauli to the Swiss physicist Jean Weigle: "You remember, that we talked about the fact, that he [Gamow] was *not* invited to the Solvay-meeting in Brussels. Now I just returned from there and heard the true reason for it: there is some trouble with the general conditions of his health, about the details I would prefer to talk rather than to write. I am very sorry for him."[83]

The thirteenth and fifteenth Solvay congresses of 1964 and 1973, respectively, also dealt with cosmological issues, the first being on 'Structure and Evolution of Galaxies' and the second on 'Astrophysics and Gravitation'. Møller participated in both conferences, in the first of them with Heisenberg and Strömgren among others, and in the second with Rosenfeld, Roger Penrose, and others. The twelfth congress of 1961 with the theme 'Quantum Field Theory' was the last one in which Bohr participated. He contributed with a

81. Solvay (1958). Mehra (1975), pp. 381-387.

82. Gamow (1970), pp. 124-126.

83. Pauli to Weigle, 16 June 1958, in Pauli (2005), p. 1208. The problem that Pauli did not want to write about was Gamow's excessive consumption of alcohol which on occasions led to embarrassing scenes at meetings and conferences. See Kragh (1996), p. 139, and Harper (2001), p. 367.

valuable historical report of the Solvay meetings since their start in 1911.[84] The theme of the fourteenth congress held in 1967 was 'Fundamental Problems in Elementary Particle Physics'. As president of the scientific committee since 1962 Oppenheimer was supposed to participate, but he passed away on 18 February 1967. His place as acting president was taken by Møller, who in Brussels rendered an homage to the memory of the great American physicist.

To return to the 1958 meeting, Hoyle gave an address on his field-theoretical formulation of the steady-state theory in which he suggested an explanation of the continual creation of matter without violating the principle of energy conservation: "The process of creation can … be thought of as involving no energy expenditure – a particle is created at a negative [gravitational] potential that compensates for its rest mass. Accordingly [*sic*] to quantum theory, particle creation might well be expected under these circumstances."[85] Following Hoyle's report, Møller asked him if his expression for the energy-stress tensor and its associated 'creation tensor' $C_{\mu\nu}$ was phenomenological or "ultimately should be derivable by considering suitable elementary processes in which matter is created." Hoyle answered that his expression was indeed phenomenological and had to be so, "so long at the whole gravitational theory remains outside modern particle physics … After all, the whole gravitational theory is really phenomenological!" Møller definitely disagreed with the last statement.

With regard to energy conservation on a cosmological scale, Møller used the occasion to announce a result he had recently derived within the context of general relativity theory:

> Contrary to usual beliefs it is even possible to define a consistent expression for the total energy density consisting of a matter part and a gravitational part. If this expression for the energy density, which is given in a paper appearing in the next number of *Annals of Physics*, is applied to the case of the metric for a homogenous and isotropic

84. 'The Solvay Meetings and the Development of Quantum Physics', reprinted in Bohr (1999), pp. 431-454.
85. Solvay (1958), p. 57. On Hoyle's argument, see Kragh (1996), p. 212.

> universe one finds that the energy density is zero everywhere and at all times. This means that the positive matter energy is constantly counterbalanced by a corresponding amount of negative gravitational energy. So also with ... a continuous creation of matter we will certainly have conservation of the sum of matter energy and gravitational energy for any finite region in space.[86]

In his 1958 paper mentioned in Section 7.1 Møller found from his expression for the energy density that the total energy of a closed universe is zero in the sense he reported in Brussels.[87] Although Møller was not the first to present the case of a zero-energy closed universe, he may have been the first to derive the result rigorously from the equations of general relativity.

The idea has a curious and little-known history, which is often misrepresented in scientific texts. Probably unknown to Møller and other contemporary physicists, the idea first appeared in a brief 1936 paper by the Austrian-American physicist Arthur Haas, who proposed that the universe was born with zero total energy and would remain in such a state in agreement with the law of energy conservation. A somewhat similar argument can be found even earlier in Tolman's 1934 textbook. Citing Haas' paper, in 1939 Jordan gave a more elaborate argument based on his unorthodox idea of cosmic creation of new matter. According to Jordan, if new matter m was created, its energy was compensated for by the increase in negative potential energy. With M denoting the mass of the universe within the Hubble radius $R = c/H$, he expressed the requirement as

$$mc^2 - \frac{GmM}{R} = 0$$

Many years later, in 1973, the American physicist Edward Tryon once again rediscovered the zero-energy universe, this time in the context of speculative quantum cosmology. Tryon was unaware of

86. Solvay (1958), p. 74.

87. Møller (1958b), pp. 364-368, who acknowledged private communications with Charles Misner.

the early works of Tolman, Haas, and Jordan, and also of the work of Møller preceding his own work with fifteen years.[88]

According to Mach's principle, in one of its several versions, the space-time metric is determined by the mass of the universe. Mach's principle played an important role in research on gravitation and cosmology in the 1960s, when it served as a foundation of the so-called scalar-tensor theory developed in particular by Carls Brans and Robert Dicke at Princeton University. A theory of this kind was originally developed by Jordan and is for this reason also known as the Jordan-Brans-Dicke theory. The Brans-Dicke theory was largely abandoned at about 1980, when measurements proved that the oblateness of the Sun agrees much better with the Einstein theory of gravitation than the rival Brans-Dicke theory.[89] Based in part on Mach's principle the two American physicists argued that Einstein's theory of general relativity was only approximately correct. The much-discussed Brans-Dicke theory dating from 1961 included as a central feature that the gravitational constant G was not a true constant but a quantity decreasing very slowly over cosmic time (of the order $G \sim t^{-0.1}$, where t is the age of the universe).

Møller was aware of the Brans-Dicke theory at an early date but without endorsing it. At the 1961 Varenna meeting on gravitational theories, he listened to Dicke's comprehensive lecture on Mach's principle and its role in gravitation theory. According to Dicke, the principle "is based on a logical positivist philosophical stance [and asserts] that physical concepts must be based on operational definitions through measurements."[90] While Møller disregarded Mach's principle in his early works on general relativity, he briefly commented on it in his textbook of 1972. As he showed, it follows from standard general relativity that the rest mass of a particle de-

88. Tryon (1973). According to the Wikipedia article on the zero-energy universe: "The first known publication on the subject was in 1973, when Edward Tryon proposed in the journal *Nature* that the universe ... [has] its positive mass-energy being exactly balanced by its negative gravitational potential energy." https://en.wikipedia.org/wiki/Zero-energy_universe.

89. For the origin and early development of scalar-tensor theories, see Goenner (2012) and Kragh (2016), pp. 44-58.

90. Dicke (1962), p. 32.

pends on the gravitational field of nearby masses, and this he saw as "an indication that there is some truth in Mach's ideas." However, having referred to the Brans-Dicke theory and related ideas, he refrained from going further: "Attempts having been made in recent years to incorporate Mach's ideas fully into a generalized version of Einstein's theory, but this work goes beyond the scope of the present book."[91] He evidently referred to the theories of Jordan, Brans, Dicke, and their followers.

The problem of space-time singularities as solutions to the equations of general relativity attracted attention early on, in particular in connection with the German astronomer Karl Schwarzschild's exact solution of 1916 for a uniform spherical mass M. In this case there exists an 'exterior singularity' at a distance from the centre given by

$$R_{\mathrm{S}} = 2GM/c^2$$

which was later understood as a surface or horizon from which light cannot escape. The Schwarzschild singularity was reconsidered by Lemaître in an important but little-known paper of 1933, in which he showed that it could be brought to disappear by means of a suitable coordinate transformation and thus was apparent only. Both in a cosmological and an astrophysical context the question concerned the validity of the relativistic field equations at very high density of matter and space curvature. According to Einstein, "for large densities of field and of matter, the field equations and even the field variables which enter into them will have no real significance. One may not therefore … conclude that the 'beginning of the expansion' [of the universe] must mean a singularity in the mathematical sense."[92]

Einstein and most others believed that the singularities were artefacts due to unrealistic assumptions of homogeneity and isotropy and that they could be avoided in less idealised models. However, in 1955 the Indian physicist Amalkumar Raychaudhuri at the Univer-

91. Møller (1972), p. 383.

92. Einstein (1956), p. 129. See Earman (1999) for a detailed historical examination of the singularity problem in general relativity theory.

sity of Calcutta argued that the cosmic singularity was real, meaning that it is a consequence of general relativity. His and others' work on the subject culminated in the mid-1960s with a series of singularity theorems proving from general relativity the existence of space-time singularities. In 1965 Roger Penrose proved that a gravitationally collapsing star will inevitably end in a singular state, turn into a black hole, and slightly later Stephen Hawking extended the result to apply also cosmologically. With the singularity theorems of Penrose, Hawking, Robert Geroch, and others the embarrassing singularities were brought back on stage, forcing physicists somehow to make sense of them. A singularity is a non-physical object, so how can it result from the fundamental physical theory of relativity as a necessary consequence?

The relation of the Schwarzschild singularity to the gravitational collapse of a massive star was pointed out by Oppenheimer and Hartland Snyder in a paper of 1939, which today is recognised as a pioneering work of black-hole physics. According to the two American physicists, the result of the collapse of a sufficiently massive star was that it "tends to close itself off from any communication with a distant observer; only its gravitational field persists."[93] However, it took more than two decades before the subject attracted wide interest and physicists began to speculate whether black holes are more than theoretical constructs. Do they belong to the fabric of nature? Wheeler introduced the name 'black hole' in 1967 and seven years later Hawking famously argued that black holes must have a temperature and therefore emit blackbody radiation. The so-called Hawking temperature for a black hole of mass M is

$$T_{\mathrm{H}} = \frac{\hbar c^3}{8\pi G M k_{\mathrm{B}}}$$

where k_{B} is Boltzmann's constant. It follows that a black hole must eventually evaporate, although the decay time for stellar bodies will be exceedingly long (of the order 10^{66} years or more). So-called white holes are time-reversed versions of black holes, hypothetical bodies emerging spontaneously from a singularity, such as theorised

93. Oppenheimer and Snyder (1939), p. 456.

by Novikov in 1964. However, contrary to their black counterparts, white holes are not believed to exist in nature.

In 1972, the same year that the second edition of his textbook was published, Møller attended a large symposium in Trieste celebrating Paul Dirac's seventieth birthday. There he met again with a panoply of physics celebrities such a Chandrasekhar, Peierls, Salam, Wheeler, Heisenberg, and Schwinger – not to mention the now septuagenarian Dirac. Møller, who had been invited by Casimir and Wigner (who was Dirac's brother-in-law), was supposed to contribute to the voluminous festschrift following the symposium, but he failed to meet the deadline.[94] Although he kept a low profile during the symposium, at least at one occasion he intervened in the discussions, such as Wheeler recalled more than twenty years later:

> The Dirac birthday at Trieste under the stewardship of [Jagdish] Mehra gave a chance to review many subjects. My own paper, 'From Relativity to Mutability', I found challenged by my wonderful Danish colleague Christian Moller. Moller felt that it's possible to give a definition of the local energy density of a gravitational field in contrast to, I think, the general opinion of the community. And I had to stand up for the general opinion of the community against the attacks of Moller.[95]

Contrary to Møller, Wheeler maintained that "the concept of 'total mass-energy' makes no sense for a closed universe."[96]

As chairman for the session on 'Space, Time, and Geometry' Møller listened to talks given by Chandrasekhar, Jordan, Dirac, and Sciama. The latter was not only a distinguished astrophysicist and cosmologist but also a former student of Dirac. He gave a wide-rang-

94. Telegram from Wigner and Casimir to Møller, 5 April 1972 (CMP). Møller does not appear in the proceedings volume except on the group photography, where he is seated between Sciama and Schwinger. Mehra (1973), p. xvii.

95. American Institute of Physics, interview with Wheeler (Session XII) by Kenneth Ford, 28 March 1994. https://www.aip.org/history-programs/niels-bohr-library/oral-histories/5908-12.

96. Mehra (1973), p. 211.

ing talk on 'The Universe as a Whole' in which he concluded that theoretical physics was facing a crisis:

> It follows that if general relativity is correct, ... then there is a physical singularity in the past. Either classical general relativity breaks down, or effectively negative energy densities can exist, or causality breaks down, or singularities exist in nature. One's first thought is that quantizing general relativity might resolve the crisis, but at the moment this remains only a hope.[97]

Møller very much agreed with Sciama's pessimistic diagnosis, but he thought that after all the singularity problem was not inevitable and that somehow classical relativity theory, if not necessarily in Einstein's original version, could be saved. At the time of the Trieste symposium, he had begun working on the rescue operation.

Møller followed the literature on singularities and black holes, but he only contributed to it in a memoir of 1975 in the proceedings of the Royal Danish Academy titled 'A Study in Gravitational Collapse'. Referring to the singularity theorems of Penrose, Hawking, Geroch, and others – he called it "one of the most surprising and disturbing discoveries in later years" – he investigated by means of a new system of coordinates the Oppenheimer-Snyder case of a spherical distribution of matter. Rather than considering an imploding star, he chose as an example a system of galactic mass and radius for which he took $M = 10^{45}$ g and $R = 10^{23}$ cm. For this system, which he for reasons of simplicity assumed to be non-rotating, he calculated that it would take about 160 million years before it collapsed through the Schwarzschild radius $R_S = 2GM/c^2 = 10^{17}$ cm. Following a further short time of only 19 days, a total collapse into the singularity would follow and a black hole be formed. After having derived formulae for ingoing and outgoing light signals, Møller concluded what at the time was well known: "The Schwarzschild wall $R = \alpha$ separates space-time into two regions I and II with $R < \alpha$ and $R > \alpha$

97. Sciama (1973), p. 32.

respectively. While information can pass freely from II to I, no information about happenings in I can ever reach the region II."[98]

Møller also briefly considered white holes, but without offering his opinion about the physical reality of these hypothetical objects. On the other hand, he found it conceivable that "the observable universe at the present time is a 'white hole', so that no information from distant stars outside the meta galaxy can penetrate into the interior." What he had in mind with this speculation was a spatially closed universe as described by the Friedman-Lemaître equations. In a rare mood of speculation, he wrote as follows:

> If the matter inside the sphere is uniformly distributed, the metric is … identical with the Friedman solution for a spatially closed universe with constant positive curvature. According to conventional cosmological ideas there is nothing outside this closed world, but the question now arises if the observable part of the universe in reality could be the inner part of a 'meta galaxy' immersed in a much larger closed or open universe. In this respect the usually assumed values for the radius and average mass density of the universe are strongly suggestive. For a model of the kind considered in this section with a radius of 10^{10} light years and density 10^{-29} gm/cm^3 the Schwarzschild constant would be of the order of magnitude of the radius.[99]

It is hard to tell from Møller's memoir how seriously he took this picture of the observable universe as imbedded in a much larger one to which we can have no empirical access. It was a kind of simple multiverse model, to use a term that was only coined about two decades later.

In any case, as Møller made clear only at the end of the memoir, he did not believe in the physical reality of black holes. The occurrence of true singularities, he wrote, means that the system runs into an unphysical state, which he described as "a kind of nirvana where the time stops and the notions of space and time lose their meaning." This he found to be quite unacceptable, "and one would

98. Møller (1975b), p. 25.
99. Møller (1975b), p. 29.

rather conclude that Einstein's theory, which so admirably accounts for all phenomena in the case of normal gravitational fields, breaks down in cases where the components of the curvature tensor of space-time are extremely large." Thus, his solution belonged to the same category as discussed much earlier by, for example, Lemaître, Einstein, and Tolman. By the mid-1970s Møller's insistence to explain away the singularities may still have been the standard view, but it was not shared by many specialists in black hole physics. "No one who accepts general relativity has found any way to escape the prediction that black holes must exist in our galaxy", declared Misner, Thorne, and Wheeler in their textbook on gravitation.[100]

Between 13 and 25 March 1975, Møller participated in a course of the International School of Cosmology held in Erice in Sicily. The course was mainly devoted to gravitational waves on which subject Joseph Weber, Kip Thorne, and others gave addresses. Rather than dealing with the main theme of the course, Møller contributed with a critical analysis of the behaviour of clocks near a gravitational singularity. He now accepted the Penrose-Hawking conclusion that "singularities are unavoidable in Einstein's theory, not only in the case of the universe as a whole but also for a sufficiently massive galaxy that is contracting under the influence of its own gravitational field." Given that the entrance of a standard clock into the unphysical singular state happens after a finite proper time, and this is the time associated with real clocks in nature, according to Møller it presented a serious difficulty for Einstein's gravitation theory. After a comprehensive analysis, he concluded:

> The notion of proper time ceases to represent a physical quantity in the vicinity of singularities in the metric, since we cannot imagine any physical clock that can measure this quantity. This also means that the metric components themselves lose their physical meaning in this region and that Einstein's theory, which so admirably accounts for the gravitational phenomena inside our solar system, has to be changed in the case of super strong gravitational fields.[101]

100. Misner, Thorne, and Wheeler (1973), p. 620.
101. Møller (1975c), p. 254 and p. 268.

In a lecture given to the Society for the Dissemination of Natural Science on 31 March 1976, Møller similarly spoke of the 'big catastrophe' that would imply "nothing less than the collapse of physics." He ended his lecture by sketching his own alternative, realising that so far it was just a work in progress: "It is still an open question whether or not one can construct a satisfactory divergence-free theory by introducing new variables for the gravitational field besides the g_{ik} variables."[102]

Of course, the problem of the singularity and its relation to the concept of time was not new. Thus, in the mid-1930s Milne came up with the idea of using two different but logarithmically related time-scales to avoid the initial cosmological singularity, and a similar idea appeared prominently in the cosmological theories of Dirac and Jordan. Milne introduced a new time scale τ which relates to the ordinary time t as

$$\tau = t_0 \log(t/t_0) + t_0$$

where t_0 is the present epoch. On t time the universe expands from a singularity, while on τ time it is static and stretches infinitely back in time to $\tau = -\infty$. Møller did not refer to the original proposals of two time-scales but instead to a more recent discussion by Misner, who agreed with Møller that "the singularity occurs at a finite *proper* time in the past, and proper time is the most physically significant, most physically real time we know."[103] Nonetheless, in 1973 Misner speculated, as he had done in a paper four years earlier, that finite proper time may describe an infinite number of physical events. However, Møller did not accept Misner's reasoning. Contrary to the American physicist, who wanted to retain the singularity as a legitimate part of Einstein's theory, Møller thought that it required modification in the case of extremely strong gravitational fields.

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102. Møller (1977c), a lecture in Danish on 'Victories and Defeats in the General Theory of Relativity'.

103. Misner, Thorne, and Wheeler (1973), p. 813. In Misner (1969) he suggested that "the universe is meaningfully infinitely old because infinitely many things have happened since the beginning."

In a later memoir of 1978 Møller elaborated on what he perceived not only as a crisis in general relativity, but more dramatically also as a breakdown of physics. He now admitted the consensus view that singularities are inevitable consequences of Einstein's classical theory. According to Hawking's 'randomicity principle', as Møller called it, the future state of a system may be undetermined even if the initial state is well-defined. This he considered a much more radical and even inadmissible break with classical physics than the one brought about by the uncertainty principle in quantum mechanics. It was "such a serious departure from the philosophy, which has been the mainstay of physics since Galileo, that many physicists will ask if this step is really necessary."[104] Of course, Møller was one of the many physicists. He thought that the step was unnecessary and consequently looked for "a theory in which there are no black holes and which gives the same results as Einstein's theory at least for weak fields."

After careful deliberations he concluded that among the fundamental assumptions of general relativity only one could possibly be changed, namely that the gravitational field is exhaustively defined by the metric tensor g_{ik} alone. Consequently, Møller suggested in his 1978 memoir that "the g_{ik} are not among the truly fundamental gravitational variables, but that the latter are a set of tensor variables from which the metric quantities can be derived uniquely." This was not a new suggestion, as he had already aired it in his works from the early 1960s. As mentioned in Section 7.1, Møller's idea was to base the metric components on the variables of the tetrad formulation of gravitation. In this way he thought to have found a loophole to the singularity theorems based on standard general relativity. Although Møller admitted that he had not yet found the appropriate tetrad formalism to replace Einstein's equations, he claimed to have shown that "the breakdown of physics predicted by Hawking on the basis

104. Møller (1978), p. 5. Møller adopted the randomicity principle from an unpublished preprint by Hawking with the title 'Fundamental Breakdown of Physics in Gravitational Collapse'. The principle is related to what is sometimes called Hawking's principle of ignorance: for an observer with limited information about a physical system, all descriptions consistent with the known laws of physics are equally valid.

of Einstein's theory does not seem to be inevitable." He repeated his message of the 'catastrophe' and his own way of avoiding it in some of his works published on the occasion of the centenary of Einstein's birth in 1879.[105]

Møller communicated his new theory in papers, lectures, and letters to his colleagues in theoretical physics, eager to know what they thought about it. Thus, to the American physicist Karel Kuchař, a specialist in general relativity at the University of Utah, he wrote:

> [My work] shows that the catastrophy [*sic*] pointed out by Hawking and others does not seem to be inevitable. Returning to my old idea that the truly fundamental gravitational variables are tetrad fields, from which the metric field is derivable, I find that the domain of possible Lagrangeans [*sic*] is much wider than in the case of a purely metric gravitational field and that there may be a possibility to avoid the singularities in this way.[106]

And to the Swiss particle theorist Konrad Bleuler:

> I have in the last two years tried hard to avoid the 'breakdown of physics' predicted by Einstein's theory of gravitation in the collapse of large heaps of mass. It is not very easy to change Einstein's beautiful theory without destroying the part which we now know is experimentally correct. However, I believe that I now see a possibility to arrive at a theory of gravitation, which agree with Einstein's to the second order of weak fields and which has no singularities in it.[107]

In a later letter to Kuchař, Møller wrote: "I am not yet completely senile, and [continue] the difficult task of generalizing Einstein's theory with the aim to remove the intolerable singularities." He enclosed a forthcoming paper, which "seems to show that it is perhaps not hopeless to arrive at a singularity-free formalism."[108]

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105. Møller (1979a) and also Møller (1979c), which is a brief programmatic account of the "alarming" situation in general relativity.
106. Møller to Kuchař, 5 September 1977 (CMP).
107. Møller to Bleuler, 24 March 1977 (CMP).
108. Møller to Kuchař, 23 February 1979 (CMP). The paper was Møller (1979b).

