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Carsten Jensen †

Controversy and Consensus: Nuclear Beta Decay 1911–1934

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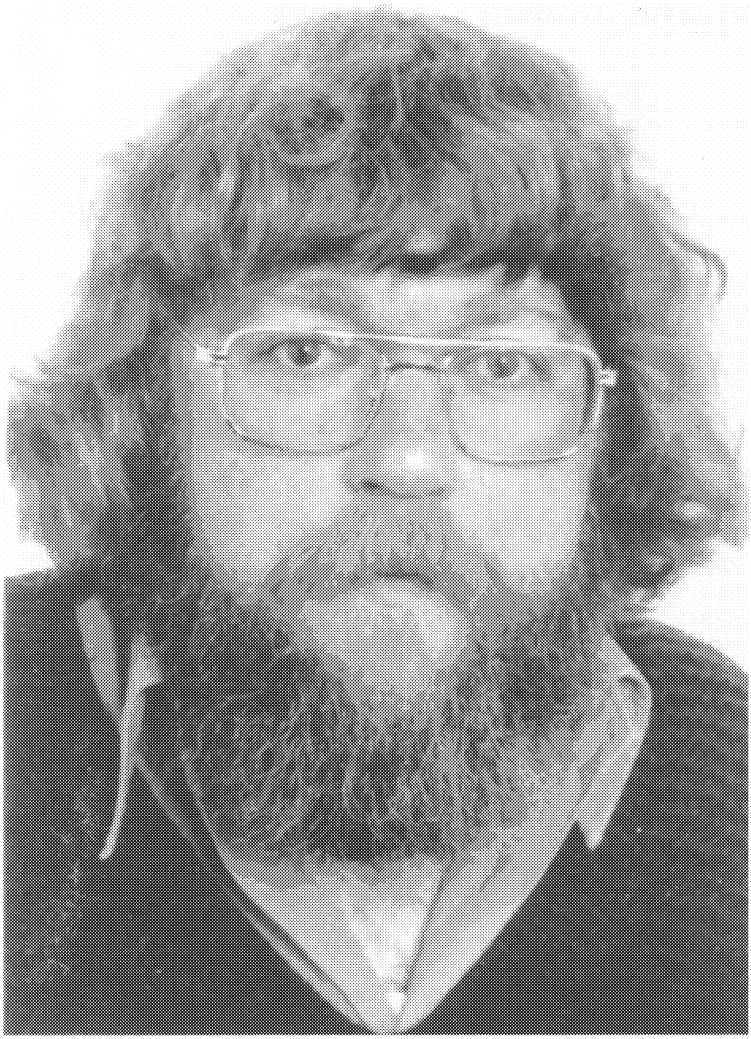
For Thomas: may he conserve his energy
– but only statistically.



Carsten Jensen.

In Carsten Jensen's Memory

Since Carsten Jensen was my one and only Ph.D. student (I have since changed to other work), my coeditors have kindly accorded me the privilege of writing a few words in his memory.

I first met Carsten when he attended my course on the history of quantum mechanics for 3rd year students at the University of Copenhagen. He passed with an excellent mark and shortly afterwards he turned to me because he wanted to write a historical paper in his course on atomic physics. Quite frankly, the thought crossed my mind that this was perhaps a student looking for a "soft option". I was soon to learn otherwise.

Carsten's paper discussed the hydrogen molecule ion as dealt with in the old quantum theory as well as in quantum mechanics, including the historically interesting case of an electron in an electric dipole field.

After completing this paper, Carsten had clearly decided on the history of physics as his field of specialization. He decided to write his master's thesis on "Some Experimental Anomalies in the Old Quantum Mechanics". Also this work earned him a very high mark; it was later published, in slightly revised form, as "Two One-Electron Anomalies in the Old Quantum Theory" (*Historical Studies in the Physical Sciences 15* (1984), 73–90).

I had by now come to appreciate the very high quality of Carsten's work so when he approached me again and asked me to be his supervisor for his Ph.D. dissertation, I accepted with pleasure. He first chose a broad theme within a rather brief period (the transition from atomic to nuclear physics, 1926 to 1932), but soon settled on a more limited topic over a longer period – the subject of the present book.

In the spring of 1990, Carsten was awarded the Ph.D. for his very successful dissertation. A few months later he died of cancer at the age of 41.

The editors have felt that this brilliant study ought to be known to scientists, historians and others interested in the topic, and have therefore prepared the present, slightly edited, version of Carsten Jensen's work for publication. The resulting book testifies to the loss that the history of science suffered by Carsten's early death. We, his friends and colleagues, will remember him above all for his kind and gentle personality, his disarming smile and his great sense of humour. Fortunately, a little of the latter is preserved in his style of writing, and hence in the present work.

Erik Rüdinger

Editors' Acknowledgements

Several individuals and institutions have taken part in the long process of editing Carsten Jensen's dissertation and otherwise making it ready for publication.

We are grateful to professor emeritus Hilde Levi for her improvement of the editors' translations of quotations from German into English, and to the late professor Allan Mackintosh for reading the manuscript for content and suggesting improvements in language. Anne Lis Rasmussen, secretary/librarian at the Niels Bohr Archive, spent months editing the manuscript on computer, introducing often complex LaTeX commands. She also entered the index terms and obtained sometimes obscure publications from which she scanned the numerous figures in the book. This work was made possible through financial support from the Foundation of the Niels Bohr Institute (*Niels Bohr Institutets Fond*) and the Niels Bohr Fund (*Niels Bohr Legatet*). We thank the Niels Bohr Institute for use of computer facilities. Peter Harper, director of the National Cataloguing Center for the Archives of Contemporary Scientists in Bath, England, helped obtaining the first names of some of the less well-known scientists mentioned in the book. Felicity Pors, academic assistant at the Niels Bohr Archive, checked the manuscript in the very last stages.

We have sought to make a conscientious effort to secure permissions to quote from unpublished material and to reproduce figures from publications. For help in improving the references to published material, as well as locating permission holders, we wish to thank, in particular: Carolyn Lye at the Churchill College Archives Centre; Adam Perkins at the Cambridge University Library; Mary Sampson at the Royal Society Archives; Margaret Kimball at the Stanford University Archives; and Marion Kazemi at the Archiv zur Geschichte der Max-Planck-Gesellschaft.

For permission to quote from unpublished letters, we wish to thank the following: N.M. Blackett (letters of P.M.S. Blackett); George Bloch (letters of Felix Bloch); Mrs. J. Batterham and Miss J. Chadwick (letters of James Chadwick); Charles E. Miller (letters of P.A.M. Dirac); Stefan S. Fajans (letters of Kasimir Fajans); Mary Nisbet (letters of Ernest Rutherford and Ralph Fowler); R. Igor and Elfriede Gamow (letters of George Gamow); Klaus Geiger (letters of Hans Geiger); Dietrich Hahn (letters of Otto Hahn); Elisabeth Heisenberg (letters of Werner Heisenberg); Martien Kramers (letters of H.A. Kramers); Ulla E. Frisch (letters of Lise Meitner); Roswitha Rahmy, for the Pauli Committee (letters of Wolfgang Pauli); Øystein Elgarøy (letters of Svein Rosseland); Ruth Braunizer (letters of Erwin Schrödinger); Helmuth Smekal (letters of Adolf Smekal); Walter Thirring (letters of Hans Thirring); and the Niels Bohr Archive (letters of Niels

Bohr). We have been unable to find the copyright holders of the letters of Ernest Marsden as well as of the letters of Charles Drummond Ellis, one of the main characters in the book.

All figures originally appearing in Carsten Jensen's dissertation have been retained. For permission to reprint them, we thank the following: S. Hirzel Verlag (reproductions from *Physikalische Zeitschrift*); Addison Wesley Longman Ltd. (reproductions from W.E. Burcham, *Nuclear Physics: An Introduction* (Longmans Green and Co. Ltd., London, 1965)); American Association of Physics Teachers (reproductions from *American Journal of Physics*); EDP Sciences (reproduction from *Annales de Chimie et de Physique*); Cambridge University Press (reproductions—and quotations—from *Proceedings of the Cambridge Philosophical Society*; E. Rutherford, *Radio-Activity* (Cambridge: Cambridge University Press, 1904); and *idem.*, *Radioactive Substances and Their Radiations* (Cambridge: Cambridge University Press, 1913)); Taylor and Francis (reproductions—and quotations—from *Philosophical Magazine*); American Physical Society (reproductions—and quotations—from *Physical Review*); The Royal Society (reproductions from *Proceedings of the Royal Society*); Springer Verlag (reproductions—and quotations—from *Zeitschrift für Physik*); and Deutsche Physikalische Gesellschaft (reproductions—and quotation—from *Verhandlungen der Deutschen Physikalischen Gesellschaft*).

The frontispiece photo of Carsten Jensen, as well as the photographs in Figures 1.7, 1.12, 2.1, 3.1, 3.4, 3.8, 6.1, and 6.8, have been added to the original manuscript by the editors. Most of these photographs stem from the photograph collection of the Niels Bohr Archive. We also thank Ingelise Christensen and the Cambridge University Library for providing, respectively, the frontispiece photo and the portrait of C.D. Ellis (Figure 3.1). We furthermore thank the Deutsches Museum for supplying the photograph of Otto Hahn and Lise Meitner (Figure 3.4) as well as the photograph of the beta-ray spectroscope and the beta spectrometer (Figure 1.12). The Rome conference photo (Figure 6.1) is from the Emilio Segrè Collection in the Niels Bohr Library of the American Institute of Physics, whereas the Solvay conference photo (Figure 6.8) is owned by the Institut international de physique Solvay.

Finally, we would like to thank Doris Wörner and Stephan Amman at Birkhäuser Verlag for continued support and trust in our work despite long silences and breaches of deadlines, as well as Kristin Roth and Edgar Klementz for competently taking over the cooperation during the last few months.

Finn Aaserud
Helge Kragh
Erik Rüdinger
Roger H. Stuewer
February 1999

Editors' Preface

To all four of us, Carsten was the best possible friend and colleague. To Finn, he was a fellow student in the history of science for several years at the Niels Bohr Institute; to Helge, he was a welcome resource for personal and intellectual interaction in an otherwise less than fertile environment for the history of science; Roger was Carsten's friend and advisor, not least in the development of the dissertation on which the present book is based; and as director of the Niels Bohr Archive, Erik was his main advisor in his historical work. Because he was the person closest to Carsten's work on his Ph.D. dissertation on the history of beta decay, on which the present book is based, it is only fitting that Erik stands as single author of the words in Carsten's memory at the very beginning of this book.

Before his untimely death shortly after the completion of the Ph.D. dissertation, Carsten had himself plans to develop the dissertation into a book. Being a true perfectionist, he wanted to rework the manuscript substantively, especially with regard to relating it to the broader discussion among historians of science. Nevertheless, we are convinced that the dissertation itself, especially its detailed narrative based on archival sources, is a unique and solid contribution to the history of science which has not been superseded and which, apart from being of interest in its own right, constitutes a particularly useful empirical basis for the continuing discussion on the character of scientific practice in this century.

For this reason, and because we thought it most proper that Carsten speak for himself, we have made few changes of substance. The diagram of the decay chains, which was presented as an Appendix in Carsten's dissertation, now appears immediately before the Contents, as it may be crucial for the understanding of several of the technical details in the text. We have provided translations into English of quotations that Carsten did not translate himself, as well as made some improvements in his use of the English language. In addition, the following changes should be noted in particular: new references have been added in a few cases, notably where a work referred to by Carsten has since been published in a new version; some of the many quotations have been replaced by paraphrases; the sources of the figures are now provided in the figure captions; as noted in the preceding Editors' Acknowledgements, a few photographs have been added; and a name index has been supplied. Since the changes hardly affect the substance of the work, we have deemed it redundant to note in the text where the published book differs from the dissertation.

Whereas it took Carsten four years to write the book in the midst of full-time high-school teaching, we four editors have taken twice as long to make the present revised version ready for the publisher. This circumstance is a measure of Carsten's tenacity of work as well as the continuing value of his writing.

Carsten dedicated his dissertation to his and Ingelise's son, Thomas. We have found it appropriate to retain this dedication. Since Carsten defended his dissertation, Thomas has grown from 14 to 22 years of age. Following in his father's footsteps by studying physics and mathematics at the University of Copenhagen, he seems indeed to continue to conserve his energy, but, as some of us have been lucky to observe at first hand, only statistically!

Finn Aaserud
Helge Kragh
Erik Rüdinger
Roger H. Stuewer
February 1999

Author's Preface

In 1985, after I had served as a teacher in a Danish Gymnasium for some years, a stipend from the University of Copenhagen enabled me to proceed with the studies in history of modern physics that I entered upon when I was a student. Originally my intention was to focus on nuclear research at the Niels Bohr Institute in the 1930s, but gradually the tremendous efforts to understand the complex beta spectrum absorbed me completely. I therefore felt like starting on a closer study of the history of the beta spectrum, and consulted Roger H. Stuewer, Laurie M. Brown, and Lawrence Badash to hear their opinions about such a project. I am grateful to them for having encouraged me to go on with it; the work has been as exciting as I expected, and the present book reveals what has come out of it.

After an introductory chapter on beta-spectrum research in the pre-nuclear years, the essential part of the account starts in the year of Ernest Rutherford's discovery of the nucleus. Owing to improved experimental techniques, the years from *circa* 1911 until 1914 revealed a growing complexity of the beta spectrum, culminating with James Chadwick's discovery of the continuous part. Furthermore, in this period the existence of two kinds of beta rays were recognized – the primary particles from the nucleus and the secondary particles from the electronic system.

In the 1920s, the interpretation of the composite spectrum was in focus, and a long-lasting controversy on this question arose between Lise Meitner and Charles Drummond Ellis. This controversy, and the reactions from the contending parties when it was settled, reflect clearly the difference between the scientific communities in Berlin and Cambridge at that time.

The Meitner–Ellis controversy ended in 1929, and it left an anomaly that attracted leading theoretical physicists. A new dispute, this time between Niels Bohr and Wolfgang Pauli, broke out. It concerned the explanation of the continuity of the primary beta particles and dominated the discussions for the next five years. Pauli argued for a new particle, and Bohr for a new theory; both suggestions were radical steps, but they reflected two different ways of doing physics.

Concurrently with the discussions of the continuous spectrum, great efforts were made, especially by the Cambridge theorists, to create a theory of internal conversion, and *circa* 1934 both the Fermi theory of beta decay and the Taylor–Mott theory of internal conversion were in place. This year thus constitutes the end of an exciting period in the history of the beta spectrum, and I have found it appropriate to finish my book there.

Many people have helped me during my project. First, I especially wish to thank my advisor Erik Rüdinger. Our many discussions, his encouragement, his close reading of the drafts and useful suggestions, and his help in guiding me to the right sources have been of inestimable importance to my work.

I am also deeply indebted to Roger H. Stuewer. His interest in my project from the very beginning has been a great pleasure to me; and his careful reading of the draft, his many comments and useful suggestions as regards content as well as language have contributed essentially to the final version of my book.

Three more people – Ole Knudsen, Helge Kragh and Abraham Pais – have read and commented on different parts of my writings at different stages, and I am grateful to them all.

I furthermore wish to thank the following people and institutions for use of manuscript material, for permission to quote from it, and for other information and assistance: Erik Rüdinger, Niels Bohr Archive, Copenhagen; Marion Kazemi, Max–Planck–Gesellschaft zur Förderung der Wissenschaften, Bibliothek und Archiv, Berlin; Marion Stewart, Churchill College Archives Centre, Cambridge; Spencer R. Weart, American Institute of Physics, Center for History of Physics [now in College Park, Maryland – eds.]; Cambridge University Library; Royal Society Library, London; Royal Institution Library, London; Library of Congress, Washington; Berta Karlik, Institut für Radiumforschung und Kernphysik, Vienna; Nikolaus Riehl, Munich; Laurie M. Brown, Northwestern University, Evanston, Illinois; Alan Q. Morton, Science Museum, London; Allan D. Franklin, University of Colorado, Boulder; Finn Aaserud, Niels Bohr Archive, Copenhagen.

As already mentioned, this work was made possible by a stipend from the University of Copenhagen; grants from the Danish Research Council for Science and the Niels Bohr Archive enabled me to visit archives in Berlin, Cambridge and London. Thanks to Paul Forman I furthermore received a grant from the Smithsonian Institution, Washington, which made it possible for me to do research at the National Museum of American History and the Library of Congress, Washington, and at the American Institute of Physics, New York. Finally, Mordechai Feingold invited me to give a talk about the Meitner–Ellis controversy at the XVth Annual Joint Atlantic Seminar in the History of Physics in April 1988 at Boston University. I am much indebted to all.

Most of my work was carried out at the Niels Bohr Institute and the Niels Bohr Archive, and it has been a great pleasure to work there. I especially would like to thank Hilde Levi, Helle Bonaparte and Judith Hjartbro for much help and many pleasant hours, and of course my neighbor on the third floor, Jan Teuber. I always looked forward to our beer-time on Friday evenings in the center of the universe (Jan's office).

Last, but not least, I would like to thank my wife Ingelise and my son Thomas. Their patience during my four years of work on this book has been exemplary, but fortunately they did not refrain from sounding the alarm when they felt that the beta spectrum occupied too much of my time.

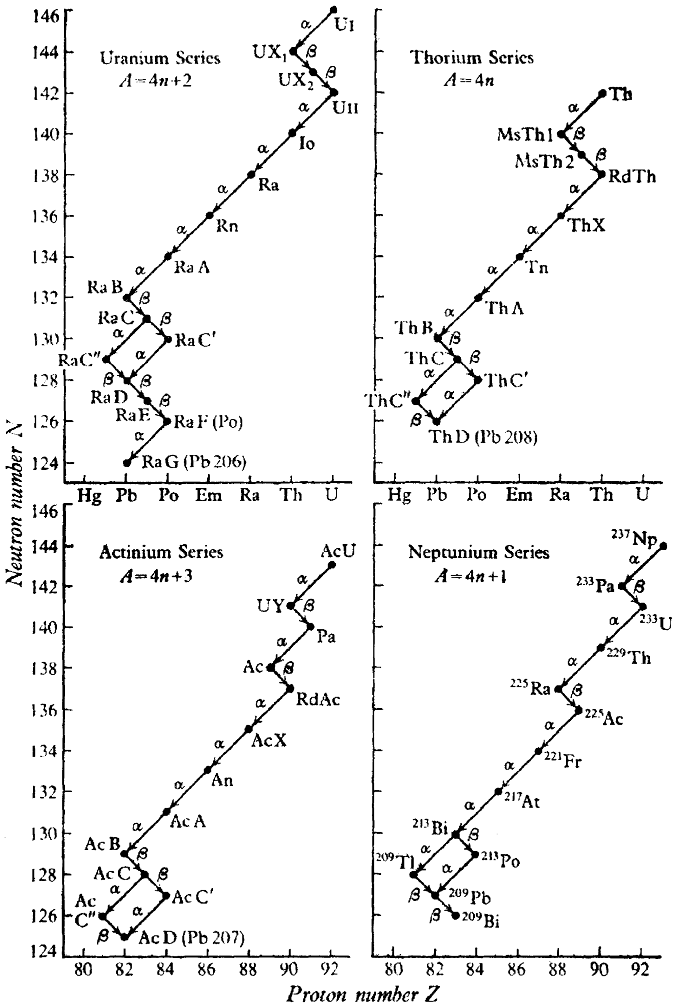
Carsten Jensen
Spring 1990

Contents

Abbreviations

AHES	Archive for History of Exact Sciences
AHQP	Archive for History of Quantum Physics (microfilm copies deposited at the Niels Bohr Archive and elsewhere)
Akad. d. Wiss. Wien	Akademie der Wissenschaften zu Wien
Am. J. Phys.	American Journal of Physics
Ann. Chim. Phys.	Annales de Chimie et de Physique
Ann. d. Phys.	Annalen der Physik
BCW	Niels Bohr Collected Works (published by Elsevier, Holland)
Biog. Mem. Fel. Roy. Soc.	Biographical Memoirs of Fellows of the Royal Society of London
BJHS	British Journal for the History of Science
Bohr MSS $(x.y)$	Bohr Manuscripts (originals deposited at the Niels Bohr Archive, Copenhagen; microfilmed as part of the *AHQP*), microfilm x, section y.
BSC $(x.y)$	Bohr Scientific Correspondence (originals deposited at the Niels Bohr Archive, Copenhagen; microfilmed as part of the *AHQP*), microfilm x, section y.
Bull. Atom. Sci.	Bulletin of the Atomic Scientists
C.R.	Comptes rendus hebdomadaires des sciences (Academie des Sciences, Paris)
C.U.L. Add. MS. 7653 x	Additional Manuscripts 7653, item x, Rutherford Papers, Cambridge University Library
Helv. Phys. Acta	Helvetica Physica Acta
HSPS	Historical Studies in the Physical Sciences
J. Chem. Soc.	Journal of the Chemical Society (London)
J. de Phys.	Journal de Physique
MTNR x/y	Item y, section x, Meitner Papers, Churchill Archives Centre, Cambridge, England
Naturwiss.	Die Naturwissenschaften
Phil. Mag.	Philosophical Magazine
Phys. Rev.	The Physical Review
Phys. Zeit.	Physikalische Zeitschrift
Proc. Camb. Phil. Soc.	Proceedings of the Cambridge Philosophical Society
Proc. Roy. Soc.	Proceedings of the Royal Society of London
PSA	Philosophy of Science Association (U.S.A.)
Rev. Mod. Phys.	Reviews of Modern Physics
SHQP	Sources for History of Quantum Physics
Z. Phys.	Zeitschrift für Physik
Verh. d. D. Phys. Ges.	Verhandlungen der Deutschen Physikalischen Gesellschaft

The Main Decay Chains



The main decay chains of the four radioactive series. The historical names are shown on the diagrams and the subsequently accepted chemical symbols on the axes. The figure is from W.E. Burcham, *Nuclear Physics: An Introduction* (Longmans Green and Co. Ltd., London, 1965), p. 37.

Chapter 1

Prelude: Beta-Spectrum Research in the Pre-Nuclear Years, 1900–1911

1.1 Discovery and identification of the beta particle[1]

In retrospect, Henri Becquerel's discovery of radioactivity in 1896 was one of the landmarks in the history of physics, and it may be said to constitute the birth of nuclear physics, even if the existence of an atomic nucleus was far from being a subject of discussion then.

At the time, however, the discovery did not attract much interest. Because of the many similarities, the consensus among physicists was that Becquerel rays were analogous to the X rays discovered a few months earlier, and not worth studying for their own sake.

Pierre and Marie Curie's discovery in 1898 of two new and very intense radioactive sources, polonium and radium,[2] made Becquerel rays much more suitable for investigation, and an increasing interest in studying them arose, but the attitude towards their identification persisted. Not even Ernest Rutherford's demonstration that uranium rays consist of two species of rays of different penetrability – an easily absorbed alpha component and a more penetrating beta component –

[1]For a more thorough discussion, see M. Malley, *From Hyperphosphorescence to Nuclear Decay: A History of the Early Years of Radioactivity, 1896–1914* (Ph.D. thesis, University of California, Berkeley, 1976); *idem.*, "The Discovery of the Beta Particle," *Am. J. Phys. 39* (1971), 1454–1461; L. Badash, "Radioactivity Before the Curies," *Am. J. Phys. 33* (1965), 128–135; *idem.*, "An Elster and Geitel Failure: Magnetic Deflection of Beta Rays," *Centaurus 11* (1966), 236–240; S. Meyer, "Zur Geschichte der Entdeckung der Natur der Becquerelstrahlen," *Naturwiss. 36* (1949), 129–132; A. Pais, *Inward Bound: Of Matter and Forces in the Physical World* (New York: Oxford University Press, 1986), pp. 142–162.

[2]P. and M. Curie, "Sur une substance nouvelle radioactive, contenue dans la pechblende," *C.R. 127* (1898), 175–178; P. Curie, M. Curie and G. Bémont, "Sur une nouvelle substance fortement radioactive, contenue dans la pechblende," *C.R. 127* (1898), 1215–1217.

C. Jensen et al. (eds.), *Controversy and Consensus: Nuclear Beta Decay 1911-1934*

changed this opinion.[3] Rutherford himself identified the two components as secondary and primary X rays, respectively.

In 1899, however, things took a new turn. At a meeting in the month of May, the two Wolfenbüttel physicists Julius Elster and Hans Geitel reported on the results of their research.[4] They had observed that a magnetic field changed the conductivity in gases produced by Becquerel rays from radium; the field proved to delay the discharge produced by the radium rays. Their interpretation of the result was that the magnetic field had deflected the gas ions, but of great importance for things to come, they mentioned the possibility that the Becquerel rays themselves were deflected. Their own tests showed that they were not, but others were challenged to do the same experiment, and different results were obtained. Friedrich Giesel, a chemist from Braunschweig, as well as the Vienna physicists Stefan Meyer and Egon Ritter von Schweidler, reported in November 1899 on a magnetic deflection of Becquerel rays.[5] From the direction of deflection the latter two furthermore concluded that the Becquerel rays, as then understood, behaved like negatively charged particles.

The magnetic deflection of the Becquerel rays was considered amazing, and stimulated greatly the study of radiation from radioactive bodies. Ernst Dorn and Becquerel himself showed that the beta rays from radium were deflected in an electric field as well, in the same direction as cathode rays,[6] and the Curies demonstrated directly that the beta rays carried with them a negative charge.[7]

With the demonstration that beta rays were deflected in both magnetic and electric fields, it became possible to determine both the velocity and the charge-to-mass ratio e/m of the beta particles. In 1900, Becquerel did so for beta rays from radium, and found an average velocity of about 1.6×10^{10} cm/sec and a charge-to-mass ratio of about 10^7 emu/g.[8] The resemblance of this e/m value with that of cathode-ray particles seemed to make it obvious to identify the beta particles with electrons, only their velocity was considerably higher than what could be impressed on cathode particles in an ordinary vacuum tube. In fact, the identification was made with mixed feelings. The great penetrability of the beta rays was difficult to reconcile with the known properties of corpuscles. The scepticism did not last

[3]E. Rutherford, "Uranium Radiation and the Electrical Conduction Produced By It," *Phil. Mag. 47* (1899), 109–163.

[4]J. Elster and H.F.K. Geitel, "Ueber den Einfluss eines magnetischen Feldes auf die durch die Becquerelstrahlen bewirkte Leitfähigkeit der Luft," *Verh. d. D. Phys. Ges. 1* (1899), 136–138.

[5]F.O. Giesel, "Ueber die Ablenkbarkeit der Becquerelstrahlen im magnetischen Felde," *Ann. d. Phys. 69* (1899), 834–836; S. Meyer and E.R. v. Schweidler, "Über das Verhalten von Radium und Polonium im magnetischen Felde," *Phys. Zeit. 1* (1899), 90–91, 113–114.

[6]E. Dorn, "Über das elektrische Verhalten der Radiumstrahlen im elektrischen Felde," *Phys. Zeit. 1* (1899), 337–338; H. Becquerel, "Contribution a l'étude du rayonnement du radium," *C.R. 130* (1900), 206–211; *idem.*, "Deviation du rayonnement du radium dans un champ electrique," *C.R. 130* (1900), 809–816.

[7]P. and M. Curie, "Sur la charge electrique des rayons deviable du radium," *C.R. 130* (1900), 647–650.

[8]Becquerel, "Deviation" (note 6); H. Becquerel, "Sur la dispersion du rayonnement du radium dans un champ magnetique," *C.R. 130* (1900), 372–376.

long, however. By the middle of 1900, the majority of scientists was convinced that the beta component consisted of high-speed cathode-ray particles, i.e., electrons.

The beta rays were the first to be identified. The alpha component was a harder nut to crack, but eventually Rutherford succeeded in measuring the charge as well as the quantity e/m, and by 1908 no one doubted that the alpha particle was a doubly ionized helium atom.[9] The third component – termed gamma rays, first reported by the Paris physicist Paul Villard in 1900[10] and corroborated by Becquerel[11] – was at first thought by Rutherford to be electrons. When Marie Curie in her *thèse*[12] of 1903 drew an analogy between gamma rays and X rays and convinced Becquerel, Rutherford agreed hesitatingly; but doubt about the nature of these rays persisted for several years thereafter.[13]

1.2 The first experiments on the velocity distribution of beta particles

In the early experiments on magnetic deflection, the photographic plates always showed a rather diffuse picture of the beta particles, indicating a wide range of velocities even if the rays were well collimated. Walter Kaufmann thus found that his radium source emitted rather slow beta rays as well as very fast ones, and this enabled him to determine both the e/m value of the beta particle and the much discussed relation between its energy, mass and velocity.[14] The slow rays were especially suitable for measuring e/m, and the fast rays were ideal for determining the energy–mass–velocity relation.

The first attempt to make a quantitative investigation of the velocity distribution of beta particles was made by Becquerel in the year 1900.[15] His experimental arrangement is shown in Figure 1.1. The beta source (radium) was placed in a lead

[9]N. Feather, "Rutherford at Manchester: An Epoch in Physics," in *The Collected Papers of Lord Rutherford of Nelson*, Vol. 2 (London: Allen and Unwin, 1963), pp. 15–33.

[10]P. Villard, "Sur la réflexion et la réfraction des rayons cathodiques et des rayons déviables du radium," *C.R. 130* (1900), 1010–1012.

[11]H. Becquerel, "Sur la transparence de l'aluminium pour le rayonnement du radium," *C.R. 130* (1900), 1154–1157.

[12]M. Curie, *Thèses présentées a la Faculté des Sciences de Paris pour obtenir le Grade de Docteur és Sciences Physiques. Recherches sur les substances radioactives. Propositions données par le Faculté* (Paris: Gauthier-Villars, 1903). Translated into English as *idem.*, *Radioactive Substances* (New York: Philosophical Library, 1961).

[13]For historical accounts, see R.H. Stuewer, "William H. Bragg's Corpuscular Theory of X Rays and γ Rays," *BJHS 5* (1971), 258–281; *idem.*, *The Compton Effect: Turning Point in Physics* (New York: Science History Publications, 1975); B.R. Wheaton, *The Tiger and the Shark: Empirical Roots of Wave-Particle Dualism* (New York: Cambridge University Press, 1983).

[14]W. Kaufmann, "Die magnetische und elektrische Ablenkbarkeit der Becquerelstrahlen und die scheinbare Masse der Elektronen," *Nachrichten von der Königlichen Gesellschaft der Wissenschaften zu Göttingen. Mathematisch–physikalische Klasse* (1901), 143–155.

[15]Becquerel, "Sur la dispersion" (note 8); *idem.*, "Note sur la transmission du rayonnement du radium au travers des corps," *C.R. 130* (1900), 979–984; *idem.*, "Sur l'analyse magnétique

groove beneath the slit Sp through which the particles went into two eccentrically placed cylinders, with slits on one side. Influenced by a homogeneous magnetic field, the beta particles were deflected into circular orbits with radii essentially dependent on their velocities. Their paths were outlined on a photographic plate parallel to the plane of the circles. By using two cylinders, it was ensured that only very narrow pencils of rays escaped. Their radii could be determined, and the product of radius and field strength was a measure of their velocity. By turning the cylinders relatively to each other the whole velocity spectrum could be investigated.

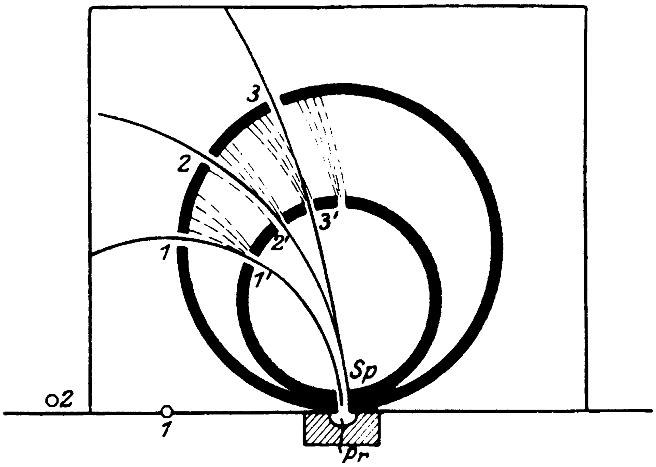


Figure 1.1: Becquerel's experimental arrangement to investigate the velocity distribution of beta particles emitted from a radioactive source. The beta source was placed in the groove beneath the slit Sp, and the particles were deflected into circular orbits by a homogeneous magnetic field. The two eccentrically placed cylinders with slits (1, 2, 3) and (1′, 2′, 3′), respectively, produced very narrow pencils. The paths were outlined on a photographic plate parallel to the plane of the circles, and their radii could thus be measured. By turning the cylinders relatively to each other the entire spectrum could be measured. Source: Kohlrausch, "Radioaktivität" (note 19), p. 198.

The method is not very accurate, but the inhomogeneity in the velocity of the beta rays seemed to be a fact, and in 1904, as well as in 1913, Rutherford wrote that "the complexity of the radiation has been very clearly shown by Becquerel."[16]

des rayons du radium et du rayonnement secondaire provoqué par ces rayons," *C.R. 132* (1901), 1286–1288.

[16] E. Rutherford, *Radio-Activity* (Cambridge: Cambridge University Press, 1904), p. 98; repeated in *idem.*, *Radioactive Substances and Their Radiations* (Cambridge: Cambridge University Press, 1913), p. 196.

In fact, he went even further by stating that "the impression on the plate takes the form of a large, diffuse, but continuous band."[17]

Shortly after the first edition of Rutherford's book on radioactivity was published in 1904, his words received further support. Friedrich Paschen, the great spectroscopist, carried out some investigations on the velocity spectrum of beta rays from radium[18] by using a method which, in the words of Karl W.F. Kohlrausch twenty-four years later, "applied apparatus that already came close to present usage."[19] Owing to some uncertainty, Paschen's experiments must be said to have been only semi-quantitative in 1904, but they were significant precursors of later, more precise, methods.

The beta source (15 mg $RaBr_2$) was sealed in a small glass vessel in the center (b in Figure 1.2A) of six lead blades surrounded by a lead cylinder (a in Figure 1.2A) insulated from b. The outer part of a was connected to an electroscope E. The whole apparatus was placed in a homogeneous magnetic field H (see Figure 1.2B). Influenced by this magnetic field, the beta particles were deflected into circular orbits. If the diameter, $2r$, of these orbits was smaller than R, the particles were not able to reach the outer lead cylinder. If, on the contrary, $r = mv/eH > R/2$, they were, and deposited their charge on the cylinder. The total charge Q deposited is then equal to $\sum Ze$, where Z is the number of beta particles with velocities v fulfilling the above inequality. By determining Q as a function of $HR/2$, Paschen obtained a curve whose first derivative is a measure of the deposited charge per field intensity H or, equivalently, the number of beta particles as a function of their velocity. Paschen's curve (I), together with its first derivative (II), is shown in Figure 1.2C. Apart from the first section, i.e., until $H = 1000$ gauss, curve II resembles surprisingly well later velocity distributions of beta particles from RaC (cf. Figure 3.11).

A threat to this apparent continuity in the beta spectrum was, however, just around the corner.

[17]Rutherford, *Radio-Activity* (note 16), p. 98; *idem.*, *Radioactive Substancees* (note 16), p. 196.

[18]F. Paschen, "Über die Kathodenstrahlen des Radiums," *Ann. d. Phys. 14* (1904), 389–405.

[19]K.W.F. Kohlrausch, "Radioaktivität," in *Handbuch der Experimentalphysik*, Band 15 (Leipzig: Akademische Verlagsgesellschaft m.b.H., 1928), p. 199.

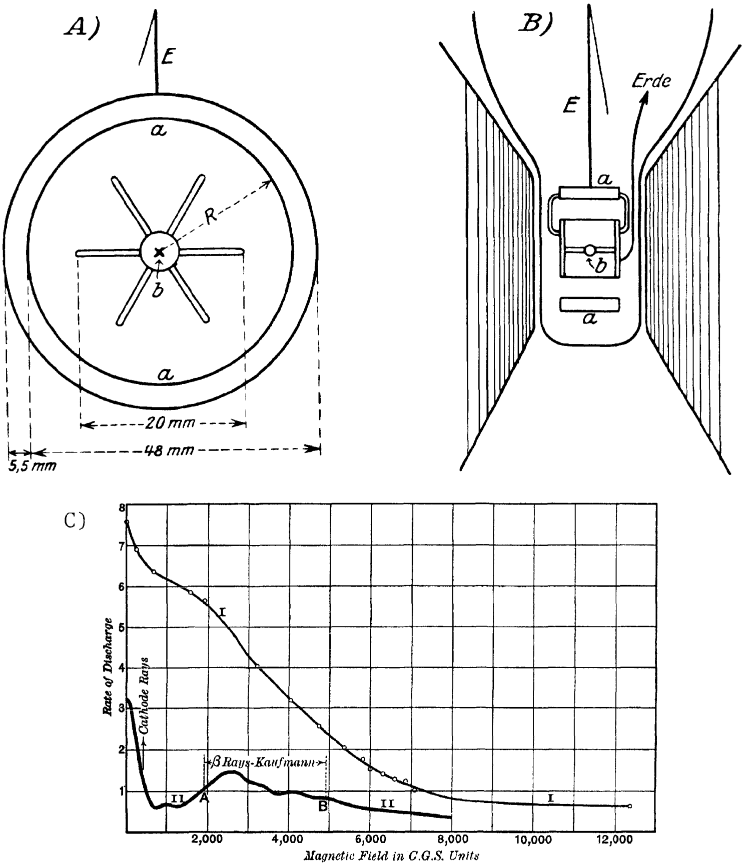


Figure 1.2: Paschen's experimental arrangement to investigate the velocity spectrum of beta rays from radium is shown in A and B. The beta source was placed in the center b of six lead blades that were surrounded by a lead cylinder a. The whole apparatus was placed in a homogeneous magnetic field H. Paschen measured the charge deposited on the cylinder as a function of $HR/2$, and he obtained a curve (I in C) whose first derivative (II in C) was a measure of the number of particles as a function of their velocity v. Sources: Kohlrausch, "Radioaktivität" (note 19), p. 200 (A and B); Rutherford, *Radioactive Substances*, (note 16), p. 251 (C). The letter "C" denoting the lower figure has been added by the author.

1.3 Absorption measurements question the inhomogeneity of the beta particles

By 1904, several investigations of the absorption of beta rays by matter had been carried out, usually with a testing vessel as shown in Figure 1.3. Experiments

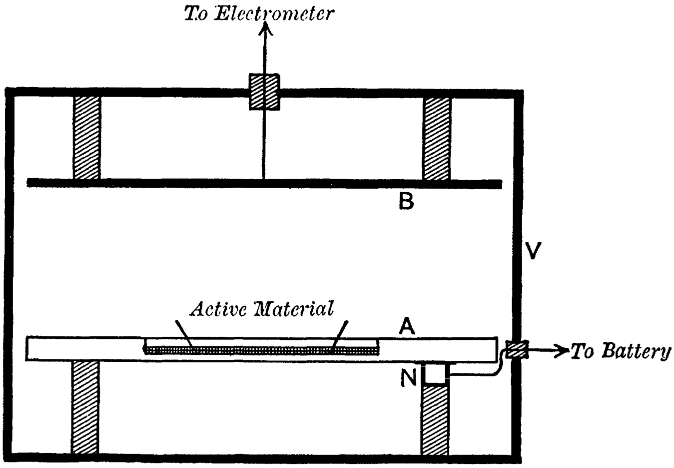


Figure 1.3: The arrangement used for testing the absorption of beta rays by matter. Two parallel insulated metal plates A and B were placed inside a metal vessel V, provided with a side door. The plate A was connected to one terminal of a battery of small storage cells, the other terminal of which was grounded; the plate B was connected to an electrometer, and the vessel V was grounded. The active material to be tested was spread uniformly in a shallow groove in the brass plate A. In order to avoid breaking the battery connection every time the plate A was removed, the wire from the battery was permanently connected to the metal block N resting on an ebonite support. Source: Rutherford, *Radio-Activity* (note 16), p. 82.

with uranium showed that the intensity I of the beta rays, after they had passed through a thickness d of matter, was expressible by an exponential law,

$$I = I_0 e^{-\lambda d},$$

where I_0 is the initial intensity and λ a constant of absorption. In 1905, Tadeusz Godlewski of McGill University in Montreal found that the same law appeared to hold for actinium, and he stated that "this indicates that the beta rays from actinium are homogeneous in character, and that their penetrating power does not change with the thickness of matter traversed."[20] This may seem a rather bold

[20] T. Godlewski, "On the Absorption of the Beta and Gamma Rays from Actinium," *Phil. Mag. 10* (1905), 375–379, p. 377.

statement. The indication was certainly not obvious, as Heinrich Willy Schmidt from Giessen pointed out in 1906.[21] Godlewski had good reasons for his interpretation, however. In his 1913 edition of the radioactivity "bible" Rutherford explained why:

> Since [Philipp] Lenard had shown that cathode rays, which are identical with β rays of low speed, are absorbed according to an exponential law, it was natural at first to suppose that the exponential law of absorption was an indication that the β rays were *homogeneous*, i.e., consisted of β particles projected with the same speed. On this view, the β particles emitted from uranium which gave a nearly exponential law of absorption, were supposed to be homogeneous.[22]

Since it was well known that a sample of radium bromide consists of, besides radium, several other decay products, where at least three – RaB, RaC and RaE – emit beta particles, the above-mentioned interpretation was fully consistent with investigations of beta rays from radium, which showed that the absorption of these rays did not follow an exponential law, and it was also considered in agreement with the outcome of Becquerel's and Paschen's deflection experiments. Rutherford concluded in his first edition of *Radio-Activity*: "The electrical examination of the deviable rays thus leads to the same results as their examination by the photographic method."[23]

In September 1906, Schmidt delivered a lecture on his investigations of absorption of beta particles from radium. He had concentrated on RaB and RaC, which he succeeded in sorting out from the radium compound. Figure 1.4 shows his results, from which he drew the important conclusion that "within certain filter thicknesses the points of the curve absorb according to a pure exponential function." He proposed the following interpretation:

> Couldn't this also be explained by saying that among all the beta rays there exists a certain group with constant absorption coefficient? Indeed, couldn't we even go one step further and explain the total effect of the beta rays by assuming that a few beta-ray groups have a constant absorption coefficient? It follows that the radiation intensity I could be expressed according to the formula
>
> $$I = a_1 e^{-\lambda_1 d} + a_2 e^{-\lambda_2 d} + \cdots$$
>
> where d is the filter thickness, e the basis of the natural logarithms, and a and λ certain constants.[24]

---

[21]H.W. Schmidt, "Über die Absorption der Beta-Strahlen des Radiums," *Phys. Zeit.* 7 (1906), 764–766, p. 765.

[22]Rutherford, *Radioactive Substances* (note 16), pp. 209–210.

[23]Rutherford, *Radio-Activity* (note 16), p. 113.

[24]Schmidt, "Über die Absorption" (note 21), p. 765.

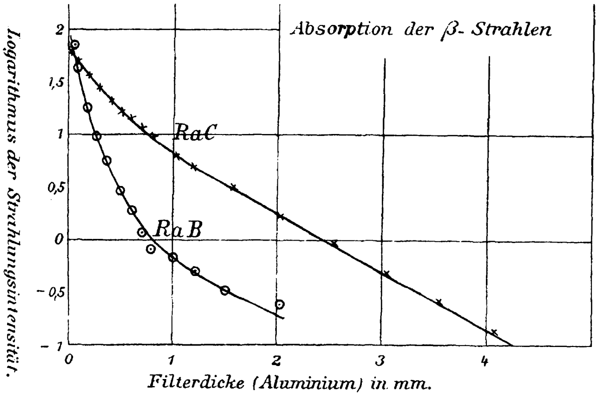


Figure 1.4: Absorption curves of beta rays from RaB and RaC, obtained by Schmidt in 1906. The logarithm of the beta-ray intensity is plotted against the filter thickness (aluminum) in mm. The dots and crosses mark the experimental values, and the curves constitute the values calculated on the assumption that the intensity as a function of thickness could be expressed as a sum of exponential functions. Source: Schmidt, "Über die Absorption" (note 21), p. 765.

He made the calculation for RaB with the help of the relation

$$I = 1100e^{-890d} + 88e^{-80d} + 2.5e^{-13.1d},$$

where d is measured in cm, and for RaC using

$$I = 49e^{-53d} + 25e^{-13.1d},$$

and obtained the two full-drawn lines in Figure 1.4. He did not explicitly draw the conclusion that the separate beta groups consisted of homogeneous particles, but he did something that seems to be equivalent. His argument and conclusion in his own words were as follows:

> Since the penetrating power of the beta rays can only depend on their velocity, it follows from the constancy of the absorption coefficient that when passing through matter the particles do not change their velocity at all.[25]

He imagined that probably a "scattering and a complete annihilation of the individual particles take place simultaneously."[26]

Even if the exponential law of absorption of homogeneous beta particles seemed to be a plausible one in 1906, it also met with opposition. Joseph John

[25] *Ibid.*, p. 766.
[26] *Ibid.*, p. 766.

Thomson made a theoretical derivation of an expression for the absorption coefficient λ based on this law, but in the second edition of his book *Conduction of Electricity through Gases* of 1906, he showed that the penetrating beta particles reduced their velocity according to the formula,

$$v_x^4 = v_0^4 - ax,$$

where v_x is the velocity at the distance x from the source, v_0 the initial velocity, and a a constant. He thereby undermined the exponential law.[27]

Nor did William Henry Bragg, professor of physics at the University of Adelaide in Australia, believe in the exponential absorption. He raised the objection that the thickness of the beta-radiating layer is of essential importance to the outcome of absorption experiments. For very thin layers, Bragg argued, an exponential law is not to be expected, since the oblique rays have to traverse a longer distance, and consequently suffer a stronger absorption. The thicker the layer, the fewer oblique particles will succeed in coming to the outlet, and the more the absorption will obey an exponential law.[28]

The exponential law was a persevering competitor, however. In 1907, Schmidt presented further investigations of absorption of beta particles, this time from RaE.[29] For several reasons, RaE was most suitable; above all it emits very few disturbing gamma rays, but at first sight the result was not promising. According to Schmidt, there was no pure exponential law,[30] and the shape of the curve even proved impossible to explain by a sum of exponential functions. Gamma rays or radioactive impurities were not the problem, but a clearly demonstrated scattering gave hope:

> Now it is quite possible that a pure exponential law holds for absorption proper, i.e., for the annihilation of radiation energy, and that the deviations from this law are only apparent. The deviations are caused precisely by the fact that part of the scattered radiation energy is not measured with the experimental arrangement chosen, and furthermore that scattered rays cover a longer distance in the filter than those passing straight through.[31]

To answer the question of a change of velocity when beta particles are passing through matter, Schmidt carried out a magnetic-deflection experiment. The arrangement and method are illustrated in Figure 1.5, and the results obtained

[27] J.J. Thomson, *Conduction of Electricity Through Gases* (Cambridge: Cambridge University Press, 1906), pp. 378–379. For further discussions of absorption and scattering of beta particles, see J. Thorsen, *Udviklingen af Niels Bohrs stødteori i perioden 1912–1927* ("The Development of Niels Bohr's Theory of Collisions, 1912–1927,"), Ph.D. thesis, University of Aarhus, 1978; J.L. Heilbron, "The Scattering of Alpha and Beta Particles and Rutherford's Atom," *AHES 4* (1967/68), 247–307.

[28] See O. Hahn, *A Scientific Autobiography* (New York: Scribner's, 1966), p. 53.

[29] H.W. Schmidt, "Einige Versuche mit Beta-Strahlen von Radium E," *Phys. Zeit. 8* (1907), 361–373.

[30] *Ibid.*, p. 362.

[31] *Ibid.*, pp. 370–371.

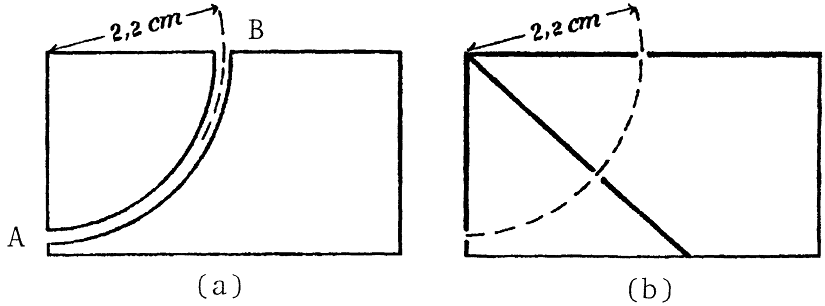


Figure 1.5: In his deflection experiments, Schmidt used these two different arrangements: (a) a block of lead with a small circular channel; (b) an arrangement consisting of three brass diaphragms soldered on a common sheet. The apparatus for measuring ionization was placed near hole A, and the source and filters, if any, at hole B. By varying the homogeneous magnetic field Schmidt obtained velocity distributions for different filters. Source: Schmidt, "Einige Versuche" (note 29), p. 371. All letters have been added to the original figure by the author.

are placed together in Figure 1.6. All four curves resemble each other; they are symmetric and have nearly identical maxima. Since he furthermore was able to show that even a small change in velocity would have caused a considerable change in the absorption coefficient, he felt certain about his conclusion

> ... that the energy given up by the beta rays when passing through matter does not correspond to a reduction of the particles' velocity. In all probability, an absorption, i.e., an annihilation of radiation energy, originates from the fact that for equally thick layers the same percentage of particles are stopped completely each time, whereas the remaining particles fly through with undiminished velocity.[32]

Schmidt also emphasized the result of Philipp Lenard and others that cathode rays showed a similar behavior.

Also in 1907, the Austrian physicist Lise Meitner started her distinguished career in Berlin. In 1905, she received her Ph.D. in Vienna under Franz Exner, and then transferred to Ludwig Boltzmann's Institute for Theoretical Physics where Stefan Meyer, an assistant of Boltzmann, introduced her to the new subject of radioactivity. After having published two papers on absorption and scattering of alpha and beta rays, she decided in 1907 on a temporary stay in Berlin to attend Max Planck's lectures. She also wanted to do some experimental work, however,

[32] *Ibid.*, p. 372.

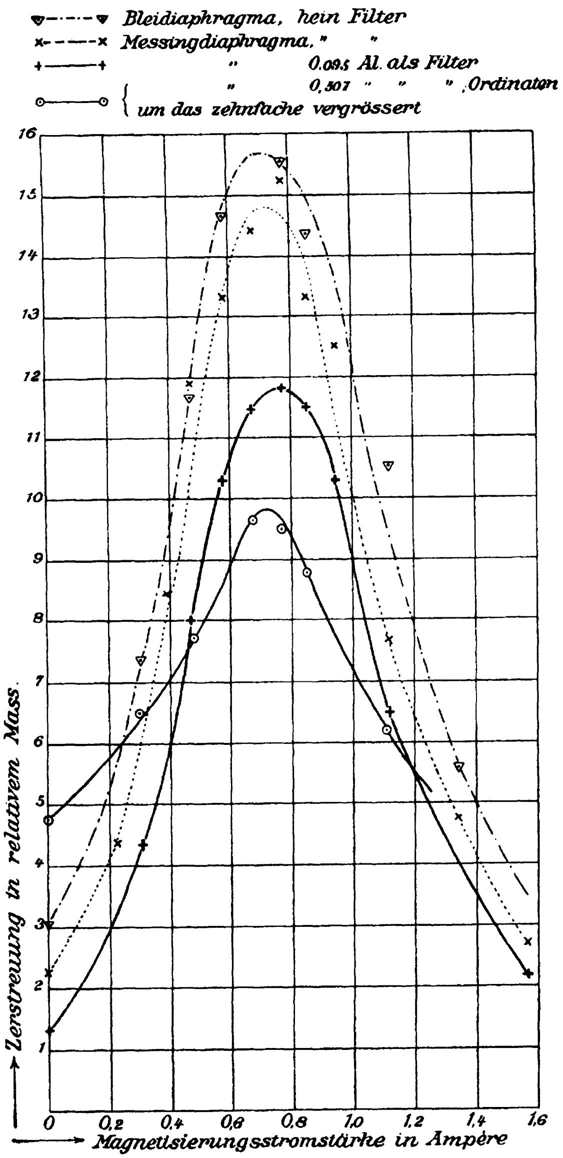


Figure 1.6: Schmidt's beta-particle velocity-distribution curves of 1907. The scattering per unit mass (of the filter) is plotted against the magnetizing strength of the current in ampères. It appears that the thickness of the filter did not have any essential influence on the shape of the curve. Source: Schmidt, "Einige Versuche" (note 29), p. 372.

and since Otto Hahn already worked with radioactive substances, they decided to collaborate.[33] She would stay with Hahn for about thirty years.

Meitner and Hahn's first joint work was an extensive investigation of the absorption of beta particles from different radioactive elements with the purpose of settling the dispute between Schmidt and Bragg (they did not mention J.J. Thomson).[34] At the outset, Hahn and Meitner questioned Bragg's result, since he had not stated whether he had used pure RaC. A possible explanation of his result was that his source had been the unseparated active deposit RaA + B + C or RaB + C.

Their investigation confirmed their prejudice against Bragg's result. A curve measured with ThB + C (now ThC + C′ where only ThC emits beta particles) in a thin layer showed clearly a straight line on semi-logarithmic paper, i.e., an exponential absorption, which was inconsistent with Bragg's view according to which one should find a bent curve.

Since ThA (the later ThB) and other substances behaved in the same way, Hahn and Meitner felt certain of their conclusion:

> The fact that under our controlled conditions the absorption measurements gave a pure exponential law also for infinitely thin layers, led us naturally to the assumption that also the other beta-ray emitting substances, as long as they are uniform, would show a similar behavior, and that in cases where the apparent absorption coefficient decreases with increasing thickness of the penetrated layer, we were not dealing with uniform beta-ray emitting substances.[35]

In line with their hypothesis, Hahn and Meitner assumed that aberrant sources, e.g., mesothorium 2, were complex. They admitted the assumption to be "somewhat speculative," but claimed that it found strong support in the absorption curves of the uniform beta emitters.[36] Schmidt had accounted for the observed deviation from the exponential law for RaC beta rays by assuming the existence of several groups of emitted beta rays, but that explanation did not appeal to Hahn and Meitner:

> When one considers the analogy with alpha rays, our assumption that uniform beta-ray emitting products also emit only one group of beta rays, seems a priori more probable.[37]

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[33]See Hahn, *Autobiography* (note 28), pp. 50–58; L. Meitner, "Looking Back," *Bull. Atom. Sci.* (Nov. 1964), 2–7; S. Watkins, "Lise Meitner and the Beta-Ray Energy Controversy: An Historical Perspective," *Am. J. Phys. 51* (1983), 551–553; R.L. Sime, *Lise Meitner: A Life in Physics* (Berkeley: University of California Press, 1996).

[34]O. Hahn and L. Meitner, "Über die Absorption der Beta-Strahlen einiger Radioelemente," *Phys. Zeit. 9* (1908), 321–333.

[35]*Ibid.*, p. 328.

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

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

Figure 1.7: Lise Meitner in the 1920s. [Niels Bohr Archive]

Concerning the cause of the straight absorption curves, however, they agreed with Schmidt "that the velocity of the beta rays would not change when they passed through matter. For since beta rays from different radioelements differ only in their velocity, then, if the velocity would change during absorption, a pure exponential law would not be obeyed for the whole process."[38]

Hahn and Meitner continued their work and investigated beta rays from actinium with the purpose of examining whether the validity of the exponential law for uniform beta rays could be established also for actinium.[39] Their absorption curve (A in Figure 1.8) presented two deviations from the exponential law: the first and the last part of the curve showed a too strong absorption. Consistent with their previous interpretation, Hahn and Meitner ascribed the first deviation to another beta emitter, whereas the last one was considered to be owing to the experimental arrangement. By changing the arrangement in various ways, they were able to change the last part of the curve (see B, C, D and E in Figure 1.8); in B they had quite removed the deviations by eliminating the oblique rays.

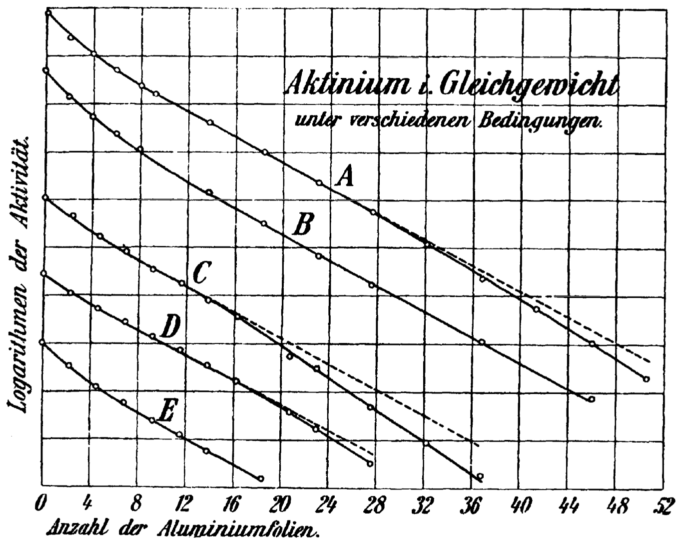

Figure 1.8: Absorption curves of beta rays from actinium, obtained by Hahn and Meitner in 1908. By changing the experimental arrangement, it proved possible to change the last part of the curve, and in B the deviation from the exponential law was removed. The first part of the curves also showed a deviation from the exponential law, which was ascribed to the existence of another beta emitter. Source: Hahn and Meitner, "Über die Beta-Strahlen" (note 39), p. 698.

[38] *Ibid.*, p. 332.

[39] O. Hahn and L. Meitner, "Über die Beta-Strahlen des Aktiniums," *Phys. Zeit. 9* (1908), 697–702, p. 697.