From 28 February to 2 March 1979 the Academy of Science of the German Democratic Republic celebrated the centenary of Einstein's birth with a large conference in East Berlin. Møller was among the invited foreign speakers, as were Bergmann, Wheeler, Otto Heckmann, and the French-American astronomer Gerard de Vaucouleurs. Participants from the Soviet Union included distinguished physicists such as Dmitri Ivanenko and Victor Ambartsumian, the latter a prominent astrophysicist and former president of the International Astronomical Union. In his contribution to the Berlin meeting Møller not only repeated what he had said earlier, namely that a singularity-free modification of Einstein's gravitation theory was imperative, he also suggested a cosmological model satisfying this desideratum.

Shortly after the meeting in Berlin, Møller attended yet another Einstein celebration, this time in Jerusalem, where he stayed for a couple of weeks as a representative of the Royal Danish Academy of Sciences and Letters, whose secretary he was. Speakers at this centennial symposium not only included physicists (such as Bergmann, Dirac, Weinberg, and Rosen) but also, and at the time somewhat exceptionally, historians of science and culture (such as Max Jammer, Loren Graham, Martin Klein, and Isaiah Berlin).[109] After having vacationed in San Cataldo, Sicily, he and Kirsten went on to Rome, "where I attended a week's symposium on 'Problems of the Cosmos'. It was a gathering of physicists, astrophysicists and philosophers and most enjoyable although not so many new results came up."[110]

From his tetrad theory of gravitation Møller derived general equations for the evolution of a homogeneous isotropic universe, limiting himself for reasons of simplicity to the case of flat space, $k = 0$. Since he also assumed the cosmological constant to be zero,

109. The proceedings of the Einstein symposium 14-23 March were published as Holton and Elkana (1982). Møller also attended another of the Jerusalem symposia focusing on elementary particle physics, but again without contributing with a talk. See Ne'eman (1981).

110. Møller to Bleuler, 12 November 1979 (CMP). The Einstein symposium in Rome took place 24-29 September.

his model resembled in some respects the one suggested by Einstein and de Sitter in 1932. Based on the tetrad theory, Møller arrived at a set of equations which included a new positive constant l with the dimension of a length but otherwise looked like the Friedman equations based on standard general relativity. For the variation of the scale factor $R(t)$ he found that it had a smallest value $R_{\min}$ which he associated with the origin of time at $t = 0$. This minimum size of the universe depended on the value of the l constant as it was given by

$$R_{\min} = (2Ml^2)^{1/3}$$

Thus, although there was a kind of origin of time in Møller's theory, the universe was not created a finite time ago. There was a universe before the big bang. With $x = ct/l$ he concluded that it is "consistent and physically meaningful to continue the solution to negative values of x." In other words, "In this model there is no beginning of the world in a finite past. The metric has no singularity and the matter density has a finite maximum at $x = t = 0$ depending on the value of the constant l." Møller's tetrad model of the universe was thus non-singular, contracting for $t < 0$ and expanding for $t > 0$. For the relationship between the present matter density and the Hubble parameter he found

$$\rho_0 = 3H_0^2/8\pi G$$

which is the same relation as in the Einstein-de Sitter model. From the 'latest value' $H_0 = 2.4 \times 10^{-18}\ \mathrm{s}^{-1}$ followed $\rho_0 = 10^{-29}\ \mathrm{g/cm^3}$ and $t_0 = 8.8 \times 10^9$ years. However, whereas in the Einstein-de Sitter model the universe had a definite age given by $t_0 = 2/(3H_0)$, "In our model the time t_0 ... is not anymore the age of the universe, it only represents the time elapsed since the matter had its highest mean density." This result, he continued, "has been obtained without any special assumptions about the property of the matter, such as internal spin, and it will be valid also for instance for a universe filled with black body radiation."[111]

111. Møller (1979b), p. 92 and p. 94.

Møller realised that his model was not entirely satisfactory, for other reasons because it did not take into account the heat radiation dominating the early universe but not its later development. Still, he thought that he had achieved his main goal, namely to demonstrate that the singularities occurring in the Einstein gravitation theory could be avoided by the tetrad generalisation of this theory.

When Møller presented his cosmological model in 1979, there were already several proposals of qualitatively similar models with no singular beginning of the universe and no absolute beginning of time. For example, in the 1950s Gamow speculated that our present universe might have arisen from a previous one which collapsed into a state of maximum density, and William Bonnor in England entertained similar ideas of a bouncing or cyclic universe. The aim of Bonnor (whom Møller had met in Jablonna in 1962) was the same as Møller's, namely to construct a cosmology with no singular state and yet according with general relativity. From about 1970 several bouncing non-singular models were proposed on the basis of a hypothetical negative pressure

$$p = -\rho c^2$$

which at very high density would revert the contraction into an expansion without the state $R = 0$ ever been reached. The extreme state of maximum compression was often assumed to correspond to a density of the order of an atomic nucleus, $\rho = 10^{16}$ g/cm^3, but it could be even higher.[112] Some modern versions of the cyclic universe operate with the ultimate density given by the Planck value, which is $\rho_P = c^5/hG^2 = 10^{93}$ g/cm^3.

Although Møller was presumably aware of these models, he did not refer to them and did not offer a physical mechanism for the non-singular state$R_{\min}$ at t_0. Surprisingly, despite his expertise in relativistic thermodynamics he also did not consider the problem of entropy, such as other physicists had done. Without making use of Møller's tetrad formalism, bouncing models continued to

112. For a history of cyclic and bouncing models of the universe, see Kragh (2011), pp. 193-215.

be proposed after his death. They are still being discussed, now predominantly in the context of quantum cosmologies.[113]

Uwe Kasper, an astrophysicist at the Academy of Science DDR, was stimulated by Møller's Berlin lecture to suggest a joint publication on the bouncing universe based on Møller's ideas of gravitation and cosmology. However, after having studied Kasper's draft manuscript and exchanged several letters with him during the summer and autumn of 1979, Møller declined collaboration. He found that the ideas of Kasper were too different from his own. In a long letter of July 1979, Møller wrote: "You say that the abrupt change ... from a contraction for $t < 0$ to an expansion for $t > 0$ necessitates a negative pressure in the matter. However, in my opinion this change is due to a particular property of the gravitational field of the cosmic matter which is moving under the influence of its own gravitational field."[114] Kasper came to agree with Møller that "nothing can be said of the origin of this changing over from contraction to expansion without further assumptions" and begged his colleague in Copenhagen to "reconsider the question of a joint publication which should clear up this remarkable difference between yours and Einstein's equations."[115] But Møller had made up his mind. In one of his last letters, he reported:

> My only worry now is that it seems quite difficult to find experimental or observational effects by which the present formalism could be distinguished from Einstein's theory. Such effects could be expected only in the vicinity of neutron stars and would be difficult to observe. You will understand that there are still some differences in point of view between us, so that I cannot agree to a joint publication of your paper.[116]

113. According to some models of string cosmology, the universe *in toto* is symmetric between $t > t_0$ and $t < t_0$, and at $t = t_0$ (the big bang) the radius of curvature was not infinitely large but about a minimum fundamental length given by 10^{-32} cm.
114. Møller to Kasper, 19 July 1979 (CMP).
115. Kasper to Møller, 11 November 1979 (CMP).
116. Møller to Kasper, 18 December 1979 (CMP).

Møller continued to work on the singularity-free tetrad formulation of gravitation until his death in January 1980. However, his efforts bore little fruit. They were not much noticed and even less appreciated by contemporary cosmologists in the mainstream tradition of the hot big bang. The lack of empirical distinguishability between the standard Einstein theory and Møller's tetrad theory, such as he pointed out his letter to Kasper, was presumably a major reason why the latter theory attracted but little interest.

Like the Brans-Dicke theory, Møller's was a modification of Einstein's field equations, but with the important difference that the scalar-tensor gravitation theory of Brans and Dicke resulted in a number of interesting astronomical and geophysical predictions which could actually if not easily be tested. It was precisely for this reason that the Brans-Dicke theory was much discussed while Møller's was not. His work was not completely ignored, though. For example, in 1984 two Spanish astrophysicists, arguing that "Møller's theory must be studied in detail", presented a cosmological study based on Møller's concept of tetrad fields. They showed that the Friedman equations can be derived from a particular tetrad and that the H_0 and q_0 parameters take the same values in Møller's theory as in ordinary Robertson-Walker cosmology.[117]

With the exception of his Berlin address of 1979 Møller did not flesh out his view of cosmology either qualitatively or quantitatively, but there is circumstantial evidence that he was sceptical with respect to the victorious hot big-bang theory. Although this theory does not necessarily rest on the assumption of an initial cosmic singularity – the ultimate beginning of the universe – many physicists and astronomers, not to mention popular writers, presented it as such. As we have seen, this was unacceptable to Møller, who resisted the notion of a singularity whether on a local or global scale. According to Strömgren, who knew him better than most, Møller disliked the standard big-bang model:

> In conversations about cosmology Christian Møller often emphasised the importance of a work by Oskar Klein from 1971, which he thought

117. Saez and de Juan (1984).

had received less attention than it deserved. In connection with Klein's considerations he tended to stress the significance of analysing the old idea of a hierarchical structure of the universe, which continues beyond the limits of the presently observable world of galaxies.[118]

Critical to both relativistic evolution theories and the rival steady-state theory, Klein had in the 1950s become interested in problems of cosmology and cosmogony. For example, in his contribution to the 1958 Solvay conference he offered an alternative, stating about the 'metagalactic system' of galaxies that "there may be any number of similar systems in the world in different stages of evolution – perhaps for ever outside the reach of our observations."[119] This was yet another anticipation of the idea of a multiverse. In later works with his compatriot Hannes Alfvén, a Nobel laureate of 1970, Klein developed his theory into a plasma cosmology based on the assumption of an initial cloud of equal amounts of matter and antimatter. Rejecting the cosmological uniformity principle, Klein suggested that "our expanding metagalaxy is too small to represent the universe, there being other metagalaxies in other phases of evolution, some expanding like our own and some contracting."[120]

Møller's use of the term 'metagalaxy' in about the same sense as used by Klein may suggest that he was inspired by the cosmological thoughts of his Swedish colleague. However, this was hardly the case. Møller never referred to the hypothesis of cosmic antimatter, which was the defining feature of the plasma cosmology argued by Klein and Alfvén. He also ignored the cosmic microwave background, which Klein in 1971 sought to explain, albeit unsuccessfully, on the basis of his alternative cosmological theory. If Møller really found Klein's cosmology important and was inspired by it, such as indicated by Strömgren, it is hard to explain why it was

118. Strömgren (1981), pp. 105-106. Klein's paper was presumably Klein (1971).

119. Klein (1958), p. 34. In the same address he considered, not unlike what Møller did in his 1975 memoir, a metagalactic system limited by the exterior singularity of the Schwarzschild solution.

120. Klein (1971), p. 341. For the Klein-Alfvén theory and other alternatives to mainstream relativistic cosmology, see Kragh (2019).

not evidenced in his own works on the structure and evolution of the universe.

CHAPTER 8

Christian Møller and the physics community

As indicated by his many honours and prizes, Møller was highly regarded in the international physics community. Not only was he an important figure in the Solvay institution and the recipient of several prizes and doctorates of honour, he was also centrally placed in the renaissance of general relativity through two decades. In 1966 he served as Lorentz Professor in Leiden (Section 7.2) and the same year he received the Ole Rømer medal which seven years earlier had been awarded the astronomers Ejnar Hertzsprung and Bengt Strömgren. In 1968 Møller was awarded the prestigious Gauss chair in Göttingen, a guest professorship established in 1955 on the centenary of the death of the great German mathematician and physicist. On the occasion of the 50-year anniversary of Åbo Academy, a Swedish-language university in Turku, Finland, established in 1918, Møller was awarded an honorary doctorate (doctor honoris causa).

Whether Møller worked on electron scattering, beta radioactivity, meson theory, or general relativity, it was his research projects that really interested him and filled much of his life. However, during the later part of his life Møller was also much engaged in and used many resources on teaching and organisational activities, some domestic and others international. As far as teaching is concerned, he gave for thirty years or more an extensive course in quantum mechanics, which eventually resulted in three mimeographed volumes with a total length of 561 pages.[1] Hundreds of physics students learned quantum mechanics by following the course and studying the associated texts. Apparently, he never thought of transforming the course material to a proper textbook for an international audience.

There are only few sources which relate to Møller's very extensive work as a teacher and supervisor for graduate and postgraduate

1. Møller, *Forelæsninger Over Kvantemekanik* (Lectures on Quantum Mechanics), parts 1-4, University of Copenhagen, 1958-1967.

physics students. An interesting account is given by Jørgen Otzen Petersen, a Danish astronomer who started studying physics in about 1955. Having passed the undergraduate exams, Petersen decided to follow the courses in quantum mechanics and relativity theory given by Møller at the institute for theoretical physics. He recalled:

> Christian Møller (1904-1980), professor in mathematical physics, had written notes for his course in quantum mechanics and a comprehensive textbook for the lectures in relativity theory. In the long run I found the theory of relativity to be the most exciting field and also the most comprehensible. Mechanics, dynamics and gravitation were here unified in a most satisfactory manner. I asked Møller if cosmology might be a suitable subject [for a Master's thesis], but he did not think so, for "Einstein has done it all" and one could read about it in Møller's book. Today this appears paradoxical, but at the time cosmology was generally considered to be almost speculative natural philosophy. Møller suggested that I took up relativistic thermodynamics and referred me to a textbook by Tolman from the 1930s. As an authority-obedient student I studied the book, which turned out to be very difficult and exceedingly mathematical, without any obvious connection to the physical thermodynamics which I knew from my undergraduate studies. So I postponed my choice of subject and just followed Møller's course in relativity theory.[2]

Petersen subsequently changed from physics to astronomy, writing his Master's thesis on stellar models. He later worked as associate professor in astrophysics, collaborating with Bengt Strömgren and other Danish astronomers.

Although Møller was not a populariser of physics in the traditional sense, on several occasions he contributed with works intended for readers with no or almost no background in physics, most successfully in the 1938 book co-authored by Ebbe Rasmussen. On a national level he served for a twenty-year period as secretary for the Royal Danish Academy, and as director of the CERN the-

2. Petersen (2015). The two textbooks referred to in the quotation are Møller (1952) and Tolman (1934).

ory group he contributed to the high international standing of the Copenhagen institute. From 1957 to 1971 he worked as director of Nordita, the new Nordic Institute for Theoretical Physics. During the same period, he got increasingly involved in the organisation of the young community of physicists working on general relativity, with the result that in 1971 he was elected president of the International Committee on General Relativity and Gravitation.

Contrary to many contemporary physicists, Møller was reluctant to step down from his ivory tower and discuss in public the broader philosophical and societal implications of modern physics. He wanted to stay neutral and yet he was not indifferent. By browsing through his many publications and letters it is possible to get at least some insight not only in his philosophical views but also in his thoughts about politics and the science-society relationship.

8.1. Popular works

While the large majority of physicists during the 1930s restricted their publications to research papers aimed at their peers, a few were actively engaged in the popularisation of the new scientific world view. Notable British scientists cultivating this genre were James Jeans and Arthur Eddington, whose popular and semi-popular books sold extremely well, witness titles such as *The Mysterious Universe* (Jeans, 1930) and *New Pathways of Science* (Eddington, 1935). Einstein too was interested in presenting the new state of science to the broader public, which he most successfully did with *The Evolution of Physics*, a book from 1939 written jointly with the young Polish physicist Leopold Infeld. The hugely popular book was quickly translated into other languages including a Danish translation of 1939 (*Det Moderne Verdensbillede*) by the physicist Niels Arley, who at the time collaborated with Møller.

Oskar Klein in Sweden was another eminent physicist who engaged in popular writing, such as he did in a charming book of 1935 dedicated to Bohr. Rather than presenting atomic and quantum physics in a factual and informative manner, he formed his book as a Galilean dialogue between two persons discussing the philo-

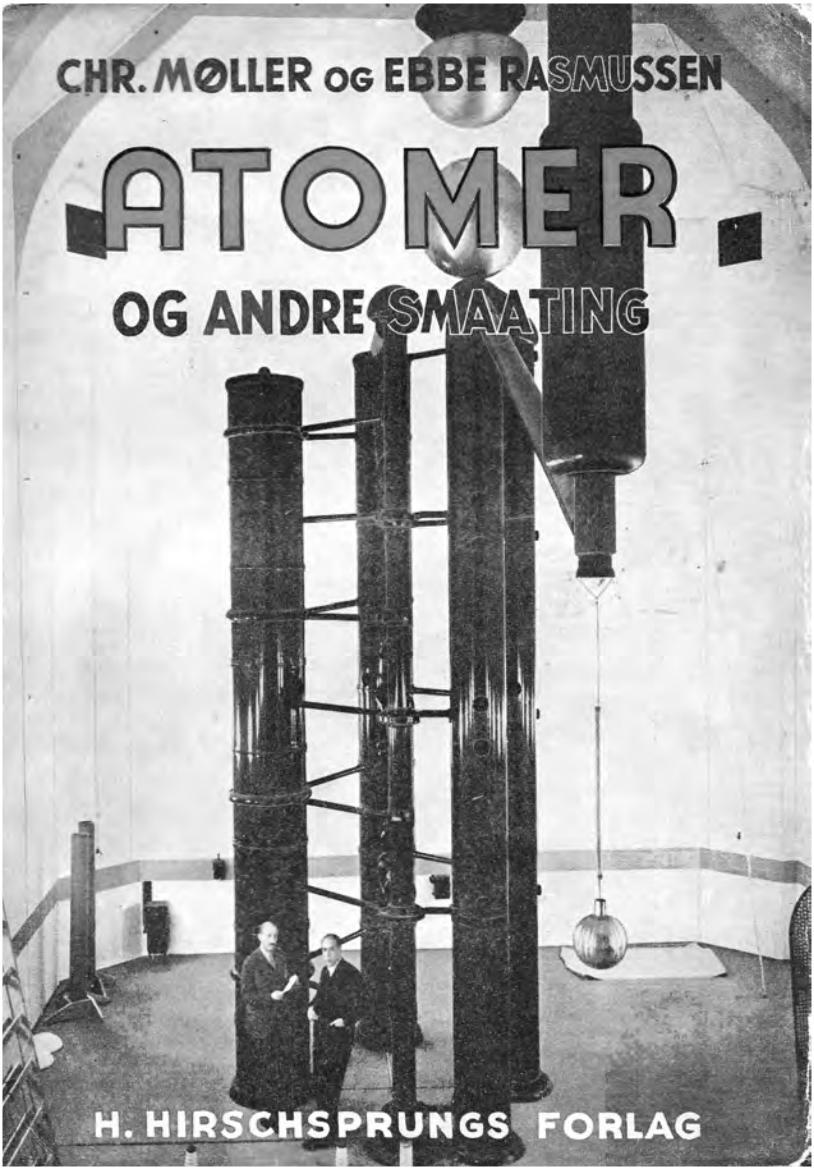

Fig. 33. Cover page of the first edition of the Møller-Rasmussen popular book on atomic and nuclear physics.

sophical consequences of the new physics.[3] This kind of free and imaginative writing was completely different to Møller's disciplined and fact-oriented idea of popular science. Only at one occasion, his 1945 contribution to the *Journal of Jocular Physics* (Section 5.4), did he try his pen with imaginative science story-telling, in this case with a story about the clock paradox.

Much like Eddington, George Gamow did not respect the strict border that traditionally was taken to distinguish research papers from popular writings. As he admitted, he started writing popular works "probably because I love to see things in a clear and simple way, trying to simplify them for myself."[4] Gamow's innovative and highly original *Mr. Tompkins in Wonderland* was published by Cambridge University Press in 1939, soon to be followed by a series of other popular works. In 1942 Gamow's book appeared in a Danish translation by Sven Werner and with a foreword by Bohr, who referred to the author's close association with the physics institute in Copenhagen.[5] As early as 1922, Bohr's atomic model had been presented in a popular format by young Hendrik Kramers and his co-author Helge Holst, a physics-trained librarian and science writer. Their book was translated into English as *The Atom and the Bohr Theory of Its Structure* and also appeared in German, Spanish, and Dutch translations.[6] It was a remarkable success which in a sense was followed up sixteen years later by a book written by Møller and Ebbe Rasmussen called *Atomer og Andre Smaating* (Atoms and Other Small things).

Although Møller was far from a populariser of the same scale and originality as Eddington and Gamow, or for that matter Klein, on a few occasions he did engage in the art of popular science writing. As mentioned in Section 3.4, he felt that it was "one's duty

3. Klein (1935).
4. Gamow (1970), p. 155. On Gamow as a popular science writer, see Bagdonas and Kojevnikov (2021) and Harper (2001).
5. However, according to Casimir, "Bohr was irritated rather than amused." Gamow played arbitrarily around with the relative orders of magnitude of constants of nature, which "struck him [Bohr] as silly rather than funny." Casimir (1967), p. 111.
6. Kragh and Nielsen (2013).

to … popularize a little what was going on in physics." About the origin of his book with Rasmussen, Møller recalled that it began with a prize competition by a Swedish publisher to write a popular science book:

> Rasmussen had seen it and he came to me, "should we sit down and write such a book and try to get the first prize?" … So we started to talk about it; how we should go about such a book, what would be important and so on. We knew of course it had to be very popular because otherwise we would not get a prize. But then we became more and more seriously interested in it, and so finally it was a rather comprehensive description of the situation in physics at that time. I don't think we left out anything significant. … We worked on that for a year, I think, in our spare time, and we had a very jolly time together while we were doing it. Then it was sent to Stockholm. We did not get the first prize. We got a second prize. That was something.[7]

Having won the second prize, the Danish publisher Hirschsprung got permission to publish the manuscript in Danish in a series of popular science handbooks.

Møller thought that much of the success of the book was due to his co-author:

> If I had written the book alone, it would not have been as successful. Rasmussen was very good in explaining the experiments and so on, and I think that was — I mean his contribution was really very important. He used to say, "Well, now I shall write this chapter and you criticize it", and this was more or less what we did. Often we sat together also and wrote together, but very often we talked about it and then he would write down a sketch of it, and I would read it and we would go over it together.

With a new and only slightly revised Danish edition of 1939 and a fourth extended edition of 1945, *Atomer og Andre Smaating* became an unexpected success not only nationally but also internationally.

7. Weiner (1971c), which is also the source for the next quotation.

A Dutch translation came out in 1939, followed by one in English in 1940, American in 1941, and in Swedish in 1945. Two years later, it was translated into Czech. As late as 1969, a heavily revised fifth Danish edition was published with Jørgen Kalckar, one of Bohr's last scientific assistants and a nephew of Fritz Kalckar, as a co-author.[8]

In style and content, the Møller-Rasmussen book was fairly conventional, an informative and chronologically organised account of breakthroughs in atomic and nuclear physics since the discovery of radium in the late nineteenth century. As Bohr pointed out in his foreword, the collaboration of a theorist and an experimentalist resulted in a balanced account of how modern physics had progressed. The book was structured in three parts, the first on pre-quantum atomic physics, the second on Bohr's theory and its development into quantum mechanics, and the third and largest part on nuclear and particle physics. As the authors mentioned in their preface, they had followed the development up to 1938, but "among the most recent discoveries and progress we have only included those whose correctness can be regarded as beyond any doubt." That is, contrary to some other popular science books, such as Eddington's and Gamow's, they avoided more speculative issues and kept to well-established physics.

As expected from a book coming from Bohr's institute, Møller and Rasmussen dealt at some length with the complementarity principle and also with Bohr's recent compound model of atomic nuclei. To illustrate the idea of complementarity the two authors referred to their own book, where readability and clarity required a renunciation of equations and technical details:

> Thus, clarity and detail stand in a complementary relationship. The clarity increases if only few details are taken into account, and vice versa.

8. Møller and Rasmussen (1938). Translations: *Atomen en Andre Kleine Deeltjes* (The Haag, 1939); *The World and the Atom* (London, 1940; New York, 1941); *Atomens Sällsamme Värld* (Stockholm, 1945); Od *Atomu K. Atomové Bombě* (Prague, 1947). Jørgen Kalckar's father and Fritz Kalckar's brother was Herman Kalckar (1908-1991), an outstanding biochemist who since 1939 worked in the United States.

> On the other hand, both clarity and comprehensiveness are necessary if one wants an exhaustive description, and yet one cannot achieve both at the same time. So, it is only by making a choice that one can obtain the right balance between these two complementary concepts.[9]

Without using equations, the two authors discussed the quantum wave equations of Schrödinger and Dirac. With regard to the latter equation, they explained how it resulted in the surprising prediction of the antielectron and its verification in the form of the positrons found in the cosmic rays. Dirac's theory also predicted the existence of an antiproton, "but this particle, if it exists at all, still waits to be discovered." Only in a footnote did they mention the meson (mesotron, now muon) and then without naming the particle: "By means of cloud chamber photographs one has very recently found that sometimes the cosmic rays contain yet another new particle with a mass between that of the proton and the electron, but so far its properties remain unclear."[10]

In the last part of their book, Møller and Rasmussen referred to the possibility of the exploitation of atomic energy (Section 4.1). Moreover, they indirectly entered science policy, arguing that research in physics should be left to the physicists without too much interference from either government or industry. Sure, much modern technology had its roots in modern physics, "but it does not further the development of physics by making exclusive demands for such results which can be immediately applied for practical purposes. ... It serves physics as well as technology best to let the physicists work along their own paths, which are determined wholly by the desire to attain to greater knowledge of the wonderful laws and the rich life that govern and penetrate even what is called lifeless nature."[11]

The second edition of 1939 was a reprint of the first edition except that the new one was supplied with an index. One might have expected that the authors updated the book by referring to

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9. Møller and Rasmussen (1938), p. 94.
10. Møller and Rasmussen (1938), p. 115.
11. Møller and Rasmussen (1938), p. 168.

the sensational splitting of the uranium nucleus, but they did not. On the other hand, they did so in the English version of 1940 where they described Hahn's experiments in Berlin and the subsequent recognition of so-called fission "on the analogy of the processes of splitting the cell in biology." Møller and Rasmussen briefly described how measurements made by "Frisch in Austria" had confirmed the fission hypothesis. "Furthermore, it was very soon shown that in the uranium fission neutrons are also set free, so that the conditions ... for carrying the process further seemed to be present", that is, a chain reaction.[12] Curiously, the two Danish authors did not so much as intimate that Frisch did his work at the Copenhagen institute, where they both worked, or that the name 'fission' was a Copenhagen invention. Nor did they mention that the possibility of a fission chain reaction was first considered by Møller.

The Danish edition was briefly but positively referred to by Paul Bergsøe, the engineer, radio broadcaster and science writer who in February 1939 interviewed Møller and other physicists on the recent discovery of the uranium fission process (Section 4.1). Bergsøe's essay review was mostly concerned with a new book in the same genre written by Bengt Strömgren, whose work on astronomy and astrophysics he considered to be a "twin brother" to the one of Møller and Rasmussen. Bergsøe praised Strömgren for being a "calm and restrained" author, who "not a single time tends to exaggerate or make use of lyrical expressions", a characteristic which was valid also for the authors of *Atomer og Andre Smaating*.[13] The English edition titled *The World and the Atom* was reviewed in *Nature* together with a review of Gamow's newly published *The Birth and Death of the Sun*. According to the reviewer, the British physicist James Arnold Crowther, the Møller-Rasmussen book was a clear and well-balanced "scholarly account of the rise and progress of atomic physics" written at a level "somewhere between the entirely 'popular' and the elementary text-book." This was also the opinion

12. Møller and Rasmussen (1940), pp. 187-188. Of course, Frisch did not make his fission experiments in Austria.

13. Bergsøe (1941), pp. 96-99. Strömgren (1940). Strömgren reviewed the Møller-Rasmussen book in *Nordisk Astronomisk Tidsskrift* **19** (1938): 155-156.

of the reviewer in *Journal of the Franklin Institute*, who thought that the book was not popular in the ordinary sense but that it should rather be categorised as "midway between the first step after the technical treatise and the popular work."[14]

Of course, by the late 1960s, when nuclear and particle physics had evolved explosively, the Møller-Rasmussen book was obsolete. Rasmussen had passed away in 1959 only 58 years old, so Møller decided to join forces with 34-year-old Jørgen Kalckar in writing a substantially revised and updated version of the book.[15] Since a large part was taken over from the old book, Møller and Kalckar kept the title and added Rasmussen as posthumous co-author. The new book published in 1969 included a solid chapter on modern high-energy or elementary particle physics, a subject which scarcely existed thirty years earlier. It was more comprehensive than the earlier one, more reader-friendly and with many more illustrations. The conclusion at the end of the book – that research in pure physics should be granted autonomy and not be seen merely as a servant of technology – was reproduced verbatim from the old book. Møller had not changed his mind with regard to the higher aim of physics.

As mentioned in Section 4.1, Møller wrote in 1943 a popular account on the future use of atomic energy in *Danfoss Journalen*, a periodical published by the large industrial company Danfoss, a manufacturer of thermostats and other automatic control devices. Three years later, after the atomic bomb was no longer a secret, he wrote in the same journal a detailed article in which he explained the principle of the bomb and how uranium might be used in the commercial nuclear reactors which at the time were on the drawing board. Yet another and very different use of the marvellous atomic energy was the medical and industrial applications of radioactive isotopes, which he also described to readers of *Danfoss Journalen*.[16] Møller's articles reflected the optimism of the coming atomic age which at the time was shared by most people whether scientists or

14. J. A. Crowther, *Nature* **147** (1941): 689-690. R. H. Oppermann, *Journal of the Franklin Institute* **232** (1941): 297.

15. Møller, Rasmussen, and Kalckar (1969).

16. Møller (1946c). Møller (1948).

not. They were clear and informative, popular but probably more aimed at engineers and high school teachers than to the average citizen.

In 1944, during the worst period of the German occupation, a large group of Danish scientists and scholars published a comprehensive work on the state of science addressed to the general but educated reader. The two volumes covered not only physics, chemistry, biology, and astronomy, but also philosophy, archaeology, linguistics, and psychology. Bengt Strömgren wrote on astrophysics, Ebbe Rassmussen on atoms and nuclei, and Møller contributed with an insightful chapter on quantum mechanics and its epistemological implications. In close agreement with the views of Bohr and Heisenberg, he explained the consequences of the uncertainty principle and the associated principle of complementarity. With a reference to those unnamed philosophers and physicists who wished to reinstate classical causality in new formulations of quantum mechanics, Møller stressed that this was a retrograde step doomed to be a failure. On the contrary, "in the future it will be necessary to sacrifice even more of our usual ideas and customary thoughts in order to accommodate new phenomena in the scientific world picture."[17]

Had Bohr still been in Denmark, he would undoubtedly have written the chapter on quantum theory in *Videnskaben I Dag* (Science Today). Now it was left for Møller, who substituted for Bohr and did it excellently. His contribution was very much in the spirit of Bohr, only written more clearly and comprehensibly. The same was the case with an article published in a Danish engineering magazine, which he wrote on the occasion of Bohr's seventieth birthday. With regard to the complementary principle and Bohr's general ideas about physics, Møller prophesised, albeit in this case wrongly, that "they will undoubtedly have an impact on future philosophical thinking comparable to the one that the Newtonian system of the world had for the philosophy of Laplace and Kant."[18]

Møller wrote a couple of other works for the educated Danish lay audience which more had the character of undergraduate

17. Møller (1944), p. 457.
18. Møller (1955b), p. 797.

textbook material than popular writings as normally understood. In 1964 he published a small book, an 'Elementary Exposition of the Foundation of Atomic Physics', together with his friend and colleague Mogens Pihl who since 1957 had served as professor of physics at the University of Copenhagen.[19] The booklet developed the subject of Møller's 1944 essay, only this time in a much more elaborate and quite demanding form. As a reviewer pointed out, the book was a 'puritan' exposition of quantum philosophy beyond the reach of most of its intended readers.[20] Once again, Bohr and his interpretation of quantum mechanics were at the centre, with no indication that there were other interpretations than the one of the Copenhagen physicists. The two authors stressed the objectivity of the quantum world as understood by Bohr, perhaps to counter the often expressed misunderstanding that Bohr's 'observer' of a physical event referred to a conscious human individual and that the act of observation therefore included an element of subjectivism.

In his later career, Møller wrote at a few occasions on the quantum world for a general audience, always identifying the quantum world with Bohr's ideas.[21] He never tried to do the same with his passion since the mid-1950s, the general theory of relativity. Contrary to Rosenfeld, Møller only wrote on quantum philosophy for a local audience and he refrained from entering the international academic debate on the rival interpretations of quantum mechanics.

8.2. The Royal Danish Academy

The Royal Danish Academy of Sciences and Letters or what originally was called the Society of Lovers of Science and Learning in Copenhagen, was established on 13 November 1742, three years later than the corresponding Swedish academy in Stockholm.[22] During

19. Møller and Pihl (1964).
20. Review in *Information*, 4 May 1965, by David Jens Adler.
21. For example, a feature article in *Politiken* on 'Niels Bohr i historiens lys' (Niels Bohr in the Light of History), 18 November 1972.
22. For the history of the Royal Danish Academy, see Pedersen (1992) and Kragh et al. (2008), pp. 145-148, 257-261.

the first century or so the Royal Danish Academy initiated a series of research projects, some important and others less so, but later on its activity was largely restricted to meetings and exchange of communications between the members. The Academy was and still is divided in two classes, a mathematical-scientific class and a humanist class. It was considered a great honour to be elected a member of the elitist Academy, which in the mid-twentieth century comprised approximately 110 scientists and scholars. As in other European academies, prominent foreigners could be elected as members in addition to the ordinary domestic members. For example, in 1889 the famous Russian chemist Dmitri Mendeleev became a member, and in 1920 Einstein, Planck, Rutherford, Marie Curie, and Heike Kamerlingh Onnes were granted membership. In 1946, after the end of World War II, several more physicists were elected foreign members. They included L. de Broglie, J. Chadwick, J.-F. Joliot, P. Kapitsa, O. Klein, and L. Meitner.[23]

The 1920 batch of foreign members was proposed by Bohr, who since his election in 1917 played a central role in the Royal Danish Academy. Already in 1927, he was asked to become president, but at the time he was too busy to take up the position. Twelve years later he agreed and was elected president, a position he held until his death despite his absence from Denmark during most of the years 1943-1945.[24] The last meeting he presided over took place on 16 November 1962, just two days before he passed away. Bohr and his associates at the institute for theoretical physics, whether they were Academy members or not, published much of their research in the Academy's proceedings series called *Matematisk-Fysiske Meddelelser* (Communications in Mathematics and Physics), which in the period from about 1930 to 1970 counted as an internationally important physics journal. As mentioned in chapters 5 and 7, Møller published the major part of his work on meson theory and general relativity

23. Marie Curie was elected on 21 January 1920 as the first female member of the Academy. J. R. Oppenheimer became a foreign member in 1950, L. Rosenfeld in 1951, and A. Pais in 1988. For a list of members 1942-1992, see Blegvad (1992).

24. On Bohr and the Royal Academy, see Pedersen (1992), pp. 292-304, Pais (1991), pp. 464-470, and Pedersen (1967).

Fig. 34. Heisenberg, F. Bloch, and Møller eating "smørrebrød" (open sandwiches) at the 1963 Bohr memorial conference. Credit: Niels Bohr Archive, Photo Collection, Copenhagen.

in *Meddelelser*, often in memoirs much too long to be accepted by more standard journals such as *Physical Review* or *Proceedings of the Royal Society*.

Thirty-eight-year-old Møller was elected a member of the Royal Danish Academy at a meeting of 2 May 1943, the same year he was appointed professor of mathematical physics at the University of Copenhagen. As Møller realised, the election was in part due to Bohr, whom he thanked for "everything you have done over the years for my development and my work."[25] A few months later Bohr fled to Sweden and the next two years he was inactive although nominally still president. In agreement with a well-established tradition the important and time-consuming post as secretary was occupied by a member of the mathematical-physical class, which from 1945 to 1959 was the mathematician Jakob Nielsen. Møller was elected secretary in October 1959, a post he held until his death in early 1980. After Bohr had passed away on 18 November 1962, it was left to Møller to present to the Royal Danish Academy the traditional

25. Møller to Bohr, 3 April 1943 (BSC).

éloge of its former president, which he did at a meeting one month later. In his careful account of Bohr's scientific life, he emphasised the role of the correspondence principle and Bohr's insistence that the new quantum mechanics should be seen as an extension of classical physics rather than a complete break with it.[26]

The same year that Møller became secretary of the Academy he was elected a member of the Royal Physiographic Society in Lund, Sweden, an institution founded in 1772, and also a foreign member of The Royal Norwegian Society of Sciences and Letters. The latter institution was established in 1767 in Trondheim, Norway, at a time when the country was under Danish rule. After independence the larger and more internationally oriented Norwegian Academy of Science based in Oslo was established in 1857. Møller was a foreign member of this academy too, elected in 1963. Nine years later he became a member of yet another European academy, the Leopoldina Academy in Halle, Germany, founded as early as 1652 under the name *Leopoldina Naturae Curiosorum*. Among the more recent members of this prestigious academy were notables such as Wilhelm Ostwald, Max Planck, Otto Hahn, and Carl Friedrich von Weizsäcker. Einstein too was a member, but only until the beginning 1933 when he was excluded for being Jewish.

As secretary of the Danish Academy, Møller used at some occasions his position to invite foreign scientists to give talks at the meetings of the Academy. One such occasion was when Heisenberg on 26 November 1965 gave a general talk on his ideas of a unified field theory, which he combined with an informal discussion at the Bohr institute.[27] The position also caused Møller to get involved in the early phase of Danish research policy, which essentially took its start only at about 1950. His primary aim was the same as Bohr's, to place the Academy as a central player in the political negotiations concerning the government's attempt to create a new basis for research and development in the country. However, it soon turned

26. Møller (1963c), meeting of 14 December 1962.

27. Møller to Heisenberg, 8 November 1965, and Heisenberg to Møller, 16 November 1965 (CMP). The title of the talk was 'The Basic Ideas of a Unified Field Theory of the Elementary Particles'. Blegvad (1992), p. 83.

Fig. 35. Møller, photograph of 1971. On his desk in front of him is a copy of his and Rasmussen's popular book on atomic and nuclear physics. Credit: Niels Bohr Archive, Copenhagen.

out that the politicians did not consider the Academy nearly as important as Møller and his fellow members thought it was. In 1968 the government created a system of research councils and four years later a Planning Council for Research (Planlægningsrådet for Forskning) was established as the central body of national research policy. Møller took part in many of the negotiations, but without succeeding in giving the Academy an important role in the new institutions. In his capacity of secretary, he also participated in discussions related to UNESCO and other international organisations including the important European Science Foundation ESF which Denmark joined in 1975.

The statutes of the Carlsberg Foundation founded in 1876 stipulated that its board of directors should be appointed by the Royal Danish Academy. At the time, the rich Carlsberg Foundation was

Denmark's most important private actor in science and science policy. In 1963 Møller was elected to join the board of the Carlsberg Memorial Foundation (Carlsbergs Mindelegat), a branch of the Carlsberg Foundation dating from 1938.[28] Bengt Strömgren had been a member of the Academy since 1939, but in 1957 he moved to Princeton with no intention to return to Denmark. Møller considered it his patriotic as well as scientific duty to lure his old fellow student back to the country, and as a new board member of the Carlsberg Memorial Foundation he figured out how to do it. At Christmas time 1965 he wrote to Strömgren suggesting the possibility that the Danish astronomer might be offered to move into the Carlsberg Mansion of Honour and also, if he returned, become extraordinary professor at Copenhagen University.

According to the will of Jacob Christian Jacobsen, the wealthy founder of the Carlsberg Brewery, his villa should be used as a residence of honour "by a man or a woman deserving of esteem from the community by reason or services to science, literature, or art, or for other reasons."[29] The first resident was the prominent philosopher Harald Høffding, who assumedly influenced the younger Bohr's thinking about complementarity and related matters. Høffding stayed in the villa 1914-1931 and was succeeded by Bohr. After Bohr's death, the archaeologist and prehistorian Johannes Brøndsted resided in the mansion, but he died on 16 November 1965 and so it was temporarily empty. "I was very moved by learning about your ideas in connection with the mansion of honour", Strömgren wrote in reply. "As Brøndsted only lived there shortly, I regard it as Niels Bohr's residence, as we probably both do, and so I don't have to explain to you that it is a bit overwhelming to me."[30]

Møller persuaded the chairman of the Carlsberg board of directors to follow his plan and by 1 March 1966 the board sent the formal

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28. Blegvad (1992), p. 88. Two years earlier, Møller had become a board member of another of the Danish foundations supporting science, the Rask-Ørsted Foundation established in 1919. For the importance of this foundation for Bohr and his institute, see Aaserud (1990).

29. For the history of the Carlsberg Mansion, see Nielsen (2021).

30. Strömgren to Møller, 14 January 1966, as quoted in Rebsdorf (2005), p. 427.

invitation to Strömgren, who finally returned to Denmark a year later. After Møller had directed Nordita for fourteen years, in 1971 Strömgren succeeded him as director. Moreover, from 1968 to 1975 Strömgren served as president of the Royal Danish Academy with Møller as its secretary. The close relationship between the two scientists is evidenced by the memorial speech Strömgren gave to the Academy at its meeting on 12 March 1981 and in which he referred both to Møller's scientific work and to his work as secretary for the Academy. Throughout his long period as secretary, Strömgren noted, Møller "always spoke with conviction against proposals of a radical increase in the number of members of the Academy."[31] Less formally and more emotionally, Strömgren wrote a letter to Møller's widow Kirsten in which he expressed his condolences:

> I write these words in the room [at Nordita] that Christian and I shared and at the desk by which we both worked. It is so difficult to comprehend that I will no more be met with Christian's welcoming smile and that I shall no more have the feeling, which I always had, of peace of mind and happiness by being together with him. Time and again it goes through my head; my best friend is dead.[32]

Møller's former student, the historian of science Olaf Pedersen, summarised his work for the Academy in the following words:

> Besides continuing his research into the theory of relativity until the very end, he always found time for a meticulous preparation of all matters to be discussed at the meetings, being much loved by the members for his modest and likable personality. In his last years he took an active part in the new public activity of the Society, which celebrated his 20th anniversary as Secretary at an especially festive meeting 1979 Novem-

31. Strömgren (1981), p. 107. In 1943, the number of members in the humanist class was 30 and in the scientific class 40. In 1954, it was agreed to increase these numbers to 40 and 80, respectively. Suggestions of further increase were opposed by Møller. See Pedersen (1967).