The first part of curve A showed, according to their hypothesis, that actinium must consist of more than one beta emitter. They tried, therefore, to separate its decay products, first radioactinium whose remarkable absorption curves appear in Figure 1.9 (A and B). Each consisted of two exponential parts. They felt certain that the first part represented very soft beta rays, whereas the interpretation of the last part was more doubtful as it might be ascribed to easily absorbable gamma radiation.[40]

From an investigation of the absorption of beta rays from the active deposit with and without actinium X, Hahn and Meitner inferred that actinium X does not emit beta rays whereas the active deposit emits two groups, and by separating the active deposit into its components they showed that the two beta emitters were actinium A and C. The investigation thus had yielded what they had expected:

> Our assumption, already made earlier, that uniform substances also emit uniform beta rays, and that their absorption in aluminum follows an exponential law, has fully retained its status as a working hypothesis also for actinium, and has led to the discovery of the new groups of beta rays.[41]

That radioactinium possibly emits two groups was considered too uncertain to be taken seriously. Shortly afterwards, however, Hahn found the presence of hard beta rays most probable[42] and together with Meitner he concluded:

> Among the rays investigated, we found that, under our controlled conditions, those of thorium A, thorium D, actinium A, and actinium C are homogeneous, whereas the rays from mesothorium II and radioactinium consist of several groups.[43]

Thus, consistent with their hypothesis, they assumed that mesothorium 2 and radioactinium were not single substances.

The next Hahn–Meitner investigation on this subject was presented in October 1909 and dealt with radium.[44] Several experiments had shown that RaB, RaC and RaE emit beta rays, and it was often inferred that the radiation from these substances was not homogeneous. As we have seen, Schmidt proposed that the beta rays from radium B and C consist of three and two groups, respectively.

Regarding RaE, Hahn and Meitner agreed, and their new investigation of radium B and C showed that also RaC emits complex radiation. As a consequence, it had to consist of more substances and in fact, by using the successful recoil

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[40] *Ibid.*, p. 699.
[41] *Ibid.*, p. 702.
[42] O. Hahn, "Über eine neue Erscheinung bei der Aktivierung mit Aktinium," *Phys. Zeit. 10* (1909), 81–89.
[43] O. Hahn and L. Meitner, "Nachweis der komplexen Natur von Radium C," *Phys. Zeit. 10* (1909), 697–703, p. 697.
[44] *Ibid.*

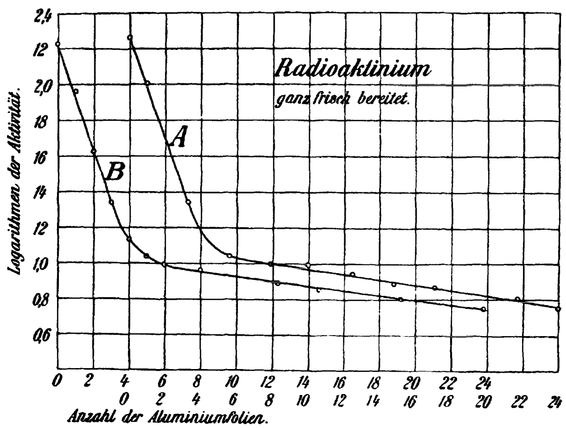

Figure 1.9: Absorption curves of beta rays from radioactinium, obtained by Hahn and Meitner in 1908. The curves were obviously made up of two exponential parts. Source: Hahn and Meitner, "Über die Beta-Strahlen" (note 39), p. 699.

method,[45] Hahn and Meitner succeeded in separating RaC into two components, radium C_1 and radium C_2, where the latter was again considered to be complex. Radium B, however, seemed to be homogeneous even if the result was uncertain because of the rapid decay into radium C.

A further result of the investigation was that radium itself emits very soft beta rays, and since radium also emits alpha rays it led to the following conclusion:

> On the basis of our hypothesis that complex rays correspond to complex substances, one must conclude from the existence of this beta radiation that radium has a complex character.[46]

Thus, Hahn and Meitner had sharpened their hypothesis to include alpha as well as beta radiation. In 1909, they were convinced that one single substance is emitting only one sort of homogeneous particle radiation. Their view was soon challenged, however, and it turned out that their hypothesis, in spite of its success in predicting new radioactive substances, was very far from the truth.

[45]The recoil method utilized the fact that, when an atom ejects an alpha or beta particle, conservation of momentum requires that the daughter atom recoil with an equal and opposite momentum. By collecting the recoil atoms on a wire or plate it proved possible to separate various decay products.

[46]O. Hahn and L. Meitner, "Über eine typische Beta-Strahlung des eigentlichen Radiums," *Phys. Zeit. 10* (1909), 741–745, p. 745.

1.4 The Hahn–Meitner vs. Wilson controversy

The most serious challenge to the Hahn–Meitner hypothesis came from the laboratory in Manchester where Rutherford had suggested to one of his students, William Wilson, to establish a connection between the absorption and velocity of beta rays.[47] This may appear to be a rather ordinary assignment, but because Wilson earlier had questioned the general assumption "that a beam of homogeneous rays is absorbed according to an exponential law,"[48] it resulted in a far-reaching novel conclusion.[49] By deflecting the beta rays in a magnetic field *before* they entered the absorbing material, Wilson made certain that the particles really were homogeneous, and it turned out that the absorption is not at all exponential. Instead, it proved to be practically linear. Consequently, exponentially absorbed beta rays from substances like uranium X, radium E, and actinium are not homogeneous as inferred by Hahn and Meitner. They have to be heterogeneous, and Wilson showed "that it is possible to obtain a heterogeneous beam of particles, of which the different types of rays are absorbed according to a linear law, but the absorption of the whole beam takes place according to an exact exponential law."[50]

The experimental arrangement leading to Wilson's conclusion is shown in Figure 1.10. The beta rays were emitted from the source J, consisting of a quantity of radium emanation in equilibrium with its decay products, and deflected through a circular tube by a magnetic field that was found to be practically uniform. At the entrance of the electroscope E, where Wilson considered the beta particles to be nearly homogeneous, he placed the absorbing sheets of metal, and then measured the ionizing power of the particles that had penetrated the sheets. The lead screens MMM aimed at reducing as much as possible the amount of scattered beta radiation entering the electroscope, and the effect of secondary beta rays caused by gamma rays was measured and subtracted from the total ionization.

The linear absorption law of homogeneous rays suggested, contrary to what was asserted by Schmidt, that beta rays suffered a decrease in velocity in their passage through matter, for which Wilson presented a convincing demonstration. He obtained a curve, a in Figure 1.11, of the ionization as a function of the magnetic field strength, then placed aluminum sheets of thickness 0.489 mm and 1.219 mm, respectively, immediately below the electroscope, obtained the curves b and c, and finally repeated the experiment with the sheets placed at the source J (d and e in Figure 1.11). The interpretation of the results seemed obvious:

[47] W. Wilson, "On the Absorption of Homogeneous Beta Rays by Matter, and on the Variation of the Absorption of the Rays with Velocity," *Proc. Roy. Soc. A82* (1909), 612–628.

[48] *Ibid.*, p. 612.

[49] See F. Soddy, *Annual Progress Report to the Chemical Society for 1908/1909*, Vol 6, p. 240; reproduced in *Radioactivity and Atomic Theory*, T.J. Trenn (ed.) (London: Taylor & Francis Ltd., 1975), p. 192.

[50] Wilson, "Absorption" (note 47), p. 624.

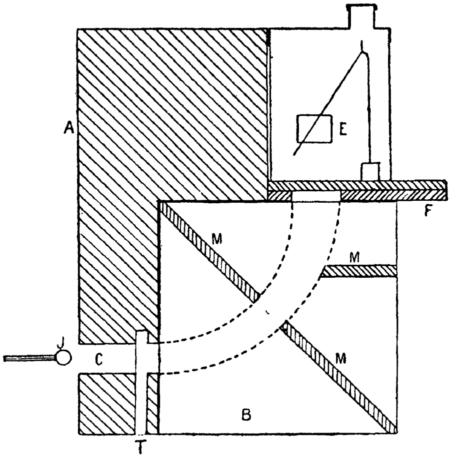

Figure 1.10: Wilson's experimental arrangement to investigate the connection between absorption and velocity of beta rays. J is the source, and E is an electroscope. The purpose of the lead screens MMM was to reduce the number of scattered beta rays entering the electroscope. The absorbing material was placed immediately below E. Source: Wilson, "Absorption" (note 47), p. 613.

> If the particles do not decrease in velocity in passing through matter, the curves obtained in this case connecting ionisation with strength of magnetic field should fall on *b* and *c*. If the velocity decreases, however, they should fall to the left of these. This was found to be the case, the curves being shown in the figure [Figure 1.11] at *d* and *e*, and the particles therefore decrease in velocity as they pass through the matter.[51]

Wilson was also able to explain why Schmidt's experiments apparently showed no change in velocity:

> According to the views expressed in this paper he was dealing with heterogeneous rays and the position of the maximum should therefore move to the higher fields if the velocity of the rays does not change. The actual decrease in velocity, however, brings the maximum point back to practically the same position as before.[52]

What was the immediate reaction to Wilson's significant new results? In England there seems to have been a reticent wait-and-see attitude; more experiments were needed. Frederick Soddy expressed this as follows in his *Annual Progress Report* to the Chemical Society for 1908–1909:

[51] *Ibid.*, p. 627.
[52] *Ibid.*, p. 627.

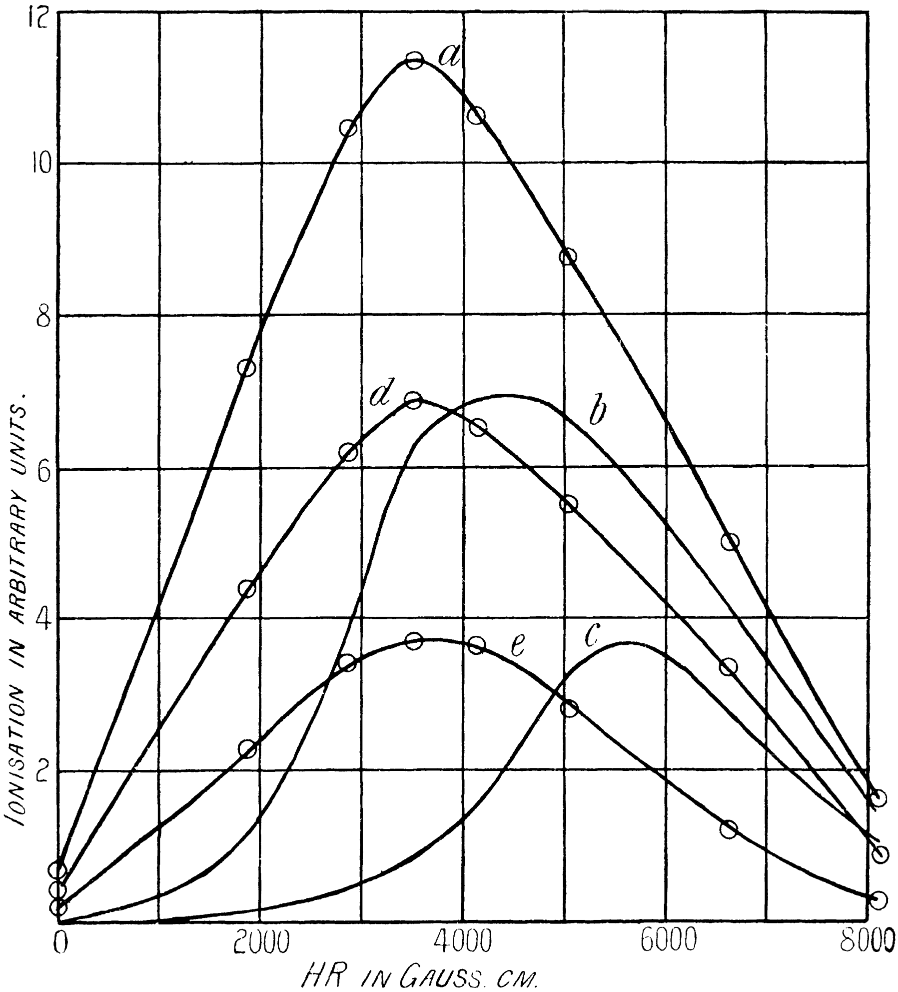

Figure 1.11: Wilson's velocity-distribution curves, obtained in 1909. The curve *a* showed the ionization as a function of the magnetic field-strength without an absorption sheet. The curves *b* and *c* were obtained with aluminum sheets of 0.489 mm and 1.219 mm, respectively, placed immediately below the electroscope. The curves *d* and *e* were obtained with the same two sheets placed at the source J. Source: Wilson, "Absorption" (note 47), p. 626.

> Revolutionary conclusions, it is true, as to the heterogeneity of β-rays and their linear rather than exponential absorption by matter have been recently put forward, but it remains very open to question whether the methods employed were refined enough to bear such an interpretation.[53]

Rutherford appears to have had greater confidence in Wilson's work, even if he also called for more experiments. In a letter to Hahn of 24 October 1909, he argued for a greater open-mindedness on the question:

> *Don't* necessarily commit yourself to the view that each line corresponds to a distinct product. There are other possible explanations between which experiments will ultimately decide.[54]

The attitude in Berlin was different, and apparently Rutherford's warning did not make any impression on Hahn and Meitner. They still had a strong belief in their working hypothesis, and were of the opinion that Wilson had misinterpreted his results.[55]

First, Hahn and Meitner felt certain that the investigated rays were not homogeneous. The size of Wilson's aperture, they argued, was too large. For instance, RaC (RaC_1 + RaC_2), as it was then assumed, emits two very close beta rays, and they could not be separated by Wilson's arrangement. Second, they denied that the results showed a decrease of velocity:

> We believe to be able to show that Wilson's experiments not only constitute no proof at all of a velocity change, but, when correctly interpreted, even lead to the conclusion that the velocity remains unchanged.[56]

Hahn and Meitner's argument against the decrease of velocity is worth closer study. They agreed with Wilson that the displacement of the curves *b* and *c* on Figure 1.11 arises because slow rays are absorbed more than fast ones, but thereafter agreement came to an end. "The situation is different," they wrote, "if the rays pass through aluminum *before* they are deflected by the magnetic field."[57] To illustrate their view they considered the case in which beta rays of two different velocities are emitted from the source. The intensity of the slow rays is J_1, that of the fast rays J_2. If no absorption material is present, the magnetic field for which the maximum ionization occurs will sort out an amount $\alpha_1 J_1 + \alpha_2 J_2$ of the radiation, where α_1/α_2 is determined by the field strength. "If one now lets the rays pass through aluminum before they enter the magnetic field," Hahn and Meitner pointed out, "then the emerging rays will always have lower intensity, say $\beta_1 J_1$,

[53]Soddy, *Annual Progress Report* (note 49), p. 192.
[54]Rutherford to Hahn, 24 October 1909, C.U.L. Add. MS. 7653 H40.
[55]O. Hahn and L. Meitner, "Über das Absorptionsgesetz der β-Strahlen," *Phys. Zeit. 10* (1909), 948–950, p. 948.
[56]*Ibid.*, p. 949.
[57]*Ibid.*, p. 949.

and $\beta_2 J_2$, respectively, where β_1 may of course be smaller than β_2."[58] They were thus quite aware that the intensity distribution of the rays changed from $J_1 + J_2$ to $\beta_1 J_1 + \beta_2 J_2$ while passing through the sheets. Nevertheless, in the case of no decrease of velocity, they asserted:

> Then the maximum ionisation will always occur at the same field strength, as in the case of curve a [in Figure 1.11], as long as sufficiently soft β rays are present to ensure that the rays appear in the ratio α_1/α_2.[59]

That was exactly what happened with the curves d and e in Figure 1.11. If a decrease of velocity took place, the maximum of the curves would have been displaced, according to the argument of Hahn and Meitner, in the direction of lower field strength (relative to a).

Finally, Hahn and Meitner pointed out that the above considerations were only valid if the investigated rays were inhomogeneous, and that Wilson would not have observed any displacement of the maximum at all had he used homogeneous rays.[60]

Wilson replied to Hahn and Meitner's sharp criticism in the *Physikalische Zeitschrift* in early 1910.[61] He repudiated their argument against the change in velocity by means of their own example. In case of no decrease of velocity, the intensity distribution had to be $\alpha_1\beta_1 J_1 + \alpha_2\beta_2 J_2$, both when the absorbing sheets are placed at the entrance of the electroscope and when they are placed immediately behind the source, i.e., the maxima of the curves c and d in Figure 1.11 had to coincide. The reason why they do not coincide is exactly the decrease in velocity. Wilson did not understand Hahn and Meitner's argument. "I am not in a position," he admitted, "to understand the reasons justifying such a conclusion. Apparently, Hahn and Meitner have not taken into account the different degrees of absorption for slow and fast β-rays."[62] This was certainly strange, considering that they were quite aware of this velocity-dependent absorption, and mentioned it explicitly in their paper.

Concerning the homogeneity of the beta rays, Wilson also had strong arguments. He admitted that the velocity varied within certain limits in his first experiments, but later investigations of much more homogeneous rays confirmed the linear absorption law with closer accuracy. Furthermore, he stated, it was easy to see that a random mixture of homogeneous beta rays, each group obeying the exponential law of absorption, cannot be absorbed linearly. If this were so, a curve of the logarithm of the ionization current as a function of the thickness of the

[58] *Ibid.*, p. 950.
[59] *Ibid.*, p. 950.
[60] *Ibid.*, p. 950.
[61] W. Wilson, "Über das Absorptionsgesetz der Beta-Strahlung," *Phys. Zeit. 11* (1910), 101–104.
[62] *Ibid.*, p. 102.

absorbing material has to be convex with respect to the reference point, and his curves always showed a marked concavity.

Wilson concluded with a remark that in retrospect may foreshadow the discovery of the continuous beta spectrum. According to the hypothesis of Hahn and Meitner, the rays from radium B and C consist of a few groups, each with a certain velocity. "One should then expect," Wilson wrote, "that the photographic exposures of [Walter] Kaufmann and [Alfred] Bucherer would show a series of bands and not, as was actually the case, a continuous intensity change from one velocity to the next."[63] As Abraham Pais has mentioned, one may wonder why Wilson did not go on to prove that the primary beta spectrum is continuous. I agree with Pais that Wilson was probably too involved in absorption questions to take that step.[64]

Hahn and Meitner abandoned further absorption experiments. In a retrospective account Meitner recalled that, following a discussion with Otto von Baeyer, she and Hahn realized that in order to say anything about the velocity of radiation, they had to use deflection in a transverse magnetic field.[65] They did not, however, abandon their idea that single products emit only a single beta-ray group of a well-defined velocity. As we shall see in the next section, their early magnetic-deflection experiments made them stick to this idea.

1.5 From unity to complexity: magnetic-deflection experiments, 1910–1911

"Congratulations on your magnetic photographs which I received this morning," Rutherford wrote to Hahn in October 1909. "They are really fine and much clearer than I would have anticipated . . . . Drive ahead with the matter."[66] So Hahn did, together with Baeyer from the Physical Laboratory in Berlin and Meitner, and apparently with great success.[67]

Their arrangement, shown in Figure 1.13, was similar to the one used by Rutherford in his experiments on the magnetic deflection of alpha rays, and originally conceived by Becquerel. An active wire was placed in the groove A. The rays then passed through a narrow slit B and hit a photographic plate C. The apparatus was enclosed in a cylindrical vessel P that could be rapidly evacuated, and placed in a uniform magnetic field parallel to the wire and the slit. If x is the deflection on the photographic plate, b the distance of the slit from the source, and a the distance of the plate from the slit, then the radius of curvature of the

[63] *Ibid.*, p. 104.
[64] Pais, *Inward Bound* (note 1), p. 153.
[65] Meitner, "Looking Back" (note 33), p. 6.
[66] Rutherford to Hahn, 24 October 1909, C.U.L. Add. MS. 7653 H40.
[67] O. v. Baeyer and O. Hahn, "Magnetische Linienspektren von Beta-Strahlen," *Phys. Zeit. 11* (1910), 488–493; O. Hahn and L. Meitner, "Eine neue Beta-Strahlung beim Thorium X; Analogien in der Uran- und Thoriumreihe," *Phys. Zeit. 11* (1910), 493–497.

Figure 1.12: Hahn and Meitner's beta-ray spectroscope. In foreground, Baeyer's beta spectrometer. [Deutsches Museum München]

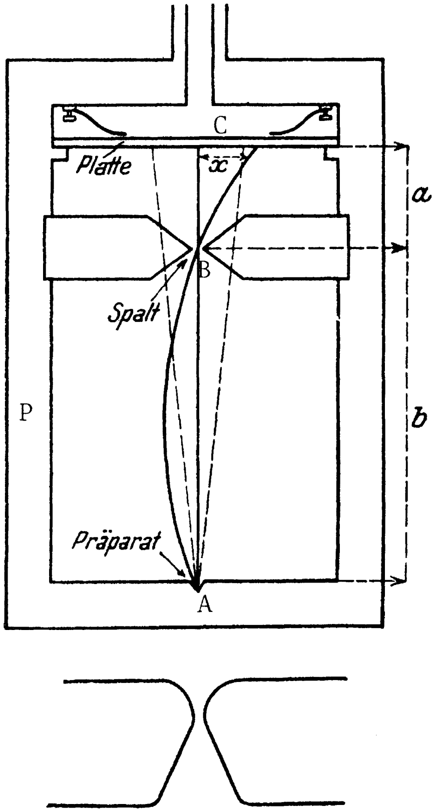

Figure 1.13: The apparatus used by Baeyer, Hahn and Meitner in their magnetic-deflection experiments. The active wire was placed in the groove A, and B is a narrow slit through which the beta rays passed before they hit the photographic plate C. The apparatus was enclosed in the vessel P which could be evacuated. Source: Kohlrausch, "Radioaktivität" (note 19), p. 203. The letters A, B, C and P have been added to the original figure by the author.

circular arc ρ is given by

$$\rho = \sqrt{\left(\frac{x^2 + ab + a^2}{2x}\right)^2 + \frac{b^2}{4}},$$

or, if x is small, approximately by

$$\rho = \frac{ab + a^2}{2x}.$$

Then, from the relativistic formula

$$E = m_0 c^2 \left[\frac{1}{\sqrt{1 - v^2/c^2}} - 1\right] = m_0 c^2 \left[\sqrt{1 + \left(\frac{H\rho e}{m_0 c}\right)} - 1\right],$$

E or v can be calculated.

As mentioned, magnetic-deflection experiments of radium beta rays had been carried out earlier and indicated a continuous spectrum, but Meitner, Hahn and Baeyer did not find them conclusive. "Kaufmann and Bucherer did not obtain discontinuous spectra of the radium β rays," Hahn wrote to Rutherford in his letter of 13 May 1910, "but we thought, this might be due to the complexity of those rays (RaB, RaC_1C_2, E etc.)."[68] Furthermore, the velocity distribution of secondary radiation was unknown, and might well be the reason for the blurred spectrum.

The Berlin group set to work on some of the thorium products and RaE, and as it appears from Hahn's letter to Rutherford, the results were considered strong support for their hypothesis:

> We ... believe to have a proof, that radioelements emit single and definite types of β rays, and very likely only one type for single products.[69]

What were these "convincing" results? Consistent with the hypothesis, ThA (later ThB) + ThD showed two rather sharp lines, but in addition two very faint ones, and after an exposure time of about 15 hours, the beta rays from radium E had produced a single, but rather blurred line. The picture of the mesothorium-2 rays appeared to be a broad, diffuse and nearly undeflected band indicating two partly overlapping beta groups, quite in accordance with Hahn and Meitner's demonstration of non-exponential absorption of the beta rays from this product. In addition, however, four sharply separated and strongly deflected lines were present, which they could not account for. Hahn admitted in his letter to Rutherford that there were difficulties in several cases, but he did not think that they would affect the general results. If ever lost, their great confidence in their hypothesis was regained, and they summed up the results of their first magnetic-deflection experiments in this way:

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[68]Hahn to Rutherford, 13 May 1910, C.U.L. Add. MS. 7653 H41.
[69]*Ibid.*

> The present investigation proves that during the decay of radioactive substances, not only alpha rays, but also beta rays, leave the radioactive atom with a velocity that is characteristic of the substance in question. Thus, new support has been gained for the assumption made by Hahn and Meitner on the basis of their absorption measurements, that such beta rays, which are absorbed according to an exponential law, are homogeneous. Probably, each beta-radiating substance emits only one group of characteristic beta rays. In cases where this does not apply, we may be dealing with complex substances.[70]

This new development in beta-ray research caused an optimistic atmosphere. Rutherford was, it turned out, too optimistic when in August 1910 he wrote to Hahn that "the whole question of β rays is in a very interesting state and should be reasonably cleared up before long,"[71] but the Berlin group was encouraged to go on with their deflection experiments. They proceeded with the same apparatus, but the exposures were gradually improved, and stronger sources in thinner layers produced brighter pictures.

In the autumn of 1910, they still had confidence in their hypothesis, and were "very sceptical about Wilson's interpretation of his results."[72] Around New Year, however, problems appeared. It seemed that swift rays behaved quite differently from soft ones. The soft rays were completely homogeneous, whereas the swifter rays were not to be obtained within a band narrower than several percent. "RaE is the worst of all," Hahn confided to Rutherford in a long letter of 11 January 1911, and continued:

> We can only obtain a fairly broad band. We formerly thought that it was as narrow as the other bands; but this is not true. It looks as if secondary or such effects had a maximum influence on rays of a medium velocity like RaE.[73]

Meitner, Hahn and Baeyer clung to the secondary effects. Of course, they could not exclude a primary inhomogeneity, but owing to the homogeneity of soft beta rays they found it improbable. A secondary effect might be either a change in velocity as well as direction when the rays passed through the layer of radioactive material, or a secondary radiation created by the primary beta rays. To find the answer, extensive absorption experiments were carried out.[74] The result was that slow rays suffered a considerable decrease in velocity, but preserved homogeneity. Very little secondary radiation emerged. For hard rays only a slight change in velocity was found, but a considerable secondary radiation was present. This secondary radiation was considered to be the reason for the

[70] Baeyer and Hahn, "Magnetische Linienspektren" (note 67), pp. 492–493.

[71] Rutherford to Hahn, 2 August 1910, C.U.L. Add. MS. 7653 H44.

[72] Hahn to Rutherford, 7 August 1910, C.U.L. Add. MS. 7653 H45.

[73] Hahn to Rutherford, 11 January 1911, C.U.L. Add. MS. 7653 H50.

[74] O. v. Baeyer, O. Hahn and L. Meitner, "Über die Beta-Strahlen des aktiven Niederschlags des Thoriums," *Phys. Zeit. 12* (1911), 273–279.

observed inhomogeneity of swift beta rays, and that was a convenient result for the Berlin group. Even if they now had to admit, to my knowledge for the first time, that exponential absorption could not be a criterion for homogeneity, they had strengthened their belief in a discontinuous magnetic line-spectrum of beta rays.

The rigorous version of the discontinuity, i.e., that each line corresponds to a distinct product, had to be abandoned, however. During 1911, it became evident that nature was not that simple. Meitner, Hahn and Baeyer themselves investigated the active deposit of radium, RaB + RaC, found nine lines and ascribed five of them to RaB and four to RaC.[75] This and other investigations showed that there was no denying the complexity, but as we shall see in the next chapter, only the peak of the mountain had as yet emerged.

[75] O. v. Baeyer, O. Hahn and L. Meitner, "Magnetische Spektren der Beta-Strahlen des Radiums," *Phys. Zeit. 12* (1911), 1099–1101.

Chapter 2

The Origin of Beta Rays, and the Growing Complexity of Their Spectrum: The Rutherford Era, 1911–1919

2.1 Introduction

Ernest Rutherford had a life-long partiality for alpha rays.[1] In the early years of radioactivity he simply considered beta and gamma rays to be of minor interest. "I have often pointed out," he wrote in 1906, "what an important part the α particle plays in radioactive transformation. In comparison the β and γ rays play quite a secondary role."[2] His attitude changed around 1911, however, and for some years Rutherford placed an essential part of his efforts on beta and gamma rays. I think that the reasons for this change are twofold. In the first place, magnetic-deflection experiments had revealed a complexity of the beta rays from certain radioactive products whereas alpha rays always proved to be homogeneous, and this lack of analogy between alpha and beta rays attracted Rutherford's attention. Second, his nuclear model of the atom, put forward in 1911, encouraged him to propose a theory of the origin of the beta and gamma rays. This theory started a development which, owing to a fruitful interplay between theoretical considerations and experimental work, led to important results. Unfortunately, this development was almost completely interrupted by the war, but the results obtained became of great importance for the post-war researchers.

[1]On the occasion of Rutherford's 60th birthday, O. Hahn and L. Meitner wrote: "There is no problem of nuclear processes that has not benefited decisively by Rutherford's investigations, but his special love was and is, as he said himself a few years ago, the α-rays"; see *Naturwiss. 19* (1931), 57.

[2]Quoted in N. Feather, "Rutherford at Manchester: An Epoch in Physics," in *The Collected Papers of Lord Rutherford of Nelson*, Vol. 2 (London: Allen and Unwin, 1963), pp. 15–33, p. 26.

C. Jensen et al. (eds.), *Controversy and Consensus: Nuclear Beta Decay 1911-1934*

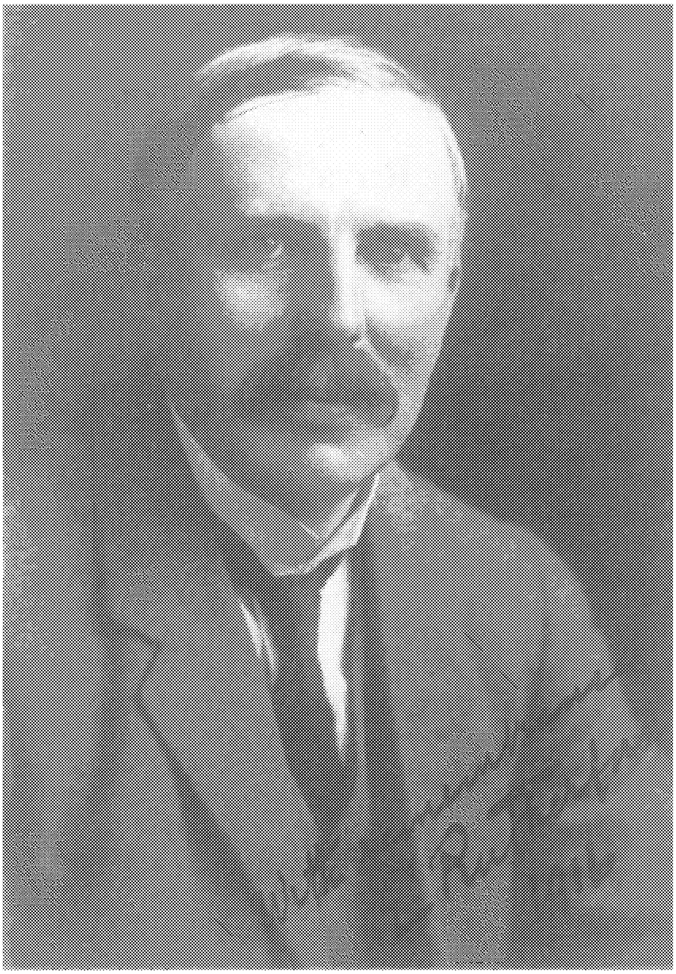

Figure 2.1: Ernest Rutherford in 1916. [Niels Bohr Archive]

2.2 Rutherford's 1912 theory, and reactions to it

In Paris, the Polish-born physicist Jean Danysz had recently carried out some experiments, to which I shall return later.[3] They showed that the beta spectra were even more complex than assumed, about 30 homogeneous groups appearing from radium B + C. An obvious explanation was that many beta particles are emitted in every disintegration, but experiments seemed to show that on the average only one beta particle is emitted.[4] Another possibility was that an atom breaks up in a number of distinct ways, each characterized by the emission of beta particles of a definite velocity. This also was rejected, however, since it might be anticipated that different modes of transformation would give rise to a series of new products, and only one was observed. The most probable explanation, according to Rutherford, was that the beta rays change their velocity in some definite way during their escape from the disintegrating atom.[5] This hypothesis appealed to Rutherford's mind since it also made possible an understanding of the gamma emission and the connection, if any, between beta and gamma rays. But did it fit with experiment? This question was of decisive importance to Rutherford.

After extensive calculations, Rutherford succeeded in expressing nearly all of the energy differences of the beta lines of radium C in the form $pE_1 + qE_2$, where $E_1 = 0.456$ MeV, $E_2 = 1.556$ MeV, and p and q are integers. "It does not appear likely," he stated in his paper, "that the connection observed is accidental."[6] In fact, it seems to have been accidental, since Rutherford calculated the energy of the beta particles from the wrong formula

$$E = \frac{1}{2} m_0 c^2 \frac{\beta^2}{\sqrt{1-\beta^2}},$$

where m_0 is the mass of the electron at slow speeds, c the velocity of light, and β the ratio of the velocity of the electron to the velocity of light. Henry Moseley became aware of the mistake, and in a letter of 14 October 1912 to his mother he described Rutherford's reaction:

> Today I was surprised to find a sad blunder in Rutherford's latest paper, in which he gives a new theory of β rays. I fear all his calculations are wrong, but when I demonstrated it to him he philosophically acknowledged his error, and declared that even if the calculations did

[3] J. Danysz, "Sur les rayons β de la famille du radium," *C.R. 153* (1911), 339–341; *idem.*, "Sur les rayons β de la famille du radium," *Le Radium 9* (1911), 1–5.

[4] H.G.J. Moseley, "The Number of β-Particles Emitted in the Transformation of Radium," *Proc. Roy. Soc. A87* (1912), 230–255.

[5] E. Rutherford, "The Origin of β and γ Rays from Radioactive Substances," *Phil. Mag. 24* (1912), 453–462.

[6] *Ibid.*, p. 458.

> no longer fit the theory (which was made to suit them) he is sure the theory is right all the same.[7]

Rutherford corrected himself in a letter to the editors of the *Philosophical Magazine*, published in December 1912:

> Mr. Moseley drew my attention to the fact, which I had overlooked, that according to the Lorentz–Einstein theory the total energy of the electron is not given by the above formula but by
>
> $$E = m_0 c^2 \left(\frac{1}{\sqrt{1-\beta^2}} - 1\right)$$
>
> The latter formula agrees nearly with the former for small values of β, but departs widely from it when β approaches unity.[8]

Actually, the mistake did not undermine the theory. Rutherford was able to show that the difference between energies of successive groups could still be expressed by a relation of the same form as that given in his paper, i.e., $pE_1 + qE_2$, only new values had to be ascribed to E_1 and E_2. The values fitting best with the data were now $E_1 = 1.12$ MeV and $E_2 = 0.356$ MeV.

Rutherford's interpretation of his relation was that the same total energy is emitted during the disintegration of each atom, but that the energy is divided between beta and gamma rays in varying proportions for different atoms. Thus, if a beta particle of initial energy E_0, before escaping the atom passes through two regions where the energy required to excite a gamma ray is E_1 and E_2, respectively, its energy outside is $E_0 - (pE_1 + qE_2)$, where p and q are the numbers of gamma rays excited in the two regions.

Some sources behaved anomalously. Radium E and uranium X emitted penetrating beta rays, but relatively weak gamma radiation, and, in addition, their beta spectra were diffuse bands instead of sharp lines. Rutherford offered a possible explanation of the first problem:

> It is possible that the atomic structure of uranium X is such that only an occasional β particle loses energy by conversion into γ rays in its escape from the atom. In the case of radium E where the γ rays are very weak in intensity and of slight penetrating power, it seems probable that the β rays originate near the surface of the atom, and consequently do not traverse the regions where penetrating γ rays can be set up.[9]

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[7] Moseley to his mother, 14 October 1912, quoted in J.L. Heilbron, *H.G.J. Moseley: The Life and Letters of an English Physicist, 1887–1915* (Berkeley: University of California Press, 1974), p. 194.

[8] E. Rutherford, "On the Energy of the Groups of β Rays from Radium," *Phil. Mag. 24* (1912), 893–894, p. 893.

[9] Rutherford, "Origin" (note 5), pp. 460–461.

The question of the peculiar heterogeneity that expressed itself in the diffuse bands had to be left open.[10]

Rutherford's paper concluded with some considerations of models. Could the emission of radiation be understood on the basis of his new atomic model? This question was no doubt of great importance to Rutherford. He imagined that the instability leading to a disintegration were of two kinds, one associated with the central nucleus and one with the electronic system. Alpha emission is due to the former kind, whereas the latter is responsible for the appearance of beta and gamma rays. The electrons were assumed to be distributed in concentric rings, and the expulsion of a beta ray was then due to an instability in one of these rings. On its way out the beta particle passes through the electronic distribution external to it, and in traversing a ring of electrons it may lose a part of its energy by exciting one or more gamma rays which are then emitted with a definite energy characteristic of that ring. Some of the energy of the gamma rays may again be converted into beta rays, and that may explain the presence of groups of slow particles.

I have found only a few reactions to Rutherford's theory. Frederick Soddy, Rutherford's collaborator on the transformation theory of 1902, was sceptical.[11] He considered it to be "more or less tentative" and continued:

> At first sight ... the complexity of the β-rays and apparent simplicity of the γ-rays, taken in conjunction with the existence of β-rays of high velocity unaccompanied by γ-rays ... seems additional evidence in favour of regarding the two kinds of radiation as not necessarily causally connected.[12]

Kasimir Fajans as well as Owen Richardson found Rutherford's work interesting,[13] but at first Fajans did not believe in the nuclear model of the atom, expressing strong doubt

> ... that the whole positive charge is collected at the centre of the atom. I think that the simple relationship between the radioactive changes and the chemical properties of the elements shows conclusively that the α-particle comes not from the inner but from the surface of the atom.

[10] J.A. Gray, "The Distribution of Velocity in the β-Rays from a Radioactive Substance," *Proc. Roy. Soc. A84* (1911), 136–141.

[11] T.J. Trenn, *The Self-Splitting Atom: The History of the Rutherford–Soddy Collaboration* (London: Taylor & Francis Ltd., 1977).

[12] F. Soddy, *Annual Progress Report to the Chemical Society for 1912*, Vol. 9 (1913), pp. 289–328, on pp. 298–299; reproduced in *Radioactivity and Atomic Theory*, T.J. Trenn (ed.) (London: Taylor & Francis Ltd., 1975), pp. 300–301.

[13] Fajans to Rutherford, 10 April 1913, C.U.L. Add. MS. 7653 F5; Richardson to Rutherford, 13 June 1913, C.U.L. Add. MS. 7653 R30. See also R.H. Stuewer, "The Nuclear Electron Hypothesis," in *Otto Hahn and the Rise of Nuclear Physics*, W.R. Shea (ed.) (Dordrecht: Reidel, 1983), pp. 19–67, on p. 21.

> By use of this idea I believe I shall be able to explain the Periodic Law and the valency question in general.[14]

As is evident from a letter of 31 December 1913 to Rutherford, Niels Bohr's theory was required to convince Fajans of the justice of the nuclear atom:

> I have followed Bohr's papers with extraordinary interest, and now I no longer doubt the complete correctness of your atomic theory. The reservations I expressed in my last letter have been entirely removed by Bohr's work. There is no doubt that the solution of this fundamental problem will give rise to an exceedingly lively development in physics as well as in chemistry, and I want to express to you my sincere congratulations on this great success.[15]

A serious blow to the Rutherford theory soon emerged, however. It was, as we shall see in the next section, the hypothesis of the nuclear origin of the beta particle.

2.3 The beta particle as a nuclear constituent

In 1962, in an interview conducted by Thomas S. Kuhn, George Hevesy recalled the following conversation in Manchester between Rutherford, Bohr and himself:

> It was a Sunday afternoon. It was at the House of Rutherford. Bohr was also present. I asked Rutherford: "Alpha particles clearly come from the nucleus, no doubt. But where do the beta particles come from?" Rutherford answered, "Ask Bohr." Bohr was present. And with no difficulty answered that electrons involved in transmutation processes come from the nucleus, and all of the other electrons come from the exterior of the atom.[16]

It is difficult to say exactly when this conversation took place. Bohr published his hypothesis of the beta particle as a nuclear constituent for the first time in Part II of his great trilogy,[17] which was sent to Rutherford on 10 June 1913, but probably the idea of the nuclear origin of the beta particle already occurred to him during the year 1912. From a letter of 7 February 1913 to Hevesy it is evident that Bohr was certain of his hypothesis in early 1913. From the knowledge of the structure

[14] Fajans to Rutherford, 10 April 1913, C.U.L. Add. MS. 7653 F5. K. Fajans, "Über eine Beziehung zwischen der Art einer radioaktiven Umwandlung und dem elektrochemischen Verhalten der betreffenden Radioelemente," *Phys. Zeit. 14* (1913), 131–136; F. Soddy, "Intra-Atomic Charge," *Nature 92* (1913), 399–400.

[15] Fajans to Rutherford, 13 December 1913, C.U.L. Add. MS. 7653 F6.

[16] Interview with George Hevesy, 25 May 1962, transcript p. 3, *AHQP*.

[17] N. Bohr, "On the Constitution of Atoms and Molecules. Part II. – Systems Containing Only a Single Nucleus," *Phil. Mag. 26* (1913), 476–502. The trilogy has been reprinted, with an introduction by L. Rosenfeld: N. Bohr, *On the Constitution of Atoms and Molecules* (Copenhagen: Munksgaard, 1963). It is also reproduced in *BCW*, Vol. 2, *Work on Atomic Physics (1912–1917)*, U. Hoyer (ed.) (Amsterdam: North-Holland, 1981), pp. 161–233.

of the electronic system surrounding nuclei, he hoped for a detailed understanding of what he called chemical and physical properties of matter, but continued:

> By the notation "chemical and physical" I exclude gravitation and radioactivity, which are independent of the chemical and physical state and which, according to the point of view taken, are only dependent on the internal structure of the nuclei; while the other properties are only dependent on the system of electrons, which according to the theory in question is fully determined by the total charge on the nucleus.[18]

That radioactivity is an exceptional phenomenon was not a new idea in 1913. In October 1911, at the first Solvay conference, Marie Curie expressed the view that many physical properties depend on the peripheral structure of the atom, but not radioactivity:

> Radioactive phenomena form a world apart, without any connection with the preceding [i.e., thermal, optical, magnetic, and other phenomena]. It seems therefore that radioactive phenomena originate from a deeper region of the atom, a region inaccessible to our means of influence and probably also to our means of observation, except at the moment of atomic explosions.[19]

She did not, however, mention the nucleus as a conceivable place of origin, and apparently Rutherford, who was present at the conference, saw no reason to draw Curie's attention to that possibility. He himself was at that time convinced that alpha particles came from the nucleus but, as we have seen, in 1912 he still assumed that beta particles originate outside the nucleus.

Bohr's hypothesis was not supported merely by the considerations mentioned above. One further argument was stressed by Bohr when he submitted his paper for publication. In a letter to Rutherford of 10 June 1913, with which he enclosed the manuscript, he wrote:

> From the point of view of the theory the argument of the invariance of the angular momentum of the electrons during the expulsion of an α particle seems to me strongly in support of the hypothesis that the β-rays have their direct origin in the nucleus.[20]

His argument was as follows:

> In the first place, the spontaneous expulsion of a β-particle from the cluster of electrons surrounding the nucleus would be something quite

[18] Bohr to Hevesy, 7 February 1913, quoted in Rosenfeld's introduction (note 17), p. xxxii (minor misspellings corrected).

[19] "Discussion du rapport de M. Sommerfeld" in *La théorie du rayonnement et les quanta*, P. Langevin and M. de Broglie (eds.) (Paris: Gauthier-Villars, 1912), pp. 373–392, on p. 385. Translation taken from A. Pais, *Inward Bound: Of Matter and Forces in the Physical World* (New York: Oxford University Press, 1986), p. 223.

[20] Bohr to Rutherford, 10 June 1913, *BSC* (6.3).

> foreign to the assumed properties of the system. Further, the expulsion of an α-particle can hardly be expected to produce a lasting effect on the stability of the cluster of electrons. The effect of the expulsion will be of two different kinds. Partly the particle may collide with the bound electrons during its passing through the atom. The effect will be analogous to that produced by bombardment of atoms of other substances by α-rays and cannot be expected to give rise to a subsequent expulsion of β-rays. Partly the expulsion of the particle will involve an alteration in the configuration of the bound electrons, since the charge remaining on the nucleus is different from the original. In order to consider the latter effect let us regard a single ring of electrons rotating round a nucleus of charge Ne, and let us assume that an α-particle is expelled from the nucleus in a direction perpendicular to the plane of the ring. The expulsion of the particle will obviously not produce any alteration in the angular momentum of the electrons; and if the velocity of the α-particle is small compared with the velocity of the electrons – as it will be if we consider inner rings of an atom of high atomic weight – the ring during the expulsion will expand continuously, and after the expulsion will take the position claimed by the theory for a stable ring rotating round a nucleus of charge $(N-2)e$. The consideration of this simple case strongly indicates that the expulsion of an α-particle will not have a lasting effect on the stability of the internal rings of electrons in the residual atom.[21]

The nuclear origin of the beta particle was soon generally accepted. Rutherford proclaimed his agreement with Bohr in a letter to *Nature*, dated 6 December 1913, and typically for his experimentally based view, he considered the strongest evidence in support of the Bohr hypothesis to be, first, that physical and chemical conditions were independent of the beta-ray transformations and, second, that the energy emitted in the form of beta and gamma rays by the transformation of an atom of radium C is much greater than could be expected to be stored up in the external electronic system. He did not at all mention Bohr's theoretical invariance argument.[22]

Rutherford's first argument was well known, but where did he get his second one from? Together with Harold Robinson he had carried out some investigations on the heating effect of radium and its emanation, which were published in February 1913.[23] No conclusions about the origin of beta and gamma rays were drawn in this paper, but Rutherford's argument is probably due to that investigation.

At the end of 1913, Soddy presented what, in his own words, "amounts to a proof that the electrons expelled as β rays come from a nucleus."[24] He argued

[21] Bohr, "Constitution" (note 17), pp. 500–501.

[22] E. Rutherford, "The Structure of the Atom," *Nature 92* (1913), 423.

[23] E. Rutherford and H.R. Robinson, "Heating Effect of Radium and Its Emanation," *Phil. Mag. 25* (1913), 312–330.

[24] Soddy, "Intra-Atomic Charge" (note 14), p. 400.

that "if the β-particles came from the outer ring and were the same electrons as are concerned in electrochemical changes of valency, the loss of two β-particles by uranium-X_1 in the formation of uranium-II should be equivalent to the loss of two electrons by uranous salts in their electrochemical oxidation to uranyl salts, and uranium in uranous salts should be isotopic with thorium."[25] Uranium and thorium salts are very similar in chemical character, but recent experiments had shown that they are separable from one another by chemical methods. Thus the conclusion was that the nucleus must contain electrons – at least six in the case of uranium to account for the six beta rays expelled in its decay chain.

That the disintegration electron originates in the nucleus became evident during 1913. That does not mean, however, that all the beta rays emitted from radioactive substances necessarily come from the nucleus. Rutherford found it very likely, as he expressed in his *Nature* letter, that a considerable fraction are secondary electrons emitted because of a secondary effect resulting from the primary expulsion of a beta particle from the nucleus.

2.4 An extreme complexity of beta line-spectra is brought to light: deflection experiments in the years 1911–1913

In the previous chapter we have seen that Otto Hahn and Lise Meitner were forced to abandon their original idea about the analogy between alpha and beta spectra. From their own experiments in 1911, it became evident that, in general, beta spectra are line spectra, each consisting of rather few lines. This, however, also proved to be an oversimplification. Nature is much more complex than that, and some of this complexity was recognized in the years from 1911 to 1913. In these years, the number of lines observed in the RaB + C line spectrum increased from 9 to 64.

The reason why this extreme complexity was brought to light is easy to trace. A new experimental technique was introduced that proved to be much superior to that used by Otto von Baeyer, Hahn and Meitner. It was developed by Jean Danysz, who was probably somewhat inspired by the early deflection experiments of Henri Becquerel.[26] Danysz lived in Paris, did research on beta rays from the radium family, and finished his doctoral thesis in 1913. He was unknown in the scientific community in 1911 when he published the first results of his research; Rutherford simply referred to him as "a Frenchman" in a letter to Hahn in October 1911.[27] By 1914, however, he was considered to be one of the leading beta-spectrum researchers, and Rutherford and Stefan Meyer suggested him, as the only one

[25] F. Soddy, *Annual Progress Report to the Chemical Society for 1913*, Vol. 10 (1914), 262–288; reproduced in *Radioactivity*, Trenn (ed.) (note 12), pp. 331–358, on p. 342.

[26] For a discussion of these experiments, see Chapter 1, Section 1.2.

[27] Rutherford to Hahn, 2 October 1911, C.U.L. Add. MS. 7653 H55.

outside Berlin and Manchester, as a participant in a forthcoming congress on beta and gamma rays and their origin.[28] To my knowledge, the congress was not held. The war broke out, and Danysz became one of its casualties. He died in 1914.

Danysz's arrangement is shown in Figure 2.2. A very strong radiation source S was placed in a little capillary tube, and 1 cm above this source was a slit F,

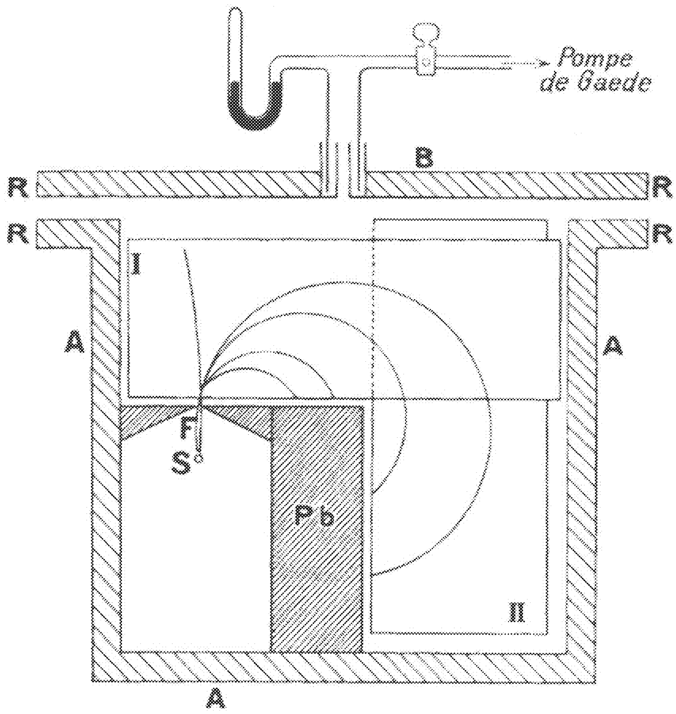

Figure 2.2: The experimental arrangement used by Danysz in his magnetic-deflection experiments. S is the source, and F the slit. In the first experiments, the photographic plates were placed vertically as I and II in the figure. Later on, however, Danysz realized that he obtained more accurate results by placing them horizontally in the plane of the slit. Source: Danysz, "Recherches expérimentales" (note 29), p. 273.

0.4 mm wide. The beta rays emitted from S and through F were then deflected by a strong magnetic field. In his first experiments Danysz, like Becquerel in 1900, put the photographic plate vertically (see I and II in the figure). In this way he obtained very clear pictures, but since beta rays not exactly perpendicular to the magnetic field influenced the plate in a disturbing way, especially near the slit, his

[28]See Meyer to Rutherford, 20 June 1914, and Rutherford to Meyer, 29 June 1914, C.U.L. Add. MS. 7653 M186.

determination of the radius of curvature, and thus the velocity of the rays, was inaccurate. That could be ameliorated, however. Danysz realized that he could avoid the disturbing effect by placing the photographic plate horizontally in the plane of the slit. The rays then approximately ran through a semicircle before hitting the plate on which a number of well-marked lines were outlined. From these lines, and by correcting for the decrease of velocity through the glass wall of the capillary tube, Danysz made very accurate velocity determinations.

The most impressive result of Danysz's investigations was, however, the number of lines. In his thesis of 1913, he presented 27 lines from RaB + C.[29] Compared to the nine lines found by Baeyer, Hahn and Meitner, Danysz revealed an extreme complexity by his new method. According to Rutherford, it paved the way for a possible explanation of the anomalous beta spectra. In the 1913 edition of his book on radioactivity he wrote:

> In the light of the results given above [Danysz's] it appears not improbable that the continuous β ray spectrum observed for uranium X and radium E may be ultimately resolved into a number of lines. It is quite possible also that the somewhat diffuse band given by thorium D may be caused by the overlapping of several groups of rays.[30]

In Manchester, researchers decided to check the work of Danysz. Together with Rutherford, the man responsible for magnetic spectroscopy, Harold Robinson, improved on Danysz's apparatus and developed his method into a highly successful one. Their arrangement appears in Figure 2.3. It very much resembles that used by Danysz, but Rutherford and Robinson saw and took advantage of the so-called semicircular-focusing effect caused by the magnetic field. As a result, homogeneous beta rays, even when they pass through a relatively wide slit, concentrate in a narrow line on a photographic plate placed horizontally in the plane of the slit. In this way groups of rays of very small energy could be detected. "The Prof. is finishing up some work on the β ray spectrum of RaB and C," Ernest Marsden wrote to Bohr in June 1913, and he continued: "He gets an enormous number of lines."[31] That was no exaggeration. No less than 64 groups from RaB + C were measured; 16 were ascribed to radium B, and 48 to radium C, and they observed an additional number of very faint, but not measurable, lines.[32]

Since it appeared possible to express the energy of the swiftest 29 groups of beta rays as an integral number of one single energy value, Rutherford and Robinson considered their result to be in fair agreement with Rutherford's 1912 theory, revealing Rutherford's belief in this theory still in July 1913.

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[29] J. Danysz, "Recherches expérimentales sur les rayons β de la famille du radium," *Ann. Chim. Phys. 30* (1913), 241–320.

[30] E. Rutherford, *Radioactive Substances and Their Radiations* (Cambridge: Cambridge University Press, 1913), p. 256.

[31] Marsden to Bohr, 12 June 1913, *BSC* (5.1).

[32] E. Rutherford and H. Robinson, "The Analysis of the β Rays from Radium B and Radium C," *Phil. Mag. 26* (1913), 717–729.

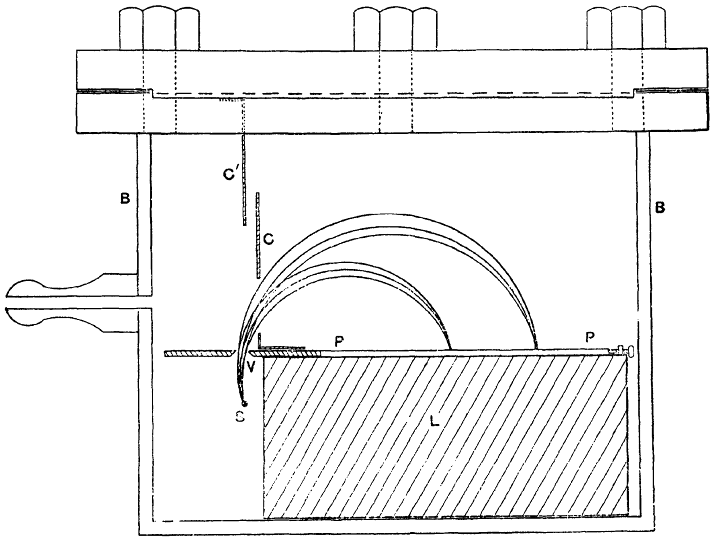

Figure 2.3: The experimental arrangement used by Rutherford and Robinson in their magnetic-deflection experiments in 1913. Like Danysz, they placed the photographic plate horizontally in the plane of the slit; but in contrast to him, they took advantage of the semicircular-focusing effect. According to this effect, electrons that begin their motion at any small angle from the normal to the plate will follow different orbits, but will all strike the photographic plate at about the same distance from the origin. As a consequence, a relatively wide slit could be used. Source: Rutherford and Robinson, "Analysis" (note 32), p. 720.

In Berlin, the trio of Baeyer, Hahn and Meitner continued their investigations on magnetic beta spectra, but contrary to their colleagues in Paris and Manchester they adhered to their old method. According to a letter of 22 November 1913 from Hahn to Rutherford, they had planned to change to the new method, but this involved difficulties, which they did not overcome before the outbreak of war. "Before your paper was out," Hahn wrote, "we had put up our mind, to use a similar apparatus as you and Danysz have used for the magnetic spectra. We hope to succeed in getting better photographs by this way. But the great trouble is the little quantity of active material, we can use; and another trouble is the difficulty of having an electromagnet strong enough to secure homogeneous magnetic fields."[33]

If the Berliners lagged behind in their method, they were more comprehensive in their investigations, however. Whereas the British stuck to radium B + C and Danysz to the radium family, the Berliners experimented with many different radioactive sources, and always with the same result – a number of more or less

[33]Hahn to Rutherford, 22 November 1913, C.U.L. Add. MS. 7653 H65.

distinct lines.[34] Even in the uranium X spectrum, which so far had not revealed any lines, they found two faint lines after 37 hours of exposure.[35]

The only spectrum behaving anomalously was thus the RaE spectrum, a real *enfant terrible* in beta-spectrum history. No trace of even a single line was observed; only a broad, diffuse band emerged on the plate. Danysz had examined a RaE source originating from radium emanation, and when the swiftly decaying products were still present he obtained a very distinct line spectrum whereas, after these products had decayed, he saw only a blurred band.[36] Baeyer concluded "that the inhomogeneity of these β rays cannot be faked by secondary influences."[37] Thus, according to Baeyer, the continuity was due to the RaE beta rays themselves, and not to electrons excited in the apparatus by gamma rays, or to other secondary electrons.

The consensus in early 1914 was, however, that beta spectra were line spectra, and apparently only the RaE spectrum was anomalous from this point of view.

2.5 Continuity as well as lines: The composite beta spectrum

In 1913, the Manchester student James Chadwick received his M.Sc. degree, and was then awarded an 1851 Exhibition Scholarship.[38] He decided to go to the Physikalisch–Technische Reichsanstalt in Berlin to do research in Hans Geiger's recently established laboratory in the *Magnetische Haus*.[39] To go to Berlin was the obvious thing to do, Chadwick said later; the only alternative was Marie Curie in Paris, and her interests were more on the chemical side.

After returning to Berlin from an eventful six-year-long stay in Manchester, Geiger had succeeded in developing the point counter, an improved form of the counting device used in his and Marsden's famous alpha-scattering experiments

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[34] In addition to radium, the Berlin group investigated the thorium group as well as the actinium group. See O. v. Baeyer, O. Hahn and L. Meitner, "Das magnetische Spektrum der β-Strahlen des Thoriums," *Phys. Zeit. 13* (1912), 264–266; and *idem.*, "Das magnetische Spektrum der β-Strahlen des Radioaktiniums und seiner Zerfallsprodukte," *Phys. Zeit. 14* (1913), 321–323.

[35] O. v. Baeyer, O. Hahn and L. Meitner , "Das magnetische Spektrum der β-Strahlen des Uran X," *Phys. Zeit. 15* (1914), 649–650.

[36] J. Danysz, "Sur les rayons β des radiums BCDE," *Le Radium 10* (1913), 4–6.

[37] O. v. Baeyer, "Bericht über die magnetischen Spektren der β-Strahlen der radioaktiven Elemente," *Jahrbuch der Radioaktivität und Elektronik 11* (1914), 66–84, p. 80.

[38] Interview with James Chadwick, 15–24 April 1969, Niels Bohr Library, American Institute of Physics, p. 17. I am grateful to Spencer R. Weart for permission to quote from the interview. See also H.S.W. Massey and N. Feather, "James Chadwick," *Biog. Mem. Fel. Roy. Soc. 22* (1976), 11–70, and Andrew Brown, *The Neutron and the Bomb: A Biography of Sir James Chadwick* (Oxford: Oxford University Press, 1997).

[39] The "magnetic house" was a rather small place in the middle of the garden of the Reichsanstalt. On account of the tram cars, it could not be used for magnetic work any longer. See the Chadwick interview (note 38) and Geiger to Rutherford, 12 October 1912, C.U.L. Add. MS. 7653 G23.

leading to the Rutherford atomic model,[40] and to his great pleasure the new counter proved to be a promising beta-particle counter as well. "The arrangement is so sensitive," he informed Rutherford in April 1913, "that also the β rays give a good effect ... . I have some hope that the arrangement will be useful later for some of the β ray problems."[41] A few months later Geiger reported to Rutherford that his counting device did beta-ray counting very well,[42] and it was thus natural for Chadwick to make use of the new point counter when he started his research work in Berlin.

Chadwick became familiar with the method very quickly,[43] and intended to observe the scattering of beta particles. The idea was to bend the particles in the usual way in a magnetic field, bombard a thin metal foil, and count the scattered particles; but to be able to say anything about the ratio of the scattered particles to the incident particles, it is also necessary to count the number of beta rays in the different groups before they enter the foil. Chadwick started on that work by using the arrangement shown in Figure 2.4, and ran into unexpected difficulties. Geiger noted them in a letter of 12 December 1913 to Rutherford:

> Chadwick is trying to determine the number of β-rays in the different groups from Ra-Emanation. He has got the counters going well and there is apparently no difficulty in that direction. But the lines don't come out sharply yet when observed with the counter, and we don't know yet what the reason is.[44]

Chadwick himself mentioned his problems in a letter of 14 January 1914 to his former teacher Rutherford:

> I have not made much progress as regards definite results. We wanted to count the β particles in the various spectrum lines of RaB + C & then to do the scattering of the strongest swift group. I get photographs very quickly & easily, but with the counter I can't find even the ghost of a line. There is probably some silly mistake somewhere.[45]

Chadwick made some progress, but observed "only 4 lines and all these belong to RaB. The rest are merely small variations in a continuous spectrum, variations of less than 10%."[46] He presented his investigation in a paper submitted in April

[40] As to these scattering experiments, see J.L. Heilbron, "The Scattering of α and β Particles and Rutherford's Atom," *AHES 4* (1967/68), 247–307.

[41] Geiger to Rutherford, 14 April 1913, C.U.L. Add. MS. 7653 G30. See also H.W. Geiger, "Über eine einfache Methode zur Zählung von α- und β-Strahlen," *Verh. d. D. Phys. Ges. 15* (1913), 534–539; and *idem.*, "Demonstration einer einfachen Methode zur Zählung von α- und β-Strahlen," *Phys. Zeit. 14* (1913), 1129.

[42] Geiger to Rutherford, 23 June 1913, C.U.L. Add. MS. 7653 G38. Rutherford was quite surprised at the success of the point counter. On 10 May 1913, he wrote to Geiger: "It is rather surprising that the pointed conductor which we tried to avoid so much should prove after all the best thing"; C.U.L. Add. MS. 7653 G32.

[43] See Geiger to Rutherford, 25 November 1913, C.U.L. Add. MS. 7653 G45.

[44] Geiger to Rutherford, 12 December 1913, C.U.L. Add. MS. 7653 G46.

[45] Chadwick to Rutherford, 14 January 1914, C.U.L. Add. MS. 7653 C21.

[46] Chadwick to Hevesy, 12 March 1914, Hevesy Scientific Correspondence, Niels Bohr Archive.

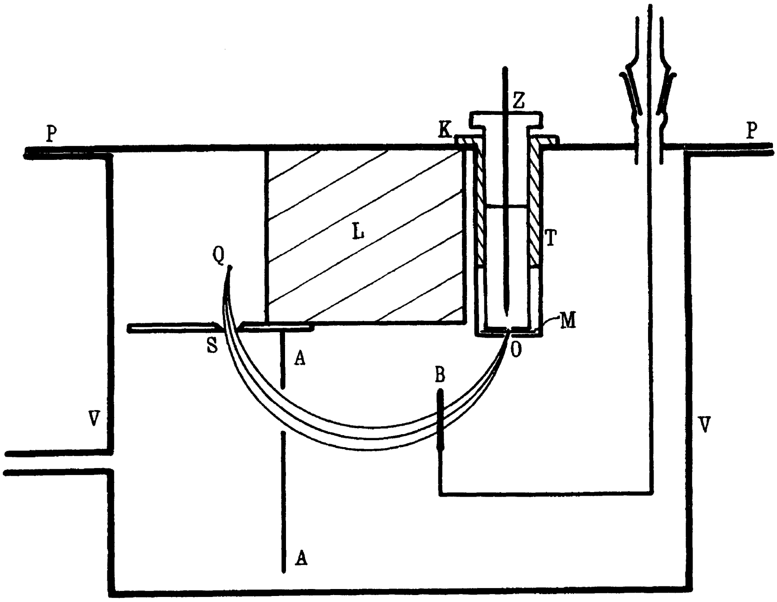

Figure 2.4: The experimental arrangement used by Chadwick in his counting experiments in Berlin, 1913–1914. To determine the number of beta particles he used Geiger's new point counter. Source: Chadwick, "Intensitätsverteilung" (note 47), p. 384.

1914 and the outcome, i.e., the RaB + C spectrum, appears from curve A in Figure 2.5.[47] To ensure that the counting experiment was reliable, Chadwick, in addition, obtained a spectrum by the ordinary ionization method (curve B in Figure 2.5), and he confirmed his result. The beta spectrum of RaB + C is essentially continuous, and a line spectrum, which with a few exceptions is relatively weak, is superimposed upon it.

This result seemed to be inconsistent with the previous photographic measurements, which showed many lines on a weak background, but according to Chadwick this inconsistency could be explained. The distinctness of the lines was a delusion, because a photographic plate is very sensitive to even a small change in intensity. To make certain that the continuous spectrum really was due to the beta particles from the source, Chadwick inserted a small lead screen (B in Figure 2.4) to stop these particles before counting. He then counted only the stray and scattered beta rays, and it appeared that they did not contribute essentially to the continuity.

[47] J. Chadwick, "Intensitätsverteilung im magnetischen Spektren der β-Strahlen von Radium B + C," *Verh. d. D. Phys. Ges. 16* (1914), 383–391.

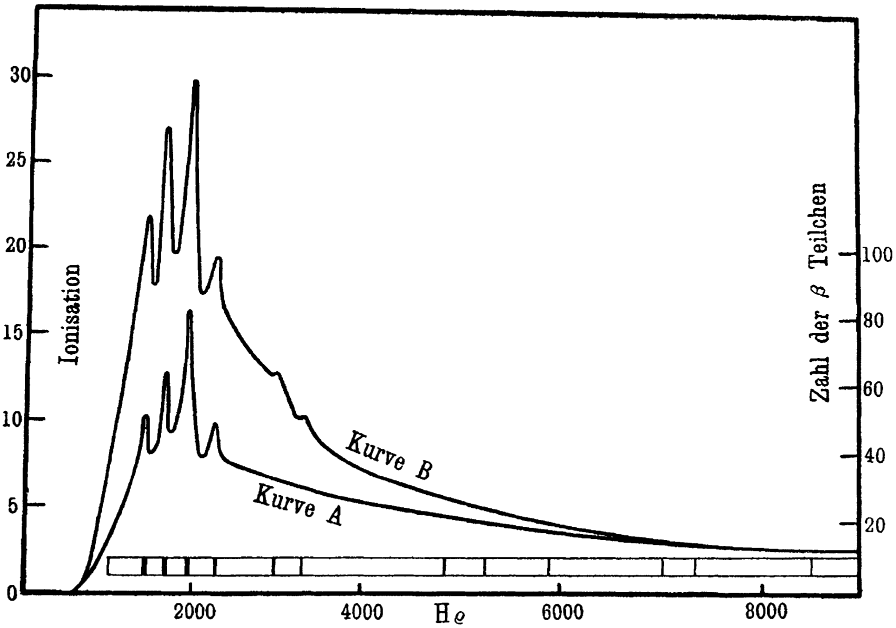

Figure 2.5: Chadwick's beta-ray spectrum of 1914. Curve A was obtained by using Geiger's point counter, and curve B by using the ordinary ionization method. The source was RaB + C. Source: Chadwick, "Intensitätsverteilung" (note 47), p. 389.

In retrospect, Chadwick's discovery of the composite beta spectrum is considered a turning point in the history of nuclear physics, and with justice. The immediate reactions to it were different, however. Even Chadwick himself was somewhat disappointed in the result. In a letter of 12 March 1914 to Hevesy he wrote:

> This [i.e., the result of his investigations] is of course interesting, but at the same time disappointing; for it means that the scattering problem will be very difficult.[48]

Rutherford's reaction is also worth mentioning. He received information about Chadwick's result from Geiger in March 1914, and answered as follows:

> I received your letter last night and was much interested in the results you tell me. I always considered it probable that there was a general β radiation on which the lines were superimposed. I think, if I remember

[48]Chadwick to Hevesy, 12 March 1914, Hevesy Scientific Correspondence, Niels Bohr Archive.

> aright, that in one of my papers I introduced a cryptic remark, "Apart from the question whether a continuous spectrum of β rays is superimposed on the line spectrum, β rays are excited in all parts of the vessel on which the γ rays fall; in addition, the rays falling on the photographic plate are partially scattered in all directions." (Rutherford and Robinson, Phil. Mag. Oct. 1913.)
>
> I did not, however, appreciate the facts which you mention, that the lines show clearly even when the radiation is a small percentage of the fraction of the continuous radiation.[49]

The mentioned sentence indeed appears in the Rutherford–Robinson paper.[50] The presence of a composite beta spectrum had thus entered Rutherford's mind before he became aware of Chadwick's result. What surprised him, however, was that the continuous spectrum was the dominant one.

Rutherford continued his letter with the following remark:

> As a matter of fact, when Robinson thought of starting the work tackled by Chadwick, I told him it would be a very difficult matter to pick out any but the stronger lines by the electrical method.[51]

It turned out that Rutherford was quite right, but his foresight on this point had the negative effect that it prevented Robinson from carrying out the experiment, and thus perhaps also from being the discoverer of the composite beta spectrum. On this occasion, Chadwick was probably fortunate in being in Berlin.

2.6 Rutherford's 1914 theory

Things had changed considerably since Rutherford's 1912 theory of the emission of beta and gamma radiation. In 1914, it was generally accepted that at least some of the beta particles have their origin in or near the nucleus, and Chadwick's experiment seemed to show that a beta spectrum is essentially continuous and that usually a line spectrum of rather low intensity is superimposed upon it.

A new theory was thus required, and again Rutherford was the founder.[52] The assumption that a high-speed beta particle is expelled from the nucleus enabled him to explain the continuous part of the spectrum:

> This β particle in passing through the outer distribution of electrons will, on the average, suffer several collisions of an ordinary type with the electrons, and will share its energy with them. As a statistical result of a large number of atoms, the velocity of the escaping β particles will, on the average, be continuously distributed within certain limits

[49] Rutherford to Geiger, 18 March 1914, C.U.L. Add. MS. 7653 G50.
[50] Rutherford and Robinson, "Analysis" (note 32), pp. 720–721.
[51] Rutherford to Geiger, 18 March 1914, C.U.L. Add. MS. 7653 G50.
[52] E. Rutherford, "The Connexion Between the β and γ Ray Spectra," *Phil. Mag. 28* (1914), 305–319.

> of velocity. This would give rise to the continuous spectrum of β rays which is most typically illustrated by the β rays from radium E.[53]

To explain the line spectrum, Rutherford imagined, as he also did in 1912, that there are certain well-defined regions in the electronic distribution that can be set into definite vibrations by the escaping beta particle. These regions were considered to be the origin of the characteristic gamma radiation, and if some of the beta particles from the nucleus pass through one or more of these regions on their way out they give rise to a line spectrum of gamma rays. Some of these gamma rays, however, may be reconverted into the beta-ray form before they escape the atom, and thus create the homogeneous groups of beta particles.

The above considerations accounted for the continuous spectrum as well as for the beta and gamma line-spectra, but why does radium E not emit gamma rays at all, or at any rate very few? Rutherford explained this anomaly by assuming that the primary beta particle from a given radioelement is always expelled in a fixed direction with respect to the structure of the atom itself. The absence of gamma rays from radium E, then, according to Rutherford, arises because the escaping beta particle does not pass through any region where characteristic gamma radiation is generated.

At first sight, this assumption seems to be purely *ad hoc*, but actually it also accounted for another observation. General evidence had indicated that radium B and radium D both have the same physical and chemical properties as lead, and thus the same electronic distributions. Since the gamma rays were assumed to originate in this electronic system, the two radioactive sources should reveal identical gamma spectra. Experiments, however, showed something quite different. Radium B and D emit gamma rays that are widely different in relative amount and penetrating power. This apparent discrepancy was understandable if the beta particles from the two elements were emitted in different directions with respect to the atomic structure.

Rutherford's new theory differed fundamentally from the one of 1912. It was, for instance, a novel assumption that the entire line spectrum should be due to conversion of gamma rays into beta rays. Rutherford had pointed to this conversion process already in 1912, but only as a possible cause of a few of the lines. What made him change his mind on this point? Together with Robinson and W.F. Rawlinson, he had carried out some experiments for the purpose of determining the distribution of velocities of beta rays excited by gamma rays.[54] Their hope was to reveal a connection between the excited beta spectra and the natural beta spectra, i.e., the spectra from the gamma sources themselves.

Their arrangement was similar to that used by Rutherford and Robinson, which is shown in Figure 2.3. The gamma source (radium emanation) was placed in a glass tube thick enough to absorb the alpha rays and the low-velocity beta

[53] *Ibid.*, p. 308.

[54] E. Rutherford, H. Robinson and W.F. Rawlinson, "Spectrum of the β Rays Excited by γ Rays," *Phil. Mag. 28* (1914), 281–286.

rays, and this glass tube was surrounded by a layer of the absorbing substance. The swifter beta rays from the source are capable of penetrating the matter, but they lose their homogeneity and become undetectable against the continuous background. Thus, the only detectable beta rays were those excited in the sheet by the gamma rays. Fairly broad bands with sharp outside edges emerged on the photographic plate. These edges consisted of beta rays excited at the outer surface of the absorbing screen, i.e., those with no loss of velocity, and were thus comparable to the beta lines from the gamma source itself.

Experiments with lead as the absorbing material were of special interest, since it was assumed that radium B and lead had the same nuclear charge and the same external distribution of electrons, i.e., that they were isotopic. If the beta line-spectrum from radium B and the beta spectrum excited in lead proved to be identical, it could then be inferred that the radium B line-spectrum results from a conversion of gamma rays into beta rays. They found that "the stronger groups of excited β rays corresponded very nearly in position with the primary β-ray spectrum of radium B,"[55] and Rutherford did not hesitate to draw the conclusion he had hoped for.

Measurements were made with different metals to determine whether the velocity of the excited beta rays depends on the material. No certain difference was observed with silver, whereas the velocities of a number of rays from gold were distinctly higher than those of the rays from lead.

The investigation was in some respect preliminary, and Robinson and Rawlinson contemplated continuing the experiments. The war spoiled these plans, but the experiment was taken up again after the war, as we shall see in the next chapter.

2.7 The Bohr–Sommerfeld quantum conditions and the beta line-spectrum

So far, the application of quantum theory to radioactive phenomena was restricted to using the relation $E = h\nu$; but in 1916, while detained as an enemy alien in Munich, the Polish physicist Paul S. Epstein tried to apply the Bohr–Sommerfeld quantum conditions to explain the beta line-spectra from radioactive sources.[56] Arnold Sommerfeld's successful theory of the hydrogen atom and the X-ray spectra, and his own and Karl Schwarzschild's accounts of the Stark effect, encouraged Epstein to go further. "... having found what I thought was a general rule, I

[55] *Ibid.*, p. 285.

[56] P.S. Epstein, "Über den lichtelektrischen Effekt und die β-Strahlung radioaktiver Substanzen," *Phys. Zeit. 17* (1916), 313–316; *idem.*, "Versuch einer Anwendung der Quantenlehre auf die Theorie des lichtelektrischen Effekts und der β-Strahlung radioaktiver Substanzen," *Ann. d. Phys. 50* (1916), 815–840.

Calculated values		Measured by			
		Danysz (RaB,C)	Rutherford & Robinson RaB	RaC	Baeyer, Hahn, Meitner (RaC)
Z	$\beta = v/c$	β	β	β	β
92	0.747	0.753	0.751	0.750	0.74
91	0.732	0.733	0.731	–	–
90	0.718	0.722	0.719	–	–
89	0.704	0.706	0.700	–	0.69
88	–	–	–	–	–
87	0.677	0.682	–	0.675	–
86	0.664	0.660	–	–	–
85	0.652	–	0.656	–	–
84	0.640	0.642	0.635	0.648	0.63
83	0.628	–	–	0.632	–
82	0.616	–	–	–	–

Table 2.1: With $n = 1$ and $n' = 0$ in the Sommerfeld quantum conditions, Epstein calculated the above values of $\beta = v/c$ for the cited elements. As seen, by choosing $Z = 82$ to $Z = 92$, he obtained a fair agreement with the experimental values of v/c for the beta rays from RaB and RaC. Source: Epstein, "Versuch einer Anwendung" (note 56), p. 835.

considered it my duty to apply it to as many things as I could," he said in an interview many years later.[57]

Epstein's basic question was whether, in addition to Sommerfeld's ellipses, there exist quantized hyperbolic orbits, i.e., quantized orbits of positive energy. If so, electrons transferred to these orbits would move away from the atom, appear outside as cathode rays or beta rays, and, it was conjectured, in the case of beta rays form the line spectra. Epstein agreed with Bohr that the beta particle may well originally come from the nucleus, and for some reason then be transferred to a hyperbolic orbit.

By making use of the quantum conditions

$$\int\!\!\int dp_\phi d\phi = nh \quad \text{and} \quad \int\!\!\int dp_r dr = n'h,$$

where r and ϕ are polar coordinates, and by choosing the initial value of p_r that led to a finite integral, Epstein obtained an affirmative answer to his basic question; such hyperbolic orbits do exist. For different sets of quantum numbers, he calculated the velocities of the electrons, and for each set he obtained separate groups

[57] Interview with P.S. Epstein, 25 May 1962, transcript p. 14, *AHQP*.

of values that he compared with the measured ones. As an example, the calculated group for $n = 1$ and $n' = 0$, together with the corresponding experimental values, is shown in Table 2.1.

By interpreting the different lines as originating from different radioactive elements, instead of following the general opinion that they all are emitted from RaB + C, Epstein obtained a fair agreement. He was not able to explain the swifter beta rays, yet found the theory to agree remarkably well with experiment.[58] The agreement was there, but it certainly rested on a controversial interpretation of the beta line-spectrum.

Compared to the optimistic view Epstein expressed in his paper, his answer in an interview of 1962 to the question of whether many people thought that this was the way to attack the beta-decay problem is surprising:

> Well, some people did. You see by that time I didn't believe it myself already. [Koenigsberger] in Freiburg took it quite seriously, but I wrote him that I was not quite sure of it.[59]

Why, even though the quantum conditions were a great success at that time, Epstein did not believe in them is not clear; but perhaps his memory is correct. Epstein's idea was never followed up, either by himself or by anybody else.

	Values of s for			
k	$\rho = 10^{-12}$	0.8×10^{-12}	0.6×10^{-12}	0.4×10^{-12}
0.9	144	114	86	57
0.8	125	100	75	50
0.7	111	89	67	44
0.6	100	80	60	40
0.5	91	72	54	36
0.4	82	66	49	33
0.3	75	60	45	30

Table 2.2: Wolff's values for the number s of electrons in the nucleus besides the beta particle for different values of the nuclear radius ρ and the constant k. Source: Wolff, "Theoretische Betrachtungen" (note 60), p. 640.

The swifter beta rays, which could not be handled by Epstein, attracted the attention of the Dresden physicist Hans T. Wolff. In 1915, he presented an attempt to explain their origin, and he elaborated his idea in a paper of 1917.[60] Wolff imagined that the nucleus of a beta-radioactive element consists of a positive

[58] Epstein, "Versuch einer Anwendung" (note 56), p. 840.

[59] Epstein interview 25 May 1962, *AHQP*, p. 14.

[60] H.T. Wolff, "Zur Theorie der β-Strahlen," *Phys. Zeit. 16* (1915), 416–419; *idem.*, "Theoretische Betrachtungen über den Ursprung der schnellsten β-Strahlen," *Ann. d. Phys. 52* (1917), 631–648.

central charge surrounded by s electrons moving in a circle of radius ρ with a velocity very close to that of light, and that one additional electron, the beta particle, moves with roughly the same velocity in a smaller circle of radius $k\rho$, where $k < 1$. For reasons that were not discussed, the beta particle is then expelled from the nucleus, and by assuming a velocity of $0.998c$ outside the atom (the highest measured velocity in the RaB + C spectrum), Wolff calculated the related values of k, ρ and s shown in Table 2.2. Only $\rho \leq 10^{-12}$ cm was considered since, according to Rutherford, the radius of a heavy nucleus did not exceed that value.

Did these values of s fit with the actual number of electrons in a nucleus? As an example, Wolff considered radium B of atomic number 82. Its atomic weight was supposed to be 214. A RaB nucleus then consists of either 53 He + 2 H, 2 He + 206 H nuclei, or a combination of these two extremes, causing s to satisfy the inequality

$$25 \leq s \leq 127 \text{ (for RaB)}.$$

All of the values in Table 2.2, apart from one, satisfy this inequality.[61]

Wolff also showed that Bohr's angular-momentum quantum condition,

$$\ell = \frac{\tau h}{2\pi},$$

could be applied to the beta particle in equilibrium in the nucleus, though only with $\tau = 1$. He then obtained the following values of s and k:

$$\begin{array}{ccccccccc} k & = & 0.8 & 0.7 & 0.6 & 0.5 & 0.4 & 0.3 & 0.0 \\ s & = & 160 & 104 & 82 & 71 & 64 & 60 & 55 \end{array}$$

Finally, he made calculations showing that $k > 1$ was also a possibility. Yet, in this case it was not possible to apply Bohr's quantum condition.

Wolff pursued his speculations on nuclear structure,[62] but in common with other more or less speculative nuclear models put forward during the war years, they did not receive much attention.[63]

2.8 Rutherford and the gamma rays

To obtain a comprehensive knowledge of the development in understanding the beta spectrum, it is essential also to devote some time to the work and views on gamma rays. I shall not go into detail, and only concentrate on what was of greatest importance for the post-war research on beta rays.

Just as in the work on beta rays, Rutherford was a main figure in gamma-ray research in the years 1911–1919. We have discussed his theories of the emission

[61] *Ibid.*, p. 641.

[62] H.T. Wolff, "Atomkern und α-Strahlung," *Ann. d. Phys. 60* (1919), 685–701; *idem.*, "Zur Theorie der primären β-Strahlen," *Phys. Zeit. 25* (1924), 348–352.

[63] Stuewer, "Nuclear Electron Hypothesis" (note 13), p. 22.

of beta and gamma rays, and how he considered these two sorts of radiation to be closely connected. In the pre-war period, it seemed to be beyond doubt that the gamma rays originate in the external electronic distribution, and that they are analogous to X rays. This assumed analogy between X rays and cathode rays on the one hand, and gamma rays and beta rays on the other, was seemingly supported by several experimental results and became a strong guiding principle that led as well as misled Rutherford in his theoretical considerations.

One of the conclusions that arose in the wake of this assumed analogy became of great importance to the post-war discussion of the order of emission of beta and gamma rays. Together with Edward N. da Costa Andrade, Rutherford started in 1914 an analysis of the gamma radiation from radium B and C by the recently developed X-ray diffraction apparatus, which proved to be much superior to the previously used absorption method.[64] The soft radiation from RaB gave a well-defined spectrum containing two strong lines for diffraction angles of about 10 and 12 degrees. By extrapolating from Moseley's newly published law,[65] Rutherford and Andrade found that these lines fit well with the L spectrum for an element of atomic number 82, in perfect agreement with the deduction of Fajans and Soddy that radium B and lead are isotopic.[66]

This result attracted much interest. Bohr wrote to Hevesy in May 1914:

> I am sure that you have seen Rutherford's most important result in the last Phil. Mag. about the identity of the γ-Rays from radium B and the characteristic Röntgenrays from lead.[67]

The difficulty of the experiments increased rapidly with increasing radiation frequency, but Rutherford and Andrade succeeded in observing a strong radium B line in fair agreement with the strong line of the K series of lead. The radium C and thorium B spectra caused problems, however. In both cases, a line of a frequency too high to be ascribed to the K series was observed, but this did not

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[64] E. Rutherford and E.N. da C. Andrade, "The Wavelength of the Soft γ Rays from Radium B," *Phil. Mag. 27* (1914), 854–868; *idem.*, "The Spectrum of the Penetrating γ Rays from Radium B and Radium C," *Phil. Mag. 28* (1914), 263–273. Before these experiments, Rutherford and Richardson had carried out several absorption experiments with gamma rays; see E. Rutherford and O.W. Richardson, "The Analysis of the γ Rays from Radium B and Radium C," *Phil. Mag. 25* (1913), 722–734; *idem.*, "The Analysis of the γ Rays from Radium D and Radium E," *Phil. Mag. 26* (1913), 324–332; *idem.*, "Analysis of the γ Rays of the Thorium and Actinium Products," *Phil. Mag. 26* (1913), 937–948.

[65] Moseley's law states that "the frequency of any line in the X-ray spectrum is approximately proportional to $A(N-b)^2$, where A and b are constants." N is the atomic number. See H.G.J. Moseley, "The High-Frequency Spectra of the Elements," *Phil. Mag. 26* (1913), 1024–1034; and "... Part II," *Phil. Mag. 27* (1914), 703–713.

[66] For a history of the development of the concept of isotopy, and for references, see M. Malley, *From Hyperphosphorescence to Nuclear Decay: A History of the Early Years of Radioactivity, 1896–1914* (Ph.D. thesis, University of California, Berkeley, 1976). See also G. Bruzzaniti and N. Robotti, "The Affirmation of the Concept of Isotopy and the Birth of Mass Spectography", *Archives Internationales d'Histoire des Sciences 39* (1989), 309–334.

[67] Bohr to Hevesy, 25 May 1914, *BSC* (3.3).

call into question the analogy between X rays and gamma rays. Rutherford and Andrade drew a quite different conclusion:

> In other words, it is possible, at any rate in heavy elements, to obtain a line spectrum which is of still higher frequency than the '*K*' type. This may for convenience be named the '*H*' series, for no doubt evidence of a similar radiation will be found in other elements when bombarded by high speed cathode rays.[68]

With the advantage of hindsight, the assumed analogy is not surprising, since what Rutherford and Andrade actually observed were in fact X rays from the radioactive source – X rays emitted in the wake of the internal-conversion process causing the beta line-spectrum. They were only able to observe a small part of the gamma radiation. More than 95 percent of the gamma rays of radium C lay outside the range of analysis of the method they were using. Another failure, of greater importance, will be discussed in the next chapter.

In the above interpretation of the experimental results, Rutherford was led astray by his analogy, but in other cases it contributed to much progress. Thus, a comparison with the nature of X rays made Rutherford propose the possibility of conversion of the energy of a gamma ray into beta-ray energy. "This point has been emphasized by Bragg in his papers on the nature of X rays," he wrote in 1914, and continued: "He supposed that the energy of a single X ray could be converted by its passage through matter into the energy of a single β ray of appropriate speed, and that no loss of energy occurred in the process."[69]

To explain the complicated beta line-spectra, it was necessary to consider the gamma radiation originating in a certain region as a train of waves or quanta of a certain frequency. A number of these energy quanta, occasionally the whole train, might then be transferred to a beta particle. "I am now quite clear that some of the γ rays consist of a train of 20 waves, each of the same energy," Rutherford wrote to Bohr in February 1913.[70]

The "train" interpretation of gamma radiation was abandoned during the war. In spite of his war duties, Rutherford managed to carry out some experiments on absorption of rays from a newly developed high-voltage X-ray tube, and he found that, even for a voltage of about 200 kV – corresponding to a still shorter wavelength than those observed together with Andrade – the mass absorption coefficient was about 20 times that of penetrating gamma rays. The conclusion was obvious: "[t]he wave-length of the main gamma rays is much shorter than was previously supposed."[71] Gamma rays from radium C proved to correspond mainly to waves generated by voltages between 600 and 2000 kV, exactly the

[68] Rutherford and Andrade, "Spectrum" (note 64), p. 272.
[69] Rutherford, "Connexion" (note 52), p. 307.
[70] Rutherford to Bohr, 24 February 1913, *BSC* (6.3).
[71] E. Rutherford, "Penetrating Power of the X Radiation from a Coolidge Tube," *Phil. Mag.* *34* (1917), 153–162, p. 160.

energy interval in which most of the beta-ray groups are found. This agreement made Rutherford abandon his idea of trains of gamma rays:

> The results as a whole suggest that the groups of β rays are due to the transformation of the gamma rays in *single* and not *multiple* quanta, according to the relation $E = h\nu$.[72]

At the same time, a new way of investigating gamma rays was opened up:

> If the single quantum relation should prove to hold generally for the conversion of γ rays into β rays, the magnetic spectrum of β rays should afford a reliable method of extending the investigation of X-ray spectra into the region of very short waves where the crystal method either breaks down or is practically ineffective, and thus places in our hands a new and powerful method of analysing waves of the highest obtainable frequency.[73]

Rutherford had thus paved the way for fruitful post-war researches on beta and gamma rays, and, not surprisingly, one of the main tasks in Cambridge in the early 1920s was to test Rutherford's new idea.

[72] *Ibid.*, p. 162.
[73] *Ibid.*, p. 162.

Chapter 3

The Rise of a Controversy: Ellis, Meitner and Smekal Advance Different Beta-Spectrum Theories, 1920–1922

3.1 Introduction

When the Great War ended, a new era in the history of the beta spectrum began. During wartime, Ernest Rutherford had returned to his *grande passion*, the alpha rays, and his first great achievement after his return was his famous discovery of artificial disintegration, published in 1919.[1] The post-war study of the beta rays was undertaken by the young Charles Drummond Ellis, who during the 1920s developed into a world authority on the nature and behavior of beta and gamma rays.

Until the war, nothing indicated that Ellis, born on 11 August 1895, was going to be a distinguished physicist. He had chosen an army career in the Royal Engineers, and on 13 September 1913 he entered the Royal Military Academy in Woolwich as a cadet. In July 1914, however, he decided to go to Germany on a holiday and to improve his German, and was trapped by a sudden internment order. He was sent to an internment camp for civilians in Ruhleben on the outskirts of Berlin, where he had to stay for four years. By a fortunate coincidence James Chadwick, who also failed to get out of Germany before the war broke out, was interned in the same camp. Though under primitive circumstances, Chadwick formed a scientific society in the camp, excited Ellis's interest in physics, and together they did some necessarily rather crude research work.

In 1919, back in England, Ellis secured a place for himself in Trinity College, Cambridge. Under the immediate post-war relaxation of rules, he was able to take Part I of the Tripos in two terms with a first class in mathematics. A year later,

[1] E. Rutherford, "Collision of α Particles with Light Atoms. IV. An Anomalous Effect in Nitrogen," *Phil. Mag. 37* (1919), 581–587.

C. Jensen et al. (eds.), *Controversy and Consensus: Nuclear Beta Decay 1911-1934*

he took Part II, with a first class in physics, and received the B.A. degree. He was then awarded a College Graduate Scholarship and started his research career in the Cavendish Laboratory.[2] In 1921, he published his first paper, to which we shall return in the next section.

In Berlin, Lise Meitner became the leading figure in beta-ray research in the 1920s. During most of the war, she volunteered as a *Röntgenologin* in the Austrian army, but from 1917 she was occupied with the establishment of a *Physikalisch-Radioaktiven* department at the Kaiser Wilhelm Institute for Chemistry in Berlin-Dahlem.[3] She was head of the department until she was forced to leave Germany in 1938, and during the 1920s she developed it into a famous and highly respected center of experimental beta-ray and gamma-ray research.

A third person, the Austrian Adolf Smekal, took part in the discussion of beta rays. Contrary to Ellis and Meitner, he was a theorist, one of the few concerned with the beta spectrum in the 1920s. Before he turned to beta-ray problems, he had contributed much to the understanding of the fine structure of X-ray spectra, but he achieved widest recognition for his theoretical prediction in 1923 of the Raman effect, which, in German-language texts, is often referred to as the Smekal–Raman effect. He worked in Vienna until 1928, when he took up the post of professor at the University of Halle in Germany.[4]

Both Meitner and Ellis began their post-war research on beta rays by proposing, independently, an interpretation of the beta line-spectrum, and shortly afterwards they were joined by Smekal. As we shall see, they addressed the problem in different ways, however.

3.2 Internal conversion, nuclear levels, and Ellis's interpretation of the beta line-spectrum

On Rutherford's suggestion, Ellis started his career by taking up again the experiment of the three Rs – Rutherford, Harold Robinson and W.F. Rawlinson. The war interrupted their promising investigation, but they had succeeded in showing that gamma rays from radium B and radium C, when passing through heavy metals such as gold and lead, cause the emission of several groups of electrons and, more importantly, that the general structure of this excited spectrum is quite similar to that of the natural line-spectrum, i.e., the beta-ray spectrum emitted by the gamma source itself or one of its isotopes. In addition, they had observed that the electrons from gold had a velocity 1 to 2 percent higher than those from lead.

[2]For further biographical information, see K. Hutchison, J.A. Gray and H.S.W. Massey, "Charles Drummond Ellis," *Biog. Mem. Fel. Roy. Soc. 27* (1981), 198–233, and Allan R. Mackintosh, "The Third Man: Charles Drummond Ellis, 1893–1980," *Notes and Records of the Royal Society 49* (1995), 277–293.

[3]For further biographical information, see O.R. Frisch, "Lise Meitner," *Biog. Mem. Fel. Roy. Soc. 16* (1970), 405–420; R.L. Sime, *Lise Meitner: A Life in Physics* (University of California Press, 1996).

[4]J. Wagner, "Adolph Gustav Smekal," *Physikalische Blätter 15* (1959), 270.

Figure 3.1: Charles Drummond Ellis in the 1920s. [Cambridge University Library]

Ellis aimed at an experimental confirmation of the assumption that:

> The energy of the emitted electron is equal to some energy characteristic only of the γ-ray minus the energy necessary to remove the electron from the atom. The difference in the energies of the electrons from gold and lead by the same γ-rays is then explained by the difference in the work of removal of these electrons from their respective atoms.[5]

This amounts to saying that Albert Einstein's photoelectric relationship is valid. In Ellis's notation:

$$E = W_n - W_a,$$

where E is the energy of the ejected beta particle, W_n the energy of the nth gamma ray, and W_a an energy characteristic of the atom, interpreted by Ellis as the energy necessary to remove the electron from its position inside the atom to its surface.

If Ellis's assumption were correct, it could be inferred from the apparent identity of the excited spectrum and the natural line-spectrum that the latter spectrum is also due to a photoelectric effect. Rutherford had advanced the idea of an internal photoelectric effect already in 1917, and he now looked forward to having it confirmed experimentally by Ellis.

Metal Atomic No.	Barium 56	Tungsten 74	Platinum 78	Lead 82	Uranium 92
Energies	–	1.66	1.58	1.49	1.22
in volts	2.53	2.20	2.12	2.03	1.74
10^{-5}	–	2.76	2.69	2.60	2.31

Table 3.1: Ellis's energy values for the main spectral lines in the excited beta line-spectra of Ba, W, Pt, Pb and U. Source: Ellis, "Magnetic Spectrum" (note 5), p. 264.

By using a method nearly identical to that of Rutherford, Robinson and Rawlinson, Ellis investigated the excited beta line-spectra from the five elements barium, tungsten, platinum, lead and uranium (Ba, W, Pt, Pb, U). With each of these elements, apart from Ba, three main spectral lines were found. The energies of these three lines are shown in Table 3.1. It appears that the three tungsten lines all possess about 8,000 volts higher energy than the platinum lines, and the same difference is seen between the lines of platinum and lead. "It is clear from this," Ellis pointed out, "that there are three distinct γ-lines acting, each being responsible for one line";[6] and from the magnitude of the change of W_a from atom to atom he concluded that the electrons giving these lines must come from the K rings.

[5] C.D. Ellis, "The Magnetic Spectrum of the β-Rays Excited by γ-Rays," *Proc. Roy. Soc. A99* (1921), 261–271, p. 261.
[6] *Ibid.*, p. 264.

Barium	Tungsten	Platinum	Lead	Uranium
–	2.35	2.36	2.38	2.40
2.90	2.89	2.91	2.92	2.92
–	3.46	3.46	3.49	3.48

Table 3.2: The energies of the three gamma rays responsible for the three main lines in the excited beta spectra. They were calculated by adding, for each element, the energy necessary to remove the electron from the K ring to the spectral-line energies. Source: Ellis, "Magnetic Spectrum" (note 5), p. 265.

When Ellis added the energy necessary to remove an electron from the K ring to each of the energies of the ejected electrons, he obtained the values in Table 3.2. In Ellis's words, "these results show definitely that the main β-ray emission under these conditions is due to three main γ-rays, and that the β-rays emitted come from the K rings."[7] So far, the investigation had fully confirmed his and Rutherford's assumption.

Convinced of the validity of the photoelectric-effect relationship, and thus of the existence of an internal conversion of gamma rays into beta rays, Ellis turned to the beta spectrum of radium B. He showed that all of the swifter lines, strong as well as faint, could be accounted for by supposing the existence of three additional gamma lines, i.e., six in all, each characterized by a definite energy, and that each of these gamma rays ejected electrons from one of two definite levels in the atom. The first of these was the K ring, and the second was supposed to be the Λ' level, i.e., the lower sublevel of the L group.[8]

Possibly as a result of a too strong admiration for his great teacher, Ellis considered his result in complete agreement with Rutherford's 1914 theory:

> Since radium B and lead are isotopic, and as has just been shown, the emission is due to the ejection of electrons from the electronic system of the atom, this is in complete agreement with Rutherford's theory of the connection between the β-ray and γ-ray emission of radioactive atoms. According to this view the primary phenomenon is the emission of a β-particle from the nucleus. It may happen, through some mechanism at present unknown, that this β-particle gives rise to a γ-ray. This γ-ray in traversing the electronic system of the atom may be absorbed and eject a high speed electron. It is these last electrons which constitute the β-ray line spectrum.[9]

Nevertheless, the agreement was not complete. According to this theory, the gamma rays originated in well-defined regions in the electronic distribution. If

[7] *Ibid.*, p. 265.

[8] J.L. Heilbron, "The Kossel–Sommerfeld Theory and the Ring Atom," *Isis 58* (1967), 450–485.

[9] Ellis, "Magnetic Spectrum" (note 5), pp. 265–266.

so, the end state of a transition had to be very deep in the atom, and definitely inside the K ring. Rutherford and Edward N. da C. Andrade had pointed to the possibility of such states in 1914, but their existence had been ruled out by some recent work of Chadwick.[10] Chadwick had investigated the field of force between the nucleus and the K ring in heavy atoms by observing the deflection of alpha particles passing through the region, and his result proved that no electrons are present there. The gamma rays must have their origin in the nucleus, and no doubt Rutherford now agreed with that view.[11]

The origin of the gamma rays, however, was not discussed by Ellis until his second paper, published in early 1922.[12] In his first paper, he had concentrated on the photoelectric-effect interpretation of the beta-ray line-spectrum. He succeeded in accounting for eleven of the sixteen beta-ray lines observed by Rutherford and Robinson in the RaB spectrum, and that was considered a most satisfactory result. The remaining five lines might well be due to internal conversion of characteristic X-ray quanta. Ellis was thus convinced that the *entire* beta line-spectrum was due to an internal-conversion process.

The regularities in the intensities of the beta lines were another outcome of great interest. In all of the cases examined by Ellis, the most firmly bound electrons gave the most intense lines, which suggested "that the chance of an electron absorbing γ-radiation depends on the relations between the energy of the binding of the electron and the energy characteristic of the γ-ray, and that the probability of absorption becomes very small when the energy of the binding is small compared to the γ-ray energy."[13]

Ellis's second paper was primarily concerned with gamma rays. The demonstration of the validity of an internal photoelectric effect was a great step forward in the understanding of beta spectra, but according to Ellis it was "of far greater importance ... that these experiments give a method of finding the wavelengths of γ-rays."[14] This method was also one of Rutherford's visions of 1917. Up to then, the crystal method had been used, but its short wavelength limit of 0.07 Å seemed to be an impossible limitation, and many gamma rays are of much shorter wavelengths.

The previous investigation had made it probable that radium B emits six gamma rays, and Ellis calculated their wavelengths by using the new method. His results are shown in Table 3.3. The question now was whether these gamma rays could contribute to the knowledge of the structure of the nucleus. In 1917, Rutherford had indicated the existence of numerical relations between the frequencies of the gamma rays:

[10]J. Chadwick, "The Charge on the Atomic Nucleus and the Law of Force," *Phil. Mag. 40* (1920), 734–746.

[11]Rutherford's attitude to the origin of gamma rays probably changed in 1916–1917 when he abandoned his "train" waves. See Chapter 2, Section 2.8.

[12]C.D. Ellis, "β-Spectra and their Meaning," *Proc. Roy. Soc. A101* (1922), 1–17.

[13]Ellis, "Magnetic Spectrum" (note 5), p. 270.

[14]Ellis, "β-Spectra" (note 12), p. 1.

Energies in volts	λ in Å
4.000×10^5	0.0308
3.639	0.0339
3.492	0.0354
2.918	0.0423
2.529	0.0488
2.385	0.0519

Table 3.3: Ellis's values for the energies and wavelengths of RaB's six gamma rays. These six gamma rays were necessary to explain the essential part of RaB's beta line-spectrum. Source: Ellis, "Magnetic Spectrum" (note 5), p. 267.

> The multiple relations observed between the energy of some of the groups of β-rays must ... indicate approximate multiple relations between the frequencies of the gamma rays.[15]

And once again Ellis found what Rutherford had expected, this time a simple arithmetical connection between the energies of the radium B gamma lines. Each of the first three proved to have an energy that was 1.1×10^5 eV higher than each of the last three.

This arithmetical relationship encouraged Ellis to propose the energy-level diagram of the RaB nucleus shown in Figure 3.2. For comparison, the modern level diagram of ^{214}Bi, together with the transitions corresponding to the eight strongest gamma lines, is shown in Figure 3.3.

As a necessary consequence of the existence of the levels in Figure 3.2, there should also be gamma rays corresponding to transitions 5 to 4, 5 to 3, 4 to 3, and 2 to 1. Such gamma rays were actually observed by Rutherford and Andrade in 1914; their agreement with the diagram satisfied Ellis "and lends support to the view that part at least of the structure of the nucleus can be expressed in terms of stationary states."[16]

The information contained in the level diagram did not satisfy the British propensity for models and *Anschaulichkeit*, however, and Ellis was quite aware of that. "There is no evidence," he wrote, "which indicates whether these levels are occupied by positively charged particles or by electrons, nor is it possible to associate level 5 or level 1 with any special part of the nucleus."[17] Yet he presented a picture of the beta-transformation process:

> The radium B nucleus comes into existence after the α-particle has left the radium A nucleus, but one would not anticipate that all the radium B nuclei were absolutely identical. The particle which does the

[15] E. Rutherford, "Penetrating Power of the X Radiation from a Coolidge Tube," *Phil. Mag.* *34* (1917), 153–162, p. 162.
[16] Ellis, "β-Spectra" (note 12), p. 14.
[17] *Ibid.*, p. 15.

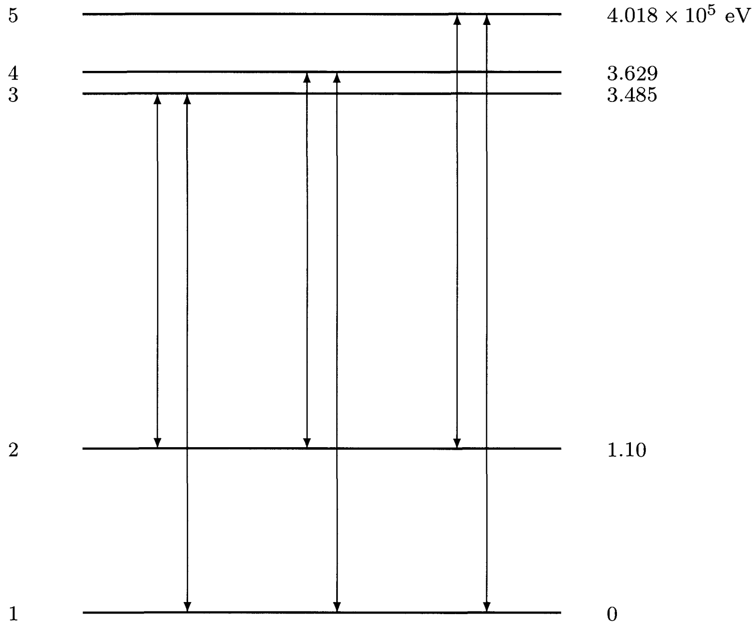

Figure 3.2: Ellis's energy-level diagram of the RaB nucleus, and the transitions corresponding to the six gamma rays shown in Table 3.3. Source: Ellis, "β-Spectra" (note 12), p. 14.

> transitions between the levels of [Figure 3.2] will be in one of these levels, but it need not be in the same level in each nucleus. Subsequently, during the life of the nucleus this particle passes to another energy level with the emission of a γ-ray. Finally, the particle will arrive in one of the states which is connected with instability, and the nucleus disintegrates with emission of an electron.[18]

Ellis thus held the view that gamma rays are emitted *before* the beta disintegration. This view was essentially brought about by the pre-war investigation of Rutherford and Andrade, which had shown that radium B emits the complete K and L spectrum of a body of atomic number 82, and this suggested strongly that the internal-conversion process takes place before the disintegration process. It could not be asserted definitely, of course. There might well be a time of relax-

[18] *Ibid.*, p. 16.

ation so that the rings of the nucleus of atomic number 82 persist for a certain time after the nuclear charge has changed to 83; but Ellis considered his view to be the simplest. It did not require an additional hypothesis of a time of relaxation, and it was *a priori* as probable as the other view.

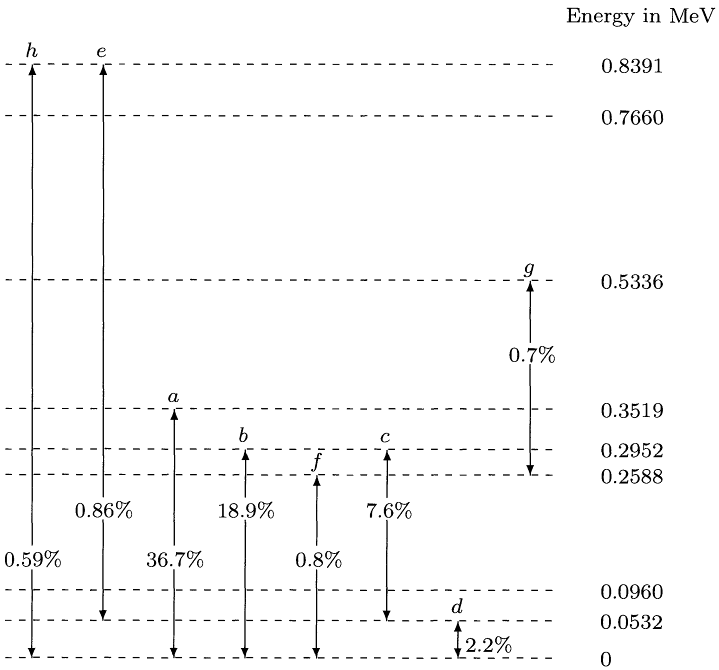

Figure 3.3: A modern energy-level diagram of ^{214}Bi with the transitions corresponding to the γ lines of intensities above 0.5 percent. The intensities of the lines are given in percentages in the diagram, and their energies in MeV are as follows: *a*, 0.3520; *b*, 0.2952; *c*, 0.2419; *d*, 0.0532; *e*, 0.7860; *f*, 0.2588; *g*, 0.2748; *h*, 0.8392. The old RaB diagram has to be compared with ^{214}Bi, and not with ^{214}Pb, since the gamma rays are emitted from the daughter nucleus and not, as it was assumed in 1922, from the parent nucleus. Source: C.M. Lederer and V.S. Shirley (eds.), *Table of Isotopes*, Seventh Edition (New York: Wiley, 1978), pp. 1365–1366.

Ellis's first work on beta-ray and gamma-ray spectra, presented only about two years after his matriculation at Trinity College, was of great importance. He convincingly demonstrated internal conversion, introduced nuclear energy levels, and offered an interpretation of the beta line-spectra; but there is no denying Rutherford's great influence. Rutherford's ideas, put forward in 1917, of an internal photoelectric effect and multiple relations between the energies of the gamma rays guided Ellis strongly. The main purpose of his work was obviously to confirm these ideas, and the consensus in Cambridge was that he had.

3.3 Analogy between alpha and beta emission, and Meitner's interpretation of the beta line-spectrum

Meitner's proposal of an interpretation of the beta line-spectrum was submitted to the *Zeitschrift für Physik* in February 1922.[19] The starting point, according to her, was a recent investigation carried out together with Otto Hahn.[20] The two had found that some alpha-radioactive sources, for example radium and radioactinium, are beta emitters as well. If the beta particles, like the alpha rays, have their origin in the nucleus, a decay product corresponding to the beta disintegration had to be present. Hahn and Meitner made thorough attempts to trace such products, but in vain:

> With regard to radium and radioactinium we could now show that these hypothetical decay products could in any case only be present with less than 1/10,000 of the intensity that one must attribute to them as ordinary transformation products, i.e., they do not exist.[21]

This result was presented in 1920 without, as Meitner admitted, any satisfactory explanation.[22] About one year later, however, Meitner considered it obvious to suppose that "somehow with the emission of α rays there is associated a γ radiation, originating in the nucleus, that produces the observed β rays as a secondary effect."[23] Her main source of inspiration for that explanation was undoubtedly again Rutherford's 1917 paper. Lack of communication immediately after the war makes it probable that Hahn and Meitner had not read that paper in 1920.

With these new results, an old attachment of Meitner's came alive. She regained her strong belief in a close analogy between alpha and beta emission,[24] and guided by that analogy she began testing the following hypothesis:

> The β rays expelled from the nuclei of the disintegrating atoms all have the same velocity. One fraction of these β rays passes through the atom unchanged and is measured outside the atom with its full velocity. The other fraction is transformed in the nucleus into γ rays of corresponding frequency, and the γ rays eject electrons from the electron rings, which have different velocities depending upon the ionization work performed and which form the secondary part of the β-ray spectrum.[25]

[19] L. Meitner, "Über die Entstehung der β-Strahl-Spektren radioaktiver Substanzen," *Z. Phys.* *9* (1922), 131–144.

[20] O. Hahn and L. Meitner, "Über die Anwendung der Verschiebungsregel auf gleichzeitig α- und β-Strahlen aussendende Substanzen," *Z. Phys.* *2* (1920), 60–70.

[21] O. Hahn and L. Meitner, "Über das Arbeiten mit radioaktiven Substanzen," *Naturwiss.* *9* (1921), 316–318, p. 318.

[22] Hahn and Meitner, "Über die Anwendung" (note 20), p. 70.

[23] Meitner, "Über die Entstehung" (note 19), p. 132.

[24] As to this analogy, see Chapter 2, Section 2.4.

[25] Meitner, "Über die Entstehung" (note 19), p. 132.

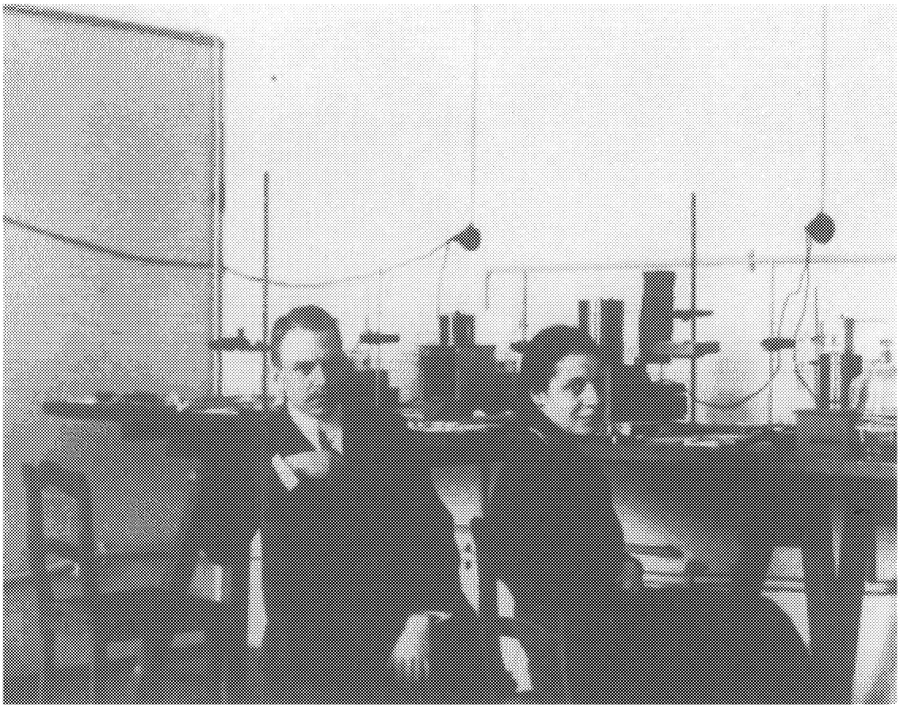

Figure 3.4: Hahn and Meitner in the laboratory. [Deutsches Museum München]

If this hypothesis were correct, the velocity of the primary beta particles, which according to Meitner constitute one of the lines in the beta line-spectrum, must agree with those of the secondary part corrected for the ionization energies. The velocities of the secondary electrons could be established by using the excited-spectrum method of Rutherford, Robinson and Rawlinson.

Like Ellis, Meitner emphasized that it was not obvious which isotopic element one had to use in the excited-spectrum method. Unlike Ellis, she held the view that the gamma ray is emitted *after* the disintegration; but since there might well be a time of relaxation, the values of the ionization energies were still not obvious. Owing to the inadequate measuring accuracy, however, the question was of no practical importance in 1922, but later it became an essential part of the controversy.

For several reasons, thorium B was chosen as the radioactive source. It has a relatively simple beta spectrum; until then, only two groups were known – a rather faint one of velocity $0.72c$ and a much more intense one of velocity $0.63c$. It emits a rather strong gamma radiation of medium penetration; and finally it is an isotope of lead, which made it possible to investigate the secondary beta spectrum of thorium B by using its gamma rays to liberate electrons from lead.

The experimental arrangement was essentially the same as the one used earlier by the Berlin group, and Meitner found good agreement, in velocity as well as in intensity, between the Pb electrons excited by ThB gamma rays and the ThB beta spectrum. Both groups thus had to be of secondary origin. Energy calculations showed that

$$E_{\beta_1} - E_{\beta_2} = 1.238 \times 10^{-7} \text{ erg},$$

where $\beta_1 = v_1/c = 0.72$ and $\beta_2 = v_2/c = 0.63$, was in good agreement with

$$E_K - E_{L_1} = 1.21 \times 10^{-7} \text{ erg}.$$

Meitner was thus convinced that the beta particles originate from the K and L_1 shells, respectively. Finally, the values of $E_{\beta_1} + E_{L_1}$ and $E_{\beta_2} + E_K$ agreed well with the energy of the gamma radiation.

Primary beta rays, calculated	Primary beta rays, observed	Secondary beta rays, observed	Origin of electron	Intensity
0.739	0.740	–	Nucleus	weak
–	–	0.723	L_1	stronger
–	–	0.714	L_2	very weak
–	–	0.630	K	strong

Table 3.4: The result of Meitner's investigation of ThB's natural beta line-spectrum. Source: Meitner, "Über die Entstehung" (note 19), p. 139.