32. Strömgren to Kirsten Møller, undated, 1980. Quoted in Rebsdorf (2005), p. 431.

ber 29 with very personal speeches by the President (P. J. Riis) and C. Møller's friend since their student days B. Strömgren.[33]

The Carlsberg Mansion was not the only residence of honour administrated by the Royal Danish Academy. Another but more modest villa was 'Lundehave' in Elsinore, which housed for free a prominent Danish scientist or scholar. When the resident Vilhelm Grønbech, an influential historian of culture, passed away, in October 1948 the Royal Academy offered Lundehave to Møller, who at the time stayed in the United States. Danish newspapers prematurely reported that an atomic scientist would now replace the humanist Grønbech,[34] but Møller declined the generous offer. Instead, the mathematician Jakob Nielsen, secretary of the Royal Danish Academy, moved to Lundehave in May 1949.[35] Nielsen was a most important figure in Danish mathematics and science policy, working closely with Bohr and others in the early 1950s to get the CERN project to Copenhagen and participating in many of the CERN meetings. In 1954-1955 he served as one of the two vice-presidents in the CERN Council and in the same period he was a member of UNESCO's executive board.[36]

8.3. From CERN to Nordita

About 1950, when nuclear and particle physics began to be increasingly dominated by large accelerators and detector devices, Europe lagged far behind the United States. Leading European physicists realised that the old world could only participate in high-energy physics by pooling manpower, money, and material resources. Not only would such an enterprise be of great scientific value, it was also envisaged as an important political and cultural project that might inspire further European cooperation. At that time, the first initiatives to organise a large-scale European research project took

33. Pedersen (1992), p. 313.
34. *Helsingør Dagblad*, 14 March 1949.
35. Blegvad (1992), p. 53.
36. Rasmussen (2002). Pedersen (1992), pp. 295-297.

place, with the French physicist Pierre Auger and his Italian colleague Edoardo Amaldi as the prime movers. The result of many and difficult negotiations was the provisional founding of CERN (Conseil Européen pour la Recherche de Nucléaire) on 15 February 1952 and the establishment of a permanent organisation on 29 September 1954, which is today considered the birthday of CERN.[37]

Niels Bohr and his closest associates in Copenhagen took an early interest in the initiative launched by Auger, Amaldi, and a few other physicists. On 6-10 June 1951, the previously mentioned informal reunion conference on 'Problems of Quantum Physics' took place at Bohr's institute, just before IUPAP (the International Union of Pure and Applied Physics) held its seventh general assembly 11-13 June in Copenhagen.[38] IUPAP was founded in 1922, initially with only thirteen member states (the Central Powers were excluded), and in 1931 it was reorganised in connection with the establishment of ICSU, the International Council of Scientific Unions. At the General Assembly in London in 1934, Bohr was chosen as new IUPAP president, but in absentia and without his consent. Nonetheless, Danish newspapers reported that now Bohr had become president.[39] The embarrassing mistake was soon corrected and instead of Bohr the Swedish physicist Manne Siegbahn, a Nobel laureate of 1924, was chosen as president of IUPAP.

In fact, for political reasons Bohr was opposed to IUPAP, which he found not to be truly international. Only after World War II, when ICSU became associated with UNESCO, did Bohr change his attitude with respect to IUPAP. In a letter to Siegbahn of 1938 Bohr made explicit his discontent with ICSU and IUPAP:

> As you know, from its very beginning I have regarded the establishment of the unions on a not truly international basis to be a fatal mistake. I

37. The early phase of CERN is detailed in Hermann et al. (1987). See also the personal recollections in Amaldi (1989).

38. Wheeler (1951). Rozental (1998), pp. 131-135.

39. *Berlingske Tidende*, 7 October 1934. Millikan, who served as IUPAP president 1931-1934 cabled Bohr from London: "Fitting climax to distinguished congress / enthusiastic election of Bohr president" (BSC, 6 October 1934).

> have kept completely outside the physics union and therefore never been a member of the committee established by the Royal Danish Academy to organise this work in the case of Denmark.[40]

Bohr's old collaborator Hendrik Kramers followed Siegbahn as president of IUPAP and was for this reason in Copenhagen, where Nevill Mott was elected new president. Kramers also participated in the preceding quantum meeting, where he discussed with Bohr the role of his institute within the framework of the new Auger-Amaldi initiative. Since Bohr, together with H. M. Hansen, J. C. Jacobsen, and Møller, was a member of the Danish IUPAP delegation, and Auger represented UNESCO, it was natural to have a meeting with Auger concerning the plan of a European accelerator project. Bohr and Kramers aired their reservations with respect to the Auger-Amaldi 'UNESCO project' and as a possible alternative they suggested to base the future European laboratory in Copenhagen in close connection with the institute for theoretical physics.[41] James Chadwick, who happened to be in Copenhagen at the same time, liked the Kramers-Bohr idea. In a letter to the British physicist George P. Thomson, he wrote: "As you know, I was strongly opposed to the proposals which Auger made some time ago, for I thought they were very impracticable ... Kramers' suggestion appeals to me very much. It is certainly practicable, and it is based on facilities, both in men and apparatus, which already exist."[42]

However, the Kramers-Bohr plan was received unfavourably by the influential Auger and other key physicists preparing the laboratory project. Auger's immediate reaction was that "Bohr, in spite of his vast experience and activity, is a little too old to undertake this international work." And according to one of Auger's young associates, "while Bohr's scientific personality is beyond dispute, confi-

40. Bohr to Siegbahn, 29 March 1938 (BSC, Supplement). The chairman of the Danish IUPAP committee was the physics professor Martin Knudsen.

41. Rasmussen (2002). Auger was director of UNESCO's Department of Natural Sciences. Amaldi too was in Copenhagen for the IUPAP meeting, where he served as one of the vice-presidents. Amaldi (1989), p. 512.

42. Chadwick to Thomson, 14 September 1951, in Hermann et al. (1987), p. 149.

dentially there may be reservations about his spirit of collaboration, his organizational abilities, and the modicum of dynamism which is essential to being a project of the size considered to fruition."[43] The Copenhagen institute was no longer the Mecca of quantum physics that it used to be, and even the glory of the great Bohr had faded.

For a short while it seemed realistic, at least to the Danes, that the CERN project might be located in the Copenhagen area. On 12 December 1951 the newspaper *Politiken* brought an article with the headline 'Huge UNESCO Project: Europe's Atomic Centre Possibly in Copenhagen'.[44] But the utopian idea remained utopian. In the end, not much came out of Bohr's ambitious plan to revitalise and expand the Copenhagen institute by linking it closely to the CERN project. Still in the spring of 1952 Copenhagen was a candidate for the new laboratory, but it clearly lost out to Geneva which a few months later was unanimously chosen as the site of the accelerator laboratory. When a director general had to be found, Bohr proposed Møller's old collaborator, Swiss-American Felix Bloch, who half-heartedly accepted and in 1954 became CERN's first general director. However, already the following year Bloch resigned his post to return to his professorship at Stanford University.

Although Bohr's plan of locating CERN in Copenhagen failed, it was not without beneficial consequences for his institute. The Council decided to establish four study groups, two of them experimental, one mostly administrative, and the fourth a theoretical study group placed in Copenhagen with Bohr as its nominal director. Apart from Bohr, the leadership of the new theory group also comprised Møller, Rozental, and J. C. Jacobsen. In reality (but not officially) a part of the Bohr institute, the group had its own identity and contrary to the other groups it was fully operative from the beginning. While Bohr and his collaborators wanted the theory group to be a more permanent institution, this is not what happened. In June 1953 it was resolved that theorists at the group could under certain circumstances be offered five-year contracts

43. Hermann et al. (1987), p. 154.

44. Rasmussen (2002), p. 36. The preliminary plan was to place the 'atomic city' in the outskirts of Copenhagen, far from Bohr's institute.

but only if they agreed to move to Geneva after three years if the Council of CERN so decided.

To a large extent it was Møller and Rozental who ran the theory group. At a meeting of the CERN nomination committee in October 1953 it was agreed that Møller should take over Bohr's position. On 1 September 1954 he was officially appointed director of the group with Rozental as his right hand being responsible for much of the administrative work. "I have now become master of the keys and can sign Møller's letters", Rozental reported to Pauli in May 1956. "Møller and I will be in Geneva next week, when there is yet another meeting of CERN."[45] With CERN funds available to hire theorists from the twelve member states, the theory group grew rapidly, attracting fellows such as Gunnar Källén (Sweden), Gerhart Lüders (Germany), Kurt Alders (Switzerland), Louis Michel (France), and Bernard d'Espagnat (France) in addition to the Copenhageners Aage Bohr and Ben Mottelson. By the autumn of 1954, when Møller took over the directorship, twenty-four theoretical physicists and two secretaries were working within the group.

In 1956 Møller invited Wheeler, who at the time stayed in Leiden as Lorentz Professor, to come to Copenhagen to give a talk to the CERN theory group. "I spoke with Møller about your lectures to the CERN group", Bohr wrote him, "and we agreed that it would certainly be inspiring to all of us to learn about your new views regarding the fundamental problems."[46] What these views were about appears from a long and interesting letter in which Wheeler told Bohr about his ideas of wormholes and quantum foams, and generally about his vision of building up elementary particles purely from field fluctuations. The key problem that Wheeler wrestled with was, "How does it come about that the vacuum has no mass density?" After having described his thoughts in some detail, Wheeler wrote:

45. Rozental to Pauli, 27 Mai 1956, in Pauli (2001), p. 574.

46. Bohr to Wheeler, 26 April 1956 (BSC).

> Is it really appropriate to make a talk along such unconventional lines as response to Møller's kind invitation to speak? Would not people at the Institute feel happier if I spoke on something more conventional and more acceptable, and would it not be better to reserve the points I have just mentioned for private discussions? ... You will understand that the topic I mentioned to Møller, 'Problems and Properties of a Universe Built of Fields of Zero Rest Mass' is very much closer to my heart than any other question. It is constantly with me, and I shall be so grateful for any illumination which my friends in Copenhagen can give me. So, I will wait to hear when I arrive whether Fields of Zero Mass should be the topic of my seminar or should be reserved for private discussions.[47]

Wheeler, who with his wife Janette stayed in Copenhagen 1-6 May, attributed some of his ideas to conversations with Bohr. As he wrote, "I puzzle over the issue you have so many times emphasized, how the energy of the several fields combines to give for the vacuum a zero mass density."[48]

As to the scientific work done by the theory group, much of it focused on particle physics and quantum field theory. Møller was responsible for the first lecture course given to the theory group in early October 1952. The subject of the course was the pseudoscalar meson theory, an area of research which still attracted interest at the time.[49] Although particle physics dominated, also theoretical areas of no obvious relevance to accelerator experiments were cultivated by the theorists in Copenhagen. One of them was nuclear structure as investigated by Aage Bohr and Mottelson; at a later stage another was Møller's favourite area of research, general relativity and gravitation, which was investigated by a few of the visiting

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47. Wheeler to Bohr, 24 April 1956 (BSC). About ten years later, it was suggested that the vacuum energy density is not zero but given by Einstein's cosmological constant by $\rho \sim \Lambda/G$. This idea, which today is generally accepted, was originally put forward by Lemaître in a paper of 1934.

48. On Bohr and the vacuum energy problem, see Kragh and Overduin (2014), p. 58.

49. Rasmussen (2002), p. 58. Møller's lecture notes are available as a CERN publication, see https://cds.cern.ch/record/212207/files/p1.pdf.

physicists. According to John Iliopoulos, a distinguished Greek particle physicist, "as far as the level of scientific output is concerned, the Centre [in Copenhagen] was a success."[50] The CERN decision that the theory function should be gradually moved to Geneva was followed, with the result that on 1 October 1957 the work of the theory group in Copenhagen was brought to an end. In Geneva, the Theoretical Study Division (as it was now called) soon expanded to about forty members.

Although the Copenhagen theory group closed down, in a sense it lived on in the shape of a new institution, the still existing Nordita or Nordic Institute for Theoretical Atomic Physics, which was later renamed the Nordic Institute for Theoretical Physics. And yet this latter institution was not established as a substitute for the CERN theory group as it was planned and first discussed early on, first by a group of physicists from the three Scandinavian countries at a meeting in Gothenburg on 17 January 1953.[51] Participants in the meeting included Bohr and Rozental from Denmark, Egil Hylleraas from Norway, and Torsten Gustafson from Sweden. At about the same time the Nordic Council (Nordisk Råd) was established as an inter-parliamentary organisation with the aim of strengthening economic, political, and cultural cooperation between the five Nordic countries (including Iceland and since 1955 also Finland). The physicists' proposal of a joint Scandinavian or Nordic research institution was well received by the Nordic Council and especially by the influential Swedish Prime Minister Tage Erlander, who in his youth had studied physics and with whom Gustafson had close connections.[52] It was natural to include also Iceland and Finland into the project, which by 1956 had become truly Nordic rather than merely Scandinavian.

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50. Iliopoulos (1996), p. 289.

51. Pais (1991), pp. 521-523. Rozental (1998), 149-154. Rasmussen (2002), pp. 65-69. For a more comprehensive history of Nordita during the Copenhagen years 1957-2007, see Gudmundsson et al. (2021).

52. Erlander served as Sweden's Prime Minister 1946-1969. At Lund University, he studied for a while physics together with Gustafson, but then changed to political science and economy.

Fig. 36. Board meeting of directors of Nordita, 1958. Niels Bohr is sitting to the right with H. Wergeland to his left side. Standing between the two are Møller and S. Rozental, and to left O. Klein, Aa. Bohr, J. Lindhard, and E. Hylleraas. Credit: Niels Bohr Archive, Photo Collection, Copenhagen.

At a meeting in Helsinki on 21 February 1957 the Nordic Council approved the plans of Nordita, which started its activities on 1 October the same year, the very day that the CERN theory group was terminated. With the exception of one secretary, the whole staff of the theory group remained in Copenhagen to continue under the wings of the new Nordita organisation. From a formal and economic point of view, Nordita was owned by the Nordic Council. Throughout the planning process it was taken for granted that the new Nordic institute should be located in Copenhagen as a kind of annex to Bohr's institute. In 1964 the mathematicians at the University of Copenhagen left the building which had been erected thirty years earlier as part of the Bohr institute complex and with Niels Bohr's brother Harald as its director. After the mathematicians had moved to the new H. C. Ørsted Institute completed in 1962, the building at Blegdamsvej became occupied by Nordita.

The first governing board of the Nordic institute had Bohr as its chairman and Gustafson as deputy chairman. The board included Møller and Rozental from Denmark as well as prominent physicists from the other countries, among them Hylleraas and Svein Rosseland from Norway and Oskar Klein and Ivar Waller from Sweden. With a few exceptions, it was the old boys' network from the happy days of Bohr's institute. When Rozental in 1968 described the history, structure, and activities of Nordita, he stressed its close links to Bohr's old institute founded nearly half a century earlier:

> While the administrative bases of the Niels Bohr Institute and of Nordita are fundamentally different and have to be kept strictly separated, the scientific activities are united. Seminars and lectures are in common. The question of who works with whom, the links between the staff and the visitors and between professors and younger physicists depend only on what subject they are working on. ... When considering theoretical research, one has to treat the Niels Bohr Institute and Nordita together.[53]

According to Pais' biography of Niels Bohr, "Nordita is an important part of the legacy Bohr left to the world of physics in general and of Denmark in particular."[54] One might also and perhaps more justifiably say that Nordita was part of the legacy of Møller, who more than anyone else was responsible for its early and very successful development. Although he was not one of its fathers, from 1957 to 1972 he was professor at Nordita and until 1971 its director. During this early phase it was he, assisted by Rozental, who was in charge of the institution.

The start of Nordita took place less than three months after Møller had hosted the quantum gravity meeting (Section 6.4) and at a time when he had left Copenhagen to work as a guest professor in Pittsburgh at the Carnegie Institute of Technology. Consequently, during a part of Nordita's first year he was unable to participate in its operations and for this reason temporarily replaced by Gustafson as director. "The board meeting at the Nordic institute should take

53. Rozental (1968).

54. Pais (1991), p. 523.

Fig. 37. Møller and Oskar Klein in a conversation of 1968 with Polish physicist Jerzy Plebański, a specialist in general relativity and visitor at Nordita. Credit: Pernilla Klein.

place tomorrow", he wrote to Rozental. "I look forward hearing from you and hope that everything at home proceeds without frictions."[55] Källén, who was one of the first physicists employed at Nordita, wanted Pauli to visit the new institution:

> The Nordic Institute has now started to function with Gustafson as 'leader' [*Führer*] (Møller is now in America and will be back only in the spring). The institute has some money to invite people, so my question is if you could possibly come. ... If this is possible, you will be most welcome and a more official invitation from Gustafson will follow later.[56]

While staying in America, Møller was uncertain about how to refer to the new institution and consulted Rozental on the matter. Should it be Nordic Institute for Theoretical Atomic Physics or Nordita? "I have just written an essay as a contribution to the Hylleraas fest-

55. Møller to Rozental, 3 October 1957 (NBA, Rozental Papers).
56. Källén to Pauli, 23 October 1957, German original in Pauli (2005), p. 579.

schrift", he wrote from Chapel Hill. "There I was first confronted with the problem of how to refer to the institution. I wrote the University's Institute for Theoretical Physics and NORDITA. What do you think is correct? Let me add that the Americans are startled at *Nordic* Institute etc. because of Hitler, but they have nothing against the abbreviation NORDITA. Is this abbreviation now official?"[57]

During Møller's directorship Nordita quickly grew to become an important research and training institution in fundamental physics. Since the late 1960s, the more traditional focus on nuclear and particle physics was expanded to include also research in condensed matter and complex systems. Astrophysics and cosmology attracted interest in particular after Strömgren replaced Møller as director in 1971. Several seminars of astrophysics were arranged and leading astrophysicists (including Thomas Gold and Jesse Greenstein) came to Nordita as visiting professors. Yet another branch of physics cultivated at Nordita was general relativity with research fellows working under the wings of Møller. Altogether a large number of brilliant young physicists got temporary positions at Nordita and also a few not so young. To the latter category belonged Rosenfeld, who joined the Nordic institute in 1958 and stayed there until his death in 1974.

Magnus Magnusson was the first Icelandic fellow at Nordita, where he stayed 1958-1960 working on general relativity. He recalled the atmosphere at the institute as most stimulating from both a scientific and social point of view. "Once during Heisenberg's visit, I was standing on Blegdamsvej waiting for a tram when I saw him in Niels Bohr's office and Bohr walking around in the room. I then recalled the story of their famous meeting, probably in the same office, during WWII, which Aage Bohr had told me about."[58] In

57. Møller to Rozental, 2 February 1958 (Rozental Papers, NBA). The superiority of the Nordic or Nordic-Germanic race was a corner stone in Nazi ideology. Still in 1958, thirteen years after the end of the war, the name 'Nordic' could be associated with Nazi racial belief. Källén's reference in his letter to Pauli to the Nordita director as a 'Führer"' may have been an allusion to the same issue.

58. Gudmundsson et al. (2021), p. 179. See Section 4.2 for the Bohr-Heisenberg meeting.

1973 Magnussson became a member of the Nordita board of directors and in this capacity, he arranged a series of lectures given by the Nordita staff at the University of Iceland, Reykjavik. Among the lecturers were Møller, Strömgren, and Aage Bohr.

On some occasions Møller used his position to invite prominent physicists of the old guard to give lectures at Nordita. The most famous of the lecturers was the legendary Dirac, who came to Copenhagen in March 1960 to speak on his new work on electrodynamics. In a letter to Bohr, Dirac wrote: "My wife and I will come to Copenhagen on 22 March and stay about a week. I have been invited by Møller to the Nordita Institute and shall talk to them about some recent work I have been doing on the Born-Infeld electrodynamics. It seems the electrodynamics is very satisfactory in the classical theory, but there are difficulties with quantization."[59] Dirac returned to Nordita in the autumn of 1966 to give another talk, this time on the quantisation of fields in constrained volumes. On the same day Iver Brevik, a young Norwegian physicist, arrived in Copenhagen to work as a Nordita fellow under Møller. Brevik recalled his excitement when he realised that Dirac would give a seminar in the afternoon:

> Dirac! As all other theoretical physicists, I knew he was regarded as one of the luminaries of theoretical physics in the centenary, and I think I had read his book on quantum mechanics at that time, but I had no idea whether he was still alive. I felt it almost as if Albert Einstein should be expected to come in through the doors! ... The one who presented the speaker to the audience turned out to be Professor Møller, the man whom I primarily had come here to meet. I had never seen him before then.[60]

As representatives of Nordita, in July 1959 Møller and Rosenfeld went to Kiev in Soviet Russia – now Kyiv in Ukraine – to attend

59. Dirac to Bohr, 19 February 1960 (BSC, Supplement). The Born-Infeld theory was proposed in 1934. Dirac's paper on a reformulation of this theory and as a possible alternative to standard electrodynamics was published on 21 March, see Dirac (1960).
60. Gudmundsson et al. (2021), pp. 123-124.