The line corresponding to the primary electrons was not present, but that was due to the apparatus, Meitner argued. The magnetic field, and thus the deflection, was too weak to produce it. She therefore changed to the method of Jean Danysz, and indeed, two additional lines of velocities $0.714c$ and $0.740c$ emerged. The swifter one had to be the primary line, and the other one was supposed to consist of electrons from the L_2 shell. Meitner's results are recapitulated in Table 3.4.

From the intensities of the lines, Meitner inferred that a considerable part of the primary beta rays are converted into gamma rays, which suffer a rather strong absorption in the K shell and a weaker absorption in the succeeding outer shells, quite in agreement with Ellis's observations.

Meitner was satisfied with her results and convinced of the validity of her hypothesis. She summed up her interpretation of the beta line-spectrum in this way:

> The experiments described above have shown that one can divide the β-ray spectra into a primary part belonging to the nucleus and a secondary part originating in the electron shells, and that one can obtain

> a satisfactory explanation of the observations for a number of substances by making the following assumption: for typical beta emitters, electrons leave the nucleus with uniform velocity. The β rays are transformed in part into γ rays having the same energy, and in the various electron shells these γ rays release secondary β rays, the velocity of which is determined by the velocity of the primary β radiation and by the ionization work in question. The highest observed velocity must always correspond to the primary β ray and its measurement provides in addition the wavelength of the γ radiation.[26]

In her next paper in the *Zeitschrift für Physik*, Meitner elaborated her view on the conversion of beta energy into gamma energy in the nucleus.[27] She emphasized that the measured velocity of the primary beta particles is the velocity *outside* the atom. Inside the nucleus, they must possess a considerably higher velocity. The nuclear electron, however, does not give up all of this energy when creating a gamma ray. "The nuclear electron gives up only just so much energy in the form of γ radiation that it can escape from the decaying atom," Meitner stated, and continued: "This explains that the energy of the γ rays corresponds exactly to the measured energy (outside the atom) of the primary β radiation."[28]

It might be necessary, Meitner admitted, to introduce some minor modifications in this hypothesis. RaB, for instance, possibly emits two primary beta groups instead of one; but on the whole she was very confident of her interpretation. In a special note appended to a letter of 15 February 1922 from Hahn to George Hevesy, she thus wrote that she now believed that she was "able to show beyond any doubt that the β-ray spectrum [i.e., the line spectrum] is composed of a primary part originating in the nucleus and a secondary part excited in the electron shells, between which one can distinguish quite sharply."[29]

Guided by her old attachment – the analogy between alpha and beta emission – Meitner had thus presented an interpretation of the beta line-spectrum that differed fundamentally from that of Ellis. The controversy had started, and it did not end definitively until 1929.

[26] *Ibid.*, p. 143.

[27] L. Meitner, "Über den Zusammenhang zwischen β- und γ-Strahlen," *Z. Phys.* *9* (1922), 145–152.

[28] *Ibid.*, p. 146.

[29] Hahn to Hevesy, 15 February 1922, Hevesy Scientific Correspondence, Niels Bohr Archive.

3.4 Ellis's response to Meitner's hypothesis, and his interpretation of the continuous beta spectrum

In early 1922, before he had read Meitner's papers in the *Zeitschrift für Physik*, Ellis expressed great confidence in his own hypothesis. On 9 February, he wrote to Meitner:

> I have ... recently taken some photos of the spectrum excited in platinum by the radium B γ-rays and the results are such as to make me quite confident that all the lines are secondary in origin, and that the interpretation I have given is correct. I have obtained all the weaker lines from platinum and all shifted by the right amount.[30]

His hypothesis rested mainly on the beta line-spectra of RaB and RaC, however; and since investigations of other line spectra, such as those of thorium B, "have led Frl. Meitner to propose quite a different theory of β-ray disintegration,"[31] Ellis decided to make some measurements of these spectra too. They were special in the sense that they were mainly due to one intense gamma ray instead of several less intense ones.

No	$H\rho$	Intensity	Energy obs. volts	γ ray	Origin	Energy calc.
1	2015	m.f.	2.813×10^5	2	L_3	2.80
2	1798	m.s.	2.327	1	M	2.32
3	1738	s.	2.197	1	L_3	2.20
4	1677	m.	2.066	2	K	2.07
5	1382	v.s.	1.475	1	K	1.47
6	1110	m.	0.993	...	...	...

Table 3.5: The results of Ellis's investigation of ThB's natural beta line-spectrum. Source: Ellis, "Interpretation" (note 31), p. 123.

Ellis's measurements were carried out by the usual method of Rutherford and Robinson, and the results of his investigation of the beta line-spectrum of ThB are shown in Table 3.5. They differed considerably from those of Meitner (see Table 3.6, where Meitner's and Ellis's values are compared). First, her energy values for the lines 2 and 3 were higher. According to Ellis, "it was possible that in her experiments the faster rays penetrated into the weaker regions of the magnetic field and so traveled in a larger circle."[32] Second, three new lines (nos. 1, 4, 6) were present, and Ellis was certain that these lines really came from ThB. Excellent

[30] Ellis to Meitner, 9 February 1922, MTNR 5/4.
[31] C.D. Ellis, "The Interpretation of β-Ray and γ-Ray Spectra," *Proc. Camb. Phil. Soc. 21* (1922), 121–128, p. 123.
[32] *Ibid.*, p. 123.

Line	$H\rho$-values		Energies in MeV		Origin	
	Meitner	Ellis	Meitner	Ellis	Meitner	Ellis
1		2015		0.281		L_3
2	1866	1798	0.248	0.233	nucleus	M
3_1	1823	1738	0.238	0.220	L_1	L_3
3_2	1800		0.233		L_2	
4		1677		0.207		K
5	1588	1382	0.188	0.148	K	K
6		1110		0.099		?

Table 3.6: A comparison of the results of Meitner's and Ellis's investigations of the natural beta spectrum of ThB. Later, Meitner reinvestigated the spectrum and obtained results that were much closer to those of Ellis (see Table 3.9). Sources: Meitner, "Über die Entstehung" (note 19), pp. 137–139, and Ellis, "Interpretation" (note 31), p. 123.

No.	Energy in volts	λ in Å
1	2.36×10^5	0.0523
2	2.96	0.0417

Table 3.7: To explain ThB's natural beta line-spectrum, Ellis assumed that these two gamma rays were emitted from the ThB nucleus. Source: Ellis, "Interpretation" (note 31), p. 124.

agreement was obtained by assuming that two gamma rays (see Table 3.7) act on the levels and produce the lines. "It is certain therefore," Ellis wrote, "that ThB emits at least these two γ-rays."[33]

Measurements of the beta line-spectrum of radium D strengthened Ellis's belief in his hypothesis. He found five lines, one more than Danysz, and by assuming the existence of only one gamma ray, he was able to obtain a good explanation of the lines (see Table 3.8). The first two lines did not agree perfectly, but Ellis was satisfied all the same.[34]

Armed with an extensive support of his hypothesis, Ellis turned to Meitner's theory of beta decay. He considered her assumption that an electron converting a part of its energy into gamma-ray energy preserves an amount just sufficient to take it to the surface of the atom to be of an "arbitrary nature,"[35] but he desisted from discussing it further. Instead, he emphasized a point that could be tested by

[33] *Ibid.*, p. 124.
[34] *Ibid.*, p. 124.
[35] *Ibid.*, p. 127.

No.	$H\rho$	Intensity	Energy obs.	Origin	Energy calc.
1	742	m.f.	0.466×10^5	N	0.463 approx.
2	717	m.s.	0.436	M	0.432 –
3	628	m.f.	0.338	L_1	0.337
4	605	m.	0.314	L_2	0.315
5	600	v.s.	0.309	L_3	0.309

Table 3.8: The result of Ellis's investigation of RaD's natural beta line-spectrum. All five lines could be explained by assuming only one gamma ray of energy 0.467×10^5 volts ($\lambda \sim 0.264$ Å). Source: Ellis, "Interpretation" (note 31), p. 125.

experiment. It appeared that the line Meitner associated with the disintegration electrons is always the weakest. "This would mean that the disintegration electron rarely escapes with any energy, but usually excites γ-rays," Ellis argued, and continued: "Even allowing for secondary electrons ejected by the γ-rays it is clear that the total number of electrons emitted would be less than the number of atoms disintegrating."[36]

Ellis thus drew attention to an important point. If Meitner's view were correct, the number of electrons had to be less than the number of disintegrating atoms, whereas more than one electron per disintegrating atom would be emitted if his own hypothesis were correct. An obvious possibility of settling the dispute seemed to be present, and it certainly became an important factor in the controversy. In 1922, however, the experimental results were inconclusive.

In support of his own viewpoint, Ellis emphasized the results of an experiment of Danysz and William Duane.[37] They had found that radium B + C emits about three electrons for every pair of atoms disintegrating. The measurements were difficult, however, and Ellis admitted in his next paper, written in German and on the whole a repetition of his earlier one, that they should not be attributed too much importance.[38] He could have added, but refrained from doing so, that other experiments on the number of emitted beta particles were consistent with his hypothesis: Walter Makower had found that about 1.5 beta particles were emitted per disintegration, and according to experiments by Henry Moseley the number was 1.1.[39]

Another matter at issue was the beta line interpreted by Meitner as consisting of primary electrons. As to the ThB spectrum, Ellis did not observe this line at all:

[36] *Ibid.*, p. 127.

[37] J. Danysz and W. Duane, "Sur les charges électriques des rayons α et β," *Le Radium 9* (1912), 417–421.

[38] C.D. Ellis, "Über die Deutung der β-Strahlspektren radioaktiver Substanzen," *Z. Phys. 10* (1922), 303–307.

[39] H.G.J. Moseley, "The Number of β-Particles Emitted in the Transformation of Radium," *Proc. Roy. Soc. A87* (1912), 230–255; W. Makower, "On the Number and the Absorption by Matter of the β Particles emitted by Radium," *Phil. Mag. 17* (1909), 171–180.

> The measurements I have given have a relative accuracy of 1 in 300 and it can be seen from the table [compare Tables 3.4 and 3.5] that this line does not occur.[40]

In his beta line-spectrum of radium D (see Table 3.8), and in the lower part of the RaB spectrum as well, the controversial line did occur, and its energy agreed well with the quantum energy $h\nu$ (i.e., that of the primary electrons). Yet Ellis found another interpretation more reasonable. He suggested that the lines in question were due to conversion of gamma rays in levels near the surface of the atom, the N level for example. "This last explanation is natural," he wrote, "and one would anticipate that N level lines might be found in cases like this where the energy of the γ-ray is nearer the energy of the N ring."[41] Furthermore, the relative intensity was in accordance with what was expected.

A serious objection against Meitner's theory, and to Ellis perhaps the most important objection, was that it did not leave room for an explanation of the continuous spectrum, "whose existence was demonstrated by Chadwick beyond any doubt for radium B and C."[42] Ellis felt certain that the continuous spectrum consists of primary electrons, i.e., those from the nucleus, and he now presented the following picture of the beta-disintegration process:

> One of the electrons in the nucleus must be considered as a result of the previous disintegration to occupy one of the higher energy stationary states out of the series of such states that it can occupy. After an interval depending only on the conditions inside the nucleus this electron will make transitions to lower energy states, emitting γ-rays in the process. Some of these γ-rays are absorbed in the electronic structure of the same atom and eject electrons with characteristic energies. These electrons form the β-ray line spectrum. Finally, the electron arrives in a stationary state in which it is not permanently stable and it flies out from the nucleus. The kinetic energy of this electron must be considered to depend on other factors besides those of the stationary state, and the variable kinetic energy is possibly connected with the two facts that the nuclear field must vary considerably in distances comparable with the diameter of the electron and that the electron cannot be considered as rigid under these conditions.[43]

This time Ellis stated explicitly that the particle making the transitions is the electron. It furthermore appears that he assumed that the continuous spectrum arises inside the nucleus or, at any rate, very close to the nucleus. By comparing Ellis's statement with the following words of Rutherford, the great influence of the Professor is again obvious:

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[40] Ellis, "Interpretation" (note 31), p. 128.
[41] *Ibid.*, p. 128.
[42] Ellis, "Über die Deutung" (note 38), p. 305.
[43] Ellis, "Interpretation" (note 31), p. 122.

> It is to be anticipated that under the intense forces in the latter [the nucleus], the electrons are much deformed and the forces may be of a very different character from those to be expected from an undeformed electron, as in the outer atom. It may be for this reason that the electron can play such a different part in the two cases and yet form stable systems.[44]

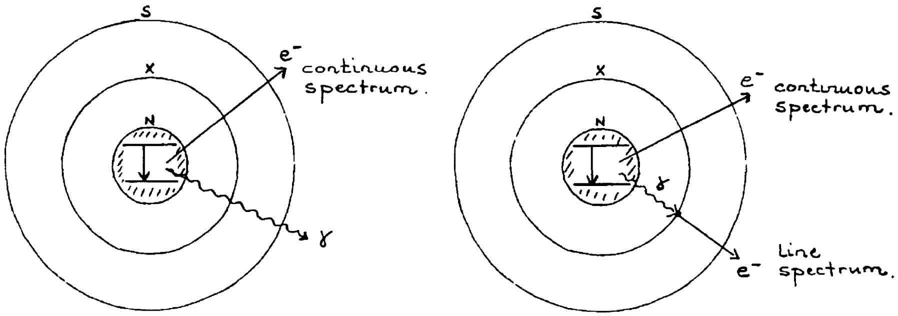

Figure 3.5: An illustration of Ellis's view on beta-ray emission. N is the nucleus; X is an electronic level, e.g., the K shell; and S is the surface of the atom. As a result of a previous disintegration, Ellis imagined, one of the nuclear electrons occupies one of the higher energy states. The electron would then make transitions to lower energy states, emitting gamma rays in the process. Some of these gamma rays are absorbed in the electronic structure, and electrons with characteristic energies are ejected. The nuclear electron will finally reach an unstable state and fly out of the nucleus. Its kinetic energy was supposed to depend upon other factors than those of the stationary state, and in this way the continuous spectrum was explained. According to Ellis's view, the gamma ray was thus emitted before the nuclear electron; and the continuity in energy of the beta particles arose inside the nucleus or, at any rate, very close to it. Drawing by the author.

Ellis had put forward his view on beta-ray and gamma-ray emission along the same lines. I have tried to illustrate it (Figure 3.5), together with Meitner's view (Figure 3.6).

[44]E. Rutherford, "Bakerian Lecture: Nuclear Constitution of Atoms," *Proc. Roy. Soc. A97* (1920), 374–400, p. 378. The expression "the Professor" is from S. Devons, "Recollections of Rutherford and the Cavendish," *Physics Today 24* (December 1971), 38–44. Devons writes: "Rutherford was not only one of Cambridge's illustrious Professors; in physics, in the Cavendish, he was *the* Professor" (p. 39). For a full account, see R.H. Stuewer, "The Nuclear Electron Hypothesis," in *Otto Hahn and the Rise of Nuclear Physics*, W.R. Shea (ed.) (Dordrecht: Reidel, 1983), pp. 19–67, on p. 25.

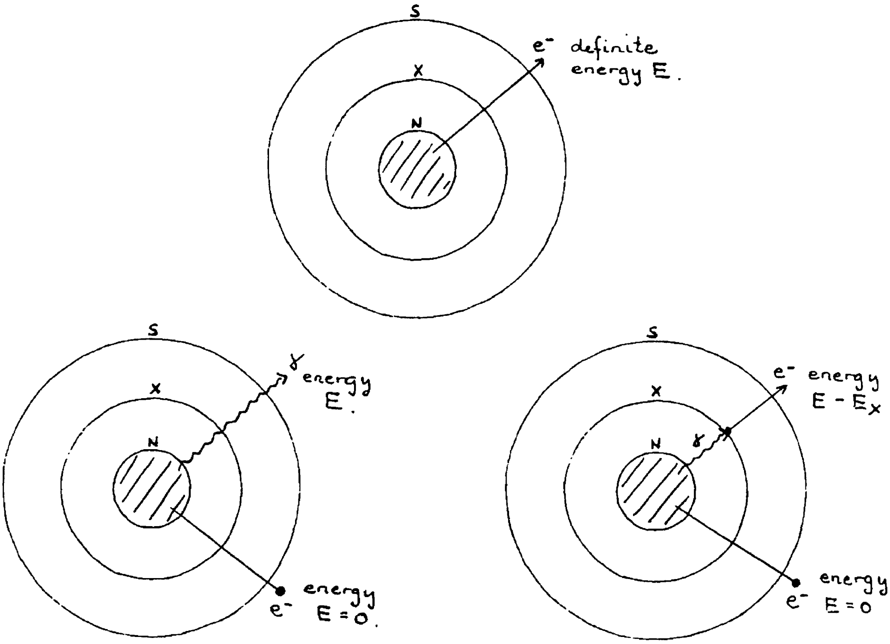

Figure 3.6: An illustration of Meitner's view on beta-ray emission. N is the nucleus; X is an electronic level, e.g., the K shell; and S is the surface of the atom. According to Meitner's view, all of the beta particles emitted from the nucleus possess the same energy. Some of the beta particles in the nucleus created gamma rays, but they still kept an amount of energy exactly sufficient to leave the atom. Some of the gamma rays were again converted into beta-ray energy in the electronic structure, and these electrons – together with those from the nucleus – constituted the beta line-spectrum. The continuous spectrum was due to secondary processes in the electronic system and in the measuring apparatus. Drawing by the author.

3.5 Meitner replies to Ellis, and reveals her view on the continuous beta spectrum

As we have seen in the previous section, Ellis essentially built his rejection of Meitner's theory on three points. First, he considered Meitner's beta line-spectra of ThB and RaD to be wrong. He relied more on his own experiments, which showed line spectra supporting his hypothesis. Second, Meitner's theory did not leave room for an explanation of the continuous beta spectrum; and third, her view was in contradiction to experiments on the number of emitted electrons. As to the third argument, Ellis admitted that it was weak, and Meitner also rejected it, but certainly not with conviction. "Against this," she wrote, "I can only state that E. Rutherford found about 0.7 β particles per decaying atom, and that W. Makower in very careful experiments arrived at the same too small value."[45] Quite true, Rutherford's result was 0.7 beta rays per disintegration, but already in 1909, it was well known, at any rate in Manchester, that this figure was incorrect. He had compared the total number of beta rays emitted from RaB + C, in radioactive equilibrium with radium, with the total number of alpha rays emitted from 1 gram of radium, and since he had used a far too high value for the number of alpha rays (6.2×10^{10}) he obtained an incorrect result. In 1909, Makower got about the same total number of beta rays, but he used the value 3.4×10^{10} for the number of alpha rays, and therefore he obtained 1.5 beta rays per disintegration or, as he put it, "one or possibly two."[46] It is strange, then, that Meitner considered Makower's result to be in agreement with Rutherford's.

Nor did Meitner yield to the other two arguments. She repeated her experiments on the beta line-spectra of ThB and RaD with an improved apparatus. Instead of brass it was made of aluminum, which decreased the number of secondary electrons and, indeed, the photographic exposures became much clearer. The sharpness of the lines was improved by using a silver wire of 0.2 mm thickness instead of a 0.5 mm phosphorous–bronze wire, and by securing a better parallel position and centering of the radioactive source.

Her new ThB line-spectrum, shown in Table 3.9, has a great resemblance to that obtained by Ellis; only the $H\rho$-values are a little higher. Meitner now agreed with Ellis that two gamma lines were necessary to interpret this result, but still she stuck to the primary line, even if she admitted that from the measurements of the ThB spectrum one could not decide whether the swiftest particles partly, or even entirely, originated from the outer levels. She thus recognized that her experimental results might prove to be incorrect, but her theory might still well be correct:

[45] L. Meitner, "Über die β-Strahl-Spektra und ihren Zusammenhang mit der γ-Strahlung," *Z. Phys. 11* (1922), 35–54, p. 49.

[46] Makower, "On the Number" (note 39), p. 180. As to Rutherford's result, see E. Rutherford, "Charge Carried by the α and β Rays of Radium," *Phil. Mag. 10* (1905), 193–208.

γ-ray energy in erg	β-ray energy in erg	$H\rho$	Origin	Intensity
3.75×10^{-7}	2.355×10^{-7}	1385	K	very strong
$\lambda = 5.2 \times 10^{-10}$ cm	3.534	1750	L	strong
	3.738	1809	nucleus	weak
4.72×10^{-7}	3.327	1689	K	very weak
$\lambda = 4.16 \times 10^{-10}$ cm	4.506	2020	L	–

Table 3.9: The results of Meitner's reinvestigation of the beta line-spectrum of ThB with an improved apparatus. Source: Meitner, "Über die β-Strahl-Spektra" (note 45), p. 40.

> However, I would like to emphasize already here that this question is certainly decisive for the theory of Mr. Ellis, for according to his view, no primary β rays with a well-defined velocity are allowed to be present at all; by contrast, the assumptions that I have suggested for the mechanism of β-ray transformation can still be fully maintained if it should turn out that these fastest lines are due to electrons from the outermost energy levels. This would only show that the total primary β radiation has been transformed into monochromatic γ radiation.[47]

Beta rays	$H\rho$	Energy in erg	Origin
A	741	7.486×10^{-8}	nucleus
B	718	6.859	M
C	602	4.966	L

Table 3.10: The results of Meitner's investigation of RaD with the improved technique. Source: Meitner, "Über die β-Strahl-Spektra" (note 45), p. 45.

Meitner's small hesitation disappeared when she turned to the beta line-spectrum of radium D. Here, the interpretation of the swiftest line is obvious, she argued. Its energy was almost identical with the gamma-ray energy, whereas she considered the deviation from the energy of the N electrons to be even larger than stated by Ellis. Using Dirk Coster's recently obtained binding energies, she found the difference to be 1.5 percent, which far exeeded the accuracy of 1/300.[48] Furthermore, there was a very interesting discrepancy in the experimental results. Whereas Ellis observed five lines in the RaD spectrum, Meitner's new measurements still revealed only three lines (see Tables 3.8 and 3.10). She did not find even the slightest hint of Ellis's L_1 and L_2 lines. This was a surprising result, since

[47]Meitner, "Über die β-Strahl-Spektra" (note 45), p. 45.

[48]D. Coster, "Zur Systematik der Röntgenspektren," *Z. Phys. 6* (1921), 185–203.

"according to Ellis the intensity of the L_1 line should be higher than, and that of the L_2 line as high as, the intensity of the line I have designated as A in my exposure."[49]

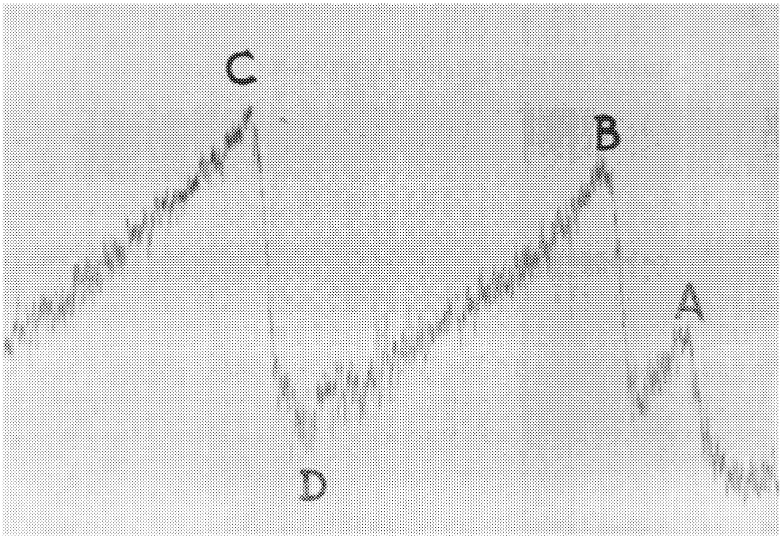

Figure 3.7: A photometric exposure of the radium D beta line-spectrum. The three lines A, B and C distinctly appear, whereas there is only a very faint indication of a fourth line D. Source: Meitner, "Über die β-Strahl-Spektra" (note 45), p. 46.

In the hope of settling the question, Meitner asked the *Staatslaboratorium* in Hamburg to make a photometric exposure of the RaD line-spectrum; this resulted in the curve shown in Figure 3.7. The three lines A, B and C distinctly appeared, and there was a faint indication of a fourth line D; but probably only one L line was present. The photometric curve thus agreed well with Meitner's results, and, in addition, it provided further support for her theory:

> I would like to draw attention to the fact that also the photometer curves contradict Ellis's explanation that line A essentially originates from the N level, and due to the superposition of weaker O and P lines shows an energy higher than that calculated for the N level. The decrease of intensity towards the fast β rays is so steep that one must probably conclude that the majority of the electrons possesses exactly the maximum (i.e., primary) energy, which to me seems incompatible with Ellis's view.[50]

[49] Meitner, "Über die β-Strahl-Spektra" (note 45), p. 46.
[50] *Ibid.*, pp. 47–48.

To Meitner, the presence of only one L line in the ThB as well as in the RaD beta spectrum was a most important result, and in favor of her view. It proved that the intensities of the beta lines are not entirely determined by the ionization energies of the levels, as had been stated by Ellis from measurements obtained by the excited-spectrum method, and thus that the natural and excited spectra are not necessarily analogous. It might well be, Meitner inferred, that "excitation by nuclear γ rays from the same atom is somewhat different in principle from excitation by radiation entering the atom from the outside. According to Bohr's theory, the configuration has of course a very pronounced asymmetry which for radiation from the outside can assert itself differently than for radiation from the nucleus."[51]

Even though Meitner had so far argued convincingly for her view, she still had to provide an explanation of the continuous spectrum. Ellis had pointed this out several times, and in a letter of 16 June 1922 Meitner told him why she had not gone into that problem earlier:

> Today I would only want to say that I have not discussed the existence of the continuous spectrum because a doctoral thesis, also dealing with this topic, is just underway at our institute; it will appear soon. On the basis of this work, and also from my own viewpoint, I believe to possess a quite plausible explanation of the continuous spectrum. The paper by Rutherford, Robinson and Rawlinson, which I know of course in detail, does not seem convincing to me on account of the experimental arrangement employed.[52]

The *Doktorarbeit* to which Meitner referred was an investigation of the beta spectrum of ThB + C carried out by *Studienrat* Werner Pohlmeyer, and I will discuss it in further detail in a subsequent section. It was not submitted to the *Zeitschrift* until August 1924,[53] but Meitner made use of Pohlmeyer's results already two years earlier. They were a welcome support for her theory.

Meitner opened the discussion of the continuous spectrum by expressing her dissatisfaction with Ellis's view on the disintegration process:

> I must admit that this assumption seems to me to be difficult to reconcile with our usual conceptions of radiation processes, for generally it is always assumed that under emission of radiation, the electron passes to a more strongly bound state. Here exactly the opposite should take place: by means of transitions leading to γ radiation, the electron reaches a less stable state. Moreover, the assertion that the electron leaves the nucleus with an arbitrary velocity that is in no way unambigously defined by the nuclear state can hardly be regarded as an

[51] *Ibid.*, p. 48.

[52] Meitner to Ellis, 16 June 1922, MTNR 5/4.

[53] W. Pohlmeyer, "Über das β-Strahlenspektrum von ThB + C," *Z. Phys. 28* (1924), 216–230.

> explanation of the existence of a continuous spectrum, but rather only as another formulation of the same circumstance.[54]

She did not go further into a discussion of the disintegration process itself, but she was convinced that the electron leaves the nucleus with a definite velocity. The continuous spectrum thus had to be of secondary origin, but what secondary processes might be responsible for the continuity? Meitner pointed to two possibilities.

The British interpretation of Chadwick's experiment[55] is incorrect, she first argued, since the resolution of the apparatus was too small. Because the beta particles moved in circles, the resolution was reduced considerably. With a radius of curvature of 3 cm, Chadwick used a slit of 1 mm in the counter and 2 mm in the ionization chamber. That caused an uncertainty in the measured $H\rho$-values of the beta rays of 1.6 and 3.2 percent, respectively, and in addition stray particles of different values of $H\rho$ were present. Besides, Chadwick could only change the magnetic field in steps of more than 1 percent. Since the $H\rho$-values of the beta lines of RaC vary by about 3 percent, and the swifter rays by even less than 1 percent, Meitner felt certain that a resolution of the lines was not possible in Chadwick's experiment, not even if the lines were strictly homogeneous before entering the counter or the ionization chamber.

As another secondary process that might make the beta particles inhomogeneous, Meitner pointed to collisions on their way out through the electronic system of the atom. It was known from experiments on excitation of X rays by cathode rays that such processes do take place, and that their frequency increases with increasing energy of the electrons. According to Meitner, radium E supported that view. It does not emit gamma rays, but yet it emits characteristic K and L radiation, which hence must be excited by the primary beta particles. In contrast to the gamma rays, these beta particles give up only an arbitrary part of their energy in the collisions. In this way, the beta particles are made inhomogeneous, and capable of constituting a continuous spectrum.

The relative importance that Meitner attached to these two possibilities does not appear in her paper; but in a letter of 16 November 1925 to Ellis, she expressed it as follows:

> I always held the opinion that on leaving the nucleus, the primary β rays must have a well-defined velocity. But already in my second paper (Ztschr. f. Phys. <u>11</u> 35 (1922))[56] I drew attention (p. 51) to the fact that, by excitation of the characteristic radiation in the mother atom as well as because of the arbitrary collision conditions, the β rays can lose any amount of energy, and so must become inhomogeneous.
>
> ...
>
> It is therefore a mistake when Mr. [Ronald Alfred Ransom] Tricker,

[54] Meitner, "Über die β-Strahl-Spektra" (note 45), p. 50.

[55] J. Chadwick, "Intensitätsverteilung im magnetischen Spektren der β-Strahlen von Radium B + C," *Verh. d. D. Phys. Ges. 16* (1914), 383–391.

[56] Meitner, "Über die β-Strahl-Spektra" (note 45).

> and recently also Mr. [Ronald W.] Gurney, say that I maintain the view that the continuous background is due in the main to insufficient resolution of the lines only. That this circumstance must contribute something to the continuous spectrum is clear, but I held this effect to be decisive only in my first paper.[57]

According to this letter, Meitner had considered the collision processes to be the essential reason for the continuity in energy as early as August 1922. It is not unlikely, however, that the attitude she expressed in the letter was influenced by the later development. Whether Meitner had any idea of the relative importance of the different secondary effects at that time is not clear.

3.6 The atom as a unity: Smekal joins the discussion, and is met with a sharp reaction

In June 1922, when the controversy between Meitner and Ellis was a few months old, the Austrian physicist Adolf Smekal presented his interpretation of beta-ray and gamma-ray emission.[58] He concentrated on the line spectra. Probably influenced by his own previous occupation with X rays, he considered the continuous beta and gamma spectra to be analogous, in a sense, to the continuous X-ray spectra, and "thereby the continuous spectra at present lose considerable interest."[59]

Smekal was a theorist, one of the very few concerned with the beta spectrum at this time, and his aim was to put forward a completely quantum-theoretical interpretation of the beta and gamma line-spectra. He succeeded in advancing interesting considerations, but their significance was only realized later. In the eyes of the experimentalists, he went too far, and, as we shall see later in this section, Meitner especially reacted sharply.

The essentially new idea in Smekal's interpretation was that he considered "the whole atom, nucleus and surrounding electrons, jointly quantized as a whole."[60] So far, everyone had always assumed that the nucleus and the surrounding electrons could be treated independently, but Smekal broke with this assumption.

Smekal distinguished between two types of quantum transition. The primary result of the first type was the emission of a nuclear electron, and to investigate the additional processes in this transition he divided the energy released into three parts:

---

[57] Meitner to Ellis, 16 November 1925, MTNR 5/4.

[58] A. Smekal, "Zur quantentheoretischen Deutung der β- und γ-Strahl-Emission," *Z. Phys. 10* (1922), 275–302; *idem.* "Zur quantentheoretischen Deutung des radioaktiven Zerfalls," *Akad. d. Wiss. Wien 16* (1922), 129–132. I am grateful to Prof. Dr. Berta Karlik of Vienna for having drawn my attention to the latter paper, and for having sent me a copy of it.

[59] Smekal, "Zur quantentheoretischen Deutung" (note 58), p. 277.

[60] *Ibid.*, p. 280.

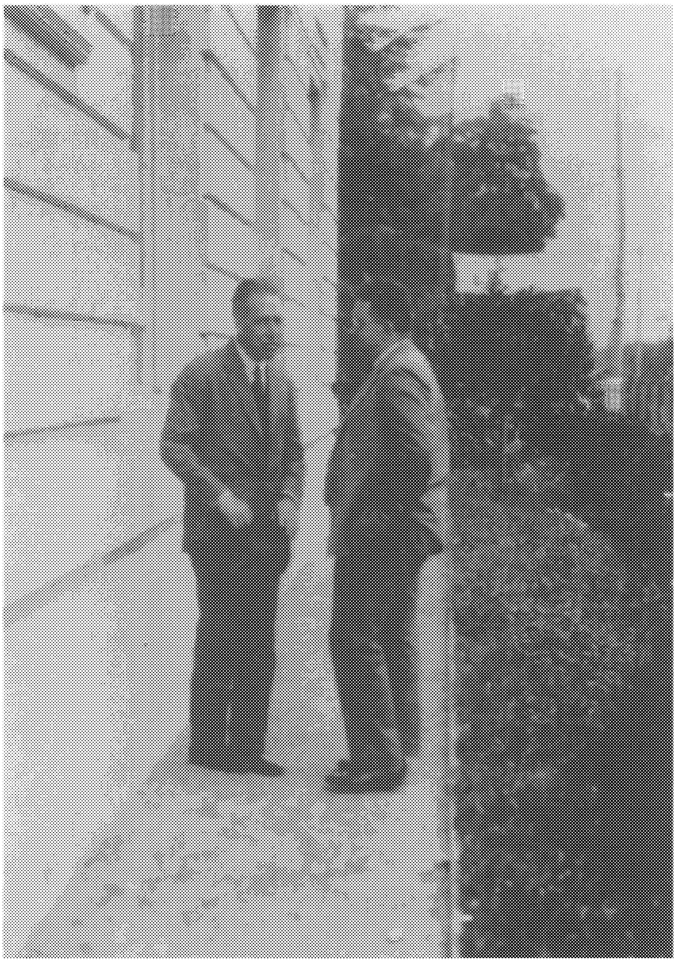

Figure 3.8: Niels Bohr and Adolf Smekal, probably at the Como conference in 1927. [Niels Bohr Archive]

a) The energy E_i necessary to bring the electron to the surface of the atom.
b) The energy E_∞ required to move the electron from the surface of the atom to infinity, i.e., the ionization energy of a loosely bound peripheral electron.
c) The additional energy E_a which might appear as kinetic energy of the free electron.

In Figure 3.9, I have tried to illustrate the different processes falling under the first type. It appears that one example of a unified [*einheitlich*] quantum transition is a transition from the ground state to a K-ionization state. Smekal considered such transitions to be analogous to Oskar Klein and Svein Rosseland's collisions of the second kind.[61] They do not involve gamma rays at all, and therefore he called them radiationless [*strahlungslose*] transitions.[62]

Except for his idea of unified transitions, Smekal's first type of transition has a strong resemblance to Meitner's interpretation (Ellis referred to it as the Meitner–Smekal theory). The second type, however, was partly borrowed from Ellis. In this process, illustrated in Figure 3.10, either a nuclear gamma ray or a secondary electron, for instance a K electron, is emitted. Smekal ascribed this emission to a nuclear transition, and he thus agreed with Ellis that a nucleus can exist in different, discrete energy states.

Consequently, Smekal had put forward a quantum-theoretical interpretation of the beta line-spectra that included essential parts of Meitner's as well as Ellis's theory. The experimental aspect of the matter still remained to be examined, however. Do both types of transition actually take place?

In principle, Smekal argued, it is possible to determine whether a secondary beta line is due to the first or second type of transition since, if energy is conserved, the first type "requires necessarily that the ionization work, $E_K, E_{L_1}, E_{L_2}, \ldots,$ must be chosen not for the decaying element, but for the element preceding the decay,"[63] i.e., the energy E_{β_K} of the secondary electron from the K level must be determined by the equation

$$E_{\beta_K}(z) = E_a + E_\infty(z+1) - E_K(z+1),$$

where z is the atomic number. If, on the contrary, the line is due to the second type where no disintegration takes place, the equation is

$$E_{\beta_K}(z) = E_a + E_\infty(z) - E_K(z).$$

For the moment, this way of deciding between the two types was only theoretical, however. The uncertainty in the experimental determination of especially the K-level energy of heavier elements was still considerable.

[61] O. Klein and S. Rosseland, "Über Zusammenstösse zwischen Atomen und freien Elektronen," *Z. Phys. 4* (1921), 46–51.
[62] Smekal, "Zur quantentheoretischen Deutung" (note 58), p. 280.
[63] *Ibid.*, p. 280.

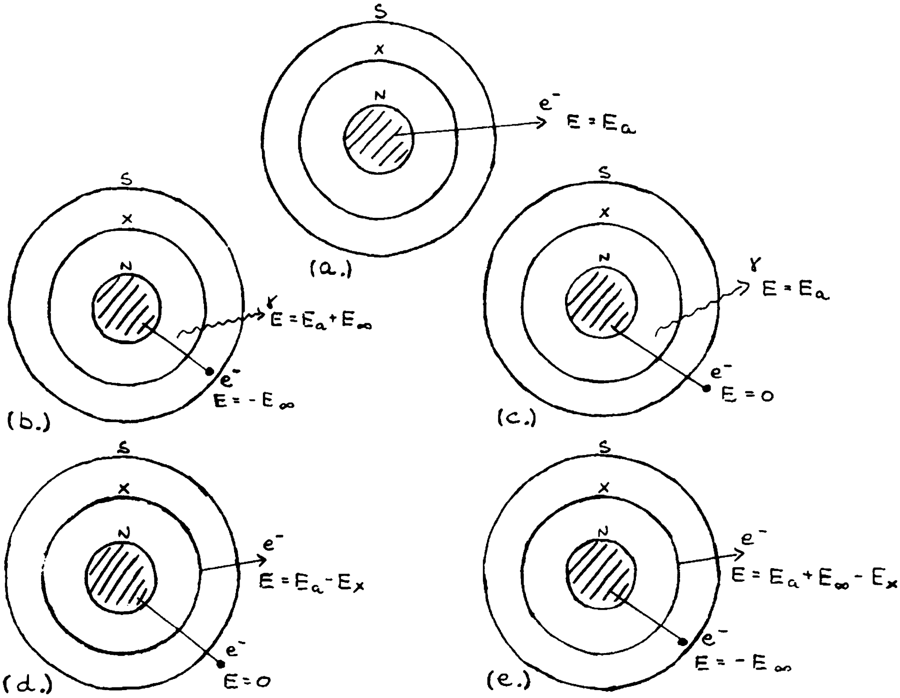

Figure 3.9: An illustration of the different processes falling under Smekal's first type of transition. N is the nucleus, X is an electronic level, and S is the surface of the atom. In (a) an electron is emitted from the nucleus with a kinetic energy E_a outside the atom. In (b) the nuclear electron is captured on the periphery of the atom, and a gamma ray of energy $E_a + E_\infty$, where E_∞ is the ionization energy of a loosely bound electron, is emitted. In (c) the electron leaves the atom with zero kinetic energy, and the energy of the emitted gamma ray is then E_a. Instead of a gamma ray, the atom may emit an electron from one of the electronic levels; the energy of this electron will be $E_a - E_X$ and $E_a + E_\infty - E_X$, respectively. These two processes are shown in (d) and (e). Drawing by the author.

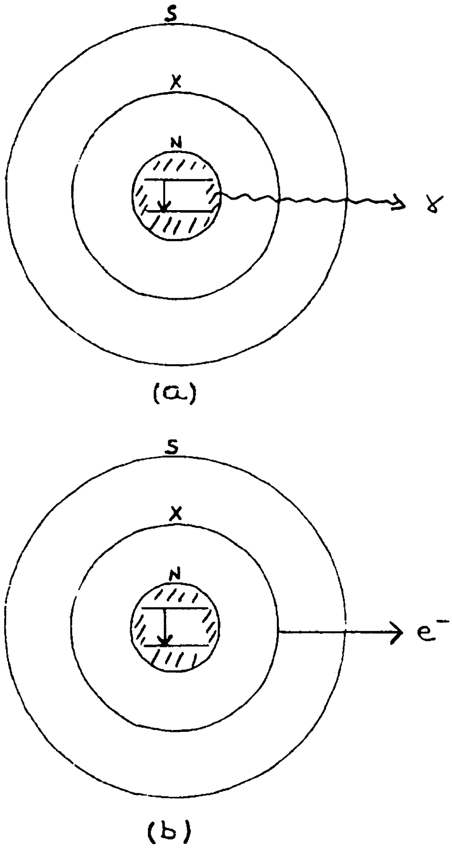

Figure 3.10: An illustration of the two processes falling under Smekal's second type of transition. N is the nucleus, X is an electronic level, and S is the surface of the atom. After an energy transition in the nucleus, Smekal argued, either (a) a gamma ray is emitted, or (b) an electron from one of the electronic levels is emitted. In accordance with Ellis's view, Smekal thus believed in the existence of discrete nuclear-energy states. Drawing by the author.

	Energy in volts	λ	Origin
a	4.000×10^5	0.0308×10^{-8}	β-decay γ ray
b	3.639	0.0339	nuclear γ ray
c	3.492	0.0354	–
d	2.918	0.0423	β-decay γ ray
e	2.529	0.0488	nuclear γ ray
f	2.385	0.0519	–
g	1.246	0.099	?
h	1.08	0.114	nuclear γ ray
k	0.537	0.230	β-decay γ ray
l	0.380	0.325	–
m	0.145	0.853	nuclear γ ray

Table 3.11: Smekal's classification of the gamma rays from RaB. Beta-decay gamma rays were connected with emission of electrons from the nucleus, i.e., the type of quantum transitions illustrated in Figure 3.9. Nuclear gamma rays, however, were due to nuclear transitions without the emission of nuclear electrons, i.e., the processes shown in Figure 3.10. Source: Smekal, "Zur quantentheoretischen Deutung" (note 58), p. 286.

Smekal had to find another way of proving the existence of both types of transition. He did so by turning to the gamma rays of RaB, shown in Table 3.11. They were calculated by Ellis from the measured beta line-spectrum. According to him, all of the gamma rays were what Smekal called nuclear gamma rays, i.e., they belonged to the second type of transition. Meitner had questioned this, since the energy of two of the gamma rays, c and f, agreed well with the energy of two of the beta lines.[64] Consequently, she considered c and f to be what Smekal called beta-decay gamma rays. Smekal disagreed with both Ellis and Meitner. First, by comparing the gamma rays from RaB + C, measured by Rutherford and Andrade,[65] with the beta rays of RaB falling in the same energy domain, he inferred that k and l in Table 3.11 must be beta-decay gamma rays. Second, he argued that if there exists a relation

$$E_{\gamma_1} = E_{\gamma_2} + E_{\gamma_3}$$

between the energies of three different gamma rays, it is "impossible that all three γ rays can correspond to primary β rays of the same energy, insofar as these rays with certainty can be associated with the same radioactive element."[66] Two

[64] Meitner, "Über die β-Strahl-Spektra" (note 45), p. 52.

[65] E. Rutherford and E.N. da C. Andrade, "The Wavelength of the Soft γ Rays from Radium B," *Phil. Mag. 27* (1914), 854–868; *idem.*, "The Spectrum of the Penetrating γ Rays from Radium B and Radium C," *Phil. Mag. 28* (1914), 263–273.

[66] Smekal, "Zur quantentheoretischen Deutung" (note 58), p. 283.

gamma-ray energies only (E_a and $E_a + E_\infty$ in Figure 3.9) are associated with one beta-decay process. "Hence it follows," Smekal stated, "that γ_1, and at the same time γ_2 or γ_3, must be associated with one β transformation or none of these three frequencies at all."[67]

He used this argument on the following gamma relations set up by Ellis:

$$\begin{array}{lclclcl} a-b & = & b-e & = & c-f & = & h \\ a-c & = & d-f & = & k & & \\ a-b & = & d-e & = & l & & \\ k-l & = & m & & & & \end{array}$$

He concluded that also a and d in Table 3.11 are beta-decay gamma rays. The remaining ones are nuclear gamma rays.

The primary beta rays corresponding to the gamma rays a and d do not appear in the spectrum of radium B, but even if Smekal could not explain why line d is missing, it did not mar his pleasure:

> Concerning a this does not seem ... surprising since already the corresponding secondary β rays ... are of very low intensity. The non-appearance of a β group of the same energy as d shows, however, that a high intensity of the corresponding secondary rays does not necessarily allow an inference as to the intensity of the primary group that in theory belongs to it.[68]

Nor did it worry him that his interpretation of the gamma rays disagreed with that of Meitner. Even if she refused to accept the presence of the rays a, b and e, he argued, one relation, $d = f + k$, still had to be fulfilled, and this relation is inconsistent with Meitner's view that c and f are beta-decay gamma rays.

Smekal's theory led to an intense correspondence with Meitner, with Professor Hans Thirring of Vienna as a third participant. It started with a letter of 18 July 1922, in which Meitner expressed her great misgivings concerning the view put forward by Smekal:

> I cannot deny that I have rather strong reservations about your point of view. The assumption that there should be two kinds of γ rays does not seem very satisfactory to me and is furthermore not sufficiently substantiated by the experiments so far available. Thus, for example, it is very likely that two γ-ray groups exist for ThB, a possibility that I already referred to in my first paper. By contrast, for RaD there is only 1 γ-ray line, and the measurements of the β-ray groups seem definitely to imply that the Pb ionization work must be taken into consideration. However, I would not put too much emphasis on this point.
>
> That the agreement of the two RaB β lines with two γ lines measured by Ellis should be regarded as accidental seems doubtful to me;

[67] *Ibid.*, p. 283.
[68] *Ibid.*, p. 290.

> as far as I can conclude from the results of Rutherford and Ellis, the agreement for one line is within 3 percent, for the other line even 1 percent of the energy, whereas Ellis claims only 3 percent accuracy for his $H\rho$ measurement.[69]

Smekal answered Meitner in a letter of 26 July 1922. He drew attention to his addendum added in proof, which Meitner had not yet seen. In this addendum, written after he had acquainted himself with Ellis's recent work, he admitted that it was "at present not possible to infer with complete certainty the occurrence of primary β rays from the β spectrum alone."[70] Only if it was assumed, as Meitner had, that the continuous beta spectrum is not directly connected with the decay process, could it be inferred that a gamma-ray or a beta-ray line must be ascribed to the disintegration. Smekal still agreed with Meitner's assumption, and he still believed in his theory; Ellis's work, however, had convinced him that the question of the existence of beta-decay gamma rays could not be settled experimentally, i.e., by comparing the energies of the beta and gamma lines. He evidently had corresponded with Ellis about these points, and Ellis's attitude towards Smekal's theory was not quite as unsympathetic as Meitner had taken it to be:

> Yesterday I received a letter from Ellis that also takes into account these additions, which are still unknown to you. He writes: "If the continuous spectrum has no direct connection with the disintegration, then I think your theory must be right, in particular I agree with your remarks in the note at the end of the paper." He means to say, however – and in this I agree with him – that a definitive experimental proof of the secondary nature of the continuous β spectrum is not known (prompted by my paper, he is now going to investigate this question), and does not seem to have noticed the theoretical reasons that almost exclude the opposite (considerations of energy and atomic weight). As long as this question is not directly settled by experiment, he will stick to his viewpoint which I too would have to accept if the continuous spectrum really were of a primary nature.[71]

Apparently, Meitner did not answer this letter, but the content of Smekal's addendum made her attack his theory even more sharply, as is evident from a letter of 15 October 1922 from Smekal to Meitner:

> Thirring told me about the scientists' meeting, that you talked with him about this and stated that in the addendum to this publication I had actually withdrawn the whole content of the paper and that my theories were altogether of such a nature that they would not survive the next experimental result.[72]

[69]Meitner to Smekal, 18 July 1922, MTNR 5/16.
[70]Smekal, "Zur quantentheoretischen Deutung" (note 58), p. 301.
[71]Smekal to Meitner, 26 July 1922, MTNR 5/16.
[72]Smekal to Meitner, 15 October 1922, MTNR 5/16.

Smekal refused to accept this remark, and reacted strongly. "I ask your permission to explain," he wrote, "why I am of the opinion (and hardly only I) that such a view does not do justice to my attempt at a quantum-theoretical interpretation and that its dissemination would not be in the interest of the matter."[73] He found it unreasonable for Meitner to say that he had withdrawn the whole content of his paper, when five of its six paragraphs were unaffected, and he even considered the paragraph in question (§4) to be the least important, since it only aimed at showing, by an example, how the preceding considerations were to be applied.

The difference in opinion between the theorist Smekal and the experimentalist Meitner is evident from their correspondence. To Smekal, the general theoretical considerations were what was important, whereas to Meitner it was the agreement with experiment. She did not accept Smekal's theory, because it was not well supported by experiment. Her opinion was that he ought to have waited for such experimental support, as she told him in a letter of 24 October 1922.[74] The Vienna professor Thirring agreed completely with Meitner on this point, as he informed her on 27 October 1922.[75]

In her letter to Smekal, Meitner further expressed that in general she was sympathetic to the idea of a quantum-theoretical interpretation of the radioactive processes:

> The attempt to interpret the radioactive processes quantum-theoretically is certainly most valuable, and, just as certainly, the conception that the emission of a γ ray is connected with a quantum transition is more satisfactory than my quantum-like relation between primary β and γ rays, which is based merely on *computation*. The assumption that the excitation of secondary β rays is due to the same quantum transition of the nuclear electron is also very promising on account of the uniformity of the conception.[76]

One important condition had to be fulfilled, however. The experimental support must be at hand, and according to Meitner it was lacking in this case. She gave several examples; for instance, she considered Ellis's interpretation of the beta lines of RaB to be far from unassailable:

> That the line ... with a measured energy of 3.473×10^5 volts should originate from a γ line b and likewise the line ... with the energy 2.371×10^5 volts should be derived from a γ line e, while there are 2 *measured* γ lines with energies 3.492 (line c) and 2.385 (line f), is not plausible. An agreement within a few parts per thousand is here to be regarded as accidental, while on the other hand additive relations

[73] *Ibid.*
[74] Meitner to Smekal, 24 October 1922, MTNR 5/16.
[75] Thirring to Meitner, 27 October 1922, MTNR 5/19.
[76] Meitner to Smekal, 24 October 1922 (note 74).

> which show a deviation of several percent are to be considered laws of nature; this seems contradictory to me.[77]

Accordingly, Meitner did not believe much in the gamma-ray relations either, and she finished her letter in this way:

> In these data, I therefore really see no *criterion* for nuclear levels and therefore also not for the conclusions related to this assumption. In my own paper, I have dealt with this point with the brief statement that as yet no numerical basis for nuclear levels can be given. Perhaps it would have been more appropriate to make Mr. Ellis more thoroughly aware of the numerical discrepancies. Likewise, I have not mentioned the discrepancies in the intensity relations of the β rays from RaB. Perhaps my great aversion to polemics has led to a restraint on my part which might be misunderstood.[78]

Smekal answered Meitner in a six-page long letter of 29 October 1922, in which he admitted that the large experimental discrepancies were a surprise to him.[79] Meitner refrained from answering Smekal's theoretically based defense of his views. Apparently she realized, as she explained to Thirring, that Smekal was not able to judge objectively the seriousness of her disapproval.[80] Thirring offered to show Meitner's letter to Smekal "in order to destroy this, for him fateful, illusion."[81] It is not clear from the Meitner correspondence whether she agreed to this.

3.7 Two repetitions of the Chadwick experiment lead to contradictory conclusions

In the hope of being able to decide between the competing interpretations of the continuous beta spectrum, the two Cambridge physicists Chadwick and Ellis repeated Chadwick's experiment of 1914 and submitted their results to the *Proceedings* of the Cambridge Philosophical Society in September 1922.[82] The main purpose of their investigation was to settle whether the experimental arrangement or any other secondary process outside the disintegrating atom might be responsible for the continuous spectrum.

With an arrangement identical in principle to that used by Chadwick in 1914 (see Figure 2.4), they measured the intensity distribution in the beta-ray emission of radium B and C by the ionization method. The RaC curve shown in Figure 3.11

[77] *Ibid.*
[78] *Ibid.*
[79] Smekal to Meitner, 29 October 1922, MTNR 5/16.
[80] Meitner to Thirring, 8 November 1922, MTNR 5/19.
[81] Thirring to Meitner 12 November 1922, MTNR 5/19.
[82] J. Chadwick and C.D. Ellis, "A Preliminary Investigation of the Intensity Distribution in the β-ray Spectra of Radium B and C," *Proc. Camb. Phil. Soc.* *21* (1922), 274–280.

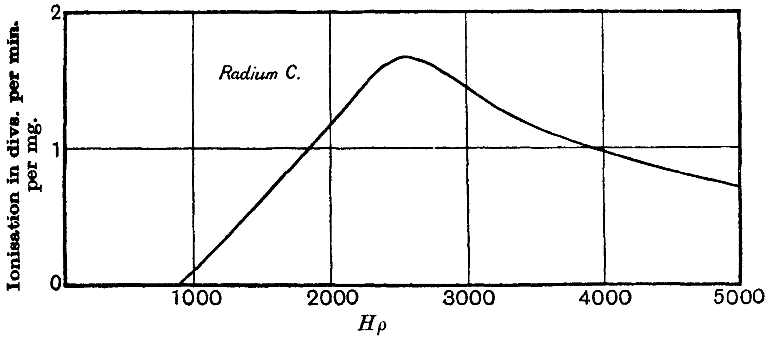

Figure 3.11: The beta spectrum of radium C, obtained by Chadwick and Ellis when they repeated Chadwick's experiment of 1914. Source: Chadwick and Ellis, "Preliminary Investigation" (note 82), p. 277.

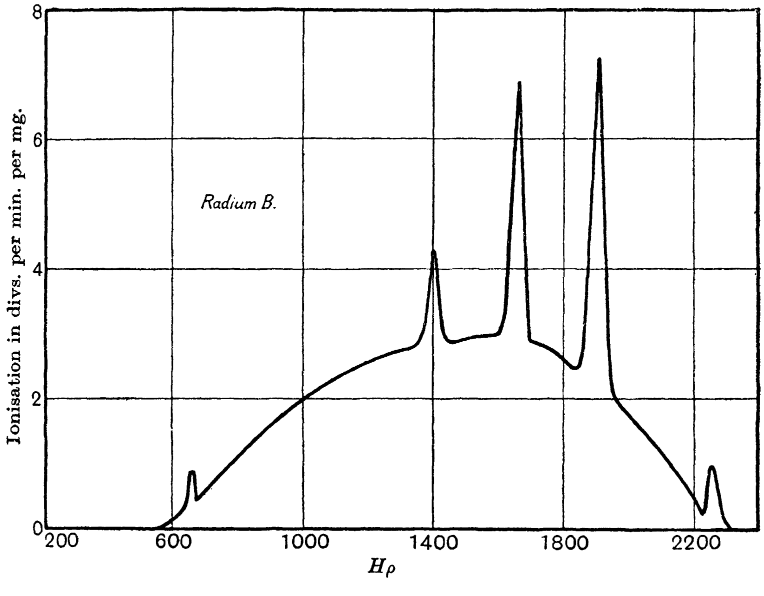

Figure 3.12: The beta spectrum of radium B, obtained by Chadwick and Ellis when they repeated Chadwick's experiment of 1914. Source: Chadwick and Ellis, "Preliminary Investigation" (note 82), p. 277.

was obtained directly with a source of pure radium C, whereas the RaB curve in Figure 3.12 was determined by deducting the effect due to RaC from the effects observed with a source of RaB + C.

Even though their investigation was preliminary and not very accurate, they considered it to be a firm confirmation of Chadwick's earlier result. The continuous spectrum is the dominant one, and Ellis and Chadwick felt certain about the consequences of that:

> These experiments and Chadwick's earlier experiments show conclusively that the continuous spectrum is emitted from the source, that is from the brass plate on which is deposited the RaB + C.[83]

Accordingly, they excluded the possibility that the experimental arrangement might be the cause of the continuity in energy, probably because they were convinced that the intensity of this part of the spectrum was too high to be explained in this way.

The possibility still existed, however, that the continuous spectrum had its origin outside the radioactive atom. Ellis and Chadwick called attention to two ways in which this could happen. Gamma rays might eject inhomogeneous electrons from the brass plate on which the radioactive source was deposited, or originally homogeneous groups might be rendered heterogeneous by being scattered backwards from the brass plate. Only if these two possibilities were ruled out could it be inferred that the continuous spectrum arose in the disintegrating atoms, and that is what Ellis and Chadwick tried to do.

They ruled out the first possibility at once by the magnitude of the effect. Against the second, they advanced "two strong arguments."[84] First, their two curves in Figures 3.11 and 3.12, when added together, agreed fairly well with Chadwick's RaB + C curve. This would not have been true if scattering at the source contributed essentially to the continuity, since the condition for this scattering is very different in the two cases: Chadwick's old experiment was carried out with a source of radium emanation contained in an alpha-ray tube of 2-cm stopping power, whereas in the later experiment the source was a small brass plate made active by exposure to radium emanation.

Their second argument originated from an experiment in which the active deposit was placed on the underside of a thin sheet of silver. In this case, there could be no back-scattering of the beta particles, and according to them it was known that passage through the thin silver sheet did not greatly affect the homogeneity of the groups. The difference between their results obtained in this way and those carried out with the brass plate should thus be a measure of the scattering effect of the brass.

Ellis and Chadwick confined their attention to the radium B curve, and observed two effects. The continuous spectrum was reduced, but not more than about

[83] *Ibid.*, p. 278.
[84] *Ibid.*, p. 278.