Fig. 38. Møller in conversation with George Uhlenbeck at Nordita conference 16-20 June 1967 in Trondheim, Norway, on statistical mechanics. Credit: Niels Bohr Archive, Photo Collection, Copenhagen.

the ninth International Conference on High Energy Physics, the first of the prestigious 'Rochester conferences' held in an Eastern Bloc country.[61] A large number of high-profile physicists showed up in Kiev, among them Landau, Marshak, Chew, Pontecorvo, Schwinger, and Salam. Møller gave a colloquium at the conference and wrote an abstract of it in the conference report. He and Rosenfeld were followed on their travel from Copenhagen to Russia by Bernard Peters, an American specialist in cosmic rays who had recently been hired by Bohr's institute. While Møller and Rosenfeld went to Kiev, Peters attended an associated conference on cosmic ray physics taking place in Moscow 6-11 July.[62]

61. The first conference in Rochester in 1950 was chiefly organised by Robert Marshak. In 1957 a commission under IUPAP took responsibility of the meetings.
62. Polish-born Peters (1910-1993) was forced to leave the United States for political reasons. He worked 1951-1958 at the Tata Institute of Fundamental Physics in

Although most of Nordita's activities took place in Copenhagen, during Møller's period as director the institute also began to arrange international meetings and workshops in the other Nordic countries. The first of these was a conference on statistical mechanics in Trondheim, Norway, from 16 to 20 June 1967. Møller and his wife were among the participants and so was the Norwegian-American theoretical chemist Lars Onsager, who the following year would be honoured with the Nobel Prize in chemistry.[63] Møller gave a lecture on his new ideas of relativistic thermodynamics (Section 7.2). The old guard was represented also by Uhlenbeck, Rosenfeld, Rozental, and Waller.

As director of Nordita, Møller made it a tradition to invite the scientific staff and the fellows to a dinner party at his and Kirsten's home at least once per year. One of the participants, Matts Roos from Finland, recalled that "here Møller taught us the noble art of smoking a cigar and told us it was seen as unsolidaric to drink Tuborg beer, as the NBI [Niels Bohr Institute] was supported by the Carlsberg Foundation." And there were other ways in which Møller could enjoy and educate the visitors. The American physicist Arthur Schild was a frequent visitor to Nordita, where he collaborated with Møller on problems of general relativity. According to the secretary Helle Kiilerich:

> One day he [Schild] arrived with a bottle of snaps, the Danish dram. He explained that he had had lunch with Møller in Nyhavn – at that time a somewhat rough place with lots of pubs for sailors and not at all surroundings where you would expect to find the fine gentlemen Schild and Møller – and had learned about the Danish drink 'a little black': put a coin in a cup, add coffee until you cannot see the coin and then snaps until the coin is visible again.[64]

Mumbai, and after a few years at Bohr's institute he became director of the Danish Space Research Institute. See *Physics Today* **46** (12), 1993: 64-65.

63. Gudmundsson et al. (2021), pp. 50-53, with pictures from the conference. Onsager (1903-1976) was originally trained in engineering in Trondheim, but since 1928 he worked in the United States.

64. Gudmundsson et al. (2021), p. 208 and p. 77. After 1970 it was no longer 'unsolidaric' to drink Tuborg beer, as that year the Tuborg breweries became a part of

The younger Nordita generation included a number of important Danish and foreign physicists of which I shall only mention a few from the early period under Møller. The American theorist Gerald E. Brown became a Nordita professor in 1960 and remained in the position until 1985; David Pines, another American physicist of renounce, worked in 1970 as a Nordita visiting professor; Gordon Baym, an American specialist in condensed matter physics, came first to Nordita in 1970 as a visiting physicist and again on many later occasions; James (Jim) Hamilton, an Irish mathematical physicist, was a Nordita professor from 1964 to 1968 and returned to Copenhagen later on. Finally, the Danish theoretical physicist Holger Bech Nielsen, one the earliest contributors to what became string theory, became a Nordita fellow in 1969 at the age of 28 and worked subsequently as a consultant to Nordita until he was appointed professor in 1985. A biography of Hamilton provides an impression of Nordita in the 1960s:

> Møller and Rosenfeld had been with Niels Bohr for many years. Møller had been at NBI since 1929 and remained there until 1975. He had also been director of the CERN Theoretical Study Group from 1954 until 1957 and was therefore a natural choice for the equivalent position at Nordita. Møller and Rosenfeld's modification of the meson theory was one of the models Jim had referred to back in the early 1940s. ... In addition to the scientific staff (the director and professors) and the administration there were about ten Nordic fellows, young physicists from around Scandinavia, supported by Nordita on one- or two-year grants. There were also other young physicists who would visit for short periods. This was the main purpose of Nordita, to provide a centre of excellence from which to promote scientific collaboration between Nordic physicists. ... Though Christian Møller had interests close to particle theory, much of the emphasis of Nordita's research until 1964 had been on the nuclear physics side. Jim was the first Nordita professor expert in particle physics.[65]

Carlsberg A/S controlled by the Carlsberg Foundation.

65. Online biography of James Hamilton, https://www.jameshamiltonphysicist.com/, chapters 12-13.

Three Nordita physicists attended the fourteenth Solvay congress in 1967, an indication of the institution's scientific reputation. One of them was Hamilton, and the other two were Rosenfeld and Møller with the latter serving as chair of the scientific committee. While Nordita originally referred to 'theoretical atomic physics', in 1989 it was decided to remove 'atomic' and change the name to the more appropriate Nordisk Institut for Teoretisk Fysik. Somewhat illogically the abbreviated form Nordita was retained. The institution still exists but no longer in the capital of Denmark. After half a century at the Bohr institute, in early 2007 Nordita moved to Stockholm, where it is currently hosted by the Royal Institute of Technology and Stockholm University with funds shared by the Nordic Council and the Swedish Research Council.

As an able science administrator with experience in national and international science committees, during the 1960s Møller got involved in several high-energy physics projects apart from those related to Nordita. Thus, Møller continued to do CERN-related work even after the Copenhagen theory group was terminated. In 1959 he was elected a member of the important CERN Science Policy Committee, a post he held until 1973, when he was replaced by Aage Bohr. At Møller's death the CERN theorists sent a telex message to the institute in Copenhagen: "We are profoundly touched by the sad loss of prof. Christian Moller. He will always be remembered for his major contributions in electrodynamics, field theory and general relativity. He was very close to CERN and to the theory division in particular, where he had many friends who admired his personal qualities, and so we deeply mourn his passing."[66]

On the national scene, in 1963 Møller became part of a Danish group known as the 'Accelerator Committee' (Acceleratorudvalget) established by the Ministry of Education with the purpose of investigating how to make best use of the Danish contributions to CERN. One of the questions discussed by the committee was the possibility of a joint Nordic accelerator, an idea promoted by Källén in Sweden and supported by Møller, who was generally in favour of Nordic collaboration related to accelerators. However,

66. Telex of 18 January 1980 (NBA, Aage Bohr Papers).

not all the physicists in the committee agreed and by late 1965 the idea was abandoned.[67]

In the early 1960s Møller also took part in the initial discussions of establishing a large international centre of particle physics aimed in particular at young physicists from third world countries. What became the International Centre for Theoretical Physics (ICTP) was the brainchild of the young Pakistani-British physicist Abdus Salam, one of the fathers of the standard model of elementary particles and a Nobel Prize laureate of 1979. Salam first suggested his plan at a 1960 conference of the International Atomic Energy Agency (IAEA), an organisation set up three years earlier in response to the controversial uses of nuclear technology. It was taken seriously enough to be considered by an IAEA panel of experts which convened in Vienna in March 1961. Among the invited experts were three Danish physicists, Aage Bohr, Rozental, and Møller.[68] Although the panel unanimously supported the idea on scientific, political, and cultural grounds, as expected there was no agreement as to the location of the future physics centre. Nor was the Scientific Advisory Committee of IAEA convinced of the need for a central institute. In a letter of 1962, Salam reported to Møller:

> You are probably aware of the battle that has been going on since September 1960 in the International Atomic Energy Agency regarding the proposal for setting up an International Institute for Theoretical Physics. ... As you probably know, the Danish Government has made an offer for one million dollars for the building if the Centre is set up at Copenhagen and the Italian Government has made a similar offer provided the Centre is set up at Trieste. ... I do very much hope though your continued interest in the Centre comes to existence as early as possible and justifies all the hopes which have been built up on the idea of a truly international collaboration in our subject.[69]

67. Rasmussen (2002), pp. 105-112. Jarlskog (2014), p. 52.
68. De Greiff (2002). Rozental (1998), pp. 155-157.
69. Salam to Møller, 28 September 1962 (CMP).

Møller and other Danish physicists including Niels and Aage Bohr saw an opportunity to get the centre to Copenhagen as part of the city's already existing physics infrastructure. The other candidate – apart from less serious offers from Turkey (Ankara) and Pakistan (Lahore) – was Trieste in Italy, which was supported by French and Italian physicists.

To study the offers on the table a panel of three advisers was set up. The three advisers were Robert Marshak from the United States, Jayme Tiomno from Brazil, and Léon Van Hove, the Dutch head af CERN's theory division. In May 1963 the IAEA expert panel summarised the choice between the two candidate cities with these words: "Copenhagen would be a more favourable location than Trieste from the point of view of existing theoretical environment whereas Trieste would be favoured on the basis of financial commitment."[70] Indeed, but the economic and political arguments won over the scientific arguments. For a brief period of time the Copenhagen physicists tried to convince Danish politicians to invest the necessary funds in the project, but with no success. Copenhagen lost to Trieste, as it had previously lost to Geneva in the case of the CERN accelerator project. On 14 June 1964 the IAEA approved the final plans for the ICTP in Trieste, which started operating the same year with Salam as its director.

Møller often got requests from graduate students and young physicists from abroad wanting to continue their studies at either the Bohr institute or the associated CERN theory group. One of them was Léon Van Hove, a 24-year-old Belgian theorist who in 1947 had visited the Copenhagen institute for just a few weeks. He now wanted to do some serious research and in 1948 asked Møller about the possibility for a six-months stay. "I should like to work under your direction, and I hope you can accept me as a pupil during that period", he wrote. "The goal of my stay in Copenhagen is firstly to improve my knowledge of physics, and secondly, if you find it possible, to treat by mathematical means a physical problem of actual interest. I should be very happy if you could choose such a

70. IAEA report of 21 May 1963, quoted in De Greiff (2002), p. 52.

problem for me."[71] Møller accepted the application and Van Hove worked in Copenhagen for half a year, after which he went on a research fellowship to the Institute for Advanced Study in Princeton. In 1959 he was appointed director of the CERN theory division in Geneva and in 1975 director-general of CERN. Møller and Van Hove, his former 'pupil', collaborated on a number of occasions both scientifically and administratively.

Another of the requests was more unusual and deserves to be dealt with at some length. In October 1953 Møller received a letter from one Steven Weinberg, a nineteen-year-old student at Cornell University who had not yet obtained his bachelor degree but claimed to have studied "classical mechanics, electrodynamics, statistical mechanics, and quantum mechanics at an advanced level."[72] He planned to apply for a Fulbright grant and optimistically thought he would get one. Could professor Møller please help him with his plan of doing graduate studies at the institute in Copenhagen? Møller, somewhat surprised by the self-confidence and arrogance of the American student, tried to dissuade him. For one thing, the graduate physics program in Copenhagen went on in Danish, which surely would be a serious problem. Moreover, although the seminars and colloquia were in English, they were "on a rather too advanced level" for the young man. "I dare not advice you at such an early date to join our group here but would suggest that you postpone your stay here to sometime in the future. I also want to draw your attention to the fact that some other, somewhat older physicists from the USA are applying for Fulbright grants and consequently, preference from the U.S. government will probably be given to post-doctoral fellows."[73]

But Steven Weinberg did not give up easily. In a letter of early 1954 he admitted the problem with the Danish language without being deterred by it: "If I receive a Fulbright grant, I will have to

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71. Van Hove to Møller, 24 January 1948 (CMP).

72. Weinberg to Møller, 9 October 1953 (CMP).

73. Møller to Weinberg, 28 October 1953 (CMP), who added: "I hope you will not feel disappointed by this answer, but that you will come back to your intentions at a somewhat later date."

attend an intensive course in this language. Also, I would study Danish independently, cross the Atlantic on a Danish ship, and spend several weeks in Copenhagen before the beginning of lectures. I have a knowledge of German, which might help. … I intend to concentrate my next year's work on field theory and nuclear physics."

Møller answered that *if* Weinberg received the Fulbright grant and was still eager to come to Copenhagen, he would be welcome. "I hope that you have not misunderstood my letter of October 28, 1953, but realize that some difficulty might exist for you here, but I am sure that in the course of a short time you will be able to master them and, subsequently, derive the profit necessary for the progress of your studies."[74] After having been informed that Weinberg had been awarded a National Science Foundation scholarship, Møller pointed out to him that courses and colloquia only started in early September. "A degree will not be obtainable for you from the University of Copenhagen, but I shall be pleased to be your scientific adviser and, perhaps, you will find opportunity to prepare your degree here so that you can take it with a University in the States not too late after your return."[75] Weinberg arrived in Copenhagen on 9 July and stayed for most of a year.

The institute secretary Betty Schultz recalled that not all visitors were equally welcome and that a few of them arrived with no recommendation. She remembered one of them, but not his name:

> [It was] a very young man, and he wrote to Moller and asked if he could come. Moller did not know anything about him and did not think he was anything. And he wrote no, we have no room. He must be here [he said] and he did nothing good, and he only did it to say that he had worked at Professor Bohr's Institute. And he rented a flat, a furnished flat, and destroyed the furniture in such a way that Rozental wrote to him, "We have never had a man like you and we hope never to get such a man again."[76]

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74. Weinberg to Møller, 3 January 1954, and Møller to Weinberg, 8 March 1954 (CMP).

75. Møller to Weinberg, 13 May 1954 (CMP).

76. Interview by Charles Weiner of 25 March 1971, American Institute of Physics. Online as https://www.aip.org/history-programs/niels-bohr-library/oral-histories/4867-1.

It is unclear whom the unwelcome young man was, but possibly Schultz had Steven Weinberg in mind.

In any case, after Weinberg had left, Møller reported to the program director for fellowships under the National Science Foundation:

> [Steven Weinberg] came to Europe right after having taken his bachelor's degree and, in the beginning, must have found it rather difficult to follow the colloquia and discussions which usually are held at a level too high for a beginner. Thus, he was obliged to devote the first months to studying before he could start on his own work. He was interested mainly in field theory and came under the advisership of Dr. G. Källén. He considered closely the Lee model, and … in continuation of the paper by Pauli and Källén, he found that the interaction between the N- and V-particle is not real. Before leaving, he gave a colloquium in which we got the impression that he has acquired a good knowledge of modern aspects of field theory and that he intends to continue on these lines. However, I cannot avoid saying that we have tried, before his arrival, to convince him that he was too young to fit in with the Institute's schedule, and I still think that he might have had greater profit of a stay in Copenhagen at some later date of his education. However, he is a bright fellow and will certainly be able to do good work in the future. Much is to be learned for him, both in scientific and personal respects. Due to his young age it is difficult to give a final statement, he might develop favorably soon.[77]

And indeed, young Weinberg did develop favourably – and more than that. At his death in July 2021, he was hailed as perhaps the greatest theoretical physicist of his time, an intellectual giant who not only received the Nobel Prize for his fundamental contributions to the standard model of elementary particles but also did very important work in cosmology and general relativity. Besides, Weinberg was a high-profile popular science writer who eagerly engaged in discussions concerning the philosophical, political, and sociological aspects of the physical sciences. Without mentioning Møller, much later he recalled his stay in Copenhagen and his fruitful collaboration with Källén as his revered teacher:

77. Møller to Bowen C. Dees, 4 July 1955 (CMP).

> In the summer of 1954, having just finished my undergraduate studies at Cornell, I arrived at the Bohr Institute in Copenhagen, where Källén was a member of the Theoretical Study Division of CERN. … But my real reason for coming to Copenhagen with my wife was that we had just married, and thought that we could have a romantic year abroad before we returned to the U.S. for me to enter graduate school. … It wasn't long before people at the Institute let me know that everyone there was expected to be working at some sort of research.[78]

In early 1955, Weinberg knocked on Källén's office door and the Swedish theorist suggested him to take a critical look at a field-theoretical model that Tsung-Dao Lee had recently proposed. "I finished the work on the Danish freighter that took my wife and me back to the U.S., and soon after I started graduate school at Princeton I had published the work as my first research paper."[79] When Møller in the spring of 1954 hesitatingly admitted the Cornell student to join the institute on Blegdamsvej, little did he know, and he certainly did not expect, that the student would soon become a celebrated leader of the theoretical high-energy physics community.

A few years after Weinberg left Copenhagen another of the coming fathers of the standard model of elementary particles, Sheldon Lee Gashow, came to Bohr's institute on a National Science Foundation fellowship. He stayed in Copenhagen 1958-1960 and returned in 1964, during which period he wrote two of the papers on electroweak interactions and quark theory that in 1979 earned him a Nobel Prize shared with Weinberg and Salam. Glashow recalled with some awe that he had several conversations with Niels Bohr in the institute lunchroom, although "Sometimes I couldn't tell whether he was speaking Danish or English. Even when I could follow the words, the precise sense of what he was saying often escaped me."[80]

78. Weinberg (2014), a written version of an oral address delivered in Lund in 2009.
79. Weinberg (1956).
80. Glashow (1988), p. 138.

8.4. Organiser and science diplomat

As stated in the statutes of the Nobel Foundation, those with a permanent right to submit candidates for a prize include "Permanent and acting professors in physics and chemistry at Swedish and other Nordic universities and technical colleges in 1900 (universities in Uppsala, Lund, Oslo, Copenhagen, and Helsinki; the Caroline Institute; the Royal Institute of Technology in Stockholm; and the Stockholm Högskola)."[81] As a tenured professor in physics at the University of Copenhagen, Møller thus could nominate candidates for a Nobel Prize. However, contrary to Niels Bohr, who took such nominations very seriously and proposed no less than 25 nominees, Møller only acted as a nominator a few times and never individually.[82]

Together with Bohr, in 1950 he nominated Born for a shared prize with Kramers. While Born belatedly received the Nobel Prize in 1954, Kramers (who died in 1952) never did. Møller also co-nominated Landau for the 1963 prize, this time in a nomination jointly with Niels and Aage Bohr, Rosenfeld, and Mottelson. Landau actually received the prize in 1962, but the Copenhagen nomination was submitted at a time when Niels Bohr was still alive and before this was known. Moreover, together with Rosenfeld and Strömgren, in early 1970 Møller nominated the Swedish physicist Hannes Alfvén for the prize, citing his important work on magneto-hydrodynamics and its role in astrophysical processes. It was indeed for this work that Alfvén was awarded the Nobel Prize later in the year.

According to the statutes of the Nobel Foundation, the nominations for the past fifty years are kept secret and for this reason it is unknown if Møller proposed nominees by himself after 1972. However, it is known from archival sources that in 1974 he and Strömgren nominated Oskar Klein for this year's prize. The two nominators highlighted as Klein's most 'spirited' work his 1927 article in *Zeitschrift für Physik*, which "in the first approximation leads to the same results as the later developed quantum electrodynamics,

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81. Friedman (2001), p. 23.

82. On Bohr as a very successful Nobel Prize nominator, see Aaserud (2001), pp. 298-307.

but without the serious divergence difficulties which blemishes the latter theory."[83] As mentioned in sections 1.1 and 1.2, Møller and Strömgren had since their youth highly appreciated Klein's paper, which however was not generally valued as a 'classic' by the physics community and was probably unknown to most physicists in the 1970s. Nor was their critical attitude to renormalised quantum electrodynamics shared by the majority of physicists at the time. Moreover, to single out a paper written 47 years ago was problematical given the rules of the Nobel Foundation, according to which the award shall be given "for the most recent achievement in the fields of culture referred to in the will [of Nobel] and for older works only if their significance has not become apparent until recently." In any case, the Møller-Strömgren initiative failed. Klein never received the prize, which in 1974 was awarded to Anthony Hewish and Martin Ryle for their astronomical discoveries (pulsars and radio astronomy, respectively).

The following year Møller co-signed a letter of nomination sent in January 1975 from Copenhagen proposing Aage Bohr and Mottelson for the prize.[84] The letter was signed by Aage Winther, a physics professor at the Niels Bohr Institute, and also by Møller and Strömgren. Whereas Winther was a specialist in nuclear structure research, Møller and Strömgren were not and they merely entered the proposal as supporters. Contrary to the earlier attempt to obtain a Nobel Prize for Klein, the nomination from the three Copenhagen scientists was successful insofar that Bohr and Mottelson were awarded the 1975 prize, sharing it with the American nuclear physicist James Rainwater.

Whereas Møller's limited involvement in Nobel Prize nominations was coordinated with the Copenhagen institute for theoretical physics, his activity within the Solvay institution was independent of it. As pointed out in earlier chapters, since 1948 he had been a member of the Solvay scientific committee in physics and partici-

83. Møller and Strömgren to the Nobel Committee in Physics, 28 January 1974 (CMP; in Danish). Klein, who passed away in February 1977, had earlier been nominated by Bohr and others.