20 percent. Only that part of the continuous spectrum could thus be accounted for by scattering from the brass plate. Furthermore, the ratio of the peaks to the continuous background did not change in their new experiment, a circumstance which was not consistent with the view that the continuity in energy is due to scattering in the brass:

> Now it is obvious that if the real emission only consists of homogeneous groups and the continuous spectrum observed under ordinary conditions is due to scattering from these lines, then in an experiment where there can be but little back scattering the homogeneous groups should be greatly increased in magnitude relative to the background. As has been stated, this effect was not observed.[85]

Thus, they concluded that the continuous spectrum must be emitted as such by the radioactive atoms. They were quite aware, however, that their experiments "do not prove that it arises directly from the nuclei of the disintegrating atoms."[86]

Meitner did not accept their arguments. In an account of the Chadwick–Ellis paper[87] in the *Physikalische Berichte* she wrote:

> The lines themselves appear broadened and in their vicinity the relative ratio between the intensity of the lines and that of the continuous background is almost the same as in the measurements with the activated brass plate, a circumstance that hence may be due to the fact that the line width, which already in itself is not small, may be increased even more by the velocity change of the β rays at the penetration of the silver foil.[88]

Her argument was taken from the previously mentioned investigation of the beta spectrum of thorium B + C by the ionization method, proposed by her and carried out by Pohlmeyer.[89] Pohlmeyer refrained from using the counting method because it was suspected of producing incorrect results for swift beta rays. Possibly, a beta particle of high velocity did not produce enough ions to release a discharge. Since the same problem might exist in a small ionization chamber like the one used by Chadwick, Pohlmeyer used a larger one; but apart from that, his arrangement was essentially identical to Chadwick's.

The results of Pohlmeyer's investigation appear in Figure 3.13, where the photographic exposures of ThB and ThC + C$''$ are also included. The intensity was corrected for the effect of gamma rays and stray beta rays. Pohlmeyer compared his curve with that obtained by Chadwick for RaB + C, and emphasized two essential differences:

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[85] *Ibid.*, p. 279.

[86] *Ibid.*, p. 278.

[87] Chadwick and Ellis, "Preliminary Investigation" (note 82).

[88] L. Meitner, [Abstract of Chadwick and Ellis, "Preliminary Investigation" (note 82)], *Physikalische Berichte 4* (1923), 595.

[89] Pohlmeyer, "Über das β-Strahlenspektrum" (note 53).

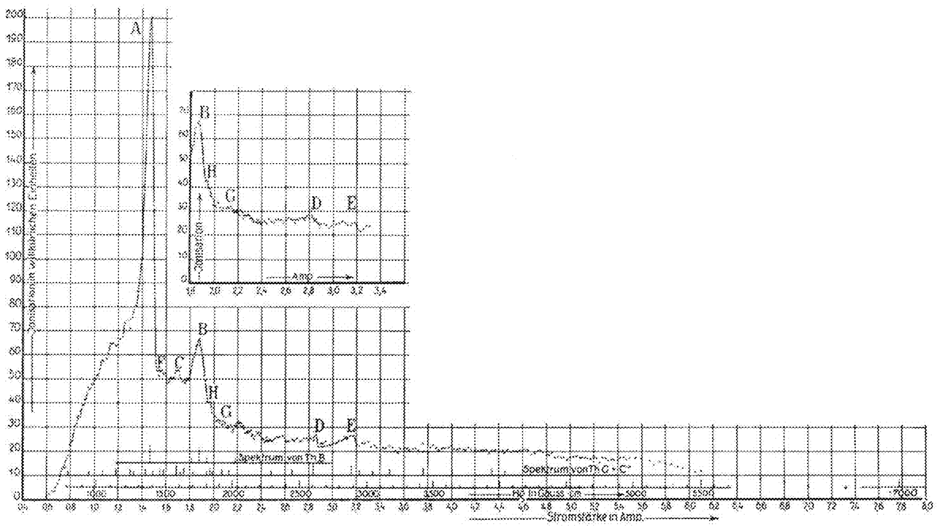

Figure 3.13: Pohlmeyer's beta spectrum of ThB + C. The graphical representation of the photographic exposures of ThB and ThC + C″ are inserted in the figure. Source: Pohlmeyer, "Über das β-Strahlenspektrum" (note 53), p. 222.

> First, in our graph not only strong groups, but also weaker ones appear; second, the large maxima dominate over a relatively weak background, whereas for Chadwick the maximum intensities exceed their close vicinity by at most 60 percent.[90]

According to Pohlmeyer, there are essentially three causes of the continuous background. First, the ability to resolve the lines is limited. As previously emphasized by Meitner, the experimental arrangement of Chadwick, and that of Pohlmeyer as well, were not able to resolve lines differing less than about 3.5 percent in their $H\rho$-values, i.e., numerous RaC and ThC″ lines could not be resolved. As an example, Pohlmeyer mentioned that there is a large number of ThC + C″ lines between the two strong ThB lines (A and B in Figure 3.13), and "only in this way is it possible to understand that the background between the two large peaks shows twice the intensity of that for larger values of $H\rho$."[91] Second, the beta particles are scattered in the wire on which the radioactive material is deposited; and third, the originally homogeneous groups are made partly inhomogeneous by collisions in the radioactive atoms.

[90] *Ibid.*, p. 224.
[91] *Ibid.*, p. 227.

Pohlmeyer thus disagreed with Chadwick and Ellis that causes of the continuity in energy *outside* the disintegrating atoms are excluded, and he did not accept their arguments:

> Since the authors themselves report that the lines were broadened by the velocity decrease in the silver foil, it is of course clear that this broadening causes an increase in the continuous background. Besides, as is also apparent from the previous experiments with ThB + C, a reduction in the intensity of the maximum is connected with the broadening of the line. This explains the fact that in the vicinity of the lines the background appears roughly as strong relative to the intensity of the lines as before. The reduction of the scattering is indeed approximately compensated by the broadening, and the result of this experiment proves once more that fuzziness of the lines, scattering, and other secondary effects play an important part in the appearance of the continuous spectrum. Thus, these results can in no way be advanced in favor of Ellis's point of view, according to which the primary β rays from the nucleus caused by the disintegration do not emerge with a uniform velocity.[92]

The two investigations had thus sharpened the controversy. Pohlmeyer's experiment had convinced Meitner that the continuous spectrum could be accounted for in a way that was in agreement with her interpretation, whereas Chadwick and Ellis had convinced themselves that the origin of the continuous spectrum was confined to the disintegrating atoms themselves, and thus probably to the nucleus.

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

Chapter 4

Secondary Effects and Order of Emission: Two Main Questions in the Controversy, 1923–1925

4.1 Introduction

I now turn to the development of the beta-spectrum controversy in the years 1923 to 1925. In the first part of this period, the origin of the continuous beta spectrum was still of paramount interest, and the efforts of the Berlin and Cambridge physicists concentrated on seeking corroboration of their respective views on this problem. Lise Meitner continued to investigate beta spectra and was also concerned with tracing secondary causes for the continuity in energy – a problem with which the young Norwegian physicist Svein Rosseland was also occupied. Charles Drummond Ellis stuck to the radium-disintegration products. Together with Herbert Skinner, he carried out a thorough reinvestigation of the beta spectrum of RaB + C.

Later on, however, attention turned towards another problem. The temporal order of the emission of beta and gamma rays developed into an urgent question in 1924, and until the end of 1925, when it was resolved, the beta-spectrum researchers in both Berlin and Cambridge spent most of their time on this problem.

4.2 Meitner investigates the beta spectrum of UX_1 and takes it as further support for her view

In January 1923, Meitner completed an investigation of the beta-ray spectrum of uranium X.[1] She was enthusiastic about the result, and did not refrain from informing Ellis immediately of her interpretation that a primary, continuous spec-

[1] L. Meitner, "Das β-Strahlenspektrum von UX_1 und seine Deutung," *Z. Phys. 17* (1923), 54–66. Even if Meitner had finished her investigation in January 1923, her paper was not submitted to the *Zeitschrift* until June 1923.

C. Jensen et al. (eds.), *Controversy and Consensus: Nuclear Beta Decay 1911-1934*

trum did not exist.[2] The following day, she drew the same conclusion in a note appended to a letter from Otto Hahn to George Hevesy.[3]

$H\rho$	$\beta = v/c$
927	0.48
1029	0.52
1163	0.581

Table 4.1: Meitner's beta spectrum for UX_1, obtained by Rutherford's method. The value $H\rho = 1163$ corresponds to the center of a blurred, rather narrow band. Source: Meitner, "β-Strahlenspektrum" (note 1), p. 57.

Mainly because of the difficulty of producing a source of sufficient activity, the investigation of the beta spectrum of UX was not an easy task; but Meitner succeeded in it. Uranium X consists of two radioactive elements, a thorium isotope UX_1 and a protactinium isotope UX_2. Meitner concentrated on the lower part of the spectrum, which was known to be the spectrum of UX_1. She carried out the investigation by Jean Danysz's magnetic-deflection method as well as by Ernest Rutherford's similar method. According to Meitner, the latter revealed a blurred, rather narrow band around the beta-ray velocity $0.59c$ and, in addition, two lines at velocities $0.48c$ and $0.52c$. She summarized the result in Table 4.1.

$H\rho$	$\beta = v/c$
927	0.48
1028	0.519
1057	0.529

Table 4.2: Meitner's beta spectrum of UX_1, obtained by Danysz's method. Source: Meitner, "β-Strahlenspektrum" (note 1), p. 58.

The photographic exposures that Meitner obtained by Danysz's method showed clearly three distinct lines (see Table 4.2), but no indication of a blurred band around $0.59c$. The absence of this band did not, however, surprise her. On the contrary:

> This corresponded completely to the expectations, for this band possesses after all an inhomogeneity of more than 6 percent. And whereas with [Rutherford's] arrangement (very narrow slit and small splitting through the small fields) mentioned above, this requires only a width of

[2]Meitner to Ellis, 19 January 1923, MTNR 5/4.

[3]Hahn to Hevesy, 20 January 1923, Hevesy Scientific Correspondence, Niels Bohr Archive.

at most 0.7 mm, it corresponds in the application of Danysz's arrangement to a splitting over more than 3 mm and thereby a reduction of the intensity, which in itself is already rather low, so that the band evades observation. I referred to this disadvantage of Danysz's arrangement and its significance for the so-called continuous spectrum in my last paper. The results obtained with UX_1 show furthermore that Danysz's method does not allow the separation between very close, unsharp lines or blurred bands.[4]

	$H\rho$	Energy	Intensity
1	1163 (band)	1.84×10^{-7} erg	faint
2	1057	1.54×10^{-7} "	"
3	1028	1.37×10^{-7} "	medium
4	927	1.144×10^{-7} "	"

Table 4.3: Meitner's beta spectrum of UX_1, obtained by putting together the results of the two methods. Source: Meitner, "β-Strahlenspektrum" (note 1), p. 59.

Meitner combined the results of the two methods, and obtained what she considered to be the beta spectrum of UX_1 (see Table 4.3). The interpretation is obvious, she argued. The energy differences between the lines 2, 3 and 4 suggest that they originate from the levels N, M and L, respectively, and by adding to their energies the ionization potentials of the mid-sublevels N_{IV}, M_{III}, and L_{II} of thorium, Meitner obtained almost identical energy values:[5]

$$\begin{array}{lclcl}
E_1 + N_{\text{IV}} & = & (1.440 + 0.011) \times 10^{-7} & = & 1.451 \times 10^{-7} \text{ erg} \\
E_2 + M_{\text{III}} & = & (1.370 + 0.064) \times 10^{-7} & = & 1.434 \times 10^{-7} \text{ erg} \\
E_3 + L_{\text{II}} & = & (1.131 + 0.312) \times 10^{-7} & = & 1.443 \times 10^{-7} \text{ erg.}
\end{array}$$

She found it surprising that this energy value coincided with the K_α radiation from thorium.[6] Thus, according to Meitner, the three distinct beta lines are excited from the levels L, M and N by the K_α radiation.

How, then, is the K_α radiation excited? Meitner found that it was natural to associate the excitation of this radiation with the action of beta rays traveling around 59 percent of the velocity of light.[7] These electrons, considered to be the primary beta particles, have an energy very close to the K-ionization energy of 1.736×10^{-7} erg, and therefore, she argued, they are inclined to transfer their energy to the K electrons. Owing to the small energy difference, the K electrons should be emitted with an immeasurably small velocity ($< 0.15c$).

[4] Meitner, "β-Strahlenspektrum" (note 1), p. 58.

[5] If these values are to be correct, two of the values in Table 4.3 must be in error (editors' note).

[6] Meitner, "β-Strahlenspektrum" (note 1), p. 60.

[7] *Ibid.*, p. 61.

The open question was still whether UX_1 emits additional gamma radiation. This was an important question to Meitner, as is evident from a letter of 1 June 1923 to Rosseland, and also the reason she did not submit her work to the *Zeitschrift für Physik* until June 1923:

> Precisely the testing of this point, which to me was important for deciding whether the narrow band of 59% represents the primary β rays, had induced me not to publish the paper even earlier. I have now, together with Prof. Hahn, in rather protracted and laborious experiments, verified the conclusion that UX_1 emits no other γ radiation apart from this K_α radiation.[8]

Meitner considered this investigation to constitute strong support for her view, but Ellis and Skinner did not agree with her and found it easy to repudiate her conclusions.[9] She had observed one electromagnetic ray, and since it lies midway between the K_{α_1} and K_{α_2} X-ray lines, she had interpreted it as being the average value of these two lines. According to Ellis and Skinner, this was an incorrect interpretation:

> Even if both these radiations were emitted, and the corresponding groups could not be separated, it follows from the experimental arrangement that the part of the photographic trace to which measurements are taken would correspond to the harder component and not to the mean of these two.[10]

In addition, Meitner found the beta line from the L level to possess an energy of 0.711×10^5 electron volts. This was more than 1 percent from any energy difference that could be formed from the sets (K_{α_1}, K_{α_2}) and (L_{I}, L_{II}, L_{III}), and "her measurements were certainly not one per cent. in error."[11]

Ellis and Skinner did not see any evidence at all that the X-ray spectrum is emitted. On the contrary, they considered the emission to be a gamma ray arising from the nucleus, as in other cases; it was a mere coincidence that its energy happened to be near that of the K radiations. They considered UX_1 to be a normal beta-ray body emitting a soft gamma ray from the nucleus, and this proved to be correct. Meitner was led astray by this coincidence in energies, but she was very reluctant to admit it. In a review article of 1924, written after she had been acquainted with the work of Ellis and Skinner, she still maintained her own interpretation, and she did not mention at all their objection to it.[12]

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[8] Meitner to Rosseland, 1 June 1923, MTNR 5/15. The work mentioned in the quotation is O. Hahn and L. Meitner, "Die γ-Strahlen von UX und ihre Zuordnung zu Uran X_1 und Uran X_2," *Z. Phys. 17* (1923), 157–167.

[9] C.D. Ellis and H.W.B. Skinner, "The Interpretation of β-Ray Spectra," *Proc. Roy. Soc. A105* (1924), 185–198.

[10] *Ibid.*, p. 196.

[11] *Ibid.*, p. 196.

[12] L. Meitner, "Der Zusammenhang zwischen β- und γ-Strahlen," *Ergebnisse der Exakten Naturwissenschaften 3* (1924), 160–181.

The diffuse band in the UX_1 beta spectrum was also discussed by Ellis and Skinner. They agreed with Meitner that it was made up of disintegration electrons; but unlike her, they took it as a "very good example of the varying velocity which these electrons always appear to possess once they are outside of the atom."[13] They continued:

> As there is no evidence of the excitation of the K spectrum, we have to assume these electrons leave the nucleus with different velocities. This phrase "leaving the nucleus" introduces difficulties. It is clear that the electron before it leaves the nucleus is in a quantised state, with definite energy, but once outside the atom its energy is found to vary over a considerable range. We have seen that there is no evidence that any inhomogeneity is introduced in passing through the K ring and out to the surface, and so the actual observed inhomogeneity must be effected before this. Whether this is called in the nucleus or not is at the moment immaterial.[14]

They imagined that the very intense electric field near the nucleus might well be the cause of the inhomogeneity. That raised problems concerning the absence of general gamma radiation, but we shall postpone a discussion of that question to a subsequent section.

As far as I know, Ellis had never before so clearly stated his view on the origin of the continuous beta spectrum. He found it quite conceivable that the inhomogeneity arose *outside* the nucleus. The essential point, according to Ellis, was that it arose *inside* the K level. Nothing indicates, however, that this marks a change in his view. He only expressed himself more explicitly; but Meitner took it as a concession that Ellis and Skinner had now abandoned their earlier point of view, that the β particles do not leave the nucleus with a well-defined velocity.[15]

4.3 Radiationless transitions: Rosseland suggests an explanation of the emission of primary, and some secondary, beta particles

In early 1923, while staying in Copenhagen, the Norwegian physicist Svein Rosseland contributed to the discussion of the applicability of quantum theory to radioactive processes. "One is above all led to invoke the quantum theory," Rosseland wrote, "because of the existence, proved by various researchers, of primary γ-ray spectra consisting of lines originating from the nucleus."[16] It was thus obvious,

[13]Ellis and Skinner, "Interpretation" (note 9), p. 197.
[14]*Ibid.*, p. 197.
[15]Meitner, "Zusammenhang" (note 12), p. 178.
[16]S. Rosseland, "Zur Quantentheorie der radioaktiven Zerfallsvorgänge," *Z. Phys.* *4* (1921), 173–181, p. 173.

in the light of the Bohr theory, to assume that the nucleus can exist only in a number of stationary states, and that homogeneous gamma rays are emitted by transitions between these states. But how could the radioactive emission of the alpha rays and primary beta rays be explained quantum-theoretically?

Rosseland saw a possible explanation in his and Oskar Klein's collisions of the second kind, developed from a rather simple consideration.[17] An ordinary collision, called a collision of the first kind, between a positively charged atom and an electron leaves the atom in an excited state, and for certain velocities the electron has a chance of being bound in the atom. If the mixture of positive atoms and free electrons is in thermodynamic equilibrium, the opposite process, that is, a spontaneous emission of an electron from an atom in an excited state also has to take place, they argued, and such a process is an example of a collision of the second kind. Thus, in general, an excited atom possesses two different possibilities of spontaneous transitions, one resulting in the emission of electromagnetic radiation, the other in the emission of particles. Rosseland imagined that these radiationless transitions take place in the X-ray domain where the energy released in a transition is sufficient to emit an electron from the atom.

The really new concept in Rosseland's paper, however, was his idea that radiationless transitions should also occur in the nucleus. He suggested that the emission of alpha and beta particles is, in a sense, analogous to collisions of the second kind. In a letter of 19 February 1925 to Pierre Auger, Bohr emphasized Rosseland's idea.[18] Auger had just established experimentally the existence of collisions of the second kind, and since he was the first to do so, they are now known as the Auger effect.[19] Meitner did Rosseland more justice by calling it the Rosseland–Auger effect.[20] In his letter, Bohr told Auger that Rosseland had suggested, on the basis of theoretical considerations, the possibility of collision processes of the same kind as those observed by Auger. Bohr continued:

> Rosseland's idea was that in such processes one should possess a quantum mechanical model exhibiting an analogy with the radioactive phenomena, where particles spontaneously may leave the nucleus of the atom.[21]

Rosseland was quite aware of the difficulty of settling experimentally the existence of radiationless transition in the nucleus, but the absence of gamma rays of very high energies made them probable:

> If ... an absorption process, such as the one considered by Ellis [i.e., a photoelectric effect], were the only possibility, one would be led to

[17] O. Klein and S. Rosseland, "Über Zusammenstösse zwischen Atomen und freien Elektronen," *Z. Phys. 4* (1921), 46–51.

[18] Bohr to Auger, 19 February 1925, *BSC* (9.1).

[19] P. Auger, "Sur le rendement de la fluorescence dans le domaine des rayons X," *C.R. 182* (1926), 773–775.

[20] L. Meitner, "Kernstruktur," *Handbuch der Physik XXII/1* (Berlin: Springer, 1933), 118–153, p. 150.

[21] Bohr to Auger, 19 February 1925, *BSC* (9.1).

> interpret the appearance of the primary β particles as arising from the absorption of a hard γ radiation originating in the same nucleus. The wavelength of this radiation would have to be much harder than that of γ radiation measured so far, and of the order of magnitude 10^{-11} cm. If this explanation were correct, one would also have to expect the appearence of such extremely hard γ radiation from all β-radiating substances. Since so far we have no indication of such a radiation, we can only explain the primary β rays by entirely radiationless quantum transitions in the nucleus.[22]

Meitner was enthusiastic about the idea of *atomic* radiationless transitions. In a letter of 1 June 1923 to Rosseland, she emphasized that the result of her recent investigation of the beta spectrum of UX_1 agreed well with this idea:

> Already half a year ago I ... obtained results for UX_1 which can be interpreted quite beautifully according to your conception of radiationless transitions with emission of electrons. ... Your interpretation is all the more unified and complete as there is no need for the intermediate step of K_α radiation.[23]

The application of radiationless transitions to the *nucleus* Meitner found more problematic:

> A carrying over of this conception to the processes in the nucleus, as Rosseland suggests, in order to reach a quantum-theoretical interpretation of β decay, would require that all transitions in the nucleus are radiationless, i.e., occur with electron emission, since of course only electron emission leads to an atomic transformation. This would furthermore imply that there could be no direct connection between β and γ radiation, while the fact that for substances emitting β and γ radiation simultaneously, the decay with time of both kinds of radiation is identical, points to a close connection between the two emission processes.[24]

The fate of Meitner's letter of 1 June is somewhat uncertain. On 20 July, Rosseland wrote to Meitner that Hevesy had informed him that her letter had arrived in Copenhagen, but probably because he was on a long tour in Norway and the letter had been forwarded several times, he had not yet received it.[25] Meitner sent him a copy, dated 24 July,[26] but in the meantime, Rosseland must have received the first letter, since he answered Meitner in a letter of 22 July 1923. In this answer, he went further into the question as to why he had suggested radiationless transitions as a probable explanation of the radioactive processes.

[22] Rosseland, "Zur Quantentheorie," (note 16), p. 175.
[23] Meitner to Rosseland, 1 June 1923, MTNR 5/15.
[24] Meitner, "β-Strahlenspektrum" (note 1), p. 64.
[25] Rosseland to Meitner, 20 July 1923, MTNR 5/15.
[26] Meitner to Rosseland, 24 July 1923, MTNR 5/15.

When he had read her paper a year and a half earlier, it had induced him to ponder how such a process could be interpreted on the basis of the quantum theory. He continued:

> I came to the conclusion that the only way to interpret your hypothesis would be to say that the emission of the primary gamma ray takes place through quantum transitions of the decay electron, in such a way that the electron loses its entire energy (precisely as suggested later by Adolf Smekal), which in practice would be equivalent to the requirement that an electron in colliding with an atomic nucleus (having its total number of outer electrons) must lose either none or all of its total energy. This requirement is – as Bohr has told me (see Z. f. Phys. 13, p. 155 top)[27] – difficult to reconcile with the fact that the maximum *frequency* of the continuous X radiation shows no Doppler shift, while this is the case for the maximum *intensity*.[28]

Accordingly, he was unable to find a quantum-theoretical interpretation of Meitner's view, and that made him propose radiationless transitions as an alternative. Meitner's objection to this alternative was incomprehensible to Rosseland:

> I must confess that I have not quite understood the remark ... in your letter that only radiationless quantum transitions should take place in the nucleus. From the basic principles of the quantum theory it can of course only be concluded that alpha, beta, or gamma transitions are possible in any order and with almost any decay probability.[29]

As we have seen, both Smekal and Rosseland introduced the concept of radiationless transitions, but they used it in different ways. The essential idea in Smekal's view was to consider the atom as a whole, and therefore he dealt with radiationless transitions from one element to another. Rosseland, however, distinguished between radiationless transitions inside and outside the nucleus.

4.4 The nuclear field and the Compton effect: Two possible reasons for the continuous beta spectrum

In his paper, Rosseland assumed, in accordance with quantum theory, that the primary beta particles are emitted from the nucleus with a definite velocity. How,

[27]N. Bohr, "Über die Anwendung der Quantentheorie auf den Atombau. I. Die Grundpostulate der Quantentheorie," *Z. f. Phys. 13* (1923), 117–165, on p. 155; translated into English as N. Bohr, "On the Application of the Quantum Theory to Atomic Structure. Part I: The Fundamental Postulates," *Proc. Camb. Phil. Soc. 22* (1924), Supplement. The latter version is reproduced in *BCW*, Vol. 3, *The Correspondence Principle (1918–1923)*, J. Rud Nielsen (ed.) (Amsterdam: North-Holland, 1976), pp. 457–499, on p. 489.

[28]Rosseland to Meitner, 22 July 1923, MTNR 5/15.

[29]*Ibid.*

then, could the continuous beta spectrum be explained? Rosseland noted that, owing to the nuclear field, the electrons are being accelerated while escaping from the atom. This acceleration causes an emission of radiation, which can be analyzed by Bohr's correspondence principle. The motion of the escaping particle is resolved into its harmonic components. Owing to the aperiodic character of this motion there must be a continuous range of amplitudes, and thus also of transition possibilities. The frequency relation ensures that the radiation emitted in one single transition is homogeneous, but the total radiation from a large number of radioactive atoms must be inhomogeneous with a short-wavelength limit corresponding to the total kinetic energy of the electron.[30]

Whether the amount of emitted radiation was large enough to cause the considerable inhomogeneity in the velocity of the beta rays, and also preserve the line character of the alpha rays, was still questionable. Rosseland made some calculations, but obtained no decisive results. He had to pin his faith on future experiments.[31]

Ellis was sympathetic to Rosseland's idea. In their joint paper, he and Skinner had argued that the inhomogeneity must be effected inside the K ring, and continued:

> There must be very intense electric fields associated with the nucleus and the inhomogeneity could well be due, as Rosseland has suggested, to the emission of general γ-radiation, resulting from the violent acceleration of the electron.[32]

Meitner, however, was not satisfied with this explanation of the continuous spectrum. In her letter of 1 June 1923 to Rosseland, she wrote that Professor Wilhelm Lenz in Hamburg also held the opinion that the continuity in energy is due to the nuclear field, and that she had answered him that these radiative processes had of course to occur, but that she saw a difficulty in that the continuous γ spectrum associated with them was not present.[33] RaE, for instance, emits intensive beta rays of velocities from $0.75c$ to $0.90c$. Thus, a rather strong and penetrating γ-radiation had to be present if Rosseland was right, but it had never been observed.

Rosseland agreed with Meitner's doubts. He was aware of the problem, but had refrained from going into experimental questions:

> I am strongly inclined to accept your conception that the beta spectrum from RaE cannot be explained in this way. I too thought about it at the time, but since I have no personal appreciation of experimental subtleties, I refrained from dealing with it.[34]

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[30]Rosseland, "Zur Quantentheorie" (note 16), p. 177.
[31]*Ibid.*, p. 180.
[32]Ellis and Skinner, "Interpretation" (note 9), p. 197.
[33]Meitner to Rosseland, 1 June 1923, MTNR 5/15. To my knowledge, Lenz did not publish anything on this subject.
[34]Rosseland to Meitner, 22 July 1923, MTNR 5/15.

On the whole, Rosseland expressed unhappiness about the way his paper was received. It was exclusively concerned with theoretical considerations, he felt, and should be evaluated from that point of view, as he had emphasized sufficiently in his introduction.[35]

In the face of the difficulty in finding a satisfactory explanation of the inhomogeneity in energy, the newly discovered Compton effect gave new hope.[36] In the autumn of 1923, Meitner suggested that here perhaps was the origin of at least part of the continuous spectrum,[37] and Marie Curie concurred in 1926.[38]

For several years, it had been known that X rays, when scattered by matter, increase in wavelength, and that this increase varies with the scattering angle. The American experimentalist Arthur Holly Compton and, independently, Peter Debye in Zurich found a theoretical explanation of this phenomenon.[39] Each assumed that the X ray is a particle of energy $h\nu$ and momentum $h\nu/c$ and considered the interaction of an X ray with an electron as a two-particle collision. Their calculated shift in wavelength agreed well with Compton's experimental data.

From the expression for the energy, E_β, transferred to an almost free electron,

$$E_\beta = h\nu_0 \frac{\frac{2}{x}\sin^2\frac{\theta}{2}}{1 + \frac{2}{x}\sin^2\frac{\theta}{2}},$$

where θ is the scattering angle, $x = \frac{mc^2}{h\nu_0}$, and ν_0 the initial frequency of the light quantum, Meitner inferred that for a certain frequency ν_0, E_β varies from zero to a maximum value given by

$$E_\beta = h\nu_0 \frac{1}{1 + \frac{x}{2}}. \tag{4.1}$$

"Therefore, when γ rays are scattered from free, or almost free, electrons, then to this scattering process is linked the appearance of a continuous β-ray spectrum whose limit on the side of the fast rays is determined by equation [(4.1)]."[40]

According to Meitner, and Curie as well, Compton scattering was "a plausible explanation"[41] of the continuous beta spectrum, or at least of part of it, but they were both quite aware that the problem remained in cases where no gamma rays were emitted. The continuous beta spectrum of RaE remained a riddle.

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[35] *Ibid.*

[36] For a detailed historical account of the Compton effect, see R.H. Stuewer, *The Compton Effect: Turning Point in Physics* (New York: Science History Publications, 1975).

[37] L. Meitner, "Über eine mögliche Deutung des kontinuierlichen β-Strahlenspektrums," *Z. Phys. 19* (1923), 307–312; *idem.*, "Über eine notwendige Folgerung aus dem Comptoneffekt und ihre Bestätigung," *Z. Phys. 22* (1924), 334–342.

[38] M. Curie, "Sur l'application de la theorie de Compton au rayonnement β et γ des corps radioactifs," *J. de Phys. 7* (1926), 97–108.

[39] A.H. Compton, "A Quantum Theory of the Scattering of X Rays by Light Elements," *Phys. Rev. 21* (1923), 483–502; P. Debye, "Zerstreuung von Röntgenstrahlen und Quantentheorie," *Phys. Zeit. 24* (1923), 161–166.

[40] Meitner, "Mögliche Deutung" (note 37), p. 309.

[41] *Ibid.*, p. 312.

4.5 Ellis and Skinner reinvestigate the beta line-spectra of RaB and C, and serious problems arise

In collaboration with the young experimentalist Herbert Skinner, Ellis decided to carry out a new investigation of the beta line-spectra of RaB and RaC. Their results, and an interpretation of them, were submitted to the *Proceedings* of the Royal Society in December 1923.[42] Previous interpretations were based on the nine-year-old measurements of Rutherford and Harold Robinson.[43] Therefore, Ellis argued, a more detailed examination with an improved technique was needed, which would probably reveal novel features. Ellis was especially dissatisfied with the absolute accuracy of earlier measurements, as he indicated in an undated letter to Meitner:

> All our measurements of the β-ray lines have a fairly high relative accuracy, but I am not altogether satisfied with the absolute accuracy. Some recent measurements I have made suggest that there may be some discrepancies.[44]

Ellis still used the well-known focusing method, but to improve the absolute accuracy of the measurements, the magnetic field was now measured directly with a search coil. In previous experiments Ellis had calibrated the field by comparison with the old results of Rutherford and Robinson.

The separation of the beta lines into RaB and RaC lines was not an easy task, and Ellis and Skinner admitted that in some cases the allocation to each was rather doubtful. Yet they succeeded in obtaining what they considered to be a complete RaB spectrum. They divided it into three groups, as shown in Table 4.4, where its interpretation and a comparison with the old results of Rutherford and Robinson are also included. As to the RaC spectrum, only the lines within the limits of the RaB spectrum were investigated.

They found new lines as well as slightly different $H\rho$-values, but even if the interpretation of some of the lines was somewhat questionable, they still strongly believed that the great majority of the beta lines were due to conversion of gamma rays:

> This is certainly correct in the case of the excited spectra where the γ-ray is emitted by one type of atom, radium B, and absorbed by another, lead or platinum. Since, however, radium B emits exactly similar β-ray groups to those ejected from its isotope, lead, by the γ-rays of radium B, it is natural to assume that the radium B β-ray groups are also due

[42] C.D. Ellis and H. Skinner, "A Re-Investigation of the β-Ray Spectrum of Radium B and Radium C," *Proc. Roy. Soc. A105* (1924), 165–184; *idem.*, "Interpretation" (note 9); *idem.*, "The Absolute Energies of the Groups in Magnetic β-Ray Spectra," *Proc. Roy. Soc. A105* (1924), 60–69.

[43] E. Rutherford and H.R. Robinson, "The Analysis of the β Rays from Radium B and Radium C," *Phil. Mag. 26* (1913), 717–729.

[44] Ellis to Meitner, undated, MTNR 5/4.

Name	Int.	Previously accepted $H\rho$	$H\rho$ measured	Energy $\times 10^5$ volts	Origin	γ-ray $\times 10^5$ volts
$C1$	8	663	660.9	0.3725	L_{I}	0.536
$C2$	3	–	667.0	0.3792	L_{II}	0.536
$C3$	1	–	687.0	0.4016	L_{III}	0.536
$C4$	6	770	768.8	0.4983	M_{I}	0.538
$C5$	4	798	793.1	0.5288	N_{I}	0.538
$C6$	2	–	799.1	0.5365	O	0.538
$D1$	1	836	833.0	0.5806		
$D2$	3	–	838.0	0.5872		
$D3$	1	–	855.4	0.6106		
$D4$	3	861	860.9	0.6172		
$D5$	1	–	877.8	0.6412		
$D6$	1	–	896.0	0.6667		
$D7$	2	914	926.2	0.7094		
$D8$	2	950	949.2	0.7426		
$E1$	2	–	1155	1.068	$K(?)$	1.943
$E2$	1	–	1209	1.160	$K(?)$	2.035
$E3$	25	1392	1410	1.529	K	2.404
$E4$	6	1470	1496	1.697	K	2.572
$E5$	3	–	1576	1.860	$K(?)$	2.733
$E6$	30	1660	1677	2.067	K	2.942
$E7$	8	1752	1774	2.275	L_{III}	2.404
$E8$	3	1815	1850	2.442	L_{III}	2.572
$E9$	40	1925	1938	2.638	K	3.511
$E10$	9	1990	2015	2.824	L_{III}	2.942
$E11$	3	–	2064	2.926	M	2.942
$E12$	1	2140	2110	3.033	L_{III}	3.163
$E13$	10	2235	2256	3.379	L_{III}	3.511
$E14$	5	2295	2307	3.502	M	3.511
$E15$	1	–	2321	3.536	N	3.511
$E16$	1	2450	2433	3.809	K	4.684
$E17$	1	–	2480	3.925	K	4.800

Table 4.4: The beta-ray lines observed by Ellis and Skinner in 1923. The interpretation of the groups C and E appears in the table, whereas the interpretation of group D was much less clear and is omitted. To explain the beta lines in the E group, Ellis and Skinner had to assume the existence of 10 gamma rays, named $E1$, $E2$, $E3$, $E4$, $E5$, $E6$, $E9$, $E12$, $E16$ and $E17$. Source: Ellis and Skinner, "Re-Investigation" (note 42), p. 170.

> to the same process. ... We regard this as the best and simplest view to take of the origin of these groups.[45]

They were quite aware that it might be necessary to take into account other possibilities. They even found it probable that certain lines in the D group of RaB were due to Rosseland's atomic radiationless transitions. Furthermore, if a strong coupling between the nucleus and the electronic distribution is present, it might be necessary to join Smekal and consider the atom as a whole, but they did not really believe this:

> We feel that at the moment the general evidence indicates that the nucleus is so distinct from the electronic system that the only method of transferring energy from one to the other is by radiation.[46]

Whatever the correct view of the origin of the beta lines might be, their analysis confirmed the existence of stationary states in the nucleus, and this was the really important result of their measurements. They succeeded in setting up an energy-level diagram of the RaB nucleus that explained all of the gamma rays except one ($E12$). This is shown in Figure 4.1, which is quite different from Ellis's earlier RaB diagram, whereas it has a certain resemblance to the modern level diagram of ${}^{214}_{83}$Bi (see Figure 3.3).

Accordingly, Ellis found some of his views to be confirmed, but he also ran into problems. One of them concerned the absolute intensity of the lines. The number of electrons in the three strong lines ($E3$, $E6$, $E9$) proved to be at least of the order of one-tenth of the number of atoms disintegrating. By taking the internal-conversion view on the origin of the beta-ray lines, the probability of absorption of gamma rays thus had to be about 0.1. Could this be explained? Ellis and Skinner tried, but in vain:

> By making special assumptions about those properties of radiation on which absorption depends we can calculate the value of the probability of internal absorption from the value of the ordinary absorption coefficient, which is known for these γ-rays from direct measurements. We have tried the assumptions that the probability of absorption is proportional to either the intensity of radiation or the density of radiation at the place where the absorption occurs (the K ring), and, further, a pure light dart hypothesis in which the probability is simply equal to the chance of hitting an area round the ring where absorption takes place. All these assumptions give values far too small, the best is the light dart one and that is fifteen times too small.[47]

This problem developed into an important one, but we shall postpone a further discussion of it to Chapter 7.

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[45] Ellis and Skinner, "Interpretation" (note 9), p. 185.
[46] *Ibid.*, p. 186.
[47] *Ibid.*, p. 187.

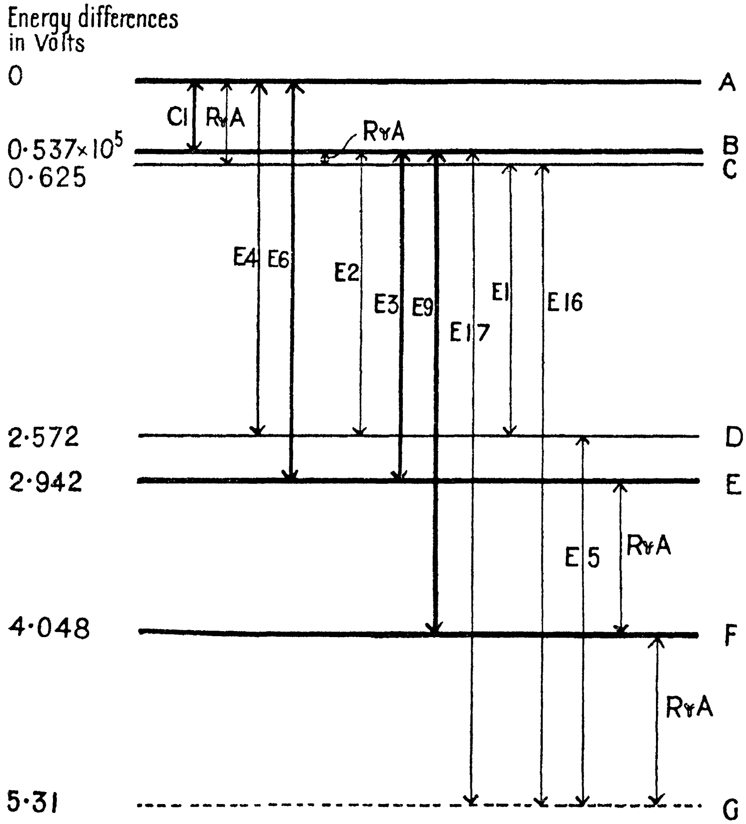

Figure 4.1: The energy-level diagram of radium B, set up by Ellis and Skinner in 1924. The R & A-transitions refer to lines observed by Rutherford and Andrade in 1914; the other designations refer to Table 4.4. Source: Ellis and Skinner, "Interpretation" (note 9), p. 190.

Relative Absorbing Powers of the L Sub-groups for Different Radiations

Region	Energy of γ-ray	Order of Intensity
Absorption Limit	0.16×10^5 volts	L_{III}, L_{II}, L_{I}
C	0.54	L_{I}, L_{II}, L_{III}
D	0.8	L_{I}, L_{III} (L_{II})
E	3.0	L_{III} (L_{II}, L_{I})

Table 4.5: The relative intensities of the L lines of RaB for four different gamma-ray energies. A level in parantheses means that the corresponding line was too faint to be seen. Source: Ellis and Skinner, "Interpretation" (note 9), p. 189.

The relative intensities of the lines originating in the L sub-levels caused further problems. These intensities appear in Table 4.5 for four different gamma-ray energies. As to the C region, for instance, the L lines are due to a gamma ray of energy 0.54×10^5 volts. Ellis and Skinner observed here that the intensity of the L_{I} line is by far the highest, and that the intensities of L_{II} and L_{III} followed in that order on a very steeply descending scale. This result seemed to be inconsistent with available evidence on X rays, where absorption in the L_{III} level always proved to be the largest; but there was no real conflict, Ellis and Skinner argued:

> [All] these absorptions really refer to absorption of radiation, whose quantum energy is only just above the characteristic energy of the level, and in the γ-ray case it is considerably above, so there is no real conflict.[48]

A recent paper by Robinson supported this view.[49] He had analyzed the electrons ejected from a target by homogeneous X rays and found that the relative absorbing power of the L sub-groups undergoes marked changes as the difference in energy between the radiation and the level in question increases. If the difference is small, the radiation is almost exclusively absorbed in the L_{III} ring, if large the L_{I} is dominant.

Accordingly, it appeared that Robinson's result agreed well with Ellis and Skinner's, but the agreement was far from complete. In the E region, all of the L lines are due to conversion of very hard gamma rays. Nevertheless, according to the two Cambridge physicists, they are all L_{III} lines, which was not consistent with Robinson's observations. Ellis and Skinner had no convincing explanation of this discrepancy, but Meitner did:

> Ellis and Skinner make calculations on the ionization work of the non-decayed parent atom. ... If one assumes on the contrary that also the E group has its origin in the daughter atom RaC, then, in order to maintain agreement with the experimental values, one has to invoke the L_{I} level as the triggering level.[50]

Even if a third problem pointed in the same direction, Ellis and Skinner were reluctant to take that step in early 1924. What happened later, and what this third problem was, I will discuss in the next section.

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[48] *Ibid.*, p. 188.

[49] H.R. Robinson, "The Secondary Corpuscular Rays Produced by Homogeneous X-rays," *Proc. Roy. Soc. A104* (1923), 455–479.

[50] Meitner, "Zusammenhang" (note 12), pp. 176–177.

4.6 Beta first, gamma second, or is it the other way around?

During the years 1923 to 1925 the question of the temporal order of emission of beta and gamma rays developed into an important controversy between Meitner and Ellis, on a par with that of the nature of the continuous spectrum.

In his early theories of 1912 and 1914, Rutherford held the opinion that the disintegrating electrons were emitted first.[51] Then, in passing through the electronic distribution, they caused the emission of the gamma rays. In 1914, however, Rutherford and Edward N. da C. Andrade's investigations seemed to show that the L X-ray spectrum of radium B was similar to that of lead.[52] Since RaB and lead are isotopic, the gradual recognition that gamma rays have their origin in the nucleus suggested that the gamma rays, producing vacancies in the electronic structure and hence causing the emission of X rays, are emitted from RaB and not from RaC, i.e., *before* the disintegration electron.

This interpretation of Rutherford and Andrade's result was not quite obvious. The electronic configuration might well persist for a certain period after the change in the nuclear charge, i.e., there might be a time of relaxation. Both Meitner and Ellis pointed to this alternative possibility in the early 1920s. Accordingly, the possibility still existed that the gamma ray was emitted after the beta ray, but before the electronic configuration had changed.

Another way of settling this question might be to measure the natural beta spectrum of RaB and the beta spectrum of platinum excited by RaB gamma rays. The energies of two corresponding lines of RaB and Pt consisting of K electrons are given by

$$E_{\mathrm{RaB}} = h\nu - K_{\mathrm{RaB}}$$

$$E_{\mathrm{Pt}} = h\nu - K_{\mathrm{Pt}},$$

where $h\nu$ is the energy of the gamma ray. The absorption energy, K_{RaB}, could then be determined from the equation

$$E_{\mathrm{Pt}} - E_{\mathrm{RaB}} = K_{\mathrm{RaB}} - K_{\mathrm{Pt}}.$$

If, on the one hand, this result agreed with the absorption energy for $Z = 83$, the gamma ray had to be emitted after the disintegration. If, on the other hand, it agreed with the absorption energy for $Z = 82$, the question could not be settled owing to a possible time of relaxation. Such experiments were carried out frequently, but not yet with sufficient accuracy. Thus, in the early 1920s, one had

[51] E. Rutherford, "The Origin of β and γ Rays from Radioactive Substances," *Phil. Mag. 24* (1912), 453–462; *idem.*, "The Connexion Between β and γ Ray Spectra," *Phil. Mag. 28* (1914), 305–319.

[52] E. Rutherford and E.N. da C. Andrade, "The Wavelength of the Soft γ Rays from Radium B," *Phil. Mag. 27* (1914), 854–868; *idem.*, "The Spectrum of Penetrating γ Rays from Radium B and Radium C," *Phil. Mag. 28* (1914), 263–273.

to be satisfied with hypothetical views, and Ellis and Meitner adopted opposite ones.

Strongly influenced by Rutherford and Andrade's experiment, Ellis assumed that the gamma rays are emitted first. In doing so, he avoided the auxiliary hypothesis of a time of relaxation, and, furthermore, he considered the continuous beta spectrum as a support for it, as is evident from a letter of 19 January 1923 from Meitner:

> It seems to me, however, that above all it is the continuous spectrum which is responsible for the difference in our views, for, as you have strongly emphasized yourself in your paper in the Zeitschrift für Physik, that is the main basis for your opinion that the γ rays are emitted before the nuclear decay.[53]

Late in 1923, however, Ellis's view was seriously threatened, and by his own results at that. In the previous section, we discussed the problems with the relative intensities of the L sub-groups, but another problem was even more serious. In the C region, Ellis observed six lines, apparently excited by the same gamma ray acting on the levels L_{I}, L_{II}, L_{III}, M, N, and O. Ellis and Skinner tried to establish agreement with the levels of a nucleus of atomic number 82, but met with unexpected difficulties. No agreement seemed possible:

> We have calculated the minimum change in the absolute energies that would be necessary to give agreement, and believe it to be far greater than the possible error in the absolute determination, and our relative values are certainly not in error to this amount. The groups are too widely separated to correspond to atomic number 82 ... .[54]

It looked as if the $C1$ gamma ray was emitted after the disintegration, whereas the E-region gamma rays might well be emitted before. Still, the difference in energy between the gamma lines $E6$ and $E3$ was almost identical with the energy of $C1$, and therefore they included $C1$ in the transition scheme of the RaB nucleus after all.

In a desperate attempt to defend their view, Ellis and Skinner put forward a curious *ad hoc* hypothesis. The electronic levels may have the power of holding absorbed radiation for a finite period before emitting the electron, they argued, and perhaps this period varies inversely as the energy of the radiation, so that soft rays like $C1$ do not emit electrons until the electronic distribution has changed.

Ellis and Skinner did not believe strongly in this hypothesis themselves, and instead proposed that the most direct interpretation was

> ... to consider the disintegration electron to be in such a position in the nucleus that its removal alters the energy value of the B and A states to approximately the same extent. In this case we are free to assume

[53]Meitner to Ellis, 19 January 1923, MTNR 5/4.
[54]Ellis and Skinner, "Re-Investigation" (note 42), p. 177.

> that whereas the majority of the transitions occur before the emission of the disintegration, there is such a coupling that the transition B–A always occurs after this event.[55]

Perhaps the most direct interpretation would have been to say that *all* of the transitions occur after the disintegration, but Ellis and Skinner's belief in the Rutherford–Andrade experiment was still too strong to go that far.

Ellis's biographers recalled that Ellis was "somewhat perplexed" and "uneasy"[56] about these problems. In 1924 and 1925, the Cambridge group was much engaged in trying to solve them. We shall follow these efforts, but before doing so, we return to Berlin, where Meitner was no less occupied with the question of the temporal order of beta and gamma emission.

In contrast to Ellis, Meitner had adopted the view that beta rays are emitted first, but until 1924 she was not much concerned with that question. In 1924, however, her idea of an analogy between alpha-ray and beta-ray emission gained further support. Previously, physicists believed that only beta rays are accompanied by gamma rays, but recent investigations had shown that this was true of some alpha-emission processes too.[57] This evidence supported the analogy between alpha and beta decay,[58] and Meitner was induced to propose a view of the decay processes built upon this analogy. A nucleus can react in two different ways when emitting an alpha or a beta particle, she argued:

1. A *radiationless* change in the configuration of the nuclear particles may take place, in analogy to that of the atomic electrons after an ionization process.
2. A *quantum* change in the configuration of the nuclear particles may occur, causing the emission of homogeneous gamma rays.

Such alpha emitters as ionium, polonium and thorium C′ belong to the first category, as do the beta emitters uranium X_1, radium E and thorium C, whereas radium B, radium D, thorium B, etc., emit gamma radiation and belong to the second category.

Meitner had thus abandoned the idea of a close connection between the beta-ray and gamma-ray energy. In Cambridge, this connection was always considered to be an "arbitrary" assumption;[59] now Meitner realized that it probably was incorrect.[60]

A necessary consequence of Meitner's view is that the beta particle is emitted before the gamma ray, or at least at the same time. It had thus become crucial to

[55] Ellis and Skinner, "Interpretation" (note 9), p. 192.

[56] K. Hutchison, J.A. Gray and H.S.W. Massey, "Charles Drummond Ellis," *Biog. Mem. Fel. Roy. Soc. 27* (1981), 198–233, pp. 207–208.

[57] O. Hahn and L. Meitner, "Das β-Strahlenspektrum von Radium und seine Deutung," *Z. Phys. 26* (1924), 161–168.

[58] L. Meitner, "Über die Rolle der γ-Strahlen beim Atomzerfall," *Z. Phys. 26* (1924), 169–177.

[59] C.D. Ellis, "The Interpretation of β-Ray and γ-Ray Spectra," *Proc. Camb. Phil. Soc. 21* (1922), 121–128, p. 127.

[60] Meitner, "Über die Rolle" (note 58), p. 170.

Meitner to settle the question of the temporal order of emission, and this question now occupied much of her time. There were several indications that she was correct, she argued, mentioning some of the observations of Ellis and Skinner that we have already discussed. Ellis was difficult to convince, however. He still clung to the old Rutherford–Andrade experiment. On 18 October 1924, he wrote to Meitner:

> There is just one point I would like to make. I think some γ-rays at least must be emitted and converted before the disintegration takes place because RaB emits the L spectrum of Pb 82 (Rutherford & Andrade) whereas if *all* the γ's came after the disintegration it could only emit [the] L spectrum of 83. What is your interpretation of that?[61]

Meitner was not so convinced that Rutherford and Andrade's old experiment was correct, and in her answer to Ellis, she proposed another possible interpretation of its result:

> As far as the question of the lead L spectrum of RaB is concerned, this seems in fact at first to speak in favor of certain γ rays being absorbed in the RaB atoms. It seems, however, that another possible explanation is not entirely ruled out. In the paper by Rutherford and Andrade, it was still assumed that the β rays, and not the γ rays, excite the characteristic X rays in other substances, and from this viewpoint, it seemed safe to use a lead slit of 6 cm. Today, when we know that it is mainly the γ rays that excite the characteristic X rays, one must keep in mind that the appearance of the lead lines may be due to the lead slit. After all, Rutherford and Andrade have also themselves excited and photographed the lead spectrum in a very small lead foil by means of rays from RaB, but simply concluded that this was only an effect of the β rays and not the γ rays, a viewpoint which probably can no longer be held today. What I have said here should of course only indicate a possibility. One could test this question by taking photographs with a platinum slit instead of a lead slit.[62]

Other Cambridge results proved to favor Meitner's view. In July 1924, Donald Black presented an investigation of the beta spectrum of mesothorium 2,[63] and about a year later, he carried out a similar investigation of the beta spectra of the thorium disintegration products ThB and ThC + D.[64] Ironically, his main purpose was to show that the gamma rays deduced from the measured beta lines obey the combination principle, i.e., to corroborate the view held by Ellis that quantum dynamics could be applied to the nucleus. He was fairly successful, but "before a

[61] Ellis to Meitner, 18 October 1924, MTNR 5/4.
[62] Meitner to Ellis, 24 October 1924, MTNR 5/4.
[63] D.H. Black, "The β-Ray Spectra of Mesothorium 2," *Proc. Roy. Soc. A106* (1924), 632–640.
[64] D.H. Black, "β-Ray Spectra of Thorium Disintegration Products," *Proc. Roy. Soc. A109* (1925), 166–176.

detailed analysis can be attempted," he wrote, "it is necessary to consider whether the γ-ray is converted before the nuclear change takes place or after it."[65]

As others had before him, Black found that he was not able to decide the atomic number of the atom from which the beta lines originated (89 or 90 in the case of mesothorium 2) on purely numerical grounds. The relative intensities, however, gave some hints. If the atomic number were 89 and the converted gamma-ray energy 1.3×10^5 electron volts, then the L_{II} line (or L_{I} since they were indistinguishable) would be stronger than the L_{III} line, whereas the L_{III} was stronger than L_{II} if they were excited by a gamma ray of energy 1.8×10^5 electron volts. These relative intensities disagreed with those of RaB, where the L_{III} line is the stronger one already for a gamma ray of 0.8×10^5 volts. If, however, the atomic number were 90, the L_{II} (or L_{I}) lines always appeared to be stronger than the L_{III} lines. "Thus it would seem," Black inferred, "that there is a slight evidence to show that the atomic number is 90, and that the β-particle is ejected after the nuclear change has taken place."[66]

The result of the investigation of the thorium disintegration products pointed in the same direction. It even gave "slight further evidence"[67] in support of Meitner's view, since it proved possible to account for two additional lines in the ThB beta spectrum if the atomic number were 83 instead of 82.

Thus, several observations were in favor of Meitner's view, and around October 1925 the question was definitely settled, in Berlin as well as in Cambridge. In Berlin, the key to the solution of the problem proved to be investigations of beta-ray spectra from alpha sources. The crucial point was that when an alpha particle is emitted, the atomic number is altered by two units. This made it easier to identify from which atom the secondary beta particles originated. Measurements of radium beta lines were not accurate enough, but investigations of beta rays from radioactinium and actinium X settled the question.[68]

Meitner classified the beta lines in groups, so that each line in a group was due to conversion of gamma rays of equal energy. For each group, she calculated the energy $h\nu$ of the gamma ray by adding the appropriate absorption energies. This was done twice, using in turn the absorption energies corresponding to the two possible atomic numbers (88 and 90 for radioactinium). Meitner's results for the gamma rays of radioactinium are shown in Table 4.6. The table shows the maximum-percentage divergence between the values obtained for the energy $h\nu$ for each of the gamma rays on the basis of the two different assumptions. The figures corresponding to $Z = 88$ are the smaller ones throughout. For $Z = 90$ the divergences are frequently two to three times the measured accuracy, and

[65] Black, "The β-Ray Spectra" (note 63), p. 636.
[66] *Ibid.*, p. 639.
[67] Black, "β-Ray Spectra" (note 64), p. 175.
[68] O. Hahn and L. Meitner, "Das β-Strahlspektren von Radioaktinium und seinen Zerfallsprodukten," *Z. Phys. 34* (1925), 795–806; L. Meitner, "Die γ-Strahlung der Actiniumreihe und der Nachweis dass die γ-Strahlen erst nach erfolgtem Atomzerfall emittiert werden," *Z. Phys. 34* (1925), 807–818.

Number of γ ray	1	2	3	4	5	6	7	8	9	10
Percentage divergence if γ ray follows disintegration	3.2	1.0	0.6	0.85	1.0	0.5	1.1	0.2	0.9	0.6
The same if γ ray precedes disintegration	5.4	2.6	1.2	2.8	1.5	3.1	3.8	2.0	2.3	2.1

Table 4.6: Meitner's maximum-percentage divergences between the $h\nu$ values obtained for each of the gamma rays from RdAc if (a) the gamma ray follows the disintegration, and (b) the gamma ray precedes the disintegration. Source: E. Rutherford, J. Chadwick and C.D. Ellis, *Radiations from Radioactive Substances* (Cambridge University Press, 1930), p. 354.

since similar results were obtained for actinium X, Meitner found the conclusion obvious: the emission of the secondary beta rays must necessarily take place in the daughter atom.[69]

Meitner dwelt a little on the question of whether she really had delivered a definite proof. Ellis had put forward the hypothesis that the electronic levels may be able to hold absorbed radiation for a finite period before emitting the electrons. Could this hypothesis be rejected? She argued that it could if one believed, as she now did, in the existence of Rosseland's radiationless transitions, i.e., that the nucleus may transform from a higher energy state into a lower one without emitting a gamma ray but instead a secondary electron of equivalent energy:

> For according to Rosseland's interpretation, emissions of γ rays, and photoeffects occurring instead of these, are processes that take place under identical conditions. It therefore would be absolutely impossible to assume that, e.g., the absorption of the γ rays might take place in the daughter atom, but their emission in the parent atom.[70]

The Cambridge group presented its contribution to the clarification of the problem in a series of papers in the November 1925 issue of the *Proceedings* of the Cambridge Philosophical Society. Rutherford and William A. Wooster felt that some observations pointed to the need for a repetition of the old Rutherford–Andrade experiment. They used the arrangement shown in Figure 4.2.[71] Their source was a radon tube of 100 millicuries activity, and they employed a slit of 3 mm width to give a beam of considerable divergence. To prevent the photographic plate from being fogged by beta rays, the source, lead slit and crystal were placed between the poles of a large electromagnet delivering a magnetic field of 2500

[69] *Ibid.*, p. 812.
[70] *Ibid.*, p. 809.
[71] E. Rutherford and W.A. Wooster, "The Natural X-Ray Spectrum of Radium B," *Proc. Camb. Phil. Soc. 22* (1923–1925), 834–837.