84. Kragh and Nielsen (2001), p. 329.

pated in most of the congresses in Brussels. When a new chairman of the scientific committee had to be found after the death of Oppenheimer in 1967, Amaldi, Géhéniau, and Heisenberg suggested that he took over. Møller accepted, if not with enthusiasm: "I feel that I have to accept – after all, 6 years ago I reluctantly accepted to be the vice-chair – and in that case I shall certainly try to do my best in this situation. ... If you can find somebody else to take over the chairmanship it is certainly all right with me."[85] In November 1978 Møller joined the meeting of the sixteenth congress, the theme of which was 'Order and Fluctuations in Equilibrium and Non-Equilibrium Statistical Mechanics'. This meeting, which was chaired by Léon Van Hove, was his last involvement with the once so important institution. The only other Danish scientist who previously had been active in the Solvay organisation was Martin Knudsen, a physics professor and oceanographer who from 1912 to 1930 served as secretary of the Solvay Institute.

Whether as a professor of mathematical physics at Bohr's institute or in his capacity as director of the CERN theory group and later of Nordita, Møller often operated behind the scene, so to speak. While Bohr was of course the undisputed head of the institute, in many cases he relied on Møller to take care of practical, administrative, and scientific tasks. We have mentioned several such cases in previous sections, such as when Bohr sent Møller to Paris in 1947 to a politically inconvenient symposium (Section 5.4) or when Møller acted as Bohr's emissary in helping Charlotte Houtermans after she escaped from Soviet Russia to Copenhagen (Section 3.3). When Bertolt Brecht turned up at the institute in the autumn of 1938, it was Møller and not Bohr who had to take time speaking with the German refugee playwright (Section 4.1). During the period from October 1943 to August 1945, when Bohr was away from Denmark, Møller more or less ran the institute together with Jacobsen (Section 4.2). It was in part due to Møller's diplomatic

85. Møller to Géhéniau, 5 April 1967 (CMP). Jules Géhéniau, a Belgian theoretical physicist, was a scientific secretary of the Solvay Institute and co-responsible for many of the congresses.

skills that Heisenberg came to Copenhagen in January 1944 and helped to end the occupation of the Blegdamsvej institute.

As mentioned, in 1955 Møller was the sole representative of the Danish physics community at the Berne jubilee conference on relativity theory. In Berne he met with the eminent Russian physicist Vladimir Fock, who was not only a leading theorist in quantum mechanics but also an expert in general relativity theory. Assumedly on Bohr's initiative, Møller used the occasion to invite Fock to Copenhagen. Only in the spring of the following year did Fock receive a formal invitation from Møller which was handed over to him by Lev Sliv, a Leningrad nuclear physicist who had visited the institute.[86] Fock happily accepted the invitation and after having received permission to travel abroad – which was needed for a Soviet physicist – he spent about a month in the Danish capital, more precisely between 12 February and 16 March 1957. After his return to Leningrad, he wrote an article about his journey West of the Iron Curtain and his conversations with Bohr, whom he described as perhaps "the greatest among contemporary physicists." According to Fock:

> I had thought about talking and working a little with Niels Bohr ... but these plans took a more concrete form in 1955, after a conference in Berne (Switzerland), which was held in commemoration of the 50th anniversary of the theory of relativity. At this conference Niels Bohr's collaborator, Professor Christian Møller, on behalf of Niels Bohr and the Institute for Theoretical Physics in Copenhagen of which he is the director, expressed the wish that I should come there and work for a while. This invitation was confirmed by Niels Bohr in 1956, ... The frightening events which took place at the end of 1956 luckily had no influence on these plans, and on the 12th of February, 1957, I flew from Moscow to Copenhagen, where I arrived in the evening of the same day.[87]

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86. Jacobsen (2012), pp. 295-301. Fock to Møller, 8 May 1956, and Møller to Fock, 13 June 1956 (CMP).

87. Pauli (2005), pp. 218-221. The "frightening events" to which Fock refers were undoubtedly the Hungarian revolt in October-November 1956 and its brutal suppression by Soviet troops.

Fock also reported about the general life at the institute, about how the lunch room was supplied with sheets of paper on the tables "upon which one can write formulae", and about the lighter side of the institute: "Everyone gathered together twice for an evening's entertainment, where one showed movies and where the participants themselves entertained (accompanied by general laughter, the theoreticians had to perform very simple experiments), and a modest evening meal was served." Much of Fock's time in Copenhagen was spent in conversation with Bohr and a few others concerning the foundation of quantum mechanics and the role of the complementarity principle in particular. Fock was generally in favour of Bohr's interpretation, while some of his colleagues in Russia considered it problematical because of its alleged elements of positivism and bourgeois idealism.[88]

Quantum mechanics was not the only subject with which Fock engaged during his stay in Copenhagen. He also took time to lecture on relativity theory:

> At the request of Professor Møller I gave in English three lectures on the theory of relativity. The theme of the first one was 'On the Concepts of Homogeneity, Covariance and Relativity', the second 'Approximate Solutions of Einstein's Equations', and the third 'Gravitational Waves'. Even if only a few physicists at the Institute were especially occupied with the problems of relativity theory, almost all of the members, including Niels Bohr, attended the lectures, and the discussion with him was very lively. ... All three lectures took place within the same week because Professor Møller, who more than anybody else was interested in them, had to go away for some time (to Italy).[89]

Fock had turned to general relativity in 1939 and by the 1950s developed a theory which in some respects differed from Einstein's.

88. According to Graham (1988), pp. 311-313, Fock may have influenced Bohr to use a less 'subjectivist' language in his later writings.

89. Pauli (2005), p. 219. Fock's influential monograph *The Theory of Space, Time and Gravitation* was published in Russian in 1955 and appeared in an English translation four years later.

Fig. 39. As Fock noted from his visit in 1957, the canteen was an important part of the Institute for Theoretical Physics. Here is Møller (right) in the canteen during the 1936 conference. To his left is Hendrik Casimir. Credit: Niels Bohr Archive, Photo Collection, Copenhagen.

Although he considered Einstein to be a great physicist, he thought he was a poor mathematician whose theory of gravitation was not quite correct.[90] There is little doubt that Møller did not share Fock's unorthodox view of the general theory of relativity.

Although Møller deeply admired Einstein, his admiration was not boundless but essentially limited to Einstein's works during the two creative decades from 1905 to 1925. He shared the attitude of most of his colleagues in relativity theory that Einstein's many attempts in the later era to formulate a truly unified theory of gravitation and electromagnetism were false trails of no scientific importance. They were clever mathematics, but not physics. When asked in 1953 by a Danish journalist of his opinion of Einstein's new field theory "about the entire universe", he said: "During the later years Einstein has developed his theories in a direction so far from the

90. Gorelik (1993).

domain of experiments that many physicists consider them to be artificial, and that in spite of the enormous esteem they naturally have for his epoch-making works of the past. One is not convinced that the road which Einstein has traversed over the later years is the right road."[91] The same year, in a review of a new edition of Einstein's *The Meaning of Relativity*, Møller repeated that the new generalised theory was "rather artificial" and beyond experimental tests. As he further pointed out, even if the theory in a formal sense unified gravitation and electromagnetism, it had nothing to say about the quantum forces and was therefore in any case incomplete.[92]

As one of the sixteen founding members of the ICGRG (International Committee on General Relativity and Gravitation), Møller could not avoid being drawn into controversial organisational and political problems directly or indirectly rooted in the Cold War atmosphere. He participated in the 1965 London conference, where it was decided to have the next one in Tbilisi in Georgia then part of the Soviet empire. Two major events on the world-political scene deeply influenced the Tbilisi meeting by threatening the unity of the international community of experts in relativity theory. One was the Six-Day War in June 1967 between Israel and the Arab states Egypt, Jordan, and Syria, which caused an interruption of diplomatic relations between the Soviet Union and Israel. The other event was the invasion of Czechoslovakia in August 1968 by troops from the Soviet Union and other Warsaw Pact countries. André Mercier openly advocated members of the ICGRG to stay away from Tbilisi, whereas others were more cautious. As president of the ICGRG, Hermann Bondi sought to steer a middle course. On the one hand he withdrew official sponsorship from the Soviet conference, but on the other hand he saw no reason to cancel the conference and he did not recommend relativists to boycott it, such as his first inclination had been.

Nonetheless, this is what many Western scientists and most members of the ICGRG did. An additional reason was that no Israeli

91. *Information*, 10 August 1953.

92. Review of the fourth edition of *The Meaning of Relativity*, in *Mathematica Scandinavica* **1** (1953): 316-318.

scientists were invited, or at least not invited in time, which was widely considered an expression of the anti-Israel attitude of the Soviet organisers. The GR5 Tbilisi conference that took place 9-13 September 1968, just a few weeks after Soviet tanks had entered Prague, was mostly attended by physicists from the Soviet Union and its allied countries, but a small group of American and European physicists also participated. This group included ICGRG members John Wheeler, Bryce DeWitt, and Møller, and in addition Frederik Belinfante, Arthur Komar, Bruce Partridge, Remo Ruffini, and a few others. Møller contributed with a talk on relativistic thermodynamics.

Among the many Russians in Tbilisi were the eminent cosmologist Yakov Zeldovich, one of the fathers of the new big-bang cosmology, and also Andrei Sakharov, a theoretical physicist who may today be better known as a political dissident and the recipient *in absentia* of the Nobel peace prize. Sakharov had in 1966 proposed a mechanism for the formation of baryon asymmetry which explained the observed asymmetry of matter and antimatter in the universe. On the instigation of Zeldovich, in Tbilisi he presented a paper in which he conceived gravitation as the result of the microscopic structure or 'elasticity' of the vacuum filled with oscillation energy. As Sakharov recalled about the conference, "I found the papers informative, and profited to an even greater degree from the personal meetings with Soviet and foreign scientists."[93] Just a few months before the Tbilisi conference his controversial essay 'Reflections on Progress, Peaceful Coexistence, and Intellectual Freedom' was published in the *New York Times*, which marked the beginning of Sakharov's life as a dissident and advocate of civil liberties.

The difficult East-West relations during the Cold War and Møller's attempts to ease them are illustrated by a letter from Møller to Ivanenko:

> I very much agree with your plea that we should all work patiently on the task to keep the unity of relativists in all parts of the world. Therefore, I beg you to use your influence to prevent any hasty actions from

93. Sakharov (1990), p. 296.

> the side of our colleagues from the 'eastern' countries, which could jeopardize this unity. ... On the other hand we from our side shall do everything possible to avoid foolish actions like the one of our Texan colleague during the meeting of our committee with the plenum of relativists.[94]

In a letter to Bondi after the conference, Møller reported: "As a whole, the Conference in Tbilisi was very pleasant and successful, although, of course, we were missing a large number of colleagues from Europe and U.S.A. It was very unfortunate that the politics of the Great Powers were able to interfere with the unity of scientists, which has worked so well in our field of research since the Berne conference in 1955."[95] The attitude of Bondi and Wheeler was that political and ethical concerns should not get in the way of fruitful scientific discussions, a pragmatic or perhaps cynic attitude which Møller seems to have shared. In Tbilisi, those who attended the meeting discussed the venue for the next conference, which Nathan Rosen had previously proposed to take place in Haifa. However, for political reasons Israel was considered unacceptable by the Soviet attendees and as an alternative Møller was asked to organise the GR6 conference in Copenhagen.

Møller accepted the request and after it had been confirmed he joined forces with Léon Rosenfeld, Stefan Rozental, and Bengt Strömgren in a local organising committee. The hosting organisation was Nordita and not the Niels Bohr Institute which from a formal point of view was not involved with the forthcoming conference. In 1971 Møller stepped down as director of Nordita and left the post to Strömgren, who was thus – apart from his expertise in theoretical astrophysics – an obvious member of the organising

94. Møller to Ivanenko, 10 September 1971 (CMP). The Texan colleague to whom Møller referred may have been Ivor Robinson, a British-American theorist of Jewish descent. See Martinez (2019), p. 126.

95. Møller to Bondi, 15 October 1968, in Lalli (2017), p. 85. Møller to Fock, 10 October 1968 (CMP). The political and organisational circumstances around the conferences in Tbilisi and Copenhagen are detailed in Lalli (2017), pp. 75-125. See also Martinez (2019).

committee. One may assume that it was due to Strömgren, at least in part, that relativistic astrophysics appeared much more prominently in the program of the Copenhagen conference than in the earlier ICGRG conferences. Moreover, for the first time the list of invited scientists included a large number of specialists in cosmology and astrophysics, including notables such as Dennis Sciama, Irwin Shapiro, Igor Novikov, Roger Penrose, and Stephen Hawking. Yet another participant in this group was Jim Peebles, a former student of Robert Dicke who six years earlier had acquired scientific fame for his co-discovery of the cosmic microwave background.[96] His mentor Dicke also took part in the Copenhagen conference.

Although busy with arranging the GR6 conference, Møller found time to participate in a meeting in Bonn on 21-23 June organised by Bleuler.[97] Among the many participants in this meeting on General Relativity and Mathematical Methods in Field Theory were, apart from Møller and Bleuler, Jordan and Jürgen Ehlers from Germany, Leopold Halpern from Austria, and Jules Géhéniau from Belgium. As usual Møller went by car, bringing his wife with him. To return to the preparations of the GR6 Copenhagen conference, the policy of following the old rule with limited attendance per invitation inevitably caused problems. There simply were many more scientists wanting to participate than the 200 which Møller and his committee had taken as a maximum. Because of the large number of participants, the conference convened at the new H. C. Ørsted Institute, which was located in Copenhagen just a short walk from the Niels Bohr Institute. Several of the physicists complained that the conference was unnecessarily elitist and not sufficiently open or democratic. Still, with a total attendance of about 230 scientists the GR6 conference was larger, broader in scope, and more internationally representative than any of the previous conferences in the ICGRG series.

96. In the spring of 1965 Dicke and Peebles were the first to demonstrate that the microwave background recorded the previous year is of cosmological origin, a fossil of the hot big bang.

97. Bleuler to Møller, 22 May 1971 (CMP). Møller to Bleuler, 2 June 1971 (CMP).

On 5 July 1971 Møller opened the meeting with a general survey of recent progress in general relativity. As he pointed out, the field had developed remarkably, not unlike the ugly duckling in Hans Christian Andersen's fairy tale metamorphosing into a beautiful white swan. In Møller's words:

> For many years, and already when I was a student in the middle of the twenties, most physicists thought that the Relativity Theory was finished, and that it did not offer any new interesting problems. ... The situation lasted for about forty years. The few physicists working in the field of relativity – the relativists as they called themselves – formed a clan, or rather a small sect which was somewhat looked down upon by other, more successful groups of physicists. However, the development of the last ten or fifteen years has brought a complete change in this respect. In the first place, extensive investigations, in particular by the younger physicists and mathematicians, have given us much better understanding of the mathematical structure and of the physical contents of the theory. Secondly, the astonishing development of experimental technique and the collaboration with our astrophysics friends has given us new experimental tests of the theory, and opened up the possibility of new exciting applications of the theory in cosmology.[98]

After Møller's opening speech followed a large number of talks and presentations, divided in morning sessions and parallel afternoon sessions. On the last day of the conference the Polish physicist Andrzej Trautman summarised what he considered to be the main results of the Copenhagen meeting. "For me", he said, "the occurrence of singularities in solutions of the field equations is an indication of the fact that classical general relativity is not applicable at very large densities and curvatures."[99] Møller most likely agreed. In between the scientific sessions there were social programs including a visit to the Carlsberg Breweries, another visit to Frederiksborg Castle in Northern Zealand, and a reception in the City Hall by the Lord Mayor of Copenhagen.

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98. Unpublished manuscript cited in Gudmundsson et al. (2021), p. 54. The allusion to H. C. Andersen's fairy tale 'The Ugly Duckling' is mine, not Møller's.
99. Trautman (1972), p. 171.

Fig. 40. Reception on 6 July 1971 in Copenhagen Town Hall on the occasion of the GR6 conference. To the very left, Christian Møller and his wife Kirsten. The person in the wheelchair is Stephen Hawking. Credit: Nordita Collection, Niels Bohr Archive.

Among the many topics at the conference, gravitational radiation was perhaps the most popular single one.[100] Discussed from both theoretical and experimental angles, it was the subject of approximately twenty talks given by Sciama, Joseph Weber, Martin Rees, and others. There were also several talks on black and white holes, for instance by Novikov, Peebles, and Hawking. The latter's address titled 'A Black Hole Must Be Either Static or Axially Symmetric Brans-Dicke Black Holes' was particularly important. Shortly after his return to Cambridge, Hawking reworked it into a paper which came to be seen as a landmark in classical black

100. Camenzind (1971). No proceedings were published.

hole theory.[101] Singularity theorems were treated by Robert Geroch, among others, and questions relating to the initial singularity in cosmological models were considered by Charles Misner. Shapiro gave a review of recent experimental and observational tests of general relativity. Among the afternoon sessions were talks given by Abdus Salam and Dimitri Ivanenko, who both discussed the interface of general relativity and quantum mechanics, as did also Peter Bergmann and Penrose. Finally, there were several talks on relativistic astrophysics, such as one by E. Margaret Burbidge on 'Quasi-Stellar Objects and Cosmology'.

Various organisational changes were discussed in subcommittees, of which the most important was perhaps Bergmann's proposal to transform the relativity and gravitation committee into a more open scientific society analogous to the American Physical Society founded in 1899 or the European Physical Society formed only three years before the Copenhagen meeting. Bondi and Mercier prepared a draft statute of what became the International Society of General Relativity and Gravitation (ISGRG), an organisation which was only formally established about half a year before the 1974 conference in Haifa.

The choice of Haifa as the next venue was highly controversial and vehemently opposed by Fock and other scientists from the Soviet Bloc countries, but after many negotiations and diplomatic manoeuvres it was nonetheless confirmed that the GR7 conference should convene in Haifa with Nathan Rosen as chair of the organising committee.[102] The non-scientific part of the Copenhagen conference was deeply influenced by the East-West confrontation which at a time seemed to jeopardise the unity of the ICGRG and perhaps split it into two rival fractions. When this did not happen, it was primarily because of the diplomacy of Møller and a few of the other members such as Kip Thorne from the United States and his friend Vladimir Braginsky from the Soviet Union.

Two of the American participants in Copenhagen were Charles Weiner, a physicist and historian of physics, and the distinguished

101. Hawking (1972), received 15 October 1971.
102. Lalli (2017), pp. 115-117.

nuclear astrophysicist and future Nobel laureate William Fowler. It so happened that three years later Weiner interviewed Fowler as part of the oral history project of the American Institute of Physics. Fowler remarked that the meeting in Copenhagen was "extremely interesting because of problems with the Soviet Union." And then:

> *Weiner*: I saw what happened there.
>
> *Fowler*: Yes, you see everyone else was willing to put up candidates to be voted on for officers on the council or whatever it was, but the Soviet Union wouldn't do that. They had to wait till they got their slate cleared. ... It was apparent even on the floor of the meeting that there was [*sic*] all kinds of shenanigans going on. That was very interesting, but as you say there was a real thrust towards some kind of formal organization in this old field — old in a sense, but brand-new field in another sense. The Danish relativists played a leading role.
>
> *Weiner*: Christian Moller?
>
> *Fowler*: Christian Moller, I think without him the whole thing would have fallen through.[103]

Since the Tbilisi meeting, Fock had served as president of the ICGRG. However, The Soviet authorities were dissatisfied with Fock's political work during the Copenhagen conference and consequently removed him from the ICGRG. He was further punished by not being allowed anymore to leave the Soviet Union.[104] At the end of the Copenhagen conference, Møller was elected new president, a position he kept until the 1974 GR7 conference. During this period many of the political troubles continued, including the question of when and where to formally establish the new society. To cut the Gordian East-West knot he suggested to decide the question by means of a mail vote among the members of the ICGRG. The proposal was accepted – as usual against the wish of the Russians –

103. Interview of 30 May 1974. https://www.aip.org/history-programs/niels-bohr-library/oral-histories/4608-5. Fowler shared the 1983 Nobel Prize with Chandrasekhar for his work on nuclear astrophysics and the stellar synthesis of elements.
104. On this and his persistent rivalry with Ivanenko, see Martinez (2019).

and by January 1974 the ISGRG had become a reality with Møller as its first president and acting as deputy president until 1977.[105] Although initially very few Russian and East European scientists joined the society, Møller optimistically believed that the situation would soon improve. As he expressed it in an earlier letter to Mercier: "It might be difficult for our Eastern colleagues to commit themselves in the first stage but as soon as the Society exists, I am sure that they will find a way to join, if not as individuals, then through their Academies."[106]

Møller did not attend the GR8 meeting in Waterloo, Canada, in August 1977, the reason being that he had already signed up for another scientific conference in Loma-Koli, a remote vacation resort in Eastern Finland. Møller much appreciated the Symposium on the Foundation of Modern Physics in Loma-Koli: "The meeting in Loma-Koli in Finland was very pleasant with quite a number of my contemporaries present, Weisskopf, Casimir, Belinfante, Ter Haar, van Hove etc."[107] The next meeting again in the ISGRG series was to be held three years later in Jena in East Germany, a decision which was somewhat controversial among American and Israeli relativists in particular. However, Møller supported it with the argument that scientific internationalism should be given higher priority than political disagreements. As he wrote in a letter to Gerald Tauber, a Canadian professor of physics at the Tel-Aviv University and a collaborator of Nathan Rosen:

> We all know that our colleagues in Eastern Europe have some difficulties in participating in our enterprise and I think we should do what we can to make life easier for them. ... I am convinced that it would make the membership of our Society truly international. We have been assured that all the conditions we have required will be satisfied so

105. For a list of ISGRG officers from 1971-1974 until the present, see http://www.isgrg.org/pastcommitte.php

106. Møller to Mercier, 5 February 1973, quoted in Lalli (2017), p. 122.

107. Møller to Kuchař, 5 September 1977 (CMP). Møller to Belinfante, 19 January 1977 (CMP): "I am getting somewhat tired of these huge conferences, and I believe the small conference in Loma-Koli in a remote and beautiful part of Finland will be much more pleasant."