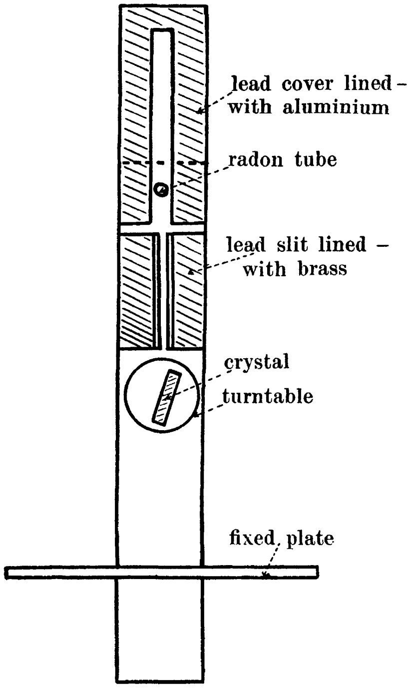

Figure 4.2: The experimental arrangement used by Rutherford and Wooster to find the angle of deflection for the two strongest lines, α_1 and β_1, in the natural L X-ray spectrum of RaB. Source: Rutherford and Wooster, "Natural X-ray Spectrum" (note 71), p. 835.

gauss. Two distances, 8 and 11 cm, of the source from the crystal were used. The photographic plate was kept fixed, and the spectrum was obtained on both sides of the main beam, the angle of deflection being calculated from the separation of the two lines without reference to the central beam. Only the two strongest lines, α_1 and β_1, of the L X-ray spectrum were measured, since a knowledge of these wavelengths was sufficient for the purpose.

The results obtained from the photographs, together with the theoretical values deduced from atomic numbers 82 and 83, are shown in Table 4.7. It appears that the separations always agreed well with those deduced from atomic number 83.

To confirm the accuracy of the measurements, and to substantiate that the natural L spectrum of radium B is distinct from that of ordinary lead bombarded by electrons, the latter was photographed with exactly the same experimental arrangement. As can be seen from Table 4.7, the result of this experiment was in close agreement with that deduced from atomic number 82.

Col. I	Col. II		Col. III		Col. IV		Col. V	
Distance of crystal from plate	Distances between corresponding lines on the photographic plate							
	Measured for radon tube		Deduced for atomic numbers				Measured for lead X-rays	
			83		82			
	α_1	β_1	α_1	β_1	α_1	β_1	α_1	β_1
cm	cm	cm	cm	cm	cm	cm	cm	cm
8.00	–	5.67	6.92	5.65	7.14	5.84	7.15	5.85
	6.93	–	"	"	"	"	7.12	5.82
	–	5.61	"	"	–	–	–	–
	6.87	5.64	"	"	–	–	–	–
11.00	–	7.75	9.52	7.77	9.81	8.03	9.80	8.01
	–	7.73	"	"	"	"	9.80	8.01
	9.50	–	"	"	"	"	9.80	8.02

Table 4.7: The results obtained by Rutherford and Wooster. The spectrum on both sides of the main beam was detected, and the distances on the photographic plate between corresponding lines were measured for the two strongest lines, α_1 and β_1, of the L X-ray spectrum. For comparison, the calculated values for atomic numbers 82 and 83, and the measured ones for the corresponding lines from lead, are included. Source: Rutherford and Wooster, "Natural X-ray Spectrum" (note 71), p. 836.

Rutherford and Wooster also carried out the experiment by using a calcite crystal instead of rock salt, and obtained an even better agreement with the values deduced from atomic number 83. There was thus no doubt that "these experiments provide strong evidence that the natural L spectrum from a radon tube corresponds with that from an atom of atomic number 83."[72] Obviously, in some way the old Rutherford–Andrade experiment was incorrect, but where was the error? Not even Rutherford knew; he only commented:

> These experiments were carried out so long ago, in the early days of X ray spectroscopy, that it is difficult to trace the error to its source.[73]

Another experiment presented in the November issue of the *Proceedings* of the Cambridge Philosophical Society was carried out by Donald Black.[74] He had

[72] *Ibid.*, p. 836.
[73] *Ibid.*, p. 837.
[74] D.H. Black, "The β-Ray Spectrum of the Natural L Radiation from Radium B," *Proc. Camb. Phil. Soc. 22* (1923–25), 832–833; *idem.*, "The Analysis of the β-Ray Spectrum Due to the Natural L Radiation of Radium B," *Proc. Camb. Phil. Soc. 22* (1923–25), 838–843.

investigated the beta-ray spectrum due to the natural L radiation of RaB, and his result showed that the agreement for atomic number 83 was far better than for atomic number 82.[75] Taking 83 as the correct atomic number, he accounted for eleven of the fifteen observed lines, including all of the stronger ones. If the atomic number were 82, it was only possible to account for six lines, none of which was strong.

A third Cambridge experiment was devised by Ellis and Wooster.[76] As previously mentioned, the question of the temporal order of emission of gamma and beta rays could be settled by measuring the natural beta spectrum of RaB as well as the beta spectrum of platinum excited by RaB gamma rays, and then determining the absorption energy K_{RaB} from the equation

$$E_{\mathrm{Pt}} - E_{\mathrm{RaB}} = K_{\mathrm{RaB}} - K_{\mathrm{Pt}},$$

where E_{Pt} and E_{RaB} are the energies of two corresponding beta lines in the two spectra. A very high accuracy was required to do that, and owing to the difficulties of measuring the magnetic field, this accuracy was hard to obtain. If, however, a measurement of the magnetic field could be avoided, the experiment was far easier to carry out, and by the simple but ingenious expedient of taking photographs of both spectra simultaneously on the same plate, Ellis and Wooster succeeded in avoiding this measurement. The separation of two corresponding lines on the photographic plate then gave a direct measure of the atomic number.

The apparatus was the same as was ordinarily employed in photographing corpuscular spectra, and the method of obtaining excited spectra was also the same as had been used previously. A platinum tube was placed outside a thin-walled glass tube filled with radium emanation. The beta rays from the RaB and C in the emanation tube were absorbed by the platinum, whereas the gamma rays from these bodies penetrated the platinum and gave rise to the photoelectrons constituting the excited spectrum. Though electrons were liberated throughout the body of the metal, only those ejected from the surface layer contributed to the heads of the lines for which measurements were taken.

Before an experiment was carried out, the platinum sheath was exposed to a small quantity of radium emanation, so that a weak source was deposited on its surface. The platinum tube was then the source of two corpuscular spectra, the natural and the excited spectrum. An important point was that all of the electrons could be considered to come from the same source, the surface of the platinum, since then, in comparing different lines, no corrections were necessary for the size of the source, and it was not even necessary to know the position of the source very accurately.

The results obtained for the three main gamma-ray lines of RaB and the strongest line of RaC are shown in Table 4.8. "It can be seen that the results

[75] *Ibid.*, p. 840.

[76] C.D. Ellis and W.A. Wooster, "The Atomic Number of a Radioactive Element at the Moment of Emission of the γ-Ray," *Proc. Camb. Phil. Soc.* *22* (1923–25), 844–848.

γ ray tested gives natural β ray line	$\delta H\rho$ obs.	$\delta H\rho$ calculated if emission	
		precedes disin-tegration	follows disin-tegration
$H\rho\, 1410_{\mathrm{RaB}}$	60 61	49	62
$H\rho\, 1677_{\mathrm{RaB}}$	56 56 58	44	57
$H\rho\, 1938_{\mathrm{RaB}}$	52 48	42	53
$H\rho\, 2980_{\mathrm{RaC}}$	57	46	56

Table 4.8: The results of Ellis and Wooster's second experiment on the question of whether gamma rays precede or follow the β-ray disintegration. Source: Ellis and Wooster, "Atomic Number" (note 76), p. 847.

indicate very clearly that the γ-ray is emitted after the disintegration," Ellis and Wooster concluded, "and there is no possible ambiguity in the interpretation."[77]

Ellis was now convinced that Meitner was correct, and in a letter to Meitner of 8 December 1925, he admitted that the old experiment of Rutherford and Andrade had led him astray:

> We have been at considerable trouble to settle the question of whether the γ-ray sometimes precedes the disintegration as I deduced from Rutherford & Andrade's measurements, or whether the simpler standpoint advanced by you that the disintegration always happens first was correct. We have had three different ways of testing this and they all show that the γ-rays come out afterwards, so you were right. I cannot help feeling a little annoyed that we were led astray by that old experiment. I don't know yet what the error was, I have seen Andrade's old plates and measurements and everything looks all right but yet there

[77] *Ibid.*, p. 847.

> is no doubt that everything was really all wrong. I am very pleased that this removes one "Streitfrage" but the other one over the nature of the continuous spectrum still remains.[78]

Meitner was of course happy that the discussion had been settled to her advantage, as she noted in a letter to Hevesy of 9 December 1926:

> I was of course happy, in my discussion with Ellis, finally to see my viewpoint vindicated by Ellis's own experiments.[79]

Later, Meitner emphasized several times that on this question she had had the correct view.[80] In retrospect, it has often been forgotten that the controversy did not deal exclusively with the continuous beta spectrum.

Thus, as Ellis stated, one *Streitfrage* had been removed, but the other one remained. How this second *Streitfrage* was settled is the subject of the next chapter.

[78]Ellis to Meitner, 8 December 1925, MTNR 5/4.

[79]Meitner to Hevesy, 9 December 1926, Hevesy Scientific Correspondence, Niels Bohr Archive.

[80]See, for example, a letter of 17 November 1958 from Pauli to Wu, MTNR 5/13. Wolfgang Pauli wrote as follows: "She [Meitner] frankly admitted that she was on the wrong track with her ideas on the primary Beta rays but emphasized, quite correctly, that in other questions she had the better view than Ellis."

Chapter 5

The End of the Beginning: The Controversy Enters the Decisive Phase, 1925–1929

5.1 Introduction

In this chapter, I deal with the period that has been called "the end of the beginning in the developments of beta decay."[1] As we shall see, the long-lasting controversy between Charles Drummond Ellis and Lise Meitner finally came to an end and left a Gordian knot waiting for the cut of an Alexander.

The period lasting from the autumn of 1925 until the middle of 1929 started with Ellis and William Wooster's reaction to the recognition that the primary beta rays precede the emission of gamma rays. They maintained their view on the origin of the continuous beta spectrum, and it seems that they were now even more eager to settle the matter. Heating-effect measurements of RaE might prove to be the key, they argued, and threw themselves into an experiment that has been called "a technical tour de force."[2] The result of their experiment was considered to be conclusive in Cambridge, but not everyone on the Continent agreed. Meitner, for example, still saw a possibility of maintaining her own view, and Wolfgang Pauli joined her enthusiastically. The optimistic atmosphere in the spring of 1929 lasted only a few months, however. At a conference in Zurich at the beginning of July 1929, Meitner delivered a lecture in which she reported on the results of her own heating-effect experiment, which definitely settled the controversy.

In addition to the heating experiments, some investigations of the number of beta particles emitted from radioactive sources were carried out. We shall also discuss these experiments and see how they were interpreted.

[1] A. Pais, "Radioactivity's Two Early Puzzles," *Rev. Mod. Phys. 49* (1977), 925–938, p. 927.

[2] K. Hutchison, J.A. Gray and H.S.W. Massey, "Charles Drummond Ellis," *Biog. Mem. Fel. Roy. Soc. 27* (1981), 198–233, p. 205.

C. Jensen et al. (eds.), *Controversy and Consensus: Nuclear Beta Decay 1911-1934*

5.2 Ellis adjusts his view on the emission process, but maintains his interpretation of the continuous spectrum

After being shown to be incorrect in his view on the temporal order of emission of beta and gamma rays, Ellis was forced to change his view on the emission process. He put forward a provisional working hypothesis that in several respects was consistent with the more general view recently proposed by Meitner.[3] He now assumed that prior to a beta decay the nucleus is in its normal state, whereas after the decay it may well be left in an excited state. A transition to the ground state then causes the emission of gamma rays. After the emission of an alpha particle, however, Ellis imagined that the electrons adjust themselves continuously to the new value of the field. He was quite aware that in certain cases alpha-ray bodies also emit gamma rays, but he considered this more the exception than the rule and preferred to concentrate on their normal behavior.

With regard to the continuous spectrum, however, a great difference still existed between the views of Ellis and Meitner, "not only about the interpretation but even about experimental facts."[4] Ellis maintained the view that the inhomogeneity in energy could not be explained as a secondary effect. Neither a Compton effect of the scattering of gamma rays from the nucleus, as proposed by Meitner and Marie Curie, nor the intense electric field of the atom was a sufficient explanation of the inhomogeneity, Ellis argued. "Some such effects as this may occur," he admitted, "but there is no question of it being the explanation of the main inhomogeneity, because radium E emits no penetrating γ-rays, whereas to account for the observed spectrum a γ-ray emission twice as strong as that of radium B would be necessary."[5] The other possibility proposed by Meitner – that the disintegration electrons collide with the planetary electrons in their escape from the atom and in this way lose varying amounts of energy – also met with difficulties in the case of radium E:

> The continuous spectrum of radium E extends to about 1,000,000 volts, showing a maximum at about 300,000 volts, and it is a question of accounting for an average loss of at least 500,000 volts per atom disintegrating if initially every electron is emitted with the maximum velocity. Only 90,000 volts are required to remove a K electron, and more than 19/20 of the disintegration electrons lose more energy than this. It is clear that if this large amount of energy, 500,000 volts, were really to be accounted for in this way then there would be a great many high speed secondary electrons which could not escape detection and also a

[3]C.D. Ellis and W.A. Wooster, "The β-Ray Type of Disintegration," *Proc. Camb. Phil. Soc.* *22* (1923–1925), 849–860; L. Meitner, "Über die Rolle der γ-Strahlen beim Atomzerfall," *Z. Phys.* *26* (1924), 169–177.

[4]Ellis and Wooster, "β-Ray Type" (note 3), p. 857.

[5]*Ibid.*, p. 859.

> large amount of K radiation. A small amount of K radiation has been detected, but it seems far too small to agree with this explanation.[6]

According to Ellis and Wooster, there thus did not seem to be any possibility of explaining the inhomogeneity as a secondary effect. One was left with the conclusion, they argued, that the primary beta particles are emitted from the nucleus with varying velocities.

In support of their view, Ellis and Wooster called attention to a recent investigation by another Cambridge physicist, Karl Emeleus, who had counted the number of electrons emitted from RaE. His result, about one electron per disintegrating atom, agreed well with Ellis's hypothesis. This and other investigations on the number of emitted electrons will be discussed in the next section.

Though feeling certain that the disintegration electrons constitute the continuous beta spectrum, Ellis and Wooster had no suggestion of how to explain the inhomogeneity. They mentioned that statistical energy conservation was a possible explanation, but they were far from being enthusiastic about this possibility:

> ... if we were to consider energy to be conserved only statistically there would no longer be any difficulty in the continuous spectrum. But an explanation of this type would only be justified when everything else had failed, and although it may be kept in mind as an ultimate possibility, we think it best to disregard it entirely at present.[7]

They admitted that in the light of quantum theory, the most natural thing was to claim that the same amount of energy was released in each disintegration; but that left the important problem of the missing energy. In the case of radium E it was a question of 0.5 MeV per atom. A direct decision between the two views was thus of paramount importance, and this could be obtained, Ellis and Wooster argued, by measuring the heating effect of RaE. How this experiment was carried out, and its result, is the subject of a subsequent section.

5.3 The number of emitted beta particles

In the mid-1920s, some significant investigations of the number of beta particles emitted from different beta sources were carried out. Earlier attempts to determine the number of beta particles emitted per disintegration are summarized in Table 5.1.[8] They differ somewhat from each other, and no certain conclusions could be drawn from them.

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[6] *Ibid.*, p. 859.

[7] *Ibid.*, p. 858.

[8] E. Rutherford, "Charge Carried by the α and β Rays of Radium," *Phil. Mag. 10* (1905), 193–208; W. Makower, "On the Number and the Absorption by Matter of the β Particles Emitted by Radium," *Phil. Mag. 17* (1909), 171–180; H.G.J. Moseley, "The Number of β Particles Emitted in the Transformation of Radium," *Proc. Roy. Soc. A87* (1912), 230–255.

Investigator(s)	Year	Source	Number
(1) E. Rutherford	1905	Ra(B + C)	0.7
(2) W. Makower	1909	RaC	1.5
(3a) H.G.J. Moseley	1913	Ra(B + C)	1.1
(3b) H.G.J. Moseley	1913	RaE	0.6
(4) J. Danysz & W. Duane	1913	Ra(B + C)	1.5

Table 5.1: The results of some early investigations of the number of beta particles emitted per disintegration from radioactive sources. Sources: (1) Rutherford, "Charge" (note 8), p. 205; (2) Makower, "Number and Absorption" (note 8), pp. 178–180; (3a and 3b) Moseley, "Number" (note 8), pp. 241–245; (4) J. Danysz, "Sur les Rayons β de la Famille du Radium," *J. d. Phys. 3* (1913), 949–960, on pp. 956–960.

One of these results was peculiar. Henry Moseley found that radium E apparently emits only a little more than half a beta particle for each atom breaking up, which seemed to agree with some ionization measurements by Hans Geiger and Alois Kovarik.[9] Moseley tried to explain his curious result by supposing that many of the electrons are emitted with very small energies, and thus not detected. Ernest Rutherford, James Chadwick and Ellis wrote in 1930 that this seemed improbable, but conceded that it was difficult to account for the low value found by Moseley.[10]

A little more than ten years after Moseley's experiment, Emeleus decided to repeat these measurements.[11] The importance of a reliable result had increased, since it might help settle the Ellis–Meitner controversy. One beta particle per disintegration, or slightly more, would strongly favor Ellis's view, since he believed that the disintegration electrons constitute the beta spectrum.

Moseley's method consisted in measuring the total charge carried away by the electrons from a known amount of radioactive material. By dividing this total charge by the product of the elementary charge and the number of atoms disintegrating per unit time, he then obtained the average number of beta particles emitted per disintegration. On account of the small current, however, Moseley found that the measurements were difficult to carry out in the case of RaE. Emeleus preferred another method. Instead of determining the number of particles by measuring their charge, he counted them directly with an electrical counter, which according to him was a far more sensitive method.[12] His experimental arrange-

[9] H.W. Geiger and A.F. Kovarik, "On the Relative Number of Ions Produced by the β Particles From the Various Radioactive Substances," *Phil. Mag. 22* (1911), 604–613.

[10] E. Rutherford, J. Chadwick and C.D. Ellis, *Radiations from Radioactive Substances* (Cambridge University Press, 1930), pp. 392–393.

[11] K.G. Emeleus, "The Number of β-Particles from Radium E," *Proc. Camb. Phil. Soc. 22* (1923–1925), 400–404.

[12] *Ibid.*, p. 400.

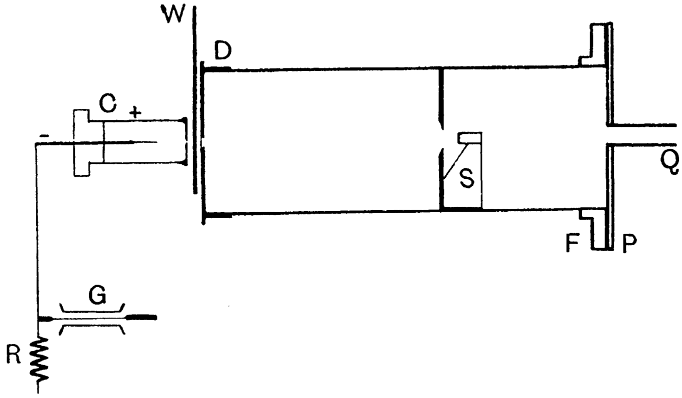

Figure 5.1: The experimental arrangement used by Emeleus in his experiment on the number of beta particles emitted from RaE. Source: Emeleus, "Number of β-particles" (note 11), p. 401.

ment is shown in Figure 5.1. The radioactive source, RaD + E + F in equilibrium, was placed at S in a brass tube, which was then closed and exhausted through Q. In front of the source was a brass diaphragm, whose aperture was sufficiently small to prevent beta particles from passing out to the counter after a single reflection in the tube. Between the counter and the brass cap D was a brass wheel W. By rotating W, 2 mm of brass – or mica of variable thickness – could be placed in the path of the beta particles. The small absorption due to the foils covering the opening of the counter and the end of the tube was equivalent to 2.5 cm of air for alpha particles.

The results of Emeleus's investigation are shown in Figure 5.2, where the number of beta particles, N, recorded per minute, corrected for spurious effects, is plotted against the total quantity of absorbing material t in their path, expressed in cm of air-equivalent for alpha particles. It was not necessary to know the amount of the absorbing material, since counts were first taken of the combined beta and alpha rays (p in Figure 5.2), whereupon the beta rays were deflected away by a magnetic field, so that only the alpha rays were counted. Subtracting the alpha counts from the total thus gave the number of beta particles from each disintegrating atom of radium E. The beta particles from RaD could not disturb the result, since they were very soft and could not penetrate even the smallest thickness of absorbing material used. A set of counts was taken with variable thicknesses of the absorbing material. The observed data are given by circles. The cross gives the number of beta particles for zero absorption, found by logarithmic extrapolation.

Figure 5.2 shows that 17.1 alpha particles correspond to 24.5 beta particles, i.e., 1.43 beta particles per alpha particle. Not all of these beta particles have

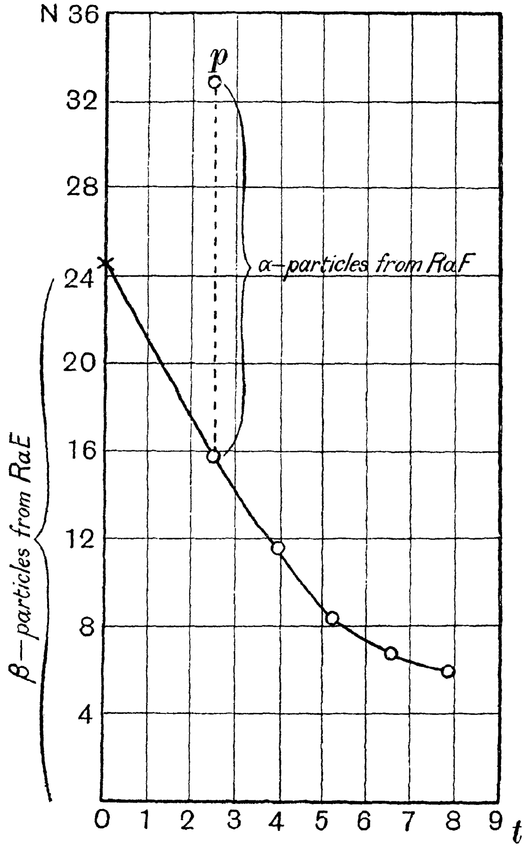

Figure 5.2: The result of Emeleus's measurements of the number of beta particles emitted from RaE. The number of beta particles is plotted against the thickness of absorbing material t, expressed in cm air-equivalent for alpha particles. For each of the five measurements, counts were first taken of the total number of particles (p in the figure); then the beta particles were deflected away, and finally the alpha particles were counted and subtracted from the total number of particles. The number of beta particles for zero absorption (the cross in the figure) was found by logarithmic extrapolation. Source: Emeleus, "Number of β-particles" (note 11), p. 403.

been projected initially towards the counter, however. Some have been emitted in the opposite direction, but were reflected by the glass source. By applying a correction factor of 23 percent for this reflection, Emeleus reduced the number of beta particles emitted from each disintegrating atom to 1.1.[13] The uncertainty was estimated to be within 10 percent.

Ellis considered Emeleus's result to be a strong support for his view, and about one year later, it gained further support from another Cambridge investigation. Ronald W. Gurney counted the number of particles in the beta spectra of radium B and C by following up some preliminary measurements of Leon Curtiss.[14] Gurney separated out the beta rays into a spectrum, measured the successive portions with a Faraday cylinder, and obtained the total number emitted by integration. This method had the great advantage of being independent of the gamma radiation, and it gave equal weight to every particle whatever its velocity. Gurney arrived at 2.3 electrons from each pair of disintegrating atoms, which together with Emeleus's result made him feel confident that Ellis's view was correct. He concluded:

> It seems unnecessary to prolong the argument, since the evidence points clearly to the view that in all three cases the continuous spectrum is formed by the disintegration-electrons from the nucleus.[15]

The Berlin physicists were very reluctant to yield to this conclusion, however. Nikolaus Riehl, a student of Meitner, chose as his dissertation an investigation attempting to determine whether a disintegrating RaE atom emits one or more beta particles.[16] In principle, his experiment bore a strong resemblance to Emeleus's. He, too, detected the electrons with a Geiger counter, but he very carefully investigated the conditions under which all of the beta particles were counted. Furthermore, by a special arrangement, he was able to reduce the initial absorption to the equivalent of 0.98 cm of air (compared to Emeleus's 2.5 cm of air). In a letter to Meitner, Ellis praised Riehl's thoroughness and the systematic way in which he investigated all possible sources of error.[17]

The result of Riehl's extrapolation was 1.33 beta particles per alpha particle, which then had to be corrected for reflection. This seemed to be in fair agreement with Emeleus's 1.43 beta particles, but as regards the reflection, the results differed

[13]Emeleus based his correction on an investigation by Kovarik. See A.F. Kovarik, "Absorption and Reflexion of the β-Particles by Matter," *Phil. Mag. 20* (1910), 849–866; Kovarik and L.W. McKeehan, "Messung der Absorption und Reflexion von β-Teilchen durch direkte Zählung," *Phys. Zeit. 15* (1914), 434–440.

[14]R.W. Gurney, "The Number of Particles in the Beta-Ray Spectra of Radium B and Radium C," *Proc. Roy. Soc. A109* (1925), 540–561; L.F. Curtiss, "A Preliminary Note on a Direct Determination of the Distribution of Intensity in the Natural β-Ray Spectrum of RaB and RaC," *Proc. Camb. Phil. Soc. 22* (1923–1925), 597–600.

[15]Gurney, "Number" (note 14), p. 557.

[16]N. Riehl, "Die Brauchbarkeit des Geigerschen Spitzenzählers für β-Strahlen verschiedener Geschwindigkeiten und die Zahl der β-Strahlen von RaE und RaD," *Z. Phys. 46* (1927), 478–505.

[17]Ellis to Meitner, 8 February 1928, MTNR 5/4.

considerably. Riehl preferred a factor found by Basil Schonland,[18] which led to the result that for every alpha particle at least 1.18 beta particles were emitted.

Riehl considered his investigation to be the more accurate one, and rejected Ellis's view that the number obtained pointed to the inhomogeneity in energy of the emitted beta particles as a primary effect:

> It may probably be concluded with certainty from these results that the number of β-particles emitted per decaying atom for RaE is greater than 1. Since the number of primary β particles originating from the nucleus is certainly 1 per decaying atom, it must be assumed that the primary β particles expel secondary electrons. In fact, the number of these secondary electrons must certainly constitute more than 20 % of the number of primary electrons, and their velocity definitely amounts to more than 40 % of the velocity of light. The demonstration that the primary β rays produce secondary β rays by the passage through the electron shells of their own atom implies, of course, that the primary β rays undergo velocity losses and become inhomogeneous. It is therefore not possible, on the basis of the inhomogeneity of the primary β rays outside the atom from which they originate, indisputably to draw the conclusion that they already leave the nuclei with different velocities.[19]

Thus, according to the Berlin physicists, these experiments on the number of emitted beta particles were inconclusive. Meitner and her collaborators apparently were still not convinced that Ellis was right. Riehl's conclusion is all the more striking, since his paper was submitted to the *Zeitschrift für Physik* in November 1927, about six months after the result of Ellis and Wooster's heating-effect measurements were first announced. This famous calorimeter experiment is the subject of the next section.

5.4 Ellis and Wooster's tour de force: A determination of the heating effect of RaE

That Meitner gained the victory in the beta-first-gamma-second question sharpened the problem of the continuous beta spectrum, and encouraged Ellis and Wooster to try their hands at measuring the heating effect of RaE. This was a promising direct way of solving the riddle of the continuous beta spectrum. If equal amounts of energy are released in each disintegration, the heating effect has to be between 0.8 and 1.0×10^6 eV per atom, and accordingly the riddle concerned the missing energy. If, however, the heating effect turned out to be close to 0.4×10^6 eV per atom, which is the mean kinetic energy of the beta particles, the solution to the riddle had to be located in the nucleus itself.

[18] B.F.J. Schonland, "The Passage of Cathode Rays Through Matter," *Proc. Roy. Soc. A108* (1925), 187–210.
[19] Riehl, "Brauchbarkeit" (note 16), p. 501.

Ellis and Wooster started their investigation in the autumn of 1925, and realized at once that they had set a difficult task for themselves. On 11 November, Ellis wrote to Rutherford, who was away on a trip to Australia and New Zealand:

> Wooster and I are now working on a photometering of β-ray spectra & trying to measure the heating effect of RaE. We have not got as far as having a source yet, it is really rather difficult & I am afraid you will think the patience is wasted on us, but the heating effect of 1 mg of RaE is of the same order as stray effects, & we shall be lucky if we get a source that size. Still we are getting better results slowly & are very hopeful.[20]

They were still hopeful 18 days later when Ellis again wrote to Rutherford:

> I am in hope, but rather faint hope, that we may have some definite advance to tell you about the RaE heating experiment, that beastly thing weighs on us, it is such a small effect, it would be dead easy if one was not limited to 1/2–1 mg RaE with a heating effect 1/50 of 1 mg of α-particles.[21]

About a year and a half was to pass before they first reported the results of their measurements, and almost another half year passed before they submitted them to the *Proceedings* of the Royal Society.[22]

Heating-effect measurements of radioactive sources were not novel. As early as 1903, Pierre Curie and Albert Laborde measured the heating effect of a quantity of radium in a Bunsen ice calorimeter, and found that 1 gram of radium in equilibrium with its decay products emits heat at the rate of about 100 gram calories per hour.[23] This experiment caused much excitement, and many calorimeter experiments followed. In 1912, for example, Rutherford and Harold Robinson succeeded in measuring the distribution of the heating effect among the decay products RaEm, RaA, RaB and RaC of radium, and, furthermore, its distribution among alpha, beta and gamma rays.[24] Ellis and Wooster also contributed to the series of calorimeter experiments at the end of 1924, when they determined the gamma-ray heat of RaB + C.[25] Thus, they were familiar with such investigations when they started their famous radium E heating-effect experiment.

Radium E was considered a particularly suitable source, since it emits very few gamma rays. Complications caused by gamma rays, and by internal-conversion

[20] Ellis to Rutherford, 11 November 1925, C.U.L. Add. MS. 7653 E8.

[21] Ellis to Rutherford, 29 November 1925, C.U.L. Add. MS. 7653 E9.

[22] C.D. Ellis and W.A. Wooster, "The Average Energy of Disintegration of Radium E," *Proc. Roy. Soc. A117* (1927), 109–123.

[23] P. Curie and A. Laborde, "Sur la chaleur dégagée spontanément par les sels de radium," *C.R. 136* (1903), 673–675.

[24] E. Rutherford and H.R. Robinson, "Heating Effect of Radium and Its Emanation," *Phil. Mag. 25* (1913), 312–330.

[25] C.D. Ellis and W.A. Wooster, "Note on the Heating Effect of the γ-Rays from RaB and RaC," *Proc. Camb. Phil. Soc. 22* (1923–1925), 595–596.

electrons as well, were then avoided. In addition, owing to the large difference between the values predicted by the rival hypotheses (1 MeV versus 0.4 MeV), the chance of obtaining a conclusive result was promising. Nevertheless, the experiment was extremely difficult to carry out. Since only a small source of RaE was available, Ellis and Wooster had to measure increases in temperature on the order of magnitude of 10^{-3} degrees Celsius. A very sensitive galvanometer thus was required. In 1977, in a letter to Abraham Pais, Wooster recalled that the galvanometer "was so sensitive to external changes in the magnetic field that we had to work between 12 midnight and 3 a.m. for a fortnight. Even so, when the policeman walked by in the street, the nails of his boots disturbed the galvanometer."[26] Meitner was impressed that the experiment could even be carried out. On 14 February 1928, she thanked Ellis for having sent her "your beautiful paper on radium E," and continued:

> Since you could use a needle galvanometer, you must be able to make measurements very undisturbed [by your surroundings]. This is not possible here in Berlin, if only because of the electric trams.[27]

After several trials, Ellis and Wooster selected for their investigation the calorimeter shown in Figure 5.3(a). It consisted of two identical lead tubes, 13 mm long and 3.5 mm in diameter, with a central hole of rather more than 1 mm in diameter. Each calorimeter tube fitted precisely into a thin outer sheath of silver, and was supported by two discs of mica, A and B, which in turn were carried by the brass screw C fitting into an ebonite base DE. The entire calorimeter system was placed in a small cavity in a copper block, as shown in Figure 5.3(b). The top portion A of this block had two small holes of diameter just larger than the internal diameter of the lead tubes, and located so that two small brass tubes of nearly the same diameter could slide smoothly from the holes in the copper block into the lead tubes. One of these brass tubes contained the source of radium E, the other an inactive wire of the same thermal capacity. The brass tubes were attached by means of two short thin glass rods to threads leading to brass rods fixed into the ebonite block B. By this means, the brass tubes could be removed from the calorimeters and kept in the upper copper block without introducing thermal disturbances. The function of the small glass rods was to eliminate any possible variation in the conduction down the threads owing to their touching the sides of the copper block at different points in different experiments. The composite copper block, which was half an inch thick at its thinnest, was housed in a wooden box lined with felt, which afforded good protection against external variations in temperature.

Knowing that RaE decays to polonium (^{210}Po) with a half-life of 5 days, that Po is a pure alpha emitter, and that the energy emitted per polonium disintegration is 5.22×10^6 electron volts, Ellis and Wooster proceeded as follows. The ratio of

[26] Pais, "Radioactivity's Puzzles" (note 1), p. 927.
[27] Meitner to Ellis, 14 February 1928, MTNR 5/4.

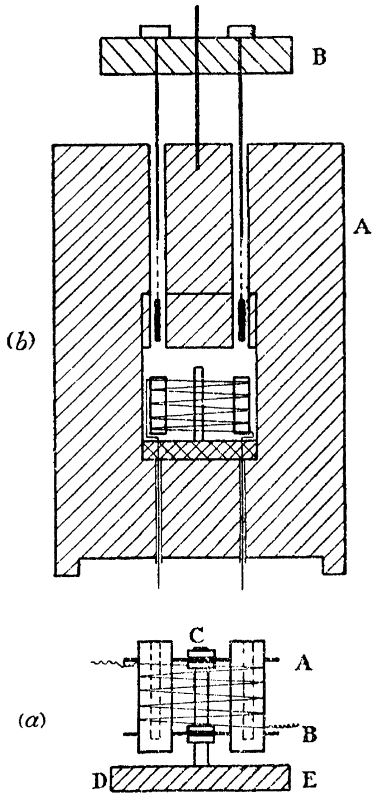

Figure 5.3: The experimental arrangement used by Ellis and Wooster in their investigation of the heating effect of RaE. The calorimeter system is shown in (a). It consisted of two identical lead tubes in a thin sheath of silver. Each tube was supported by two discs of mica, A and B, secured to an ebonite base DE by a brass screw C. In (b) the whole system is placed in a cavity in a copper block A with two small holes. Two small brass tubes could then slide smoothly into the lead tubes. One of these brass tubes contained the source, the other an inactive wire of the same thermal capacity. Source: Ellis and Wooster, "Average Energy" (note 22), p. 113.

the heating effect of RaE to that of Po at time t after the RaE source is pure is given by

$$\frac{(\lambda_{\mathrm{P}} - \lambda_{\mathrm{E}}) \cdot e^{-\lambda_{\mathrm{E}} t}}{x \cdot \lambda_{\mathrm{P}} \cdot \left(e^{-\lambda_{\mathrm{E}} t} - e^{-\lambda_{\mathrm{P}} t}\right)},$$

where x is the ratio of the known energy given out in a polonium disintegration to the average energy given out in a RaE disintegration, and λ_{E} and λ_{P} are the decay constants of RaE and Po, respectively. If this ratio could be measured as a function of time, then x, and hence the energy per disintegrating RaE atom, could be obtained.

Since in practice one could not prepare a source entirely free from polonium, one had to calculate the time at which the source would have been pure RaE. Ellis and Wooster did this by measuring the alpha activities of the source initially and 20–25 days later. The time could then be calculated from the ratio of these two activities. It proved to be about two days before the first measurement.

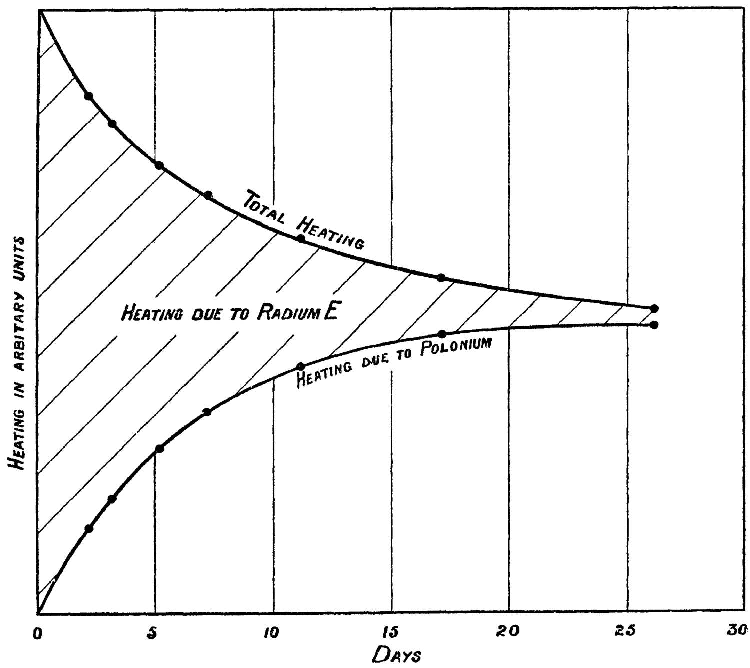

Figure 5.4: The heating curves obtained by Ellis and Wooster. The upper curve shows the total heating effect. The lower curve is the heating effect of polonium, and was obtained by extrapolation. The difference between the two curves then gave the heating effect of RaE. Source: Ellis and Wooster, "Average Energy" (note 22), p. 116.

True age	Total heating	Portion due to Po	Portion due to Radium E	x	Disintegration energy of Radium E in volts
days	mm				
2.25	22.0	3.68	18.3	15.4	339000
3.20	20.8	4.91	15.9	15.5	337000
5.20	19.0	6.99	12.0	15.5	337000
7.20	17.8	8.64	9.2	15.6	335000
11.20	16.1	10.53	5.6	14.5	360000
17.20	14.2	11.83	2.4	14.7	355000
26.20	12.85	12.18	0.67	15.1	346000

Table 5.2: The heating effect of radium E deduced from the curves in Figure 5.4 and the calculated average energies per disintegrating atom; x is the ratio of the energy given out in a polonium disintegration to the average energy given out in a RaE disintegration. Source: Ellis and Wooster, "Average Energy" (note 22), p. 118.

To obtain the heating ratio, the total heating effect due to RaE + Po was measured several times during a period of 24 days. This was done by measuring four times a day on a number of days the temperature difference between the source and the inactive wire with a system of thermocouples attached to a very sensitive galvanometer. The graph Ellis and Wooster obtained is shown in Figure 5.4. At the end of the experiment, the RaE had decayed to a small fraction (0.027) of its initial value, and the main heating effect was then due to Po. The small contribution from RaE to the total heating effect could be calculated from the observed curve extrapolated to $t = 0$, i.e., when only RaE was present. This quantity was subtracted from the total heating, and the heating effect due to Po at $t = 26$ days was obtained. Using the decay periods of RaE and Po – 5 days and 139 days, respectively – Ellis and Wooster then extrapolated backwards from this value and obtained the lower curve in Figure 5.4, showing the heating effect of polonium. The difference between the two curves gave the heating effect due to RaE, and the heating ratio could be found. As an important confirmation of the accuracy of their experiments, this difference proved to decrease exponentially with a period of 5.1 days. The heating effect of RaE deduced from the curves in Figure 5.4, together with the calculated average energies per disintegrating atom, appears in Table 5.2. "Considering the nature of the experiment," Ellis and Wooster wrote, "the agreement is excellent and shows that the energy of disintegration of radium E cannot be much different from 344,000 volts."[28]

[28]Ellis and Wooster, "Average Energy" (note 22), p. 118.

To complete the measurements, the energy escaping from the calorimeter had to be determined. The measured heating effect was due to the total radiation being stopped by an equivalent of 1.2 mm of lead. This radiation consisted of all of the beta particles and the small amount of gamma radiation emitted by radium E. Some of this gamma radiation penetrated through the calorimeter. How much energy escaped in this way? Geoffrey Aston investigated this problem for Ellis and Wooster by comparing the ionization produced by the radiation penetrating 1.2 mm of lead from the same number of disintegrating atoms of radium E, radium B and radium C.[29] Since the known penetrating power of RaE gamma rays was intermediate between that of RaB and RaC, the ratio of ionization for RaB to RaE gave a lower limit – and for RaC to RaE an upper limit – of the ratio of energy escaping. Approximate values for the gamma-ray energies of RaB and RaC were known, and Aston was thus able to obtain a figure of 5,000 electron volts per RaE atom for the energy penetrating 1.2 mm of lead. "As was to be expected," Ellis and Wooster concluded, "this amount of energy is virtually negligible in this connection, and no further discussion of the accuracy of this experiment is necessary."[30]

Owing to inherent difficulties in the experiment, its accuracy was estimated at about 10 percent. Ellis and Wooster's final result, then, was 350,000 $\pm$ 40,000 eV, which agreed well with the average value obtained by electrical methods. Ellis and Wooster did not hesitate to conclude:

> We may safely generalise this result obtained for radium E to all β-ray bodies, and the long controversy about the origin of the continuous spectrum of β-rays appears to be settled.[31]

This meant "that in a β-ray disintegration the nucleus can break up with emission of an amount of energy that varies within wide limits."[32] Ellis and Wooster admitted that this was a "curious conclusion,"[33] since the exponential law of radioactive disintegration and the homogeneity of alpha and gamma rays pointed to discreteness in the nucleus. But they did not regard this as a difficulty calling for radical change. On the contrary, they at once proposed a simple hypothesis by which the apparently contradictory observations could be reconciled on the basis of Rutherford's satellite model of the nucleus.[34]

Until the middle of 1928, when the Russian physicist George Gamow presented his theory of alpha decay, which showed that the nucleus was to be under-

[29] G.H. Aston, "The Amount of Energy Emitted in the γ-Ray Form by Radium E," *Proc. Camb. Phil. Soc. 23* (1925–1927), 935–941. See also Ellis and Wooster, "Average Energy" (note 22), pp. 119–120.
[30] *Ibid.*, p. 120.
[31] *Ibid.*, p. 121.
[32] *Ibid.*, p. 121.
[33] *Ibid.*, p. 121.
[34] For a history of the development of this model, see R.H. Stuewer, "Rutherford's Satellite Model of the Nucleus," *HSPS 16:2* (1986), 321–352.

stood in quantum-mechanical terms,[35] Rutherford's semi-classical and visualizable satellite model of the nucleus had been the most successful explanation of natural as well as artificial disintegration. Rutherford revised and developed this model throughout the 1920s, and in 1927 it comprised three regions – a concentrated inner core carrying a positive charge, a number of electrons surrounding this core, and finally a number of neutral satellites circulating around the core. These neutral satellites consisted of an alpha particle with two electrons bound to it; they were assumed to be held in equilibrium about the core by an attractive polarization force, by a magnetic force arising from the core, or perhaps by a combination of the two.

How, then, could the inhomogeneity in energy of the beta particles be explained by Rutherford's satellite model? "There is no reason," Ellis and Wooster stated, "why the outer satellite region should not be quantised, and so give the possibility of ejection of α-particles of definite energy, but yet the electronic region unquantised in the sense that the electrons have energies varying continuously over a wide range."[36] This picture of the electronic system seems to be contrary to quantum theory, but according to Ellis and Wooster it was not. For a particle to be quantized, they argued, it must be able to describe many complete orbits without disturbance, and this was hardly to be expected for electrons in the confined region of the nucleus. Nor did they consider the unquantized region to be in conflict with the law of radioactive decay. Since the decay of a substance requires time intervals immensely long compared to those involved in the frequencies of rotation or movement of the constituent parts of the nucleus, it was quite conceivable that the final statistical result followed regular laws, even if the life of the nucleus is not entirely ordered.

Ellis and Wooster had great confidence in their interpretation, and suggested that it offered hope of understanding beta decay:

> The energy resident in the electronic part will fluctuate among the electrons, and occasionally at intervals, long compared with the ordinary time scale, the energy may heap up in one electron and lead to an explosion.[37]

Ellis and Wooster also contended that the gamma rays could not be emitted from the unquantized electron region. The varying energy of the disintegration electrons and the high degree of homogeneity of the gamma rays made it impossible that the same region could be responsible for both emissions. This disagreed with the commonly held view that the electrons caused gamma emission, but a few

[35] For a historical account of Gamow's theory, see R.H. Stuewer, "Gamow's Theory of Alpha-Decay," in *The Kaleidoscope of Science: The Israel Colloquium Studies in History, Philosophy, and Sociology of Science*, E. Ullmann-Margalit (ed.) (Dordrecht: Reidel, 1986), pp. 147–186.

[36] Ellis and Wooster, "Average Energy" (note 22), p. 122.

[37] *Ibid.*, p. 123.

months earlier Werner Kuhn, as a godsend, had questioned this view.[38] Rutherford had already been convinced on this point, and was now of the opinion that the neutralized alpha particles were responsible for the homogeneous gamma rays.[39]

By combining the classical wave theory of radiation and the quantum view of matter, Kuhn had deduced an expression for the half-width of gamma-ray lines. From this expression, he had obtained for a 600,000 eV gamma-ray line a half-width of 10,000 eV, i.e., a degree of inhomogeneity of 1 part in 60, assuming that electrons were the cause of the gamma emission. Reliable observations, however, indicated a degree of inhomogeneity of less than 1 part in 1000. To obtain agreement, Kuhn argued, a system with a small charge-to-mass ratio e/m had to be responsible for the gamma emission, that is, a proton or an alpha particle.

Ellis and Wooster's hypothesis thus received good support. Two years later, Ellis still preferred it as an explanation of the continuous spectrum, as is evident from a letter of 28 July 1929 to Meitner:

> [Charles Galton] Darwin of course is quite ready to let fall the conservation of energy, on the other hand I feel more disposed to doubt the fundamental accuracy of the quantum postulate that every stable system is always in one of a small number of definite stationary states. In other words I am ready to have a small section of the nucleus in a rather indefinite state and claim that it would not affect the definiteness of the other stationary states we know of to a measureable extent.[40]

It is noteworthy that Ellis so readily proposed the existence of an unquantized region in the nucleus, and that he maintained it for such a long time. This suggests that he, and probably some of the other Cambridge experimentalists, were still too deeply rooted in the tradition of classical physics to recognize the fundamental character of the quantum postulate. This makes it easier to understand why Ellis for so many years, in his controversy with Meitner, was able to maintain a view that seemed inconsistent with the fundamental quantum postulate.

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[38] W. Kuhn, "Absorptionsvermögen von Atomkernen für γ-Strahlen," *Z. Phys. 43* (1927), 56–65; *idem.*, "Polarisierbarkeit der Atomkerne und Ursprung der γ-Strahlen," *Z. Phys. 44* (1927), 32–35.

[39] E. Rutherford, "Structure of the Radioactive Atom and the Origin of the α-Rays," *Phil. Mag. 54* (1927), 580–605.

[40] Ellis to Meitner, 28 July 1929, MTNR 5/4.

5.5 Continental reactions to Ellis and Wooster's experiment

Meitner probably heard of the result of Ellis and Wooster's experiment for the first time when Ellis visited Berlin early in April 1927.[41] Otto Robert Frisch recalled that Ellis's message was "a great shock to her."[42] She no doubt considered it as a serious blow to her beta-disintegration hypothesis. Pauli corroborated this reaction of Meitner's in a letter of 17 November 1958 to Chien-Shiung Wu. Pauli told Wu that he had just received a letter from Meitner in which she mentioned

> that already before her heat experiment with [Wilhelm] Orthmann she became rather uncertain about her own hypothesis regarding the primary beta rays and that she told two collaborators to look, whether the recoil nuclei (after beta disintegration) show an inhomogeneous distribution of their energy.[43]

Meitner's two collaborators were Karl Donat and Kurt Philipp, who submitted the results of their investigation to the *Zeitschrift für Physik* in August 1927.[44] They therefore probably began their experiments shortly after Ellis's visit. Their measurements were, however, too inaccurate to decide the question they posed.

In retrospect, the Ellis–Wooster experiment is often considered crucial in the sense that it definitely decided between the two competing hypotheses, but at the time it was not that simple. In July 1928, a conference on beta and gamma rays was held in Cambridge. I have only been able to procure the program for this important conference (see Figure 5.5), but Meitner's recollection of it, as expressed in a letter of 5 December 1956 to Pauli, is probably correct:

> In 1928, in July as I recall, there was a beta–gamma-ray conference in Cambridge at which Ellis gave a paper on his heat measurements for RaE, which were not, however, absolute measurements, without being able completely to convey his opinion about the existence of the continuous beta spectrum.[45]

A wait-and-see attitude seems to have been widespread among Continental physicists in 1927 and 1928. They realized more fully, in contrast to their British colleagues, the serious consequences of a primary continuous beta spectrum, and were therefore more reluctant to accept it.

The Ellis–Wooster experiment probably made Meitner abandon her view that the continuous beta spectrum could be due to electronic collisions, but she

[41] Concerning Ellis's visit to Berlin, see the following letters: Ellis to Meitner, 23 February 1927; Meitner to Ellis, 2 March 1927; Ellis to Meitner, 5 March 1927; Ellis to Meitner, undated, but sent from Berlin, MTNR 5/4.

[42] O.R. Frisch, "Lise Meitner," *Biog. Mem. Fel. Roy. Soc. 16* (1970), 405–420, p. 408.

[43] Pauli to Wu, 17 November 1958, MTNR 5/13.

[44] K. Donat and K. Philipp, "Die Ausbeute beim β-Rückstoss von Thorium B," *Z. Phys. 45* (1927), 512–521.

[45] Meitner to Pauli, 5 December 1956, MTNR 5/13.

23 July.	Evening meeting	E. Rutherford will refer to the subject as a whole and especially its earlier historical development.
24 July.	Morning meeting:	Beta-ray spectra. Meitner.
	Afternoon meeting:	The wavelength of gamma rays. Thibaud.
		The homogeneity of gamma rays. Kuhn.
25 July.	Morning meeting:	Absorption & scattering of gamma rays. Kohlrausch.
	Afternoon meeting:	The intensities of gamma rays. Skobeltzyn.
		Absorption & scattering of beta rays. Bothe
27 July.	Morning meeting:	Disintegration electrons. Ellis.
	Afternoon meeting:	The time of emission of gamma rays. Jacobsen
		Disintegration theories. Smekal.

Figure 5.5: The program of the Cambridge conference in July 1928 on beta and gamma rays. Source: MTNR.

clung to the hope that continuous gamma radiation was present, even if it escaped measurement. She decided to repeat the experiment; on 14 February 1928, she informed Ellis about her plans to start a heat measurement together with one of Walther Nernst's assistants.[46] She pointed out its consequences:

> The question is after all of fundamental importance, since the conception that the nuclei possess only statistically defined energy states represents something fundamentally new.[47]

Before turning to Meitner's experiment, I will place it in a broader historical context.

Many theoretical physicists, regarding the problem of the continuous beta spectrum to be an experimental one, had followed the Ellis–Meitner controversy with what may be called an at-a-distance interest. During 1928, however, several of them became more directly involved in the debates. The sharpening of the problem, together with other emerging questions that I shall discuss in the next chapter, formed a more general *Problemkomplex*, attractive to theorists.

[46]Meitner to Ellis, 14 February 1928, MTNR 5/4.
[47]*Ibid.*

Pauli was a theoretical physicist who assumed more than an at-a-distance interest in the problem. In a letter to Meitner of 3 November 1956, just after the existence of the neutrino had been demonstrated experimentally by Clyde Cowan and Frederick Reines,[48] he told her that he had been asked to deliver a lecture on the history of the neutrino.[49] He continued:

> Now I remember well the old controversy between you and Ellis which finally ended with the calorimetric experiment by you and Orthmann. I also remember that, wanting to be diplomatic, I once greeted you with the words, "It is really very nice that you are there." This was far above the limit of error with regard to my level of politeness at the time; hence you reacted very logically: "You do not usually make such compliments; you must have some hidden motive." This was precisely the case, for I was of the opinion that Ellis was right. Unfortunately, I have forgotten *where* and *when* this meeting took place. I suppose it must have been at some physicists' conference. *Do you still remember where and when?*[50]

Meitner did not remember, but suggested that it might have been in 1927 in Hamburg, where she lectured at a colloquium as Otto Stern's guest, or in Leiden, where she was Paul Ehrenfest's guest.[51] Pauli thought that it might have been even earlier:

> My remark to you that "Ellis is right" might well have been earlier than 1927. I know that I was already very impressed by his polemic against you in Zs. f. Phys. 10, 303, 1922,[52] and certainly in his favor."[53]

Whatever the merits of the details of Meitner and Pauli's reminiscences, these retrospective letters show that Pauli had followed the controversy closely.

If in 1927 Pauli felt that Ellis was right, he had changed his mind by early 1929. An essential reason can probably be traced to the questions in quantum electrodynamics on which he was then working with Werner Heisenberg.[54] Pauli reported on one such question in a letter of 18 February 1929 to Oskar Klein:

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[48]See C.L. Cowan and F. Reines, "Detection of a Free Neutrino – A Confirmation," *Science 124* (1956), 103–104. See also C.E. Atchley, *The Invention and Discovery of the Neutrino: Elusive Reality and the Nature of Scientific Acceptance* (Ph.D. thesis, University of Minnesota, 1991).

[49]Pauli gave the lecture at the Zürcher Naturforschende Gesellschaft on 21 January 1957; see C.S. Wu, "The Neutrino," in *Theoretical Physics in the Twentieth Century: A Memorial Volume to Wolfgang Pauli*, M. Fierz and V.F. Weisskopf (eds.) (New York: Interscience, 1960), pp. 249–303, on p. 301.

[50]Pauli to Meitner, 3 November 1956, MTNR 5/13; emphasis in the original.

[51]Meitner to Pauli, 5 December 1956, MTNR 5/13.

[52]C.D. Ellis, "Über die Deutung der β-Strahlspektren radioaktiver Substanzen," *Z. Phys. 10* (1922), 303–307.

[53]Pauli to Meitner, 13 December 1956, MTNR 5/13.

[54]W. Heisenberg and W. Pauli, "Zur Quantendynamik der Wellenfelder," *Z. Phys. 56* (1929), 1–61; *idem.*, "Zur Quantendynamik der Wellenfelder II," *Z. Phys. 59* (1930), 168–190.

> As applications of the theory we have (besides retardation effects), as the most important, a consideration of the Gamow model of the nucleus in the case where the radiation forces are significant. It turns out that then, even for a sharp state of the nucleus, the electrons emitted must have a continuous velocity spectrum due to additional γ-ray emission! I am quite certain myself (Heisenberg not as decisively) that γ rays are the cause of the continuous spectrum of the β rays and that Bohr with his considerations in this respect regarding a violation of the energy law is on a *completely erroneous* path! I also believe that the heat-measuring experimentalists are somehow cheating and that the γ rays have escaped them so far only due to their clumsiness. However, I understand too little of experimental physics to be able to prove this opinion (furthermore, I have had no opportunity to speak with Miss Meitner about this matter recently, which I would have liked to). Thus Bohr is in the comfortable position that by taking advantage of my general helplessness in the discussion of experiments, he is able to deceive himself and me by appealing to the Cambridge authorities (by the way, without reference to the literature).[55]

According to Pauli and Heisenberg's theory, the inhomogeneity in the beta-ray energy might be due to a retardation, and this possibility gave rise to a short and local Indian summer of Meitner's view. Pauli embraced it, not least because he felt that it offered a strong weapon against Niels Bohr's hypothesis of energy non-conservation (which I will discuss in the next chapter). On 16 March 1929, he informed Klein that on his way to an Easter conference in Copenhagen, he would stop over in Berlin "in order to, if possible, also speak with Miss Meitner with the intention of collecting material against the Copenhagen theoretical nonsense."[56]

Shortly after the Copenhagen conference, where the continuous beta spectrum no doubt was discussed thoroughly, Pauli received some welcome information from Meitner, which he immediately passed on to Bohr:

> On the occasion of a physicists' meeting in Zurich, Miss Meitner was also here. I spoke with her once more about the nuclear β rays and their continuous velocity spectrum, and it turned out that in our discussions in Copenhagen we have overlooked an important point regarding the heat experiment by Ellis and Wooster: the calorimeter for this experiment is constructed in such a way that it is transparent for γ rays. Thus, in this experiment one measures only the heat developed by the β rays; that of possible γ rays present would not be included (this is what Meitner says).

[55]Pauli to Klein, 18 February 1929, in W. Pauli, *Wissenschaftlicher Briefwechsel mit Bohr, Einstein, Heisenberg u.a.*, Vol. 1: 1919–1929, A. Hermann, K.v. Meyenn, and V.F. Weisskopf (eds.) (New York: Springer Verlag, 1979), pp. 488–492.

[56]Pauli to Klein, 16 March 1929, *ibid.*, pp. 494–495.

> A great difficulty for any theory insisting on the energy law is, however, the experiments on ThC, which seem to show the absence of any γ radiation. Personally, I still believe that there is some of that *too*. The heat experiment, of which we were so frightened before, thus does not *at all* speak against it![57]

Pauli informed Ehrenfest too about this point in his letters of 7 May and 15 May 1929.[58] In the latter, he described the Ellis–Wooster experiment as "completely harmless!"