> that we will not have a repetition of the deplorable and unacceptable situation of 1968.[108]

With around 700 participants gathering in Jena between 14 and 19 July 1980, the GR9 meeting was a success. Møller prepared to participate and in a circular of 20 December 1979 he was listed as heading a discussion group on alternative classical theories of gravitation and Mach's principle. However, he did not turn up in Jena. About three weeks after the preliminary program was circulated, he died unexpectedly by pneumonia.

At about the same time that Møller was busy with the international organisation of relativists he got involved in the no less ambitious attempt to create a forum for European physicists. The idea behind such a forum or society came from the Italian physicist Gilberto Bernardini, a distinguished specialist in cosmic rays and nuclear physics, who on 16-17 April 1966 arranged a 'Meeting on European Collaboration in Physics' in Pisa. The representatives from Denmark were Møller, Aa. Bohr, J. Hamilton, L. Rosenfeld, and S. Rozental, and other participants included Blackett, Casimir, Jost, and C. Bloch. Møller chaired the session in which the Dutch physicist Sybren de Groot discussed the key question 'Should we have a European Physical Society?' Everyone agreed, but there was some disagreement about the structure and content of the society. "Professor C. Møller raised the question whether other European centres like those of high-energy physics are desirable. In this connexion he mentioned the subject of solid-state physics. One could also think of a centre for molecular biology, for instance."[109]

A steering committee established in Pisa with Møller as one of the six members called a meeting at CERN in Geneva on 25 November 1966 where possible structures for the projected society were discussed. Less than two years later, after many negotiations and much trouble, on 26 September 1968 Bernardini's initiative was

108. Møller to Tauber, 17 September 1975 (CMP).

109. Radicati and Zichichi (1966), p. 24. For the road toward the European Physical Society, see Lalli (2020). Bernardini to Møller, 26 May 1966 (CMP). Møller to Bernardini, 21 June 1966 (CMP).

Fig. 41. Møller, first row to the right, at the Pisa meeting in April 1966 leading up to the formation of EPS, the European Physical Society. Rosenfeld sits next to him. Credit: Centro Archivistico della Scuola Normale Superiore, Pisa, Italy.

crowned with the official foundation of ESF, the European Physical Society, and with Bernardini its first president.

At the time of his involvement in the ISGRC, Møller was a retired professor with no official duties to or benefits from the University of Copenhagen. His new position did not prevent him from being very active in both scientific and administrative matters, though, and nor did it prevent him from travelling to numerous scientific conferences abroad. In 1975 he was invited to the Marcel Grossmann Meeting on general relativity held at the International Centre for Theoretical Physics in Trieste. Named after Einstein's close friend and collaborator, the Swiss mathematician Marcel Grossmann, this was the first of a series of conferences continuing to this day.[110] Among the participants in the first Marcel Grossmann meeting were Chandrasekhar, Lichnerowicz, Penrose, and Salam.

110. Møller to Bleuler, 24 March 1977 (CMP). Grossmann co-authored with Einstein an important work of 1912 which counts as the first but incomplete version of general relativity based on tensor mathematics and the principle of general covariance.

Another participant was the young British astrophysicist Brandon Carter who two years earlier had introduced the controversial anthropic principle in cosmology. However, Møller did not turn up in Trieste. He declined the invitation because he was preparing for an assignment in the United States, where he had accepted an invitation as visiting professor at the University of Utah, Salt Lake City, in the period from September to December 1975.

During this last visit to the United States, he also went to the University of Texas, Austin, invited by the Austrian-born specialist in general relativity Alfred Schild, whom he knew well from Copenhagen. Christian and Kirsten Møller were on a long trip to "the wilderness of southern Utah", as they described it in a letter to Aage Bohr and Ben Mottelson, congratulating them with their Nobel Prize: "It is also important that you receive the prize while still in the prime of your age, and there is no doubt that both the Niels Bohr Institute and Nordita will benefit from your recognition of honour."[111] As mentioned, Møller was among the nominators of the prize to the two nuclear physicists.

8.5. Broader aspects of science

Unlike some of his contemporaries in fundamental physics, Møller was not particularly interested in what may be called the broader aspects of science, with which phrase I refer to history and philosophy of science as well as to the role of science in society. The relation between science and religion may also belong to these broader aspects. While many physicists take an interest in these subjects in their older days – and only very few when younger – Møller largely stuck to his research in theoretical physics. He was on the editorial board of the journal *Foundations of Physics* established in 1970 with Henry Margenau and Wolfgang Yourgrau as editors, but without ever contributing to the journal such as did several of his colleagues in quantum and relativity physics. For example, in the first two volumes there were articles on general relativity by P. Bergmann, A. Mercier, V. Fock, and J. Synge. The editors invited him several

111. K. and C. Møller to Aa. Bohr and B. Mottelson, 28 October 1975 (CMP).

times to send an article, but he always excused himself by being too busy. Other international journals with Møller on the editorial board included *Il Nuovo Cimento*, *Annales de l'Institut Poincaré*, and *Nuclear Physics*, the latter a very important journal founded by Rosenfeld in 1956.

On a few occasions Møller wrote on the history of the physical sciences, mostly for a Danish audience, and he also indicated – but not more than that – his view concerning the philosophical aspects of quantum physics. As far as politics and the science-society relations are concerned, he likewise was reticent and rarely spoke or wrote about them. In these respects, he was quite different from his close colleague, the erudite and wide-ranging Léon Rosenfeld, who was not only an accomplished and productive historian of science but also intensely interested in philosophical, ideological and political questions.

Apart from a series of memorial articles on deceased physicists (F. Kalckar, N. Bohr, A. Einstein, O. Klein, L. Rosenfeld, G. Källén), Møller only published three essays on proper history of science. One of them, co-authored by his old friend Mogens Pihl, was a contribution to a French book on famous inventors in which they briefly surveyed the discovery of electromagnetism made by H. C. Ørsted in 1820.[112] Shortly after the death of Bohr in 1962, the same two authors wrote a concise historical review of Bohr's contributions to atomic and nuclear physics in which they covered substantial parts of the development from about 1910 to the 1950s.[113] The third of the essays, written by Møller alone, was a popular but fairly comprehensive account of Galileo's physics written in Danish during the German occupation. With what was perhaps a hidden allusion to the situation in Hitler's Germany and Stalin's Russia, Møller found Galileo's struggle with the Catholic Church to be "a particularly instructive example of the still relevant tension between the requirements of belief in authority and freedom of conscience."[114]

112. Møller and Pihl (1950).

113. Møller and Pihl (1967), a translation of an essay in Danish published in 1964.

114. Møller (1943c), p. 19. Møller conceivably had in mind his earlier conversation with Brecht regarding his play on Galileo's life (Section 4.1).

According to Møller, Galileo had shown the way not only to the truth of nature but also to the nature of truth. He had developed *the* scientific method to perfection, a method to which Møller fully subscribed and presented as follows:

> Theoretical considerations must precede the experiments insofar that one proposes generalizing hypotheses on the basis of earlier experiences and the principle of maximum simplicity in nature. The consequences are subsequently derived by means of mathematical methods. As the last link in this chain of processes, comes the experiment itself which confirms or disconfirms the consequences. This is the working method which since the time of Galileo has determined the rapid development of the exact sciences. It is as different from the speculations of the peripatetics [Aristotelians] as it is from the primitive empiricism which regards science as no more than a trivial summary of empirical data.[115]

In later publications Møller repeated this classical, hypothetical-deductive view of scientific progress. As he saw it, the role of speculation was strictly limited to the initial hypotheses. As soon as a testable theory was formulated and proved to be internally consistent, there was no longer – or should not be any longer – room for speculations. Of course, what counts as a 'speculation' and what not is somewhat arbitrary and varies from one scientist to another. In this respect, Møller was very restrictive. As he saw it, physics was in its essence about formulating theories and deriving from them predictions that would either confirm or falsify the theory in question. In this way and only in this way could the physicist hope to gain knowledge of how nature really works. Although Møller readily admitted that the history of physics was littered with mistakes and blind alleys, he believed that these were relatively harmless since physics and the exact sciences generally constitute a self-correcting system of knowledge:

115. Møller (1943c), p. 12. The essay was written on the occasion of the tercentenary of Galileo's death.

> As a rule, such mistakes have not persisted for long as a large number of scientists have always been ready to test and criticise new discoveries and theories. ... Natural science and physics in particular will never experience permanent mistakes of the kind known from other areas of intellectual life, the reason being that physics deal with the unchangeable laws of nature which exist independently of human actions.[116]

Much later, in a semi-popular essay written toward the end of his life, Møller dealt with the revolutionary changes in recent fundamental physics as seen in the context of the history of physics since Galileo. His brief account of the general trends in the history of science suggests that his view had elements in common with Thomas Kuhn's account in his famous work *The Structure of Scientific Revolutions* from 1962.

Without using the term 'paradigm' Møller spoke of the physicists' "habitual theoretical concepts which are based on a large collection of empirical facts and which they easily tend to think of as more or less self-evident truths." A crisis will occur only when numerous experiments convincingly have proved that certain anomalies are real, and even then "many physicists will tend to explain the phenomena in terms of factors unknown so far, sometimes called 'hidden parameters', rather than admit the inadequacy of the fundamental concepts in question." Contrary to Kuhn, Møller did not see rival paradigms as incommensurable and he considered the transition from one paradigm to another as rationally justified and a genuine progress in science. He thought that there had only been very few proper revolutions in science, on average just one per century except in the twentieth century, where there had been two of them, namely relativity theory and quantum mechanics.[117]

In his essay of 1977, Møller criticised philosophers of the past who had claimed that physics must necessarily be founded on certain a

116. Møller and Rasmussen (1938), p. 167.

117. Møller (1977b), p. 3. There is no documentation that Møller actually read Kuhn, but I believe that he knew about his ideas and was to some extent influenced by them. Møller was interviewed by Kuhn in July 1963 and one might imagine that during their conversations Kuhn mentioned his new book.

priori conceptions. He singled out Immanuel Kant's ideas about space and time and also his and others' false belief in deterministic causality which he associated with another German philosopher, Arthur Schopenhauer. Moreover, Møller restated his conviction that the truths obtained in the exact sciences are in some sense eternal. At least, "they will continue to be accepted as truths as long as scientific work constitutes an essential part of human activity." In relation to the situation in the quantum-mechanical measurement process he emphasised that all ideas of introducing a subjective element in science must be rejected as fundamentally wrong:

> The *objectivity* of science is preserved; it is the crown jewel which must never be thrown away, for objectivity is an essential precondition that human reason can remain superior to all those forms of superstition and obscurantism which still in our time appeal to many minds and which under certain circumstances can attain quite ominous forms.[118]

Nonetheless, Møller realised that even the best empirical data may not be enough to differentiate between one theory and a rival theory. There are cases where non-objective criteria such as simplicity and convenience legitimately enter in theory choice. This he pointed out with regard to the question of the energy-momentum of light in refractive media and also in relation to the formulae of relativistic thermodynamics (see sections 6.2 and 7.2, respectively). At least implicitly he seems to have been aware of the so-called Duhem-Quine thesis discussed by philosophers of science.[119] According to this thesis, a scientific hypothesis cannot be empirically tested in isolation but only in conjunction with certain auxiliary assumptions

118. Møller (1977b), p. 15. In a memorial address on Rosenfeld to the Royal Danish Academy, Møller likewise wrote that he, Rosenfeld, was "a declared opponent of superstitions and all forms of obscurantism, which now again, in our time, seem attractive to people at least temporarily." Møller (1975a), p. 70.

119. See Faye (2007), who refers to Møller's discussion of relativistic thermodynamics as a "very instructive example of how the explainer's ethos plays a role in the audience's belief in her explanation" (p. 63).

that do not belong to the hypothesis and may not be objective in the usual understanding of the term.

Møller summarised his semi-popular and semi-philosophical essay in an interview with a journalist from the newspaper *Berlingske Tidende*. Physics, he said, is essentially limited to those things which can be measured and therefore be expressed by numbers. "This picture is admittedly much poorer and restricted than a poet's description of nature. On the other hand, one obtains a description which is completely independent of the feelings of the individual observer." On the interviewer's question of whether physics is of any relevance to people's attitude to life, Møller answered:

> Yes, and very much so. It has accustomed people to abandon wishful thinking, although one can still deal with the world of imagination, as I do with pleasure when I read imaginative novels. However, insofar that one is concerned with physics, one is not allowed to speak of unprovable things. Sure, one can still have opinions of what happened billions of years ago, but in that case one builds on the theory of relativity or on cosmological models. Wasn't it Kierkegaard who said that "subjectivity is truth"? Physics teaches us to accept things as they are, irrespective of one's wishes how they ought to be.[120]

Contrary to Bohr and some other quantum physicists (among them Jordan), but in broad agreement with Einstein, Møller did not believe that physics could or should be extrapolated to the normative domain of human values. He subscribed to what philosophers have called 'restrictionism', whereas he was opposed to 'expansionism'.

As a result of the student revolt in the late 1960s and the popularity of the New Left, science in the traditional sense came under attack in the Western part of the world.[121] In his influential book *One Dimensional Man* (1964), a classic of the student revolt, the German-American philosopher Herbert Marcuse argued that Western

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120. *Berlingske Tidende*, 1 July 1977. In his *Concluding Unscientific Postscript to Philosophical Fragments* (1846), Søren Kierkegaard claimed indeed that "truth is subjectivity" and "subjectivity is truth."

121. Kragh (1999), pp. 401-404.

science was directed inherently toward domination of nature, as well as people.

According to Marcuse and other gurus of the period, such as Jürgen Habermas in West Germany and André Gorz in France, the essence of science was exploitation. The repressive technological society was founded on the physical sciences, and these sciences were therefore responsible for the repression and dehumanisation that were characteristics of modern society. Marcuse – who knew little about science, and even less about theoretical physics – claimed that "the mathematical character of modern science determines the range and size of its creativity and leaves the non-quantifiable qualities of *humanitas* outside the domain of exact science." Moreover, "The mathematical propositions about nature are held to be *the* truth about nature, and the mathematical conception and project of science are held to be the only 'scientific' one."[122] The views of Marcuse and other sociologists and philosophers of the same opinion were taken seriously by a large part of the younger generation, who rejected the traditional scientific project and accepted the picture of physicists as soulless machines in the service of the military and industrial corporations.

Belonging to a different tradition than Marcuse, in 1975 the Austrian-American philosopher Paul Feyerabend published *Against Method* in which he advocated an 'anarchistic theory of knowledge' and accused the so-called scientific method of being nothing but an ideological construct. The message of his book was that modern science is not inherently superior to that of non-scientific views of nature. According to Feyerabend, modern science executes a mental dictatorship on line with that of the church in the Middle Ages, and he consequently called for a stop not only of obligatory science in schools but also of government support of all science activities.

The same year another Austrian-American, the particle theorist Fritjof Capra, published his immensely popular *The Tao of Physics* in which he argued for a deep connection between quantum mechanics and Eastern mysticism. He suggested that the insights of modern quantum physicists were basically the same as those reached

122. Marcuse (1968), p. 62.

much earlier by the masters of Zen Buddhism by means of spiritual meditation and intuition. "Physicists have come to see that all their theories of natural phenomena, including the 'laws' they describe, are creations of the human mind: properties of our conceptual map of reality, rather than of reality itself."[123] What matters is that in the 1970s the classical virtues of the exact sciences were questioned from many sides, which to some extent also made an impact on young physicists and students of physics. The venerable Niels Bohr Institute was no exception.

Møller was aware of and worried about the alarming new zeitgeist. Although he never referred to it in public or in his lectures, there is evidence in his interview by Weiner that he found it most disturbing. After all, it threatened everything he believed in and had built his career upon. In reply to Weiner's question if there was any kind of anti-science feeling in Denmark at the time when Møller and Rasmussen wrote their book, he said: "No. We didn't have anything of that, no. We have it a little now. I mean, now the youth is corrupted, you know. I was in Germany now and I was horrified by — well, this confusion, this anti-natural science attitude, this mixture of humanistic viewpoints with, in an unfruitful way, mixed with objective physics, and this subjective kind of science is a dangerous thing, I think."[124] A little later in the interview:

> *Møller*: I think it [the Møller-Rasmussen popular book] was, particularly among the teachers and also among the pupils in the schools, it was quite popular, much more than now the new edition, which has sold very poorly. That is a sign that the interest has gone to other things. Well, I'm a little pessimistic about that development. It doesn't matter so much if we hurry down — if we break a little bit, it doesn't matter. But we are going back to obscurantism and believe in all kinds of this parapsychology and all kinds of —
>
> *Weiner*: Astrology?

123. Capra (1976), p. 277. See Kaiser (2011) for a fascinating account of how a group of physicists in the 1970s successfully cultivated unconventional forms of quantum physics.
124. Weiner (1971c).

> *Møller*: Astrology and that is a dangerous thing and this emphasis of faith, in contrast to knowledge and so on. I mean, faith without intellect is an extremely dangerous thing, particularly in Germany.
>
> *Weiner*: This is East Germany you're reacting to?
>
> *Møller*: That was in East Germany. But there were a lot of West Germans there and they were the worst.
>
> *Weiner*: These are young scientists?
>
> *Møller*: There were a number of young scientists. There was one scientist there, or — well, I cannot mention his name — but he was a fellow with whom I had many many violent discussions during the '30s when he was on a visit here, and he is now regarded as a big hero. When he appeared on the podium, young people were making, applauded him, like in the Nazi times, especially the ladies were completely crazy. It reminded me of the first and only Nazi meeting I attended in Germany in 1931, in a small place, Rothenburg where I had the first impression of what this — it was before Hitler was in power, but it was really frightening. And I didn't like all this applause of this man, who, I feel, is corrupting.

It is unknown to whom Møller referred with these strong words. My best but uncertain guess is that it may have been C. F. von Weizsäcker, who by the mid-1960s had largely turned from physics to philosophy and become a celebrated public figure in German intellectual life. Although Weizsäcker was by no means a Nazi, during the 1930s he sympathised with some of the developments that took place in the Third Reich. His later philosophy of nature included not only matter and fields but also and essentially the spiritual world in a version inspired by Buddhist thought, something completely foreign to Møller's world view.[125] The hypothesis that Møller had Weizsäcker in mind receives some support from the letter of 1942 from Meitner to von Laue, which suggests that Weizsäcker had an interest in astrology (see Section 4.2). Møller seems to have disagreed with his 'corrupting' views not only for scientific reasons, but also for philosophical and political reasons.

125. For the many-sided aspects of Weizsäcker as a scientist, philosopher, and public figure, see the contributions in Hentschel and Hoffmann (2015).

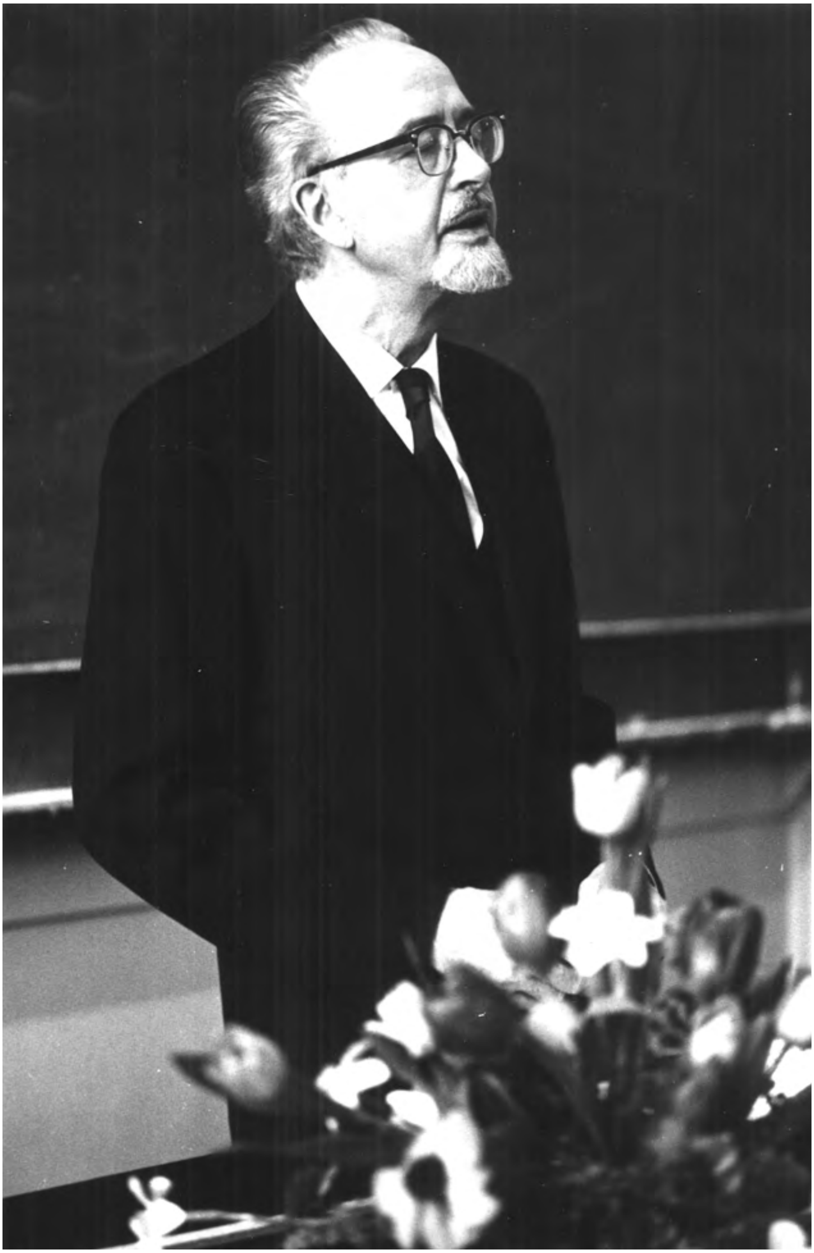

Fig. 42. Christian Møller in 1971, giving a talk on the occasion of the fiftieth anniversary of the Bohr Institute. Credit: Niels Bohr Archive, Photo Collection, Copenhagen.