The Ellis–Wooster experiment was far from "harmless," however. It was corroborated when Meitner and Wilhelm Orthmann repeated it in an improved version.[59] Their repetition was made possible by Orthmann's construction of a calorimeter that could be used without a very sensitive – and in Berlin unserviceable – needle galvanometer.[60] In contrast to Ellis and Wooster, the Berliners made an absolute determination of the heating effect, i.e., they determined directly the number of decaying radium E atoms per second. To do this, they had to prepare a very pure RaE source – a difficult task. "We have spent very much time and effort in obtaining RaE preparations as free of polonium as possible in order to be able to perform an absolute measurement of the quantity of RaE," Meitner wrote to Ellis on 20 July 1929.[61] Ellis recognized the delicacy of the experiment and wrote: "I did admire your work on RaE, the direct way you adopted was certainly the more difficult."[62]

The result of Meitner and Orthmann's investigation, an average beta-particle energy of 337,000 electron volts $\pm$ 6 percent, was a clear confirmation of the Cambridge experiment. Of paramount importance, however, was the question whether escaping gamma radiation could account for the missing energy. As we have seen, Aston had estimated the amount of energy escaping by the *known* RaE gamma rays. Should gamma radiation be able to account for the missing energy, however, its intensity maximum had to occur at a wavelength of about 2×10^{-10} cm, i.e., it had to be much more penetrating than the known gamma rays. Investigations were begun in Meitner's institute to search for such highly penetrating gamma rays. Susanne Bramson, for example, carried out very careful measurements.[63] She found only a weak radiation with a wavelength of about 5×10^{-10} cm, yet she

[57] Pauli to Bohr, 25 April 1929, *ibid.*, pp. 495–496.

[58] Pauli and Paul Scherrer to Ehrenfest, 7 May 1929, *ibid.*, p. 498; Pauli to Ehrenfest, 15 May 1929, *ibid.*, p. 500.

[59] L. Meitner and W. Orthmann, "Über eine absolute Bestimmung der Energie der primären β-Strahlen von Radium E," *Z. Phys.* 60 (1930), 143–155.

[60] W. Orthmann, "Ein Differentialkalorimeter zur Absolutbestimmung kleinster Wärmemengen," *Z. Phys. 60* (1930), 137–142.

[61] Meitner to Ellis, 20 July 1929, MTNR 5/4.

[62] Ellis to Meitner, 16 July 1930, MTNR 5/4.

[63] S. Bramson, "Absorptionskoeffizienten der γ-Strahlung von Radium D und Radium E und die Zahl der emittierten Quanten," *Z. Phys. 66* (1930), 721–740. See also L. Meitner, "Energieverteilung der primären β-Strahlen und die daraus zu folgernde γ-Strahlung," *Phys. Zeit. 30* (1929), 515–516.

could still not exclude higher-energy rays that might have escaped observation. Meitner requested further evidence, and one more experiment was begun.[64]

The idea of this experiment was simple. The number of gamma rays from RaE was compared to the number of RaB + C gamma rays that had penetrated 1 cm of lead. These RaB + C gamma rays were known to have an average wavelength of less than 2×10^{-10} cm, and thus would be even more inclined to escape observation than the RaE rays. The experiment showed that all of the RaE gamma rays constitute less than 4 percent of the very penetrating part of the RaB + C gamma rays, which left no room for any further dispute. This amount was far too small to account for the missing energy.

The long-lasting controversy thus finally came to an end. Meitner surrendered at a meeting in Zurich in early July 1929. She considered the consequences of Ellis's victory to be far-reaching:

> From this one must probably conclude that processes take place in the nucleus according to laws that are completely unknown to us today.[65]

A few days later, in her letter of 20 July 1929, she wrote directly to Ellis, stating that the result was incomprehensible to her:

> It seems to me now that beyond any reasonable doubt you are completely right in your hypothesis that the nuclear β rays are *primarily* inhomogeneous. However, I cannot at all understand this result. We have very carefully searched for a possible continuous γ radiation, but only a much too weak γ radiation is present. And yet, as long as we are not prepared to abandon the energy law, there is no theory which would not demand a continuous γ radiation equivalent to the continuous β spectrum. According to quantum mechanics, too, such a γ radiation should be there; however, it seems to be present neither for RaE nor for ThC. I am very anxious to learn the solution of this riddle.[66]

Even Pauli had to yield to the experiments, but he did so hesitantly. On 17 July 1929, he wrote to Bohr:

> In Zurich Miss Meitner gave us a beautiful lecture about the experimental aspect of the question, and she *almost* convinced me that the continuous β-ray spectrum *cannot* be explained by secondary processes (γ-ray emission etc.). So we really *don't* know what is the matter here. You don't know either, ... [67]

In her Zurich lecture, Meitner also discussed another secondary process on which she had previously pinned her hope: collisions between the primary beta

[64]See Meitner and Orthmann, "Über eine absolute Bestimmung" (note 59), pp. 153–154.

[65]Meitner, "Energieverteilung" (note 63), p. 516.

[66]Meitner to Ellis, 20 July 1929, MTNR 5/4.

[67]Pauli to Bohr, 17 July 1929, *BSC* (14,3). Reproduced in Pauli, *Briefwechsel* (note 55), pp. 512–514. The translation is from *BCW*, Vol. 6, *Foundations of Quantum Physics I (1926–1932)*, J. Kalckar (ed.) (Amsterdam: North-Holland, 1985), p. 447.

particles and atomic electrons. She now admitted that the number of beta particles obtained by Riehl, contrary to the conclusion in his paper, was far too small to account for the continuous spectrum, and this point was supported by a recent investigation of the amount of characteristic K radiation from RaE.[68]

Accordingly, after July 1929, physicists agreed that the continuous beta spectrum was an anomaly; but their diagnosis and treatment differed fundamentally. Ellis's and Meitner's reactions display this divergence, and we shall meet similar cases in the next chapter.

5.6 Some concluding remarks about the controversy

The controversy between Meitner and Ellis was not merely the result of a confrontation between two people of different views. In a wider perspective, I suggest that it can also be seen as the result of a confrontation between two different scientific communities – one deeply rooted in traditional British empiricism, the other influenced by a more rationalistic spirit of Continental thinking.

The foundation of Ellis's view was exclusively the British experimental results. Chadwick's experiment, Rutherford, Robinson and W.F. Rawlinson's experiment together with Ellis's repetition of it, and to some extent Rutherford and Edward N. da Costa Andrade's experiment, formed the basis of Ellis's "inherently difficult"[69] hypothesis on the origin of the continuous beta spectrum. The serious questions raised by this hypothesis were considered of secondary importance, if Ellis considered them serious at all. His reaction to the result of his and Wooster's calorimeter experiment indicates that he did not. Without hesitation, he was ready to abandon the fundamental accuracy of the quantum postulate, and he maintained that view for a rather long time.

Meitner, of course, also attached great importance to experimental results; like Ellis, she too was an experimentalist. However, her more rationalistic view, in addition to her deep familiarity with quantum theory, made her reluctant to accept Ellis's hypothesis. Contrary to Ellis, she fully realized the serious consequences of his hypothesis, and she therefore searched intensely for secondary origins of the continuous spectrum. Several times she pointed to the analogy with alpha rays. I agree with Leonard Loeb's assessment that "Meitner went very much further than merely stating an aesthetic idea when she assumed all β-rays to have the same velocity."[70]

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[68] Meitner, "Energieverteilung" (note 63), p. 516; Bramson, "Absorptionskoeffizienten" (note 63).

[69] This was the way in which Meitner referred to Ellis's hypothesis in her Zurich lecture. See Meitner, "Energieverteilung" (note 63), p. 515.

[70] L.B. Loeb, "Note Concerning the Emission of Beta-Rays in Radioactive Change," *Phys. Rev., 34* (1929), 1212–1216, p. 1213.

Chapter 6

From Anomaly to Explanation: The Continuous Beta Spectrum, 1929–1934

6.1 Introduction

The year 1932 was a turning point in the history of nuclear physics. The neutron, the positron and the deuteron were discovered, and the first nuclear disintegrations with artificially accelerated protons were made.[1] These events stimulated a dramatic increase in the number of publications and Ph.D. degrees awarded in nuclear physics, and the possibility for raising funds for nuclear research was much improved.[2]

Another, less conspicuous, turning point occurred in the late 1920s. At that time, many theoretical physicists turned their attention to nuclear problems and began discussing them intensely at conferences as well as in correspondence. As I have previously emphasized, their interest in the nucleus turned from a distant contemplation into an active role in discussions. Nuclear physics, instead of being mainly an experimental concern, became an essential part of a large set of problems centered around relativistic quantum physics.

In this chapter, I shall focus on the reactions, starting around 1929, to the anomalous energy continuity in the beta spectrum, especially Niels Bohr's suggestion of non-conservation of energy and Wolfgang Pauli's alternative, the neutrino hypothesis. I shall also deal with the role of experiment in the dispute between the two alternatives.

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[1] J. Chadwick, "Possible Existence of a Neutron," *Nature 129* (1932), 312; *idem.*, "The Existence of a Neutron," *Proc. Roy. Soc. A136* (1932), 692–708; C.D. Anderson, "The Apparent Existence of Easily Deflectable Positives," *Science 76* (1933), 238–239; *idem.*, "The Positive Electron," *Phys. Rev. 43* (1933), 491–494; H.C. Urey, F.G. Brickwedde, and G.M. Murphy, "A Hydrogen Isotope of Mass 2," *Phys. Rev. 39* (1932), 164–165. The spectroscopic discovery of heavy hydrogen was first reported by Harold Urey at a meeting of the American Physical Society at Tulane University, 28–30 December 1931. See also J.D. Cockcroft and E.T.S. Walton, "Disintegration of Lithium by Swift Protons," *Nature 129* (1932), 649.

[2] C. Weiner, "1932 – Moving into the New Physics," *Physics Today 25* (May 1972), 40–49.

C. Jensen et al. (eds.), *Controversy and Consensus: Nuclear Beta Decay 1911-1934*

6.2 Non-conservation of energy or a new particle? The first phase of the Bohr–Pauli dispute, 1929–1932

If Pauli distrusted the *Cambridger Autoritäten*, Bohr seems to have felt confident in their result: there is no evidence that anyone in Bohr's institute joined Lise Meitner and Pauli in their skeptical attitude towards the Ellis–Wooster experiment. In Copenhagen, as in Cambridge, the continuous beta spectrum was probably considered an anomaly already in 1928.

This anomaly challenged George Gamow, who spent the academic year 1928–1929 in Bohr's institute. Encouraged by his successful quantum-mechanical theory of alpha decay, he threw himself into trying to understand the continuous beta spectrum.[3] His efforts to explain also beta decay within the framework of quantum theory proved to be fruitless, however, and this failure strengthened Bohr's belief that the problem was a profound one. In December 1928, Bohr wrote to Werner Heisenberg:

> Gamow has of late occupied himself thoroughly with the continuous β-ray spectra; but every search for other solutions has hitherto strengthened my conviction that the difficulties lie very deep ... . In spite of Pauli's warnings, I am also still prepared for a further limitation of the applicability of the energy concept.[4]

Perhaps the beta spectrum headed the list of nuclear anomalies around the end of 1928, but other serious difficulties had also appeared. In 1926, Ralph de Laer Kronig dealt the nuclear-electron hypothesis the first blow by arguing that, contrary to observation, electrons in the nucleus had to cause hyperfine splittings as large as ordinary Zeeman splittings. And in 1928, he found another flaw in the fabric. Measurements on the band spectrum of the N_2^+ ion indicated that the nitrogen nucleus had a spin of 1 (in units of $h/2\pi$), which was inconsistent with the contemporary view of the nucleus as being built up of 14 protons and 7 electrons, that is, an odd number of spin-1/2 particles.[5]

In early 1928, Paul Dirac published his relativistic quantum theory of the electron.[6] While it was a great step forward, problems emerged in the wake of

[3] For a historical account of Gamow's theory of alpha decay, see R.H. Stuewer, "Gamow's Theory of Alpha Decay," in *The Kaleidoscope of Science: The Israel Colloquium Studies in History, Philosophy, and Sociology of Science*, E. Ullmann-Margalit (ed.) (Dordrecht: Reidel, 1986), pp. 147–186.

[4] Bohr to Heisenberg, December 1928, *BSC* (11.2); the translation has been taken from *BCW*, Vol. 6, *Foundations of Quantum Physics I (1926–1932)*, Jørgen Kalckar (ed.) (Amsterdam: North-Holland, 1985), pp. 24–25.

[5] For a thorough discussion of the difficulties of the nuclear-electron hypothesis, and for references, see R.H. Stuewer, "The Nuclear Electron Hypothesis," in *Otto Hahn and the Rise of Nuclear Physics*, W.R. Shea (ed.) (Dordrecht: Reidel, 1983), pp. 19–67.

[6] For a historical account of Dirac's relativistic theory of electrons, and for references, see H. Kragh, "The Genesis of Dirac's Relativistic Theory of Electrons," *AHES 24* (1981), 31–67; *idem*, *Dirac: A Scientific Biography* (Cambridge University Press, 1990), pp. 48–66.

its success. The Dirac equation allowed for positive as well as negative energy solutions. So did the corresponding classical equation, but in the classical case, the negative solutions could be excluded since a discontinuous transition from positive to negative energy did not occur. In quantum theory, however, such a transition was possible, and therefore the negative-energy solutions could not be disregarded.

Another difficulty brought about by Dirac's theory was the so-called Klein paradox. Towards the end of 1928, the Swedish physicist Oskar Klein discovered that an electron had a considerable probability of passing through a high and steep potential barrier, thus being transformed into a particle of negative energy.[7] It hence seemed as if electrons could not be confined within the nucleus at all.

A further indication of energy non-conservation came from experiments in the Cavendish Laboratory on the expulsion of protons by bombardment of nuclei with alpha particles. By examining the absorption of protons emitted from aluminum, Ernest Rutherford and James Chadwick had found the surprising result that the energy of the protons varied, apparently continuously, from about 0.32 to about 1.1 of the energy of the incident alpha particles. "It appears from this evidence," they wrote, "that the process of disintegration of an aluminium nucleus by an α particle of given energy is not exactly the same for each individual nucleus."[8] To Rutherford and Chadwick, the simplest explanation was that all nuclei did not have the same mass or energy content, an explanation that, they argued, was supported by the continuity in energy of the disintegration electrons from beta-emitting nuclei.

There were other possible explanations of the experiments, however, and Bohr was much interested in all conceivable ones. In a letter of 14 February 1929, he wrote to Ralph Fowler:

> In connection with Rutherford's new experiments on the expulsion of protons by bombardment of atomic nuclei with α-rays, I have been wondering whether he thinks it excluded that the observed velocity distribution of protons may arise from different discrete stages of excitation of the resulting nucleus, and if an emission of γ-rays accompanying this excitation would escape observation. If even in proton transformations we witness a want of definition of energy, new aspects indeed seem to open.[9]

Not only Bohr had noted this possibility. According to a letter from Heisenberg to Bohr of 20 December 1929, Charles Drummond Ellis had asserted that also protons emitted from nitrogen showed a continuous energy spectrum, and Heisenberg commented as follows:

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[7]O. Klein, "Die Reflexion von Elektronen an einem Potentialsprung nach der relativistischen Dynamik von Dirac," *Z. Phys. 53* (1929), 157–165.

[8]E. Rutherford and J. Chadwick, "Energy Relations in Artificial Disintegration," *Proc. Camb. Phil. Soc. 25* (1928–1929), 186–192, p. 190.

[9]Bohr to Fowler, 14 February 1929, *BSC* (10.3).

> I find Ellis's claim that also the H particles from the disintegration of N show a continuous spectrum dreadful; for how shall one then understand the sharp α-ray spectra? Do you find these experiments convincing? I am very curious what Pauli will have to say about it all.[10]

Other experimentalists obtained a different result, however. Walther Bothe and Hans Fränz, as well as Heinz Pose, were of the opinion that the proton spectrum consisted of distinct lines, and they proved to be right.[11] Because an alpha particle may be captured in the normal as well as in an excited energy state, the emitted protons may have different, but discrete energy values.

From the above letter to Fowler, it appears that Bohr had not only beta spectra in mind when he suggested questioning the conservation laws. "Lately I have been thinking a good deal of the possible limitation of the conservation theorems in relativistic quantum theory," he confided to his friend in Cambridge, "and we have just been discussing, if in the reversal of β-ray transformations we might find the mysterious source of energy claimed by Eddington's theory of constitution of stars."[12] Probably strongly influenced by Klein's paradox, Bohr now directed his attention towards relativistic quantum theory in general, even hoping to find an explanation of the energy production in stars. Quite consistent with his general attitude, Bohr thus searched for a solution embracing all of the problems in question.

In a short manuscript – entitled "β-Ray Spectra and Energy Conservation" and probably written in June 1929 – Bohr again emphasized the possibility that the failure of energy conservation might be related to stellar energy production.[13] This manuscript provides a deeper insight into the direction of Bohr's views. The immediate occasion for writing it seems to have been two papers by George P. Thomson,[14] which contained conclusions from which Bohr was anxious to dissociate himself.

Thomson attempted to explain non-conservation of energy as a natural consequence of the wave theory of matter. He imagined that the same amount of energy was liberated in every beta decay and that, at the moment of emission, the primary beta particle was associated with a short heavily-damped wave, like the sound wave caused by an explosion. Owing to dispersion, the short wave would

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[10] Heisenberg to Bohr, 20 December 1929, *BSC* (11.2).

[11] W.W. Bothe and H. Fränz, "Atomtrümmer, reflektierte α-Teilchen und durch α-Strahlen erregte Röntgenstrahlen," *Z. Phys. 49* (1928), 1–26; H. Pose, "Messungen von Atomtrümmern aus Aluminium, Beryllium, Eisen und Kohlenstoff nach der Rückwärtsmethode," *Z. Phys. 60* (1930), 156–167.

[12] Bohr to Fowler, 14 February 1929, *BSC* (10.3).

[13] N. Bohr, "β-Ray Spectra and Energy Conservation," *Bohr MSS* (12.1); the manuscript is reproduced in *BCW*, Vol. 9, *Nuclear Physics (1929–1952)*, R.E. Peierls (ed.) (Amsterdam: North-Holland, 1986), pp. 85–89.

[14] G.P. Thomson, "The Disintegration of Radium E from the Point of View of Wave Mechanics," *Nature 121* (1928), 615–616; *idem.*, "On the Waves associated with β-Rays, and the Relation between Free Electrons and their Waves," *Phil. Mag. 7* (1929), 405–417.

spread rapidly, and thus increase the space within which the electron might occur. In Thomson's view, this spreading of the wave had to be interpreted as "a marked 'straggling' in velocity";[15] accordingly, to retain both "the conception of a moving particle and the whole analytical machinery of the wave mechanics,"[16] he was forced to "allow the possibility of an electron changing speed in force-free space."[17] As a consequence, energy conservation had to be abandoned.

Bohr was sympathetic to the idea of non-conservation of energy, but he felt that Thomson's idea was not in accordance with the general principles of quantum mechanics. In Bohr's view, non-conservation of energy and momentum could not be explained within the framework of quantum theory, but had to be considered as a consequence of a new and more general theory affecting also "the problem of the constitution of the elementary electric particles which as well known has so far escaped a proper treatment on the basis of classical electrodynamics."[18]

Bohr had suggested abandoning energy conservation five years earlier in the famous Bohr–Kramers–Slater theory;[19] but at that time, he still believed in statistical energy conservation. Now he went further and wanted, in Rutherford's words, "to upset the Conservation of Energy both microscopically and macroscopically."[20] In a letter to Meitner, Gamow described Bohr's 1929 view as follows:

> Bohr was very interested in β uncertainty – he believes that we have here the real non-conservation of energy. He now goes even further and stresses that the energy need not be conserved even in the mean. In this way, he believes to be able to explain the production of stellar heat. The slow electrons (thermal motion) enter the nucleus and fly away again, but now with β-decay energies. Thus we have the eternal "β-Perpetuum-Mobile-Incorporated" in the stars.[21]

Thus, Bohr's statistical energy conservation of 1924 was quite distinct from his idea of 1929: I agree with Abraham Pais that the statements of a relationship repeatedly found in the literature are without foundation.[22]

Considering his attachment to the correspondence principle, Bohr's willingness to abandon fundamental classical laws such as conservation of energy and momentum, even at the macroscopic level, may seem strange. As he explained to

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[15] Thomson, "Disintegration" (note 14), p. 615.

[16] Thomson, "Waves" (note 14), p. 413.

[17] *Ibid.*, p. 413.

[18] Bohr, "β-Ray Spectra" (note 13), p. 87.

[19] N. Bohr, H.A. Kramers, and J.C. Slater, "The Quantum Theory of Radiation," *Phil. Mag. 47* (1924), 785–802; reprinted in B.L. van der Waerden, *Sources of Quantum Mechanics* (New York: Dover Publications, 1968), pp. 159–176. For a historical account, see J. Hendry, "Bohr–Kramers–Slater: A Virtual Theory of Virtual Oscillators and Its Role in the History of Quantum Mechanics," *Centaurus 25* (1981), 189–221; and M. Dresden, *H.A. Kramers: Between Tradition and Revolution* (New York: Springer-Verlag, 1987), pp. 41–56.

[20] Rutherford to Bohr, 19 November 1929, *BSC* (15.3). Quoted in A. Pais, *Inward Bound: Of Matter and Forces in the Physical World* (New York: Oxford University Press, 1986), p. 311.

[21] Gamow to Meitner, 27 November 1929, MTNR 5/6.

[22] Pais, *Inward Bound* (note 20), p. 312n.

Hendrik Kramers in a letter of 11 November 1926, "By [a correspondence theory] I mean a theory permitting a consistent use of the concepts of classical theory in harmony with the basic postulates of atomic theory."[23] One would thus think that he had clung to such classical laws, and in fact he did in the domain where the correspondence principle worked. His willingness to upset conservation of energy and momentum is to be seen, I think, in light of his recognition of the limitations of this principle. In classical electron theory, the electron is idealized as a charged material point, but a finite electron radius

$$r = \frac{e^2}{mc^2},$$

where e and m are the charge and mass of the electron, had to be incorporated into the theory. Within this distance, which also happened to constitute the radius of the nucleus, classical theory failed to work, and accordingly the correspondence argument could not be used. In this domain, Bohr often emphasized, "one should be prepared for surprises."[24]

Without the correspondence principle, Bohr felt at a loss in treating nuclear phenomena. He stressed "how little basis we possess at present for a theoretical treatment of the problem of β-decay disintegrations."[25] Hence, energy non-conservation could not be rejected in advance. The serious problems confronting physicists at the end of the 1920s seemed to affirm that the behavior of electrons fell outside the field of ordinary mechanical concepts, even in their quantum-theoretical modifications, and "from this point of view," Bohr stated, "the disintegration of the nucleus should rather be regarded as the creation of the dynamical individuality of the electron expelled."[26]

Bohr also advanced specific theoretical arguments in favor of abandoning conservation of energy and momentum. Since in relativistic quantum theory, "the conception of force is inherently bound up with the idea of mass," he argued, "the suggestion presents itself, that the laws of interaction between particles of different mass may conflict with a simple identification of action and reaction, as that underlying the classical conservation principles."[27] Thus, a departure from the conservation laws might well result from the close interaction of the constituent particles in the nucleus.[28]

On 1 July 1929, Bohr sent a manuscript on beta-ray spectra and energy conservation to Pauli, who was far from enthusiastic about it:

> I must say that it gave me very *little* satisfaction. It already starts so depressingly with a reference to the nonsensical remarks by G.P. Thomson, and from this the people in England will only draw the

[23] Bohr to Kramers, 11 November 1926, *AHQP* (9.5).
[24] Pauli to Weisskopf, 17 January 1957, *AHQP* (66.4).
[25] Bohr, "β-Ray Spectra" (note 13), p. 88.
[26] *Ibid.*, p. 88.
[27] *Ibid.*, p. 88.
[28] N. Bohr, "Das Wirkungsquantum," unpublished manuscript, 1930, *Bohr MSS* (12.3), 75pp.

erroneous conclusion that you regard these remarks as important. Then comes the unpleasant introduction of the electron diameter, d; I do *not* mean to say that this is actually illegitimate, but it is always a risky matter. One should then also take into account that for electrons moving almost with the velocity of light, d becomes, because of the Lorentz contraction, *much smaller* than $\frac{e^2}{m_0 c^2}$; namely,

$$\frac{e^2}{mc^2} \qquad \left(m = \frac{m_0}{\sqrt{1 - v^2/c^2}}\right),$$

at least in the longitudinal direction.[29]

Pauli admitted that he had not the slightest idea of what was wrong, but then continued:

You don't know either, and can only state reasons *why* we understand nothing. After all, you write yourself that the purpose of the note is to emphasize "how little basis we possess at present for a theoretical treatment of the problem of β-ray disintegrations." But here I must raise the question whether such a negative purpose can at all serve as a justification for publishing a note! *In any case let this note rest for a good long time and let the stars shine in peace!*[30]

Bohr never published his manuscript, but neither did he abandon his idea of a new theory in which energy and momentum were not conserved. On the contrary, he was much engaged in this problem during the following years, not sharing the pessimistic attitude towards it expressed by other physicists at that time.[31]

Dirac was another opponent of Bohr's view. Towards the end of 1929, he corresponded avidly with Bohr on the question of energy conservation. The immediate reason for their correspondence was Dirac's suggestion that the negative energy states were occupied by electrons, and thus constituted an infinite sea of unobservable entities. A transition from a negative to a positive energy state hence resulted in an ordinary electron and a hole, which Dirac identified as a proton. Bohr wrote to Dirac on 24 November 1929 to obtain further information about Dirac's new idea, and took the opportunity to explain his own view

... that the difficulties in your theory might be said to reveal a contrast between the claims of conservation of energy and momentum on one side and of the conservation of the individual particles on the other

[29]Pauli to Bohr, 17 July 1929, *BSC* (14.3). The original letter is reproduced in W. Pauli, *Wissenschaftlicher Briefwechsel mit Bohr, Einstein, Heisenberg u.a.*, Vol. 1: 1919–1929, A. Hermann, K.v. Meyenn, and V.F. Weisskopf (eds.) (New York: Springer Verlag, 1979), p. 512. The translation is from *BCW*, Vol. 6 (note 4), pp. 446–447.
[30]*Ibid.*, p. 447.
[31]Bohr to Heisenberg, 8 December 1930, *BSC* (20.2).

> side. The possibility of fulfilling both these claims in the usual correspondence treatment would thus depend on the possibility of neglecting the problem of the constitution of the electron in non-relativistic classical mechanics. It appeared to me that the finite size ascribed to the electron on classical electrodynamics might be a hint as to the limit for the possibility of reconciling the claims mentioned. Only in regions where electronic dimensions do not come into play, the classical concepts should present a reliable fundament for the correspondence treatment.[32]

Bohr thus revealed that he had in mind a new kind of complementary relationship, one between the validity of the conservation laws on the one hand and the concept of particle permanence on the other, and in this new complementarity the classical electron diameter was to play the role of the fundamental constant. Dirac replied to Bohr that he "would rather abandon even the concept of matter consisting of separate atoms and electrons than conservation of energy."[33]

Dirac's idea of an infinite sea of electrons did not appeal to Bohr. "We do not understand, how you avoid the effect of the infinite electric density in space," he wrote to Dirac on 5 December, and continued:

> I still feel inclined to see a limit of the fundamental concepts on which atomic theory hitherto rests rather than a problem of interpreting the experimental evidence in a proper way by means of these concepts. Indeed according to my view the fatal transitions from positive to negative energy should not be regarded as an indication of what may happen under certain conditions but rather as a limitation in the applicability of the energy concept.[34]

As support for his view, Bohr emphasized Klein's paradox, calling it "a striking example of the difficulties involved in an unlimited use of the concept of potentials in relativistic quantum mechanics," and a good demonstration of "the actual limit of applying the idea of potentials in connection with possible experimental arrangements."[35]

Bohr's willingness to abandon the conservation laws did not include charge conservation. "In the fact," he wrote to Dirac, "that the total charge of the nucleus can be measured before and after the β-ray disintegration and that the results are in conformity with conservation of electricity I see a support for upholding the conservation of the elementary charge even at the risk of abandoning the conservation of energy."[36]

This lack of symmetry was one of Pauli's main objections to Bohr's views. In a letter to Klein of 12 December 1930, he advanced his misgivings:

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[32]Bohr to Dirac, 24 November 1929, *BSC* (9.4).
[33]Dirac to Bohr, 26 November 1929, *BSC* (9.4).
[34]Bohr to Dirac, 5 December 1929, *BSC* (9.4).
[35]*Ibid.*
[36]*Ibid.*

> First, it appears to me that the conservation laws for energy and momentum and for charge are to a very great extent analogous, and I can see no theoretical reason why the latter should still be valid (as we know it empirically from β decay) if the former are not.[37]

In the same letter, Pauli introduced a further, very interesting, argument:

> Secondly, abandoning the energy law would make very strange things happen to the *weight*. Imagine a closed box in which there is radioactive β decay; the β rays would then somehow be absorbed in the wall and would not be able to leave the box. Individual observations of what goes on in the box could not be made; only the total weight of the box could be determined (with suitable accuracy). If the energy law thus would not be valid for β decay, the total weight of the closed box would consequently change. (This conclusion seems quite compelling to me). This is in utter opposition to my sense of physics! For then it has to be assumed that even the gravitational field – which is itself *generated* (it does not change the argument that *in practice* the field cannot be measured because of its smallness) by the entire box (including its radioactive content) – might change, whereas the electrostatic field, which is measured from the outside, should remain unchanged because of the conservation of charge (yet both fields seem analogous to me; that, incidentally, you will recall from your five-dimensional past). I would like to ask Bohr seriously whether he believes this and, if so, how he can make it plausible! So I would be *very* grateful to him if I soon received a letter from him on this matter, beginning with the sentence: "We agree of course completely, *but* ..."[38]

We shall see in a subsequent section that this problem was taken up again in 1933 by the Russian physicist Lev Landau.

Pauli did not stop at arguing against Bohr's view, however. In a famous letter of 4 December 1930, addressed to the "radioactive ladies and gentlemen" present at a meeting in Tübingen, he suggested an alternative hypothesis: an additional particle, emitted along with the beta particle, might exist in the nucleus.[39]

The exact birthday of Pauli's neutrino, or neutron as he first called it, is not easy to trace. To my knowledge, the first reference to Pauli's new particle is in a letter of 1 December 1930 from Heisenberg to Pauli,[40] but the idea may well have occurred to Pauli somewhat earlier that year. According to the letter of 12 December 1930 from Pauli to Klein, he conceived it in connection with solving a problem relating to the hyperfine structure of Li^+. Pauli there proposed that "it

[37] Pauli to Klein, 12 December 1930, in W. Pauli, *Wissenschaftlicher Briefwechsel mit Bohr, Einstein, Heisenberg u.a.*, Vol. 2: 1930–1939, K.v. Meyenn (ed.) (New York: Springer Verlag, 1985), p. 45.

[38] *Ibid.*, pp. 45–46.

[39] *Ibid.*, pp. 39–40.

[40] *Ibid.*, p. 34.

might be that the nuclei, apart from electrons and protons, contained yet other elementary particles; these would then have to be electrically neutral, obey Fermi statistics, and have a spin of 1/2."[41]

In Pauli's own judgment, his neutrino hypothesis was a desperate resolution of the problems, and in December 1930 he did not believe much in it himself. To Klein, he explained why:

> However, it depends essentially on which forces act on these neutrons [i.e., neutrinos]. If no forces whatever, or only very weak forces, act on them, they could not at all remain in the nucleus. Almost the only possible model of the neutron from the standpoint of Dirac's theory would be the one which, in an external field $F_{\mu\nu}$, satisfies the wave equation
>
> $$\gamma^{\mu}\frac{h}{2\pi i}\cdot\frac{\partial\psi}{\partial x_{\mu}}+\mu(i\gamma^{\mu}\gamma^{\nu})F_{\mu\nu}\psi-imc\psi=0$$
>
> (where, as usual, $\gamma^{\mu}\gamma^{\nu}+\gamma^{\nu}\gamma^{\mu}=2\delta_{\mu\nu}$). Because $e=0$, the terms with the potential are ignored here; it further *follows* for such particles that for small velocities they behave as a magnetic dipole of momentum μ (the constant μ has the dimension charge times length). – But there is a big snag in all of this. For, if one assumes for μ a common magneton, then, as a simple estimate shows, the ionizing power of the neutrons would not be significantly smaller than that of the β particles, and all Wilson exposures would indeed have to be crowded with neutrons. Even if it is assumed that μ is of the order of magnitude of the proton magneton and that m is as large as possible (0.01 proton mass), the ionizing power would probably not be smaller in order of magnitude than that resulting from the β rays. So if the neutrons really existed, it would scarcely be understandable that they have not yet been observed. Therefore, I also do not myself believe very much in the neutrons, have published nothing about the matter, and have merely induced some experimental physicists to search in particular for this sort of penetrating particles. I would very much like to know what Bohr thinks about it.[42]

Apparently, Bohr did not himself inform Pauli of his opinion, but left this task to Klein. This is evident from Pauli's sarcastic answer to Klein of 8 January 1931:

> Of course I am aware that the neutron hypothesis does not suit Bohr and the Bohrians. Exactly for this reason I am particularly pleased to discuss it. I interpret the many exclamation marks in your letter mainly as horror about the fact that the energy law might nevertheless be valid. Since Bohr assumes the opposite, this would lead to a serious

[41] *Ibid.*, p. 44.
[42] *Ibid.*, pp. 44–45.

> upheaval of your psychological world view. For nothing is funnier than a Bohrian; but this is not Bohr's fault. ...[43]

Obviously, Bohr was very skeptical about Pauli's neutrino hypothesis, and neither in his Rome Lecture of 1931 nor in his published Faraday Lecture of 1932 did he even mention the possibility of the particle's existence.[44] The neutrino would indeed explain the statistics problem as well as the continuous beta spectrum, but Bohr's ambitions were greater still. The problems in relativistic quantum mechanics – for instance Klein's paradox – were still present, and Bohr was convinced that only with a new fundamental theory could *all* of the problems be solved.

Pauli spent the summer of 1931 in the United States, where he gave some lectures and talked about his new particle. Although some cloud-chamber tracks indicated that neutrinos might perhaps be present in cosmic rays, he desisted from publishing his hypothesis.[45] He was probably still too uncertain about it, and during 1931 it did not receive much support, at least in public.

Through 1932, Bohr's view enjoyed the greater support. Several physicists – not only close collaborators of Bohr's such as Klein, Gamow and Heisenberg – felt the need for a new fundamental theory. In a letter to Bohr of 29 April 1931, Erwin Schrödinger also expressed his dissatisfaction with the present state of quantum mechanics and his longing for "a great new idea:" "In quantum mechanics we have come almost just as far as in electrodynamics *before* Faraday and Maxwell," he wrote.[46] Bohr answered in a letter of 8 May:

> I agree entirely with your opinion insofar as I too believe that we have reached the limit of the applicability of the present views and methods. However, at the same time I want to emphasize that to me this situation appears to be a natural consequence of the inadequacy of the classical physical theories in explaining the existence of the elementary particles. All applications and successes of the quantum theory so far are based on the assumption of their existence. To proceed further would require a conceptualization according to which the existence of the particles and of the quantum of action from the outset appear as inseparable consequences of a general principle.[47]

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[43] *Ibid.*, p. 51.

[44] N. Bohr, "Atomic Stability and Conservation Laws," in *Atti del Convegno di Fisica Nucleare Ottobre 1931* (Rome: Reale Accademia d'Italia, 1932), pp. 119–130; *idem.*, "Chemistry and the Quantum Theory of Atomic Constitution," *J. Chem. Soc.* (1932), 349–384. Bohr delivered his Faraday Lecture to the Chemical Society on 8 May 1930.

[45] J.F. Carlson and J. Robert Oppenheimer, "On the Range of Fast Electrons and Neutrons," *Phys. Rev. 38* (1931), 1787–1788. For a historical discussion of the neutrino hypothesis, see L.M. Brown, "The Idea of the Neutrino," *Physics Today 31* (September 1978), 23–28; K.v. Meyenn, "Pauli, das Neutrino und die Entdeckung des Neutrons vor 50 Jahren," *Naturwiss. 69* (1982), 564–573; and A.Q. Morton, *The Neutrino and Nuclear Physics, 1930–1940* (Ph.D. thesis, University of London, 1982). I am grateful to Alan Q. Morton for having lent me a copy of his thesis.

[46] Schrödinger to Bohr, 29 April 1931, *BSC* (25.3).

[47] Bohr to Schrödinger, 8 May 1931, *BSC* (25.3).

Samuel Goudsmit was another adherent to the view that a new theory was needed. In a paper at the Rome Conference of October 1931, he stated in his conclusion:

> It is my belief that the mechanics applicable to the nucleus must differ considerably from the quantum mechanics now used for the atom, in the same way as the latter differs from the classical mechanics for large masses.[48]

Figure 6.1: The first international conference on nuclear physics, Rome 1931. Goudsmit is standing on the far right, looking away from the photographer. Meitner is hidden behind Goudsmit. Ellis is standing fifth from the right (not counting Meitner and the man in the window). [Emilio Segrè Collection, Niels Bohr Library, American Institute of Physics]

This situation changed only after 1932, as I will discuss in a subsequent section. Meanwhile, we shall see that others besides Bohr and Pauli offered solutions to the riddle of the continuous beta spectrum.

[48]S. Goudsmit, "Present Difficulties in the Theory of Hyperfine Structure," in *Atti* (note 44), pp. 33–49, on p. 49.

6.3 Other attempts at explaining the anomalous continuity

In the wake of Gamow's as well as Ronald W. Gurney and Edward U. Condon's successful quantum-mechanical theory of alpha decay, it was natural to speculate whether beta decay could be explained similarly. We already have seen that Gamow himself tried to do so, and Gurney and Condon also made an attempt.[49] Gurney and Condon assumed that the central core of the nucleus was the habitat of nuclear electrons, and that the nuclear potential-energy curve might thus be of the type shown in Figure 6.2. In analogy to the case of alpha decay, a nuclear electron in the internal region had a certain chance of penetrating the barrier and escaping, for example with a kinetic energy given by the height of the line CD representing its energy above its ground state.

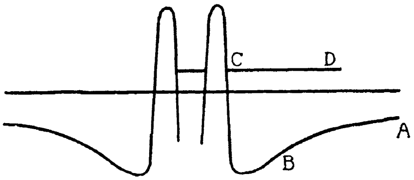

Figure 6.2: Gurney and Condon's potential-energy curve for a nuclear electron. The outer slope AB represents the Coulomb inverse-square field, and the potential barrier was assumed to be due to magnetic interactions. Source: Gurney and Condon, "Quantum Mechanics" (note 49), p. 137.

There was a fundamental difference between alpha and beta decay, however. Contrary to alpha spectra, beta spectra were continuous, and at the Schenectady meeting of the National Academy of Sciences on 20 November 1928 Irving Langmuir asked Condon whether his theory was able to explain the continuous beta-ray spectrum. Condon was embarrassed by this question, he said later, because "that was the first that I had heard of the continuous β spectrum."[50] Since Gurney collaborated with Ellis in the mid-1920s, and was on his side in the controversy with Meitner, Condon's unfamiliarity with the continuous beta spectrum is indeed surprising.

Even if Gurney and Condon's attempt at explaining beta decay as a tunneling effect thus became a signal failure, it did not discourage the Hungarian physicist Johann Kudar from trying his hand at it. He had earlier been concerned with relativistic quantum mechanics and was "one of the many fathers of the rel-

[49] R.W. Gurney and E.U. Condon, "Wave Mechanics and Radioactive Disintegration," *Nature* *122* (1928), 439; *idem.*, "Quantum Mechanics and Radioactive Disintegration," *Phys. Rev.* *33* (1929), 127–140. For a brief discussion of this work, see Stuewer, "Gamow's Theory" (note 3).

[50] E.U. Condon, "Tunneling – How It All Started," *Am. J. Phys.* *46* (1978), 319–323, p. 320.

ativistic wave equation."[51] In the autumn of 1928 he went to Berlin, and both Schrödinger and Meitner considered him to be a promising scientist. In a letter of 18 January 1929, the former characterized him as "highly nervous, but personally very congenial, and a rather unhappy man."[52] Later, since Kudar desired to go to Copenhagen, Bohr received further particulars from Schrödinger:

> It is unfortunately true that Kudar has fallen out quite thoroughly with his local authorities; and much of it is certainly his own fault, for not only could he not keep his mouth shut about the Hungarian protectionism and nepotism, which in some cases indeed took place, but he has also let his criticism be known in quite a foolish way. Moreover, in accordance with the present attitude everywhere, in any other place he always has the difficulty of being a foreigner.[53]

Bohr raised funds for Kudar, who visited Copenhagen for some time in 1931.

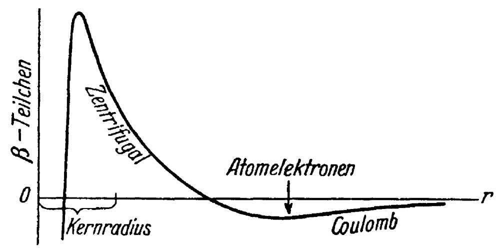

Figure 6.3: Kudar's potential-energy curve for a nuclear electron. The electrons were assumed to be emitted with a certain angular momentum. Source: Kudar, "Eigenschaften" (note 54), p. 36.

During his stay in Berlin, Kudar published several papers on the wave-mechanical character of beta decay.[54] His basic assumption was that the nuclear electrons (charge e, mass m) were emitted with a certain angular momentum, giving rise to a potential barrier. The potential *outside* the nucleus was then given by

$$U = -\frac{Ze^2}{r} + \frac{h^2}{8\pi^2 m} \cdot \frac{n(n+1)}{r^2},$$

where Z is the atomic number, n the azimuthal quantum number, and r the distance from the center of the nucleus. Kudar agreed with Bohr that quantum

[51] Schrödinger to Bohr, 18 January 1929, *BSC* (16.2).

[52] *Ibid.*

[53] Schrödinger to Bohr, 25 September 1930, *BSC* (25.3).

[54] J. Kudar, "Der wellenmechanische Character des β-Zerfalls," *Z. Phys. 57* (1929), 257–260; "... II," *ibid. 60* (1930), 168–175; "... III," *ibid. 60* (1930), 176–180; "... IV," *ibid. 60* (1930), 686–689; *idem.*, "Die β-Strahlung und das Energieprinzip," *ibid. 64* (1930), 402–404; *idem.*, "Eigenschaften der Kernelektronen," *Phys. Zeit. 32* (1931), 34–37.

mechanics in its present form could not be used inside the nucleus, and thus he assumed that the repulsive centrifugal force was neutralized by an unspecified, but strong, attractive force there. He suggested the potential-energy curve shown in Figure 6.3.

On the basis of his theory, Kudar derived an expression for the decay constant in which the energy of the beta particle, the radius r_0 of the potential well (not shown in Figure 6.3), and the azimuthal quantum number n entered, i.e., an expression that was formally analogous to the Geiger–Nuttall relationship in alpha decay.[55] For the energy of the beta particle Kudar first used the lowest value in the beta spectrum, but later realized that the most probable value gave the best results. By using his expression for the beta energy, Kudar calculated r_0 values for RaE and UX_1 for different n values, and for $n = 4$ obtained r_0 values of correct order of magnitude, but somewhat smaller than the classical electron radius. This result, together with his assumption that the classical relationship

$$m_0\rho = \text{constant}$$

between the rest mass m_0 and the radius ρ of the electon was still valid, led him to advance the following hypothesis to explain the continuous beta spectrum:

> The electron thus has a natural radius corresponding to the observed rest mass of the electron. However, at a sufficiently large azimuthal quantum number, the nuclear electron involved in radioactivity will be compressed by the narrow potential box into a smaller volume. According to the relation mentioned, this compression of the volume is associated with an increase in the rest mass. We now put forward the hypothesis that between its normal value and the larger value corresponding to the compression, the rest mass of the β particles in the nucleus has a continuous spectrum, and this rest mass spectrum corresponds to the continuous energy range of the β rays. If we let ΔE denote the difference between the upper and lower limit in the spectrum of the β-energy range, and if m is the natural rest mass, ρ the natural radius of the electron, and further r_0 the radius of the potential box, then the classical relationship allows us to assume the relationship:
>
> $$m\rho = \left(m + \frac{\Delta E}{c^2}\right) \cdot r_0.$$ [56]

Furthermore, Kudar argued that Klein's paradox did not apply here since the potential was not electrostatic.

[55] The Geiger–Nuttall relation for alpha rays, known phenomenologically since 1912, established a connection between the life-time of an alpha-emitting source and the range (or energy) of the emitted alpha particles.

[56] Kudar, "Wellenmechanische ... II" (note 54), p. 174.

When Bohr received proofs of Kudar's work, his response was critical. He pointed out that Kudar's reasoning was based on the usual quantum-mechanical interpretation of the electron being point-like, but also inconsistently invoked the classical notion of an extended electron. In particular:

> I do not see how you can reconcile the continuous spectra with the well-defined lifetime of the atoms. If the electrons, as would be the case according to your opinion, were simply compressed in the interior of the nucleus, one cannot in my view understand why a continuous spectrum is obtained, and, in particular, that one can calculate the lower limit of this spectrum by means of the usual rest mass.[57]

After receiving Bohr's letter, Kudar tried to explain the well-defined decay constant by asserting that the beta particles in the nucleus have a sharply defined energy. He then could not uphold energy conservation, but by assuming that the energy of the nuclear electrons corresponded to the average energy of the primary beta spectrum, energy was still conserved statistically.

The Cavendish experimentalists cannot have been enthusiastic about these Continental proposals. They seemed to prefer a less revolutionary solution to the beta-decay problem. In Section 5.4, I discussed Ellis and William Wooster's explanation of their calorimeter experiment. Since, in the late 1920s, few considered it likely that electrons existed freely in the nucleus, their view had to be modified somewhat. The preferred opinion, as expressed by Rutherford, Chadwick and Ellis in their book of 1930, was that "the continuous spectra show the actual energy of expulsion of the individual electrons."[58] Accordingly, they did not believe in energy non-conservation, and adopted instead the hypothesis that the principle of identity – that is, that all nuclei of a certain element are identical – was not universally valid. They suggested no reasons for a violation of this principle, but merely stated that a beta decay occurred when "an α' particle [i.e., an alpha particle with two electrons in such close combination that the whole is of nuclear dimensions] breaks up by first losing one of its electrons. ... The singly charged particle left behind is again unstable and after a certain interval will set free the remaining electron, leaving a normal α-particle in the nucleus."[59] "On this view," they continued,

> ... the energy of the emitted electrons would be the difference in the energies of the stationary states occupied by the α' particle before and after the departure of one of its electrons less the energy required to set free the electron from its state of close binding. It is this last quantity which might be considered to vary and to account for the spread of the continuous spectrum, and it would only show itself in the nucleus by a

[57] Bohr to Kudar, 28 January 1930, *BSC* (22.4).
[58] E. Rutherford , J. Chadwick, and C.D. Ellis, *Radiations from Radioactive Substances* (Cambridge University Press, 1930), p. 336.
[59] *Ibid.*, p. 409.

> slightly varying mass of the α' particles. Except for this feature all the nuclei will be identical and in the same definite stationary states.[60]

Rutherford, Chadwick and Ellis considered their view to be supported by their experiments, which I discussed in the previous section, on the artificial disintegration of aluminum. Although the violation of the principle of identity had not yet manifested itself in other respects, they continued to believe in such a violation.

A variation in the lifetime of a certain radioactive substance would have been a likely manifestation of a violation of the principle of identity, and attempts were made to detect such a variation. Already in 1924, on Rutherford's suggestion, L. Bastings investigated whether a difference between the decay constant of the RaE nuclei emitting the faster beta rays and the ordinary decay constant of RaE could be established. He found no systematic difference in the decay constants of the two RaE β emitters.[61] Yet, further experiments were carried out. In October 1929, Fowler wrote to Bohr:

> Rutherford says that he has tried to detect a difference between the rates of decay of RaE for swift & slow or average β-particles without result.[62]

Three years later, in a letter to Bohr of 2 September 1932, Ellis mentioned another experiment:

> About a year ago a student of mine carried out a rather better experiment to detect whether there was any difference in the type of emission of old or young radioactive atoms. He used the body thorium C$''$, which has a period of 3.1 minutes, and investigated the velocity distribution of the electrons from atoms which had been in existence for various times from 20 seconds to 600 seconds, and he could not find any difference in the general characteristics of the emission. It was impossible to do it very accurately, since he investigated the distribution by means of the transmission through paper, but it sufficed to show that it is unlikely that there is any effect, and the whole experiment is, I think, an improvement on Bastings.[63]

All of the Cavendish experiments appeared to favor the result that no variation in the lifetime of a certain radioactive substance existed, and they thus yielded no support for the view that the principle of identity was violated.

In 1929, the Russian physicist Georgii Pokrowski reported a deviation from the probability law for alpha particles emitted from a very small concentration of a radioactive source; he asserted that, in addition, the scintillations appeared in

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[60] *Ibid.*, p. 410.
[61] L. Bastings, "The Decay of Radium E," *Phil. Mag. 48* (1924), 1075–1080, p. 1078.
[62] Fowler to Bohr, 6 October 1929, *BSC* (10.3).
[63] Ellis to Bohr, 2 September 1932, *BSC* (19.1).

groups.[64] It thus looked as if alpha-radioactive processes in different atoms were not independent of each other. Dirac noted this result and saw a possibility of using it to explain the beta-energy continuity. On 9 December 1929, he wrote to Bohr:

> I think it is on some such basis as this, even if Pokrowski is not right, that one must look for an explanation of the continuous β-ray spectrum. Perhaps theoretically it depends on some hitherto overlooked resonance or "austausch" interaction between the electrons in different nuclei.[65]

Apparently, Bohr did not respond to Dirac's idea, and I have found no indication that it was discussed by anyone else.

Finally, I should mention that in 1929, Leonard Loeb in Berkeley suggested that when two beta decays succeeded each other, i.e., an $\alpha\beta\beta\alpha$-type decay, the *sum* of the energy released in the two beta transformations might be constant.[66] He had no experimental examples, however, and in a review article of 1930 Fritz Houtermans expressed skepticism about Loeb's idea.[67] As an argument against it, he proposed that the width of the RaE spectrum was several times larger than the maximum energy of the RaD spectrum.

Around 1930, several attempts to explain the continuous beta spectrum were thus put forward, their diversity reflecting the desperation of those years. Others followed Peter Debye's advice and remained silent.[68] A third way of reacting was recommended by Ellis. "I am convinced," he told Bohr in his letter of 2 September 1932, "that what is needed in the β-problem more than anything else is more experiments, and we must keep on investigating in the hope of coming across something that will give an indication of the direction in which the solution is to be sought."[69] Ellis himself encouraged such experimental work in Cambridge, as we shall see in the next section.

[64] G.I. Pokrowski, "Über das Wahrscheinlichkeitsgesetz bei dem Zerfall radioaktiver Stoffe sehr kleiner Konzentration," *Z. Phys. 58* (1929), 706–709.

[65] Dirac to Bohr, 9 December 1929, *BSC* (9.4).

[66] L.B. Loeb, "Note Concerning the Emission of Beta-Rays in Radioactive Change," *Phys. Rev. 34* (1929), 1212–1216.

[67] F.G. Houtermans, "Neuere Arbeiten über Quantentheorie des Atomkerns," *Ergebnisse der Exakten Naturwissenschaften 9* (1930), 123–221, p. 181.

[68] See Pauli's letter to the "radioactive ladies and gentlemen," 4 December 1930, Pauli, *Briefwechsel*, Vol. 2 (note 37), pp. 39–40.

[69] Ellis to Bohr, 2 September 1932, *BSC* (19.1).

6.4 The question of upper limits in beta spectra, and the thorium C branching problem

Earlier investigations on beta rays carried out with the usual semicircular focusing method indicated the existence of upper energy limits in beta spectra. Edwin Madgwick and Gurney had observed fairly definite end points in the spectra of RaE and RaB + C,[70] but their method was not considered reliable for this purpose, and furthermore could not be applied to short-lived products such as ThC″.

To obtain more extensive and accurate results another method had to be considered. In 1929, J. Alan Chalmers pointed to absorption measurements as a possibility.[71] Already in 1912, Joseph A. Gray had noticed that after penetrating some distance through matter, beta rays were no longer absorbed exponentially, and in addition he showed that there was a definite end point, a so-called kink, in the beta-ray absorption spectrum. Comparison with measurements of the ranges of homogeneous beta rays of known energy hence made it possible to derive the beta-ray velocity corresponding to such a kink.

Chalmers used the absorption method on ThB, ThC and ThC″, and in all three cases observed definite kinks and thus sharp upper energy limits in their beta spectra. This result, Chalmers argued, "appears to agree with the view that the electrons are emitted with maximum energy and that the dispersion is a secondary effect."[72] Although this was the most obvious conclusion, it nevertheless was immediately rejected. In Cambridge, confidence in the Ellis–Wooster experiment was so strong that this result was not sufficient to challenge Ellis's view on beta decay. In better concordance with the general attitude of the Cavendish researchers was Chalmers's opinion that his own result favored energy conservation. If energy was not conserved, a gradual tailing-off was to be expected.

This applied only to statistical energy conservation. As we have seen, Bohr went further and suggested that energy was not even statistically conserved, and nothing prevented this hypothesis from still being upheld.

Between 1929 and 1932, several experiments on the existence of upper energy limits were carried out. By using a method similar to that of Chalmers, Bernice Sargent and also Norman Feather demonstrated definite end points in the beta spectra of AcB, AcC″ and MsTh2.[73] It thus seemed experimentally demonstrated

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[70] E. Madgwick, "The β-Ray Spectrum of RaE," *Proc. Camb. Phil. Soc. 23* (1927), 982–984; R.W. Gurney, "The Number of Particles in the Beta-Ray Spectra of Radium B and Radium C," *Proc. Roy. Soc. A109* (1925), 540–561.

[71] J.A. Chalmers, "An Approximate Method of Determining the High-Velocity Limits of Continuous β-Ray Spectra," *Proc. Camb. Phil. Soc. 25* (1929), 331–339.

[72] *Ibid.*, pp. 337–338.

[73] B.W. Sargent, "The Upper Limits of Energy in the β-ray Spectra of Actinium B and Actinium C″," *Proc. Camb. Phil. Soc. 25* (1929), 514–521; N. Feather; "Concerning the Absorption Method of Investigating β-Particles of High Energy: The Maximum Energy of the Primary β-Particles of Mesothorium 2," *Phys. Rev. 35* (1930), 1559–1567; *idem.*, "Concerning the Success of the Absorption Method of Investigating the High Velocity Limits of Continuous β Ray Spectra," *Proc. Camb. Phil. Soc. 27* (1931), 430–444.

that a certain maximum energy could be ascribed to every beta-decaying source; but in early 1931, Fernand Terroux called this conclusion into question.[74]

Earlier, by using a Wilson expansion cloud chamber, Terroux had studied the passage of fast RaE beta rays through gases and observed a number of particles with velocities considerably above the limit generally assumed at that time ($\sim 5,000\ H\rho$). This remarkable result indicated the presence of a long tail tending asymptotically to zero and encouraged Terroux to begin a closer investigation of the question, still using an expansion-chamber method. The basic advantage of this method was that its sensitivity made it possible to detect even a single beta particle, regardless of its velocity.

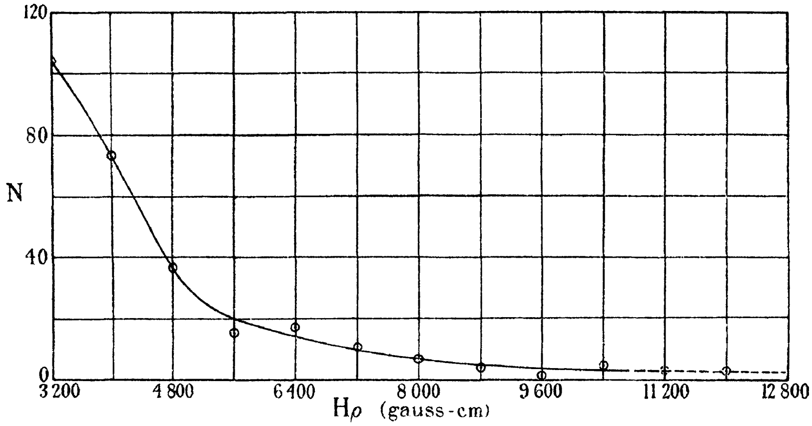

Figure 6.4: Terroux's velocity distribution of the fast beta rays from RaE. Source: Terroux, "Upper Limit" (note 74), p. 94.

Terroux placed his expansion chamber in a uniform magnetic field. He then photographed the beta-particle tracks and calculated the velocities of the particles from their curvatures. Terroux confined his attention to the region between 3,500 $H\rho$ and 10,000 $H\rho$ and found that about 15 percent of the observed tracks exceeded the limit of 5,000 $H\rho$. From this result, he estimated that the particles corresponding to these tracks amounted to about 4 percent of the total number of emitted particles. He found evidence of an end point within the investigated region, and his distribution curve suggested that the beta particles were emitted according to a Maxwellian law (see Figures 6.4 and 6.5).

Terroux's anomalous result received considerable attention and prompted further investigations both in Cambridge and in Berlin. In a letter of 19 December 1931, Meitner informed Patrick Blackett about the outcome of the Berlin reexamination:

[74]F.R. Terroux, "The Upper Limit of Energy in the Spectrum of Radium E," *Proc. Roy. Soc. A131* (1931), 90–99.