Many critics of the existing system of science were not anti-science but demanded a major reorientation away from the elitist and fundamentalist science-for-the-sake-of-science approach cultivated by 'the scientific priesthood'. They wanted a socially relevant science accessible to the people and responding to the people's needs. Such an attitude was widespread also in Denmark.

In the summer of 1975, Ritt Bjerregaard, the Social Democratic minister of education (including research), argued in a newspaper article that science was not an end in itself, but should be seen as a means to fulfil political needs. She suggested that "the researchers must be quick to demonstrate that the research they are performing is relevant to society."[126] In general she chastised the ivory-tower scientists for "communicating in closed circuits" and failing to make their research understandable to ordinary people. Although Bjerregaard did not mention the Niels Bohr Institute, a few physicists at the institute entered the debate that followed, arguing for the traditional view that scientific knowledge is of value in itself irrespective of whether or not it can be transformed into technology of social relevance. This was also the opinion of Møller, who could not possibly justify his abstract mathematical research in general relativity in terms of economic progress or social welfare. Nor could he make it understandable to 'the people'. Wisely, Møller refrained from entering the debate.

A few years earlier, at about 1970, there were political discussions of moving the entire Niels Bohr Institute including Nordita from Copenhagen some 30 km to the West. What the Ministry of Education considered was primarily an extension of the research and education activities of the institute and not necessarily that the buildings on Blegdamsvej were emptied and used for other purposes. The planned destination would be either Roskilde or the nearby Risø area, where the Danish Atomic Energy Commission had established a large research centre. Moreover, what in 1972 became Denmark's fourth university, Roskilde University Centre, was in

126. Kragh and Nielsen (2001), pp. 333-334. Shortly after Bjerregaard's article, Aage Bohr and Ben Mottelson were awarded the Nobel Prize for work that epitomised the kind of research criticised by Bjerregaard.

its early construction phase. Since the Bohr institute already had a laboratory with an electrostatic accelerator at Risø, why not move the entire institution away from crowded Copenhagen?

As the director of Nordita, Møller was involved in the plans, hoping they would never materialise. On his initiative, a circular was sent to old collaborators at the institute to enquire about their views. Aage Bohr was cautiously sympathetic to the plan, and Peierls thought that it might bring with it advantages as well as disadvantages.[127] None of them referred to the cultural heritage of the institute on Blegdamsvej. In the end, nothing came out of the plan. Bohr's institute remained where it had been since 1920 and so did Nordita, undoubtedly to Møller's great relief. He would not have felt at home anywhere else than in the environment he first experienced in 1926 and to which he remained so closely associated.

127. *Berlingske Aftenavis*, 27 June 1970. Aage Bohr to the Science Faculty of Copenhagen University, 18 November 1969 (NBA, Nordita Archive). Peierls to Møller, 19 March 1970, in Peierls (2009), pp. 729-730.

9. Appendices

Appendix I: A letter on cosmology

Handwritten letter from George Gamow to Christian Møller, 19 December 1967 (see Section 7.3). The letter is reproduced as Gamow wrote it, including his habitual misspellings. Københown = Copenhagen; Glaedelige Jul = happy Christmas.

Dear Møller,

You will see soon in Proc. Nat. Acad. Sci. U.S. (Dec. 1967 issue) my paper with Alpher and Herman in which we give the curves (analitical expressions) for the physical characteristics for the history of the universe. The results seem to be very plausible and in agreement with the observed data. I am now trying to bring in order the geometrical part of Riemann's space time continuum which is consistant with this cosmology. Since, as it is been shown in the paper, the space possesses negative curvature with $\boldsymbol{R} = 9.3 \times 10^9\sqrt{-1}$ light years, the line element must be written as:

$$ds^2 = -\frac{L(t)^2}{[1-(r/2R)^2]^2}\left[dr^2 + \sin^2\theta r^2 d\varphi^2 + r^2 d\theta^2\right], R = |\boldsymbol{R}|$$

where $L(t)$ is the time dependent scale of the universe. Since all the properties of space time are determined by the line element, I would like to have them for the comparison with the observable data, particularly. For example, for 102+ known quasars the values of $z = \Delta\lambda/\lambda$ (so that $z + 1 = \lambda'/\lambda$ =redshift) show a 'Heufungspunkt' at z = 1.95 and refuse to go bejond it. This, suggests that the space of the universe has a 'horizon' when λ'/λ approaches 3. The above given line element also diverges at $r \to 2R$ and the two things may be closely connected. Indeed the volume

$$\Omega(r) = \iiint\limits_{r\,\theta\,\varphi} \frac{r^2 dr}{[1-(r/2R)^2]^2} = d\theta^2 \sin^2\theta d\varphi^2$$

becomes ∞ at $r \to 2R$. But, somehow, I cannot get a conveniant analitical expression between the red-shift $\lambda'/\lambda = z + 1$ and the distance coordinate r in the line element. Astronomers use the relation $z \sim r$, but it seems to be wrong when $r \to 2R$. Since you have been working so much on the mathematical part of the relativity, I wander whether you can see the situation clearer than I do at the moment. If so, I would like very much to hear from you about that.

This summer my wife and I are planning to spend in Europe (mostly in Cambridge). But I hope also to visit the old alma mater Københown for a while. My best wishes for Glaedelige Jul.

Yours truly George Gamow.

Appendix II: Visitors from abroad at the institute for theoretical physics

This list is a small selection of visitors in the period 1928-1965, mainly including those with whom Møller had some kind of contact. Some of the physicists were at the CERN theory group and others at Nordita. It is based on a much longer list kept at the Niels Bohr Archive.

Alders, K.	1962-65
Beck, G.	1932; 1937-38
Bhabha, H.	1936-37
Bloch, C.	1948-50
Bloch, F.	1931-32
Bohm, D. J.	1958-59
Brown, G. E.	1957-58; 1960-64
Casimir, H. B. G.	1929-30
Chang, T. S.	1938-39
Delbrück, M.	1931; 1936
Deser, S.	1955; 1957; 1963
Franck, J.	1933-35
Frisch, O. R.	1934-39
Gamow, G.	1928-31
Glashow, S. L.	1958-60
Gustafson, T.	1960-61
Haar, D. Ter	1946-47
Hamilton, J.	1947; 1964+
Hartree, D. R.	1928
Havas, P.	1954
Heisenberg, W.	1926-28
Heitler, W.	1929; 1933; 1936; 1955
Hove, L. Van	1947-48
Hückel, E.	1929
Hulthén, L.	1938-39
Hylleraas, E.	1931; 1957

Jost, R.	1946, 1952
Källén, G.	1952-53; 1953-58
Klein, O.	1924-31
Kohn, W.	1951-52
Komar, A.	1956-57
Landau, L.	1930
Laurent, B.	1963
Lauritsen, C.	1938
Levi, H.	1934-35
Lüders, G.	1952-53
Meitner, L.	1939
Mercier, A.	1936-37; 1938
Michel, L.	1950-51; 1952-53
Misner, C. W.	1959; 1961
Morette, C.	1947-48
Mott, N. F.	1928
Mottelson, B.	1950-57+
Nishina, Y.	1925-28
Nordheim, L. W.	1928
Pais, A.	1946; 1958
Peierls, R.	1937-38
Pellegrini, C.	1960; 1962
Placzek, G.	1932-34; 1936-38
Plesset, M. S.	1933-34
Proca, A.	1934-35
Rosenfeld, L.	1930; 1940; 1955; 1958+
Rossi, B.	1938
Rozental, S.	1938-1948
Sakata, S.	1954
Schild, A.	1965
Serpes, J.	1957
Stueckelberg, E.	1947
Teller, E.	1929-30; 1934
Valatin, J.	1950-52
Weinberg, S.	1954-55
Weisskopf, V. F.	1932-33; 1935-37

Weizsäcker, C. F.	1933-34
Wergeland, H.	1941
Wick, G.	1938
Wightman, A. S.	1951-52; 1956-57
Williams, E. J.	1933-35

Appendix III: Time-line

1904 Born in Hundslev, Als, 22 December.

1920 Re-unification, CM becomes a citizen of Denmark. First awareness of relativity theory.

1923 Graduation as student from Sønderborg Gymnasium. Matriculates to the University of Copenhagen, studies in mathematics and physics.

1926 Residence at Borch's Kollegium. Spring term, follows lectures in physics and mathematics at the University of Hamburg, where he Meets Pauli. Fall term, starts graduate studies at the Institute of Theoretical Physics. Meets Niels Bohr, Heisenberg, Klein, and others. Listens to public lecture by Schrödinger.

1927 Gives student colloquium on Dirac's theories.

1928 Summer school course in Berlin, meets Schrödinger. Visit to Göttingen, meets Born and Rosenfeld. Copenhagen, meets Mott and Gamow.

1929 May, with Bohr on trip to Cambridge. Attends first Copenhagen conference. Gold medal for essay on mechanical-optical analogy. Graduates as magister of physics. First research publication, on the theory of radioactive decay.

1930 Attends Copenhagen Easter conference, meets Landau and others. Papers on radioactivity and scattering theory.

1931 Marries Kirsten Pedersen on 20 June. Scientific assistant at the Copenhagen institute. Succeeds Klein as lecturer. Paper on relativistic electron-electron scattering.

1932 Attends Copenhagen conference in April. Major paper on scattering and absorption theory. Defends doctoral dissertation on same subject, 28 November.

1933 Appointed temporary associate professor (lektor) at Copenhagen University. Participation in September conference at Bohr's institute. Paper on ferromagnetism.

1934 Paper with Plesset on perturbation theory for many-electron systems. From October 1934 to April 1935 in Rome as a Rockefeller Fellow with Fermi's group. Colloquium in Rome on Pauli-Weisskopf theory.

1935 Reports on Fermi's neutron experiments at Copenhagen seminar, ca. February. Proceeds to Cambridge in May, returns to Copenhagen in September. Anti-Eddington paper with Chandrasekhar on collapsing stars. Collaboration with F. Bloch on theory of beta decay.

1936 Participates in June Copenhagen conference on nuclear physics. In August, attends with Bohr and others the Scandinavian Meeting of Natural Scientists in Helsinki. Proceeds to Kharkov over Leningrad and Moscow. One-month stay as guest researcher in Kharkov. Meets Landau, Tamm, Houtermans, and others.

1937 Two papers on electron capture radioactivity, at the time a hypothetical process but confirmed in 1938. Copenhagen conference in September with Meitner, Heisenberg, and others. Dispute in *Fysisk Tidsskrift* with a Danish engineer concerning relativity theory and energy-mass equivalence.

1938 Møller-Rasmussen popular Danish book on atoms and nuclei. Participates 30 May to 3 June in conference in Warsaw and Cracow on 'New Theories in Physics' with Eddington, Bohr, Klein, Rosenfeld, and others. August, first paper on meson theory of nuclear forces. Meeting with Brecht about his idea of turning Galileo's life into a theatre play. From 8-22 December, CM working in Liège with Rosenfeld on meson theory.

1939 January, witness to confirmation of uranium fission in Copenhagen. Suggests possibility of chain reaction in Danish radio broadcast on 27 February. First appearance of 'meson' in physics literature. Invited as scientific secretary to the cancelled eighth Solvay congress. Continues collaboration with Rosenfeld on meson theory. Several notes on this subject, in part with Rosenfeld and Rozental.

1940 Appointed reader (docent) at Copenhagen University, 1 April. English translation of Møller-Rasmussen popular book. Major paper on meson theory in proceedings of the Royal Danish Academy. Møller-Rosenfeld meson theory of nuclear forces. Denmark occupied by German forces 9 April.

1941 In a note in *Physical Review*, CM coins the word 'nucleon' for nuclear particles.

1942 Lectures in Lund and Stockholm, late March. Meeting with Meitner.

1943 Elected a member of the Royal Danish Academy, meeting of 2 May. Appointed extraordinary professor in mathematical physics, 1 April. Paper on the clock paradox, his first work in general relativity. Bohr escapes to Sweden, 29 September. On 6 December, Bohr's institute occupied by German soldiers.

1944 Meeting with Heisenberg in January in Copenhagen, institute occupation ceases on 3 February. Heisenberg visits Copenhagen in April, discusses *S*-matrix theory with CM.

1945 First paper on *S*-matrix theory. German occupation of Denmark ends, 4 May. Bohr returns to Copenhagen, 25 August. CM visits Klein in Stockholm, September. Contribution to *Journal of Jocular Physics* on Bohr's 50-year birthday.

1946 Lecture series on *S*-matrix theory at the University of Bristol. Declines offer of professorship at Manchester University. Cambridge conference on fundamental particles, 22-27 July. Collaboration with

Pais on mass spectra of elementary particles. Introduces the word 'lepton' for light elementary particles.

1947 Dean of Science Faculty, University of Copenhagen, 1947-1948. In July, lectures at the Institute for Advanced Study, Dublin. Informal Copenhagen conference, September. Reads address by Bohr at Rutherford commemoration symposium in Paris, 7 November.

1948 Conference on nuclear physics in Birmingham, 14-18 September, and on cosmic rays in Bristol, 20-24 September. CM deals with different kinds of mesons. Participation in eighth Solvay congress, Brussels, 27 September to 2 October. Visiting professor at Purdue University, Illinois, arrives in October. Lecture series on quantum electrodynamics. Offered residence of honour (Lundehave) by the Royal Danish Academy, but declines.

1949 While at Purdue University, CM visits various American universities and institutions, including Chicago, Stanford, Berkeley, and Institute for Advanced Study, Princeton. Meets Yukawa. Returns to Denmark in April. September, conferences in Basel and Como, Italy. November, conference on elementary particle physics in Edinburgh.

1950 Conference on nuclei and fundamental particles at Institut Henri Poincaré, Paris, 24-19 July. Invited speaker at Tata Institute conference on elementary particles in Mumbai, India, 14-22 December. Paper on non-local meson field theories.

1951 Copenhagen conference on problems of quantum physics, 6-10 July. Member of the Solvay scientific committee, participates in ninth congress on solid-state physics, Brussels 25-29 September.

1952 Publication of textbook, *The Theory of Relativity*, Oxford University Press. Møller-Kristensen convergent theory of meson fields. Conference on meson theory, Copenhagen institute, 3-17 June. Lecture on meson theory, University of Liverpool, July. Awarded the Knight of Dannebrog order.

1953 Lectures at Les Houches summer school in France on meson theory, July. Congress on science and freedom, Hamburg, 23-26 July. Conference on theoretical physics in Tokyo and Kyoto, 14-24 September. Meets Yukawa, Sakata, Tomonaga, Feynman, and others. CM on non-local field theory.

1954 Møller-Belinfante theory on collision processes, his last paper on quantum physics. American experiments verify Møller scattering formula for high energies. Director of CERN theory group, Copenhagen, 1 September. Birthday of CERN, 29 September. Solvay congress, electrons in metals.

1955 Berne conference on general relativity, 11-16 July. CM speaks on time measurements in relativity theory, meets Pauli, Hoyle, Bondi, Fock, and others.

1956 Meets DeWitt in Copenhagen. Avogadro conference on constants of physics, Turin, 6-11 September. CM on experimental tests of general relativity by means of maser technology.

1957 CERN theory group moves to Geneva, 1 October, Nordita starts on same day. CM director of and professor at Nordita. Lectures in Pisa, Italy, 3-24 March. Attends lectures given by Fock in Copenhagen in early March. Meeting on quantum gravity in Copenhagen, 15 June to 15 July. From early September to end of year, visiting professor at Carnegie Institute of Technology, Pittsburgh, lectures on general relativity. Visits several other places.

1958 In January, lecture at Institute of Advances Study, Princeton. Research at Chapel Hill, University of North Carolina, 25 January to 25 February. Lectures at Cornell University, 3-4 March. Returns from New York on 6 March. Planck centennial conference, Berlin, April. Participation in Solvay congress on relativity and cosmology, 9-13 June. Member of International Commission for Gravitation and Cosmology. Colloquium at University of Wisconsin on terrestrial tests of general relativity, 15 November.

1959 Ninth International Conference on High Energy Physics, Kiev, July. Secretary of the Royal Danish Academy of Sciences, October. Member of the Royal Physiographic Society, Lund, Sweden. Foreign member of The Royal Norwegian Society of Sciences and Letters, Trondheim. Member of Science Policy Committee, CERN. Memoir on energy-momentum complex in general relativity. GR2 conference in Royamont, France.

1960 Lecture course at Brandeis University, Massachusetts. Knight of Dannebrog order, first degree.

1961 Enrico Fermi summer school, Varenna, Italy. Editor of proceedings volume published in 1962. Solvay congress on quantum field theory.

1962 Tetrad field formulation of general relativity. GR3 conference, Warsaw-Jablonna, 25-31 July. Niels Bohr dies, 18 November.

1963 Bohr commemoration meeting, Copenhagen. Interview by T. S. Kuhn. Member of Norwegian Academy of Science, Oslo. Member of Carlsberg Memorial Foundation. Member of Danish 'Accelerator Committee'.

1964 Popular book with M. Pihl. Galileo celebration meeting, Florence. Conference on cosmology, Padua, 14-16 September. Solvay congress on galaxies.

1965 GR4 conference in London. Conference on elementary particles, Kyoto, 24-30 September. Einstein memorial conference on general relativity in East Berlin, 2-5 November. Heisenberg speaks on unified theory at the Royal Danish Academy.

1966 Ole Rømer medal. Meetings in Pisa 16-17 April and Geneva 25 November on European collaboration in physics. Lorentz professorship, Leiden, fall semester. Meets Dirac in Copenhagen.

1967 President of scientific committee, Solvay congress on elementary particles. First work on relativistic thermodynamics. Nordita meeting on statistical mechanics in Trondheim, 16-20 June.

1968 Gauss professorship, Göttingen, April-June. Work on statistical mechanics in relativity theory. Foreign member of the Royal Swedish Academy of Science. Honorary doctor at Åbo Academy University, Finland, 25 May. GR5 conference in Tbilisi, USSR, 9-13 September. Meets Zeldovich, Sakharov, and others.

1969 Inaugural conference of European Physical Society, Florence, April. Lectures in Rome and Bologna, May-June. Källén memorial conference in Lund, Sweden. Colloquium in Paris on general relativity.

1970 Ørsted medal. Revised edition of Møller-Rasmussen book, with J. Kalckar.

1971 Meeting in Bonn on general relativity and field theory, 21-23 June. GR6 conference in Copenhagen, 5-12 July. Interviews by C. Weiner. CM elected president of International Committee on General Relativity and Gravitation. Meeting on general relativity in Royal Society, London, 2 November. Strömgren replaces CM as director of Nordita.

1972 Second edition of relativity textbook. Member of Leopoldina Academy, East Germany. Trieste meeting in honour of Dirac's seventieth birthday.

1973 Solvay congress on astrophysics and gravitation. Nordita lecture in Reykjavik, University of Iceland.

1974 Retires as university professor. GR7 conference in Haifa, Israel. Co-founder of International Society for General Relativity and Gravitation. Rosenfeld dies, 23 March. With Strömgren, CM nominates Klein for Nobel Prize.

1975 Work on gravitational collapse and black holes. Lectures at International School of Cosmology in Erice, Sicily, 13-25 March. Visiting professor at University of Utah, Salt Lake City. Visit to University of Texas, Austin. Co-nominates Aa. Bohr and B. Mottelson for Nobel Prize.

1976 Lecture to the Society for the Dissemination of Natural Science on problems in general relativity theory, 31 March.

1977 Conference in Loma-Koli, Finland. Attends inauguration of CERN's Super Proton Synchrotron in Geneva, May. Klein dies, 5 February.

1978 Solvay congress on non-equilibrium statistical mechanics, Brussels, 20-24 November. Essay on singularity problem in general relativity.

1979 Einstein celebration conference in East Berlin, 28 February to 2 March. Einstein conferences in Jerusalem, March, and in Rome, 24-29 September. Spends summer in San Cataldo, Sicily. Papers and lectures on limitation of Einstein's general theory of relativity. Singularity-free cosmological model based on tetrad gravitational theory.

1980 Death caused by pneumonia on 14 January, buried at Vedbæk cemetary north of Copenhagen.

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