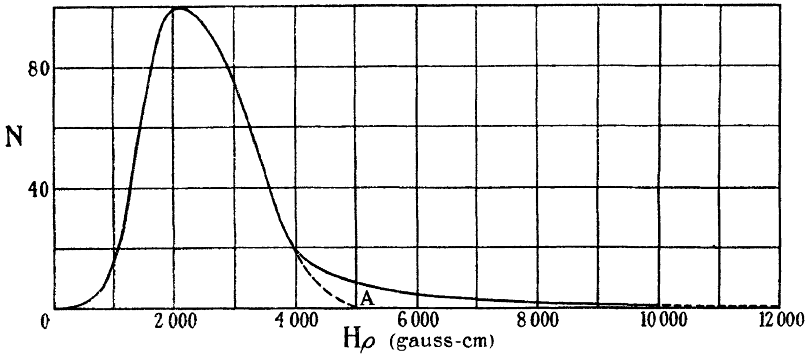

Figure 6.5: Terroux's complete beta-ray spectrum of RaE, obtained by adjusting the data of Figure 6.3 to the curve given by Ellis and Wooster at $H\rho = 4{,}000$. Source: Terroux, "Upper Limit" (note 74), p. 96.

> You probably recall that, when you reported in Rome about Mr. [F. Clive] Champion's results regarding the β rays from RaE, I told you I too had let Terroux's work be checked by a physicist, Mr. [Kan Chang] Wang, who works with me. We made absorption experiments with precisely focused rays and counted the number of β rays as a function of the absorber thickness by means of a Geiger–Müller counter. The Geiger–Müller counter registers every incoming electron practically independently of its velocity, and we have thus so to speak a plot for counting the maximum range directly. We find that beyond $H\rho = 5{,}000$, only a very weak γ radiation is present, and even if we would want to ascribe it to a β radiation, it makes up less than 1/2 part per thousand of the initial intensity. Now I would like to know from you whether the work by Mr. Champion is published, because Mr. Wang would of course refer to it in the brief communication about his results that he intends to publish. Unfortunately, I am not sure whether also Mr. Champion employs Mr. Terroux's Wilson method or whether you have applied a different method. In case Mr. Champion's work has not yet appeared, I assume that we could refer to the lecture you gave in Rome?[75]

Blackett answered Meitner in a letter of 8 January 1932:

> Champion's paper appeared in the January Proceedings of the Royal Society. As you see, it was done with the Wilson chamber, just like Terroux's work. I am glad to hear that Herr Wang has obtained the

[75] Meitner to Blackett, 19 December 1931, MTNR 5/1.

> same result by another method. It is a great pity that Terroux's mistake has led to quite a lot of more or less unnecessary work to disprove it.[76]

Thus, Champion had used a method similar to Terroux's and had obtained a very different result. In Champion's experiment, the RaE spectrum was found to end rather sharply at about 5,500 $H\rho$, in good agreement with Wang's results in Berlin.[77] What had gone wrong in Terroux's work? Champion advanced a suggestion. He had also observed several tracks that apparently had $H\rho$ values greater than 5,500, "but on closer examination," he pointed out, "these were all found to suffer from one or more deflections due to passage near an atomic nucleus and simply to be tracks near the upper limit, which, as a result of these nuclear deflections, exhibited a spurious straightness."[78] Terroux had become aware of this problem himself. In a paper written with Norman Alexander, he admitted that, due to nuclear and multiple electron scattering, he probably overestimated the number of fast particles. Incidentally, their paper dealt with the upper part of the beta spectrum of ThC$''$, and they found no trace of a tail.[79]

Experiments thus strongly favored the existence of upper energy limits, and in a letter of 2 September 1932, Ellis informed Bohr:

> There now seems no doubt that there is a definite end-point to the β-ray spectrum, though of course this can only be said in the experimental sense, and there is always the possibility of a very small tail going on to infinity. However, reviewing the whole evidence, I think it is wisest to assume that there is a definite end-point, and I feel that this is really a very curious point, and one that is difficult to understand. For that very reason it is of great importance.[80]

In 1932, Sargent made a critical survey of all of the existing measurements and carried out a few new ones. While so doing, he became aware of a very important regularity.[81] When plotting the logarithm of the disintegration constant against the logarithm of the energy of the end point, he found that the points lay on one of two slightly curved lines (see Figure 6.6). This strongly indicated that a relation analogous to the Geiger–Nuttall rule of alpha decay existed, and consequently that the end point was a very important characteristic of the spectrum.

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[76] Blackett to Meitner, 8 January 1932, MTNR 5/1.

[77] F.C. Champion, "The Distribution of Energy in the β-Ray Spectrum of Radium E," *Proc. Roy. Soc. A134* (1931), 672–681; K.C. Wang, "Über die obere Grenze des kontinuierlichen β-Strahlspektrums von RaE," *Z. Phys. 74* (1932), 744–747.

[78] Champion, "Distribution" (note 77), p. 679.

[79] F.R. Terroux and N.S. Alexander, "The Upper Limit of Energy in the β-Ray Spectrum," *Proc. Camb. Phil. Soc. 28* (1932), 115–120.

[80] Ellis to Bohr, 2 September 1932, *BSC* (19.1).

[81] B.W. Sargent, "Energy Distribution Curves of the Disintegration Electrons," *Proc. Camb. Phil. Soc. 28* (1932), 538–553; *idem.*, "The Maximum Energy of the β-Rays from Uranium X and Other Bodies," *Proc. Roy. Soc. A139* (1933), 659–673.

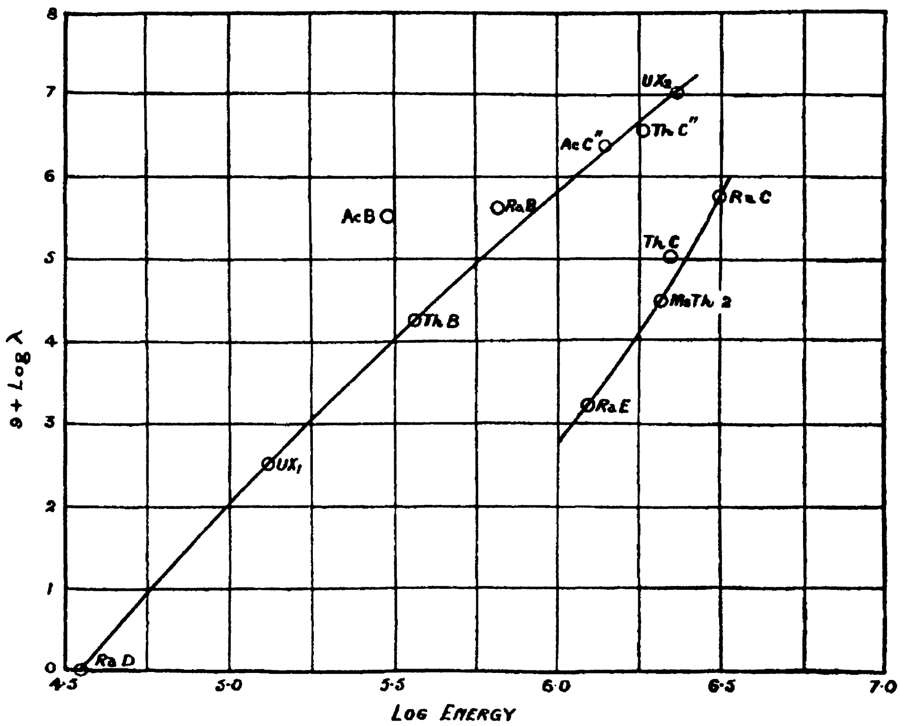

Figure 6.6: The two Sargent curves. By plotting the logarithm of the disintegration constant against the logarithm of the energy of the end point for different radioactive sources, Sargent found that the points lay on two slightly curved lines. Source: Sargent, "Maximum Energy" (note 81), p. 671.

Although the importance of the upper energy limits was established by the work of Sargent, it was not at all obvious how they should be interpreted.[82] The key to the solution of this riddle was to be found in the ThC branching problem, which Rutherford and Chadwick in 1929 described as follows for the transformation of ThC to Thorium Lead (ThD):

This takes place in two ways, according to the following scheme:

$$\begin{array}{ccl} & & \mathrm{ThC}' \xrightarrow{\alpha} \text{Thorium Lead} \\ & \overset{\beta}{\nearrow} & \\ \mathrm{ThC} & & \\ & \overset{\alpha}{\searrow} & \\ & & \mathrm{ThC}'' \xrightarrow{\beta,\gamma} \text{Thorium Lead} \end{array}$$

[82] "At present the significance ... is not apparent," Sargent wrote in *ibid.*, p. 672.

> The energy of the α particle emitted by thorium C′ is 8.83×10^6 electron volts while the energy of the [α] particle from thorium C is 5.96×10^6 electron volts. As far as the available evidence goes, it does not seem possible that such a large difference of energy, 2.87×10^6 electron volts, can exist between the β-transformation of thorium C and the β,γ transformation of thorium C″ in the case of each disintegrating nucleus. Thus either the nuclei of thorium C or those of thorium lead must differ in masses or internal energy.[83]

In the succeeding years, this problem was discussed now and then. Gradually, the Cavendish physicists realized that the energy of any nucleus was quantized, and consequently that Rutherford and Chadwick's explanation of the ThC branching problem could not be upheld. The only way out seemed to be non-conservation of energy.

In early 1933, Ellis and Nevill Mott took up the problem.[84] Considering two elements P and Q in definite energy states E_{P} and E_{Q}, such that the transformation from P to Q involved a beta disintegration, they advanced the hypothesis "that the energy difference $E_{\mathrm{P}} - E_{\mathrm{Q}}$ is *equal to the upper limit of the β-ray spectrum, i.e.*, to the maximum energy with which a β-particle can be expelled."[85] Accordingly, a beta particle might be expelled with less energy than the difference $E_{\mathrm{P}} - E_{\mathrm{Q}}$, but not with more energy. Ellis and Mott did not discuss what happened to the excess energy. They only pointed out that their assumption was consistent with Pauli's neutrino hypothesis, and that energy, in case it merely disappeared, was not even conserved statistically.

Armed with their new hypothesis, Ellis and Mott applied it to the ThC branching scheme, shown in Figure 6.7. ThC could transform into ThD by two branches, and if their hypothesis was correct, the maximum energies emitted along the two branches should be equal to each other. At first sight this seemed not to be the case. The maximum energy given out along the ThC–ThC′–ThD branch was $(2.2+8.95)\times 10^6$ eV $= 11.15\times 10^6$ eV, and along the ThC–ThC″–ThD branch it was apparently $(6.20+1.8)\times 10^6$ eV $= 8.00\times 10^6$ eV. Ellis and Mott were able to account for this difference, however. Previously, they had observed that two of the gamma rays succeeding the beta-decay process ThC″ to ThD, with energies 2.62×10^6 eV and 0.58×10^6 eV, both appeared with an intensity corresponding to that of the primary beta rays.[86] This indicated that ThC″ decayed into an excited state of ThD of energy $(2.62+0.58)\times 10^6$ eV $= 3.20\times 10^6$ eV. Instead of being equal to 1.8×10^6 eV, the difference between the ground-state energies of ThC″ and ThD was thus $(1.8+3.2)\times 10^6$ eV $= 5.0\times 10^6$ eV, which established satisfactory agreement.

[83] Rutherford and Chadwick, "Energy Relations" (note 8), p. 192.

[84] C.D. Ellis and N.F. Mott, "Energy Relations in the β-Ray Type of Radioactive Disintegration," *Proc. Roy. Soc. A141* (1933), 502–511.

[85] *Ibid.*, p. 502.

[86] C.D. Ellis and N.F. Mott, "The Internal Conversion of the γ-Rays and Nuclear Level Systems of the Thorium B and C Bodies," *Proc. Roy. Soc. A139* (1933), 369–379.

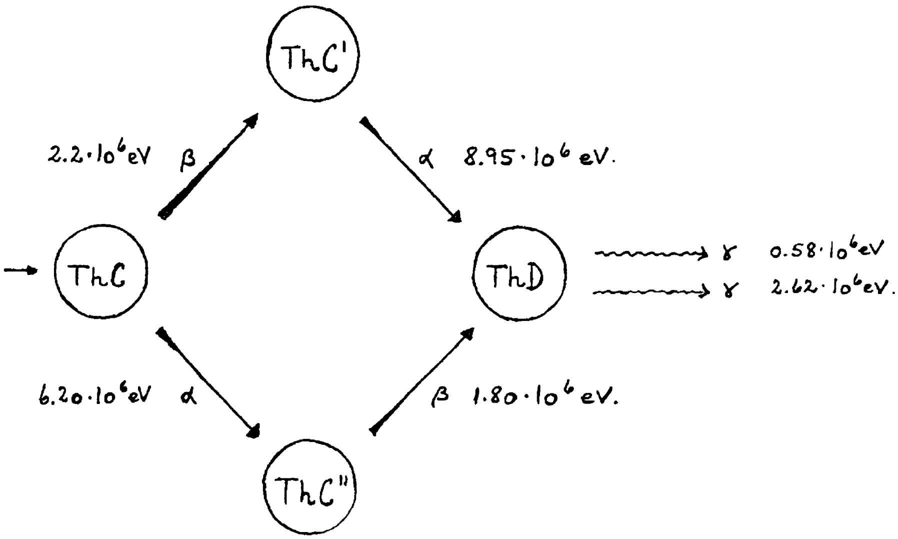

Figure 6.7: The scheme for the branching in the thorium family. The energies of the two alpha disintegrations as well as the upper limits of the two beta-ray spectra are shown. At first sight, the energies emitted along the two branches did not seem to equal each other, but by taking into account the two gamma rays succeeding the ThC″ to ThD beta decay, Ellis and Mott were able to establish the balance. Drawing by the author.

Owing to rather uncertain experimental data, Ellis and Mott recommended caution, but their hypothesis carried conviction and was soon confirmed by improved measurements by William Henderson.[87] A great step forward in understanding beta decay had been taken. The above considerations not only showed that energy conservation was valid for the maximum energies in beta-decay processes, they also revealed that in certain cases the normal transitions are suppressed in favor of transitions from excited states, which suggested the existence of selection rules.

Concerning the neutrino hypothesis, I do not think that Ellis and Mott's work as such brought about any essential change in attitude. It was, in fact, also consistent with Bohr's non-conservation view, and it proved possible, as we shall see in a subsequent section, to construct a theory satisfying both Ellis and Mott's hypothesis and Bohr's hypothesis of energy non-conservation.

[87]W.J. Henderson, "The Upper Limits of the Continuous β-Ray Spectra of Thorium C and C″," *Proc. Roy. Soc. A147* (1934), 572–582.

6.5 The impact of the miraculous year: The second phase of the Bohr–Pauli dispute, 1932–1933

During 1932, as we noted, new particles were discovered and accelerators were built. Some progress was also made in the understanding of beta spectra. I now turn to the question of the influence of these achievements on the two competing proposals to explain the continuous beta spectrum.

Chadwick announced his discovery of the neutron in February 1932.[88] His new particle was soon accepted and paved the way for much valuable research, but it did not immediately resolve the urgent problems in connection with the nuclear-electron hypothesis.[89] Heisenberg's new theory of nuclear structure,[90] advanced soon after the discovery of the neutron, involved only the heavy particles, the proton and the neutron, and hence evaded, but did not resolve, the problems with the electrons. They were, as Kronig remarked, only removed from the basement to the sub-basement, i.e., from the nucleus to the neutron. Heisenberg's theory was nonetheless a great improvement over the earlier electron–proton model, and Bohr was enthusiastic about it and the way it described beta decay. On 28 June 1932, he wrote to Klein:

> Besides, when I returned home, I found the proofs of a very beautiful and interesting paper by Heisenberg, showing how one can obtain an extensive systematic survey of the emission of α and β rays from radioactive nuclei without going directly into the problem with the electron. As nuclear constituents he takes only protons and neutrons, which are treated according to quantum mechanics, and the β-ray emission is described as a neutron disintegration for which the laws of momentum and energy are not supposed to be valid.[91]

Accordingly, Heisenberg was still sympathetic to the idea of non-conservation of energy and momentum. In general, I have found no statement, not even by Pauli, to the effect that the discovery of the neutron changed the prevailing attitude towards his neutrino hypothesis.

Quite different was the case of Carl Anderson's discovery of the positron later in 1932.[92] Even after the Cavendish physicists Blackett and Giuseppe Occhialini confirmed Anderson's discovery with an improved technique, Bohr, for a rather long time, questioned the existence of this new particle. In April 1933, he responded to Klein's enthusiasm for the positron as follows:

---

[88]Chadwick, "Possible Existence" (note 1).

[89]For a full discussion of these problems, see Stuewer, "Nuclear Electron Hypothesis" (note 5).

[90]W. Heisenberg, "Über den Bau der Atomkerne. I," *Z. Phys. 77* (1932), 1–11; "... II," *ibid. 78* (1932), 156–164; "... III," *ibid. 80* (1932), 587–596.

[91]Bohr to Klein, 28 June 1932, *BSC* (22.1). The translation is my own.

[92]Anderson, "Apparent Existence" (note 1). See also X. Roqué, "The Manufacture of the Positron," *Studies in the History and Philosophy of Modern Physics 28* (1997), 73–129.

> Regarding the positive electrons I cannot, however, quite share your enthusiasm. I am at least as yet very skeptical as regards the interpretation of Blackett's photographs, and am afraid that it will take a long time before we can have any certain knowledge about the existence or non-existence of the positive electrons. Nor as regards the applicability of Dirac's theory to this problem I feel certain, or, more correctly, I doubt it, at least for the moment.[93]

Pauli learned about Bohr's great hesitancy when the two met in Copenhagen in March. As Pauli reported to Rudolf Peierls, Bohr "would ... have nothing to do with the positive electrons and thought that Blackett even had made 'pathological photographs.'"[94] Gradually, however, Bohr yielded to the growing experimental evidence. A number of experiments showed that positrons were not only produced in cosmic rays, but also by bombarding lead with hard gamma rays, and at the Copenhagen conference in September 1933, the positive electrons were the main topic of discussion.[95]

In contrast to Bohr, Pauli was enthusiastic about the discovery of the positron. "The discovery of the positive electron has very much strengthened my earlier belief in the existence of a 'neutrino' and its emission by β decay," he wrote to Peierls.[96] This same message was contained in a letter to Blackett of 19 April 1933, here reproduced *in extenso*:

> Dear Blackett!
>
> Your and Occhialinis paper about the positive electron, is very interesting and the existence of the positive electron is very supported now by the paper of Meitner and [Kurt] Philipp in Naturwissenschaften.[97] In this moment I come back to my old idea of the existence of a "neutrino" {that means a neutral particle with a mass comparable with that of the electron; the italian name (in contrast to neutron) is made by Fermi}. If the positive and the negative electron both exist, it is not so phantastic to assume a neutral particle, consisting of both together.
>
> Further the paper of Sargent with the sharp upper limits of the energies in the β-spectra suggested to me again my old idea, that at every β-disintegration even a neutrino could be emitted and could save the conservation-law of energy (and momentum).

---

[93] Bohr to Klein, 7 April 1933, *BSC* (22.1). The translation has been taken from F. Aaserud, *Redirecting Science: Niels Bohr, Philanthropy, and the Rise of Nuclear Physics* (Cambridge University Press, 1990), p. 58.

[94] Pauli to Peierls, 22 May 1933, in Pauli, *Briefwechsel*, Vol. 2 (note 37), p. 164.

[95] This appears from a letter of Bohr to Ellis, 30 August 1933, *BSC* (19.1).

[96] Pauli to Peierls, 22 May 1933 (note 94).

[97] L. Meitner and K. Philipp, "Über die Wechselwirkung zwischen Neutronen und Atomkernen," *Naturwiss. 20* (1932), 929–932; *idem.*, "Die bei Neutronenanregung auftretenden Elektronenbahnen," *Naturwiss. 21* (1933), 286–287.

> What think the experimental physicists of the Cavendish laboratory *now* about those possibilities? Besides, I dont believe on the Dirac-"holes," even if the positive electron exists.
>
> It is very probable, that we can made in this year a "physical week" at Zurich in June. I hope, that I can write you more about that in a few days and [Paul] Scherrer and myself would be very glad, if you could come with new tracks.
>
> With the best wishes for you and your wife.
>
> Sincerely Your W. Pauli
>
> P.S. Perhaps your non-ionizing links are "neutrinos".[98]

It may seem strange that Pauli considered this particular particle to be of such great importance to his neutrino hypothesis. Certainly, the idea of a positron–electron pair was in accordance with Pauli's view of the neutrino as a magnetic dipole, and with his estimation of its mass as well. But was it not in contradiction to conservation of spin, and thus a problem for Pauli's persistent belief in the conservation laws? Only, Pauli argued, if the positron was a Fermi particle, and that was not necessarily the case. In his letter to Peierls quoted above he thus wrote:

> Furthermore, it is probable that the positive electron carries integral (perhaps zero) spin and obeys Bose statistics. To be sure, this could be deduced from the conservation laws (and from the empirical fact that the mass number of the nucleus uniquely determines its statistics and the even or odd value of its spin) only if one knew an elementary process that would create an odd number of them. It would be very desirable if the experimentalists could establish precisely by which elementary processes the positive electron is created. The idea that the positive electron might obey Bose statistics was first proposed by [Walter] Elsasser (although for different reasons), and I have advised him to send a note on the matter to Nature. (That this point of view is contrary to Dirac's hole theory only speaks *for* it. Experience tells us that positive and negative electricity do not behave in exactly the same way, and I find it very unattractive to relegate this asymmetry to the initial state of the world, as Dirac does.)[99]

Elsasser's argument was based upon a hypothesis advanced by the discoverer of the positron, Anderson, that a proton consist of a neutron and a positron.[100]

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[98] Pauli, *Briefwechsel*, Vol. 2 (note 37), pp. 158–159. The letter is reproduced in Pauli's original English.

[99] Pauli to Peierls, 22 May 1933 (note 94).

[100] W.M. Elsasser, "A Possible Property of the Positive Electron," *Nature 131* (1933), 764. See also H. Stücklen, "Kältephysik und Physik des Atomkerns," *Naturwiss. 21* (1933), 772–776, p. 776.

Since, Elsasser argued, according to Heisenberg's theory the proton and the neutron were both Fermi particles, the positron had to be a Bose particle, i.e., possess integral spin angular momentum (0 or 1). Recent experiments by Otto Stern seemed to support this view. Stern had observed that the magnetic moment of the proton was three times larger than it should be if the proton was considered to be an elementary particle in Dirac's theory.[101] Furthermore, if the positrons had symmetrical wave functions, and thus always occupied the deepest energy levels, it was easier to understand why they could be found only inside the nucleus.

Pauli was soon forced to abandon Elsasser's Bose-particle view of the positron, however. The discovery of pair creation and annihilation indicated that the positron, like the electron, was a Fermi particle. But Pauli then advanced another argument in favor of his neutrino hypothesis, again based on the conservation laws. "It seems to me that the conservation laws for all discretely quantized quantities are even more important than the conservation laws for energy and momentum in nuclear processes," he wrote to Heisenberg on 14 July 1933.[102] This, together with the assumption that the neutron was a spin-1/2 particle, implied that "a neutron cannot be transformed into an electron and a proton by an external field (or in some other way)." In further support of his view, Pauli noted "the empirical fact that *the H atom is stable* and does not decay spontaneously (under emission of light) into a neutron." Heisenberg largely agreed, but in response to Pauli still expressed reservation with regard to the absolute validity of the conservation laws.[103] Nor at the seventh Solvay conference in October did Heisenberg come to a clear decision as to the existence of neutrinos,[104] but he was close to reaching one. Shortly after the Solvay conference he asked Meitner for her view on some apparent experimental difficulties for the Ellis–Mott theory. She did not consider them to be real,[105] which Heisenberg was relieved to hear:

> It is very comforting to me that the Mott–Ellis claim faces no immediate experimental difficulties, for at the moment I am very much in favor of Pauli's hypothesis of the neutrino and have also spoken about it at length with Pauli in Zurich. Conversely, it would of course be wonderful if one could obtain more experimental evidence in favor of the hypothesis of Mott and Ellis.[106]

Pauli thus finally won Heisenberg over to his neutrino hypothesis; but what about Bohr? Actually, during 1932 and 1933 Bohr was mainly struggling with the

[101] O.R. Frisch and O. Stern, "Über die magnetische Ablenkung von Wasserstoffmolekülen und das magnetische Moment des Protons. I," *Z. Phys. 85* (1933), 4–16; O. Stern, "Über das magnetische Moment des Protons," *Helv. Phys. Acta 6* (1933), 426–427.

[102] Pauli, *Briefwechsel*, Vol. 2 (note 37), p. 184.

[103] Heisenberg to Pauli, 17 July 1933; in *ibid.*, p. 195.

[104] W. Heisenberg, "Considérations théoriques générales sur la structure du noyau," in *Structure et Propriétés des Noyaux Atomiques. Rapports et Discussions du Septième Conseil de Physique tenu a Bruxelles du 22 au 29 Octobre 1933* (Paris: Gauthier-Villars, 1934), pp. 289–344.

[105] Meitner to Heisenberg, 18 November 1933, MTNR 5/8.

[106] Heisenberg to Meitner, 28 November 1933, MTNR 5/8.

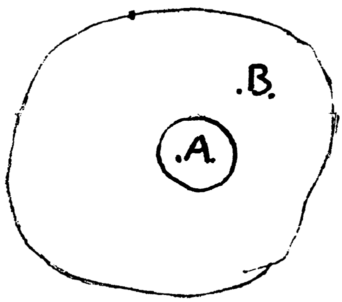
Figure 6.8: Seventh Solvay Conference in Physics, 1933. Chadwick, Meitner, and Rutherford are sitting first, second, and sixth from the right, respectively. Bohr is sitting third from the left. Pauli (in waistcoat and grey suit) is standing directly behind Rutherford. Ellis is standing fifth from the right. Mott and Gamow are the two tall men with glasses at the back immediately to the right of what appears to be the blackboard. [Institut international de physique Solvay]

question of the measurability of electromagnetic field quantities and spent only a minor part of his time on nuclear problems.[107] At any rate, one consequence of his idea of non-conservation of energy strongly affected his views. Pauli had pointed to this consequence already two years earlier, and now Landau again brought it to the fore. As Gamow explained in his greetings to Bohr for the New Year 1933:

> In the beginning of December, I was at the Institute in Kharkov to look at the fast protons which they got there. Ehrenfest, Landau, and some

[107] This work resulted in a substantial paper, co-authored with Léon Rosenfeld; see N. Bohr and L. Rosenfeld, "Zur Frage der Messbarkeit der elektromagnetischen Feldgrössen," *Det Kongelige Danske Videnskabernes Selskab. Mathematisk–fysiske Meddelelser 12* (8, 1933), 65pp. Reproduced, in original German and English translation, in *BCW*, Vol. 7, *Foundations of Quantum Physics II (1933–1958)*, Jørgen Kalckar (ed.) (Amsterdam: Elsevier, 1996), pp. 59–121, 123–166.

> other theoreticians were also there, so we organized a small conference. We discussed many problems and cleared up one matter which I believe will be especially interesting to you. It looks as if non-conservation of energy is in contradiction to the equations of gravitation for the vacuum. If the gravitational equations are correct for region B, this implies that the total mass in region A (where we do not know the laws) must be constant. If in A we have, for example, an RaE nucleus, and alter its total mass with a jump in a transmutation process, we can no longer apply the usual gravitational equations in region B.



> In what way we have to change the equations is not clear, but it must be done. What do you think about this?[108]

Bohr's reply indicates that he agreed with this theoretical consequence:

> I am very interested in what you wrote about the discussions in Kharkov, and I fully agree that a renunciation of energy conservation will bring with it equally sweeping consequences for Einstein's theory of gravitation as a possible renunciation of conservation of charge would have for Maxwell's theory, where charge conservation is after all an immediate consequence of the field equations.[109]

At the Solvay conference in October, Bohr returned to this question, stressing that "if I have advocated that one seriously consider the idea of a possible failure of the theorems of conservation of energy and momentum in connection with the continuous β-ray spectra, my intention was above all to emphasize the total inadequacy of the classical conceptual edifice for treating this problem, which could still hold great surprises for us." He continued:

> I fully appreciate the weight of the argument that such a possibility would be difficult to reconcile with the theory of relativity, and would

[108]Gamow to Bohr, 31 December 1932, *BSC* (19.4). The translation is taken from *BCW*, Vol. 9 (note 13), p. 569; the drawing is from the original letter.

[109]Bohr to Gamow, 21 January 1933, *BSC* (19.4). The translation is taken from *BCW*, Vol. 9 (note 13), p. 571.

> in particular stand in a rather unlikely contrast to the absolute validity of the theorem, analogous according to general field theory, of conservation of electric charge, which extends also to the region of nuclear phenomena. In this connection one may however remark that this comparison itself indicates how difficult it would be to prove a direct deviation from the theory of relativity, even if the total mass and energy associated with the particles and the radiation fields were not conserved in nuclear processes. Just as the conservation of charge inside a region whose boundary is not crossed by charges is, at least macroscopically, a necessary consequence of the validity of the electromagnetic field equations outside of this boundary, so, as Landau has pointed out, it is a necessary consequence of the theory of gravitation that any variation of the energy inside a certain region must be accompanied by variations in the gravitational forces outside this region, which would correspond exactly to a mass transport across its boundary. However, the question is whether we must necessarily require that all such gravitational effects are associated with atomic particles in the same way as the electric charges are associated with electrons. Therefore, until we have further experience within this area, it seems to me difficult to judge Pauli's interesting suggestion to resolve the paradoxes of the β-ray emission by assuming that the nuclei emit, together with the electrons, neutral particles, much lighter than the neutrons. In any case, the possible existence of this "neutrino" would represent an entirely new element in atomic theory, and the correspondence method would not offer sufficient help in describing its rôle in nuclear reactions.[110]

Thus, Bohr was still reluctant to accept Pauli's neutrino hypothesis, but he was ready to abandon his idea of non-conservation of energy. He had a third alternative in mind: quantized gravitational waves, as is evident from a letter from Gamow to Goudsmit of 8 March 1934:

> Bohr, on the other hand, well you know that he absolutely does not like this chargeless massless 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.[111]

Bohr explained his idea directly to Pauli one week later:

> It also pleased me that you grasped the basic feeling in my final remarks about energy conservation. However, I have since become more

[110] N. Bohr, "Sur la méthode de correspondance dans la théorie de l'électron," in *Structure* (note 104), pp. 216–228. Quoted from the English translation in *BCW*, Vol. 9 (note 13), p. 132.

[111] Gamow to Goudsmit, 8 March 1934, Goudsmit Scientific Correspondence. Microfilm copies are deposited in the Niels Bohr Library, American Institute of Physics, College Park, Maryland. The letter is quoted from *Exploring the History of Nuclear Physics*, C. Weiner (ed.) (New York: American Institute of Physics, 1972), p. 180.

> skeptical as to the hope, implicitly contained in these remarks, to employ the theory of gravitation to obtain a correspondence deduction of the law for the beta-ray emission. 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 quantization. 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 we here really have a new atomic trait before us, which could be tantamount to the real existence of the neutrino.[112]

Accordingly, Bohr did not believe in the neutrino as a quantized gravitational wave for long. Still, even though seemingly sympathetic to Pauli's neutrino in the above letter, he was far from convinced of its existence. At about the same time as he wrote this letter, he sent off his corrections to the proofs of the Solvay conference discussions, in which he advocated the Beck–Sitte theory of beta decay, which was in accord with Bohr's idea of non-conservation of energy and thus evaded the neutrino. I will discuss this theory, and Fermi's competing theory, in the next section.

6.6 The two theories of beta decay

In a paper dated May 1933, the two young physicists Guido Beck and Kurt Sitte proposed a theory of beta decay which incorporated non-conservation of energy.[113] "I didn't believe in the neutrino," Beck said in an interview many years later, "because I knew there were these experiments of Ellis and Wooster, and Meitner and Orthmann. They got the heat of the electrons but not the heat of the neutrinos in the beta decay. And Bohr thought at one time that there was a violation of energy conservation."[114] This statement may seem strange since Pauli introduced his neutrino for the purpose of *explaining* the calorimeter experiments, but Beck's recollection may well be correct. A nuclear particle with such a high penetrating power was indeed hard to imagine, as Pauli himself admitted.

In the early 1930s, Beck was occupied with the application of Dirac's theory to nuclear phenomena, which to him opened up the possibility of explaining the beta-decay riddle. "When the Dirac theory came out," he recalled in the above-mentioned interview, "my first idea was that I could use it for the beta decay; there is a lost particle. So I made a bet in '33 and started to make an inspection.

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[112] Bohr to Pauli, 15 March 1934, in Pauli, *Briefwechsel*, Vol. 2 (note 37), p. 308. The translation is my own.

[113] G. Beck and K. Sitte, "Zur Theorie des β-Zerfalls," *Z. Phys. 86* (1933), 105–119; G. Beck, "Hat das negative Energiespektrum einen Einfluss auf Kernphänomene?" *Z. Phys. 83* (1933), 498–511; K. Sitte, "Zur Theorie des β-Zerfalls," *Phys. Zeit. 34* (1933), 627–630; G. Beck, "Conservation Laws and β-Emission," *Nature 132* (1933), p. 967.

[114] Interview with Beck, 22 April 1967, Niels Bohr Library, American Institute of Physics, College Park, Maryland, p. 30. I am grateful to Spencer R. Weart for permission to quote from this interview.

And then we wrote curves down with Sitte. That wasn't so bad, but we didn't put the mass of the second particle as neutrino, we put the electron mass."[115]

Beck's idea was as follows: If the energy difference, ΔE, between the nucleus before and after beta decay was higher than $2mc^2$, a creation of a positron–electron pair just outside the nucleus occurred. The positron was then supposed to be absorbed by the nucleus, which thus increased its charge by one unit, and the electron was emitted as the beta particle. The absorption process, according to Beck and Sitte, was beyond the reach of present theories, which led them to support Bohr's plea for a new theory. The creation process, however, could be treated by Dirac's theory, and since the energy, E, was conserved in this theory,

$$E = W + W',$$

where W and W' are the energies, including the rest masses, of the electron and the positron, respectively. In the absorption process, the energy of the positron, as well as its spin, magnetic moment, etc., were lost, so the above equation described a continuous energy spectrum of the electron between the limits mc^2 and $E - mc^2$.

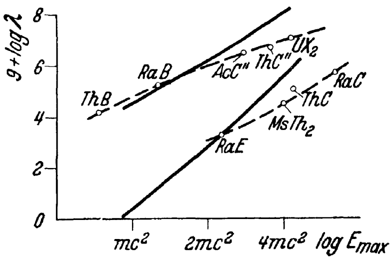

Figure 6.9: The two solid curves reveal Beck and Sitte's theoretically obtained relationship between the decay constants λ of several beta-radioactive sources and the end-point energies, $E_{\max}$, in their spectra. For comparison, the two dashed curves show Sargent's corresponding experimental results. Since the end-point energy depended upon the radius within which the positron–electron creation took place, Beck and Sitte argued that a suitable modification of this radius could easily bring the theoretical and the experimental curves into agreement with each other. Source: Beck and Sitte, "Theorie des β-Zerfalls" (note 113), p. 113.

Dirac's theory, as applied to the creation process, yielded beta-particle energy distributions in good agreement with experiment, and Beck and Sitte furthermore were able to calculate the decay constants of several beta-radioactive sources. In Figure 6.9, these decay constants are shown as a function of the end-point energies of the corresponding beta spectra. That Beck and Sitte obtained two curves, in agreement with Sargent's experimental results, was a great satisfaction to them.

[115] *Ibid.*

They concluded that Dirac's theory was able to explain beta decay in a satisfactory way if it was accepted that a treatment of positron absorption required a quite new theory, in which energy and angular momentum were not conserved.

Bohr learned about Beck and Sitte's theory when he returned home from a trip to the United States in July 1933, and it is not surprising that he was sympathetic to it. On 30 August, he wrote to Ellis:

> After my return to Copenhagen I have had great pleasure in learning about the work of Dr. Guido Beck on the β-ray spectra, of which he has also written to you. I am very much impressed by the progress as regards the explanation of the empirical effect he has achieved by – at any rate by my mind – quite reasonable theoretical assumptions; but the final judgment about the correctness of his views will of course depend on the extent to which consequences of the theory will be borne out by the continuation of the experiments.[116]

At the Solvay conference in October, Bohr again promoted Beck and Sitte's theory; but the general attitude of the participants towards it seems to have been somewhat hesitant, and a few months later a strong rival to it was published.

This rival theory of beta decay was worked out in late 1933 by the Italian physicist Enrico Fermi. Franco Rasetti recalls that Fermi had intended to announce his results in a letter to *Nature* in November, but the manuscript was rejected by its editor on the grounds that it contained abstract speculations too remote from physical reality to be of interest to its readers.[117] Instead, Fermi published a short paper on his theory in *Ricerca Scientifica* in December, and a much longer paper on it in both *Nuovo Cimento* and *Zeitschrift für Physik* in early 1934.[118]

Prior to 1934, Fermi was much occupied with the quantum theory of radiation and the techniques of second quantization, which led him to his brilliant idea of attempting to construct a theory of the emission of an electron–neutrino pair from a nucleus in analogy to the theory of the emission of photons from excited atoms. In addition to the idea of particle creation and annihilation, Fermi thus adopted Pauli's neutrino hypothesis and Heisenberg's proton–neutron model of the nucleus.

A crucial step in creating his theory of beta decay was his choice of the interaction energy between the heavy and light particles. Fermi argued that the simplest possible Hamiltonian was

$$H = g\,\{Q\,\psi(x)\,\varphi(x) + Q^*\,\psi^*(x)\,\varphi^*(x)\},$$

[116] Bohr to Ellis, 30 August 1933, *BSC* (19.1).

[117] See F.R.D. Rasetti's introduction to the published edition of Fermi's original papers on β-decay, in E. Fermi, *Collected Papers*, Vol. 1, E. Segrè et al. (eds.) (Chicago: University of Chicago Press, 1962), pp. 538–540.

[118] E. Fermi, "Tentativo di una theoria dell'emissione dei raggi 'beta'," *Ricerca Scientifica 4* (1933), 491–495; *idem.*, "Tentativo di una theoria dei raggi β," *Nuovo Cimento 11* (1934), 1–19; *idem.*, "Versuch einer Theorie der β-Strahlen. I," *Z. Phys. 88* (1934), 161–177.

where g is a dimensional constant expressing the strength of the beta-decay interaction, and x represents the coordinates of the heavy particles (protons and neutrons). Q represents a transition from a proton to a neutron, and ψ and φ are the field operators of the electron and neutrino, respectively. These light particles had to be treated relativistically, and the components of the two field operators could be combined to give five relativistic covariants: scalar, vector, tensor, pseudoscalar and pseudovector. The most general form involved all combinations of these, but Fermi stuck to his analogy. As in quantum electrodynamics, he restricted himself to the four-vector case.

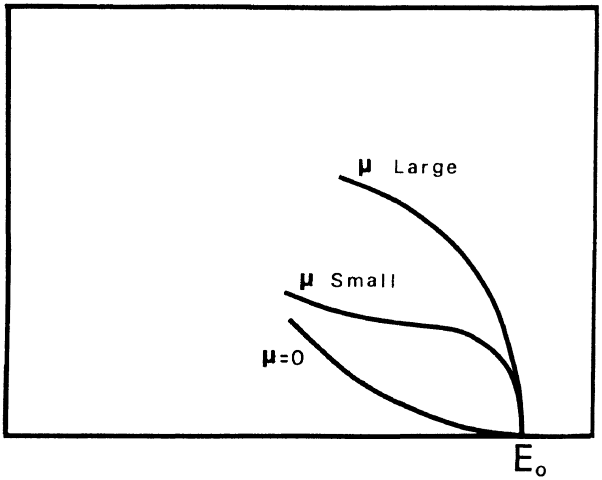

Figure 6.10: The upper part of Fermi's theoretical beta-particle curve for zero neutrino mass, i.e., for $\mu = 0$, and for large and small values of μ. The closest agreement with the empirical curves was obtained for $\mu = 0$. Source: F.L. Wilson, "Fermi's Theory of Beta Decay," *Am. J. Phys. 36* (1968), 1150–1160, p. 1156.

Fermi considered his proposed interaction to be a perturbation to the energy of the heavy and light particles, which meant that the theory could be developed in complete analogy to radiation theory. In this way, Fermi obtained a formula for the energy distribution in "allowed" beta transitions, i.e., transitions for which the matrix element $\langle f|i\rangle$ between the final proton and the initial neutron was considered to be on the order of unity. He found the greatest agreement with the experimental curves for a neutrino mass of zero, or one that was at least very small compared to the mass of the electron (see Figure 6.10).

For the beta-decay lifetime τ, Fermi obtained the formula,

$$\frac{1}{\tau} = 1.75 \times 10^{95} \cdot g^2 \cdot \langle f|i\rangle F(\eta_0),$$

Element	τ (hours)	η_0	F(η_0)	$\tau F(\eta_0)$
UX_2	0.026	5.4	115	3.0
RaB	0.64	2.04	1.34	0.9
ThB	15.3	1.37	0.176	2.7
ThC″	0.076	4.4	44	3.3
AcC″	0.115	3.6	17.6	2.0
RaC	0.47	7.07	398	190
RaE	173	3.23	10.5	1800
ThC	2.4	5.2	95	230
$MsTh_2$	8.8	6.13	73	640

Table 6.1: Fermi's values of $\tau F(\eta_0)$ for the radioactive elements for which there were sufficient data on the continuous beta spectra. As indicated by the order of magnitude of $\tau F(\eta_0)$, the above beta sources might be divided into two groups, in agreement with Sargent's experimental results. Source: Fermi, "Versuch" (note 118), p. 175.

where $F(\eta_0)$ is a complicated function of the maximum momentum η_0 of the electron. For allowed transitions, Fermi argued, $\tau F(\eta_0)$ should be roughly constant and much smaller than for forbidden transitions, for which $\langle f|i\rangle$ was considered to be very small. To test this claim, he calculated the values of $\tau F(\eta_0)$ for several beta-radioactive sources. His results, shown in Table 6.1, reveal immediately the two groups anticipated from the classification established empirically by Sargent. Finally, from the above formula, Fermi was able to provide a rough estimate for the interaction constant g, finding that

$$g = 4 \times 10^{-50}\ \mathrm{cm}^3 \cdot \mathrm{erg}.$$

Thus, by introducing a new interaction, Fermi offered a beta-decay theory based on the proton–neutron model of the nucleus and Pauli's neutrino hypothesis. Fermi's theory soon came to constitute the foundation of future studies of weak interactions, and even of efforts to combine the weak and electromagnetic interactions. But what were the immediate reactions to it? As might be expected, Bohr was skeptical towards it, as revealed in his correspondence of early 1934 with Felix Bloch. On 10 February, Bloch wrote to Bohr:

> By the way, Fermi has now developed a rather attractive theory of β decay by trying to grasp Pauli's idea of the neutrino in a quantitative manner. As far as the experiments suffice for a quantitative comparison, the β spectrum seems to come out quite beautifully. It is remarkable in this connection that in order to obtain agreement one has to assign to the neutrino a mass very much smaller than that of the electron,

> if not a zero mass. What do you think about that, in particular with regard to energy conservation?[119]

Bohr replied:

> We have naturally all also been very interested in Fermi's new work, which undoubtedly will have a very stimulating effect on the work with electrical nuclear problems, although I still do not feel fully convinced of the physical existence of the neutrino.[120]

Bohr was not alone in his hesitancy towards Fermi's theory. At an international conference on physics in October 1934 in London and Cambridge, Gamow discussed the recent state of nuclear physics, remarking that:

> There is at present no consistent theory of the β-transformations of heavy particles as this question is evidently connected with the problem of elementary particles which is beyond the limits of the present quantum theory. Fermi has recently proposed a tentative theory of β-decay which is in fairly good agreement with experimental evidence. In this theory, however, the interaction energy between heavy and light particles is introduced purely phenomenologically and its absolute value is chosen so as to fit with the observed decay periods of β-active bodies. We cannot at present give any physical interpretation of these forces as they are too weak to be considered as electromagnetic but too strong to be connected with gravitational phenomena.[121]

Accordingly, Gamow still adopted Bohr's view that a new type of quantum theory was required to explain nuclear phenomena satisfactorily.

Beck, one of the originators of the competing theory of beta decay, also contributed a paper to the London–Cambridge conference. His main objection to Fermi's theory was aimed at its foundation – Pauli's neutrino hypothesis and the proton–neutron model of the nucleus. Concerning the neutrino, we have already seen that he agreed with Bohr's objection to it; concerning the nucleus, he still adhered to earlier models. "We must assume," Beck stated, "that a nucleus contains as a rule as many α-particles as is compatible with its mass. Then the heavier nuclei must contain, beside the α-particles, a certain number of 'free' nuclear charges."[122] One of his arguments in favor of this view was as follows:

> The energy involved in a proton–neutron transition is known to be of the order of a few times mc^2; the energies to be attributed to the heavy

[119] Bloch to Bohr, 10 February 1934, *BSC* (17.3).

[120] Bohr to Bloch, 17 February 1934, *BSC* (17.3). The translation is from Aaserud, *Redirecting Science* (note 93), p. 65.

[121] G. Gamow, "General Stability-Problems of Atomic Nuclei," in *International Conference on Physics, London 1934, Papers and Discussions*, Vol. I: *Nuclear Physics* (London: The Physical Society and Cambridge University Press, 1935), pp. 60–66, on p. 63.

[122] G. Beck, "Report on Theoretical Considerations Concerning Radioactive β-Decay," in *ibid.*, p. 39.

> particles, however, are at least of the order 10 mc^2. It is difficult to see why in a nucleus, for instance, an α-particle with binding energy of 27 millions e.V. should be regarded as less stable than a neutron or a proton.[123]

Beck also pointed to difficulties concerning the low-energy end of the beta spectrum. Recent experiments, he argued, were not in accordance with Fermi's theory.[124] But since the experimental evidence on this point was very uncertain, this argument of Beck's, like his other ones, was far from convincing and probably did not attract much attention, at least from those who supported Fermi's theory.

Despite his reluctance to accept Fermi's theory, Beck did not elaborate on his own theory, for reasons that he gave in the above-mentioned interview of 22 April 1967:

> What stopped me from the question was this: Well, Bohr wanted me at that time to go on, but where these constants came from was not explained up to now. I told him I didn't believe that this can be found out at the present stage. But what stopped me afterwards from working on beta decay – When I went to Russia I wanted still to work on beta decay, but afterwards the thing became so troubled, I had no access to experimental data.[125]

Even though Fermi's theory met with some resistance, the number of physicists who immediately supported it was undoubtedly large, probably much larger than the number of its opponents. Pauli was of course enthusiastic about it. "That would be grist to our mill," he wrote to Heisenberg in early January 1934.[126] Heisenberg, who had become convinced of the existence of the neutrino in late 1933, agreed,[127] and many of the younger physicists followed in his footsteps. After 1934, the Beck–Sitte theory was largely forgotten and was not even mentioned in the "Bethe bible" of 1936–1937.[128] Why did the Fermi theory gain such strong support within such a short period? One essential reason, I think, is to be found in its explanatory promise. Heisenberg and others did not take long to determine that Fermi's new interaction might also explain the force between the neutron and

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[123] *Ibid.*, p. 37.

[124] Beck referred here to Meitner. At the October Conference in Copenhagen, she had informed him that at least in the RaE spectrum almost no electrons with energies less than 50 keV appeared, and that seemed to be in disagreement with Fermi's theory. See G. Beck, "Bemerkung zur Arbeit von E. Fermi: 'Versuch einer Theorie der β-Strahlen'," *Z. Phys. 89* (1934), 259–260, p. 259n. See also E. Fermi, "Zur Bemerkung von G. Beck und K. Sitte," *Z. Phys. 89* (1934), 522.

[125] Interview with Beck (note 114), pp. 31–32.

[126] Pauli to Heisenberg, 7 January 1934, in Pauli, *Briefwechsel*, Vol. 2 (note 37), p. 248.

[127] Heisenberg to Pauli, 12 January 1934, *ibid.*, p. 249.

[128] H.A. Bethe and R.F. Bacher, "Nuclear Physics A: Stationary States of Nuclei," *Rev. Mod. Phys. 8* (1936), 82–229; H.A. Bethe, "Nuclear Physics B: Nuclear Dynamics, Theoretical," *ibid. 9* (1937), 69–224.

proton. This constituted the beginning of the development of Fermi field theory, which for several years was the dominant fundamental theory of nuclear forces.[129]

Bohr also came to accept Pauli's neutrino hypothesis and the principle of energy conservation; Pauli thus emerged from the dispute victoriously.[130] In this respect, a letter of Pauli's of January 1957 to Victor Weisskopf, written shortly after the discovery of parity violation, is revealing. Pauli wrote:

> In my talk[131] I described how Bohr in the Solvay Congress, 1933, as my main opponent in the neutrino business thought that a violation of the energy law might be plausible in the beta decay. His opposition then became weaker later on, and he said in quite general fashion (1933), "One should be prepared for further surprises with the beta decay." I then said that I will at the end of my talk come back to those surprises which Professor Bohr promised us at that time.
>
> He certainly was not correct in respect to the energy law (He only yielded in Nature, 1936), but, who knows, with the newest events is there any stop? Perhaps the beta interactions are still too strong in order to violate also the energy law; what about still weaker interactions in which the energy law does no longer hold, as Bohr suggested?[132]

Thus, even though Bohr was shown to be wrong regarding energy conservation, his remark that "one should be prepared for further surprises with the beta decay" proved to be prophetic.

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[129] L.M. Brown and H. Rechenberg, "Field Theories of Nuclear Forces in the 1930s: The Fermi-Field Theory," presented at a conference on *The History of Modern Gauge Theories*, July 19–26, 1987, at Utah State University, Logan; *idem.*, "Nuclear Structure and Beta-Decay (1932–1933)," *Am. J. Phys. 56* (1988), 982–988; *idem.*, *The Origin of the Concept of Nuclear Forces* (Bristol and Philadelphia: Institute of Physics, 1996). For a discussion of the Fermi theory in its first decade, see also A.D. Franklin, "Experiment and the Development of the Theory of Weak Interactions: Fermi's Theory," *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association 1986*, Vol. 2 (1987), 163–179. I am grateful to Laurie M. Brown and Allan D. Franklin for having sent me preprints of their papers.

[130] In fact, it took quite a long time before Bohr yielded completely to energy conservation in nuclear processes. Only in the summer of 1936 did he state publicly his full support of the principle. See N. Bohr, "Conservation Laws in Quantum Theory," *Nature 138* (1936), 25–26.

[131] Pauli refers here to his lecture at the Zürcher Naturforschende Gesellschaft on 21 January 1957; see C.S. Wu, "The Neutrino," in *Theoretical Physics in the Twentieth Century: A Memorial Volume to Wolfgang Pauli*, M. Fierz and V.F. Weisskopf (eds.) (New York: Interscience, 1960), pp. 249–303, on p. 301.

[132] Pauli to Weisskopf, 17 January 1957, *AHQP* (66.4).

Chapter 7

Towards a Theory of Internal Conversion: The Beta Line-Spectrum, 1927–1934

7.1 Introduction

The thorough discussions of the anomalous continuity in energy of the primary beta particles in the wake of the Ellis–Wooster experiment thrust the beta line-spectrum into the background for some time. Yet, especially in Cambridge, great efforts were made to obtain a better understanding of the line spectrum. In this chapter, we shall see how this work led to a satisfactory quantum theory of internal conversion.

Throughout the 1920s, physicists generally agreed that the lines in the beta spectrum were the result of an internal photoelectric effect in the radioactive atoms; Adolf Smekal's alternative idea of uniform atoms did not attract much attention. The dominant internal-conversion hypothesis was not without problems, however. In 1924, for example, Charles Drummond Ellis and Herbert Skinner had to acknowledge its insufficiency in accounting for some high-intensity lines in the RaB spectrum; but this did not challenge the hypothesis seriously. In 1927, Ellis still opposed Smekal's view.[1] As we shall see, an elaborate experiment conducted by Ellis himself forced physicists to reconsider the matter, and an extensive search for a new theory began. First, however, I must discuss the question of the origin of the gamma rays. Until the late 1920s, the prevailing view, in Cambridge at least, was that gamma radiation was intimately connected with the beta rays, but this view had to be modified fundamentally.

[1] C.D. Ellis, "The Relative Intensities of the Groups in the Magnetic β-Ray Spectra of Radium B and Radium C," *Proc. Roy. Soc. A117* (1927), 276–288, p. 285.

C. Jensen et al. (eds.), *Controversy and Consensus: Nuclear Beta Decay 1911-1934*

7.2 Experimental evidence brings about a new view on the origin of gamma rays

During the 1920s, the two leading authorities on beta-ray and gamma-ray research differed strongly in their views on the radioactive decay processes. On the one hand, Lise Meitner was guided by a firm belief in an analogy between alpha and beta decay, and was therefore reluctant to tie the emission of monochromatic gamma rays to either of these particular nuclear particles. On the other hand, Ellis was attracted to thinking in analogy to the emission of radiation by atomic electrons and consequently assumed that the gamma rays were closely connected to the movement of nuclear electrons.

Around 1927, the Cambridge physicists changed their view, however. As we have seen, Werner Kuhn questioned the connection between beta and gamma rays by showing that electronic transitions in the nucleus could not explain the high degree of homogeneity of the gamma lines; and since this result was in accord with Ellis and William Wooster's contemporary idea of an unquantized electronic region inside the nucleus, the soil was prepared for alternative proposals. Their nestor, Ernest Rutherford, thus suggested that transitions of neutralized alpha particles might instead be responsible for the homogeneous gamma rays.

This profound connection between alpha and gamma radiation found strong experimental support around 1930, in part from the Cambridge group itself. One of the innumerable research projects then being carried out in the Cavendish Laboratory was the study of long-range alpha particles, first observed in 1916.[2] Rutherford and Alexander Wood had found that even when they placed sufficient absorbing material between their thorium C source and the zinc-sulphide scintillation screen to stop completely the normal alpha particles of 8.6-cm range, a small number of scintillations still appeared. They estimated that the number of penetrating particles producing these scintillations was about 100 for every 10^6 alpha particles emitted by the source and that their range was about 11.3 cm in air. Rutherford and Wood's observations also indicated that one-third of these long-range alpha particles had a range of about 10 cm, and Meitner and Kurt Freitag confirmed this result shortly thereafter.[3]

In 1919, Rutherford observed that also alpha particles from radium C were accompanied by a small number of long-range particles,[4] and this result led to a search for such particles emitted from other radioactive sources.[5] A large num-

[2]E. Rutherford and A.B. Wood, "Long-Range Alpha Particles from Thorium," *Phil. Mag. 31* (1916), 379–386.

[3]L. Meitner and K. Freitag, "Über die α-Strahlen des ThC + C′ und ihr Verhalten beim Durchgang durch verschiedene Gase," *Z. Phys. 37* (1916), 481–517.

[4]E. Rutherford, "Collision of α Particles with Light Atoms," *Phil. Mag. 37* (1919), 537–587.

[5]See, for example, L.F. Bates and J.S. Rogers, "Particles of Long Range Emitted by the Active Deposits of Radium, Thorium, and Actinium," *Proc. Roy. Soc. A105* (1924), 97–116; K. Philipp, "α-Teilchen grosser Reichweite beim Thorium," *Naturwiss. 12* (1924), 511; N. Yamada, "Sur les particules de long parcours du polonium," *C.R. 180* (1925), 436–439; *idem.* "Sur les particules de long parcours émises par les dépôt actif du thorium," *C.R. 180* (1925), 1591–1594; *idem.*, "Sur

Radium C	28 of c. 9 cm range 5 of c. 11 cm range 10^6 of normal particles of 7 cm range
Thorium C	65 of c. 9.5 cm range 180 of c. 11.5 cm range 10^6 of normal particles of 8.6 cm range

Table 7.1: The relative proportions of the long-range alpha particles and the normal alpha particles from radium C and thorium C. Source: E. Rutherford, J. Chadwick, and C.D. Ellis, *Radiations from Radioactive Substances* (Cambridge University Press, 1930), p. 91.

ber of experiments were carried out, and towards the end of the 1920s, it was generally accepted that radium C as well as thorium C emitted two groups of long-range alpha particles (see Table 7.1), whereas there was no certain evidence of any particularly penetrating alpha particles from actinium C and polonium.

The origin and nature of these long-range alpha particles was the subject of much discussion. Shortly after the Great War, Rutherford suggested that they were not emitted from the source, but produced by collisions in the gases traversed by the ordinary alpha particles. Furthermore, he examined their deflection in a magnetic field and concluded that they were of mass three, that is, another isotope of helium.[6]

Further investigations proved that Rutherford was incorrect.[7] The observed long-range particles were in fact alpha particles, and towards the end of the 1920s, physicists also generally agreed that they were emitted from the radioactive source. Since only very few groups were observed, however, it was still difficult to see any connection between them and the emission of gamma rays, which at least the Cambridge physicists had in mind.

Further experiments were required, and once again the brilliant Cavendish experimentalists paved the way. In 1929, they introduced a new method for detecting alpha-particle groups, the so-called differential method, which was especially suited for counting particles present in small numbers as compared to the main group.[8] The idea was to use two ionization chambers instead of one (see Figure 7.1) to count, not the total number of particles exceeding a certain range, but the

les particules de long parcours émises par le dépôt actif du radium," *C.R. 181* (1925), 176–178; M.P. Mercier, "Sur les particules de long parcours émises par le dépôt actif $B+C$ de l'actinium," *C.R. 183* (1926), 962–964.

[6]E. Rutherford, "Bakerian Lecture: Nuclear Constitution of Atoms," *Proc. Roy. Soc. A97* (1920), 374–400.

[7]E. Rutherford, "The Mass of the Long-Range Particles from Thorium C," *Phil. Mag. 41* (1921), 570–574.

[8]E. Rutherford, F.A.B. Ward, and C.E. Wynn-Williams, "A New Method of Analysis of Groups of Alpha-Rays. – (1) The Alpha-Rays from Radium C, Thorium C, and Actinium C," *Proc. Roy. Soc. A129* (1930), 211–234.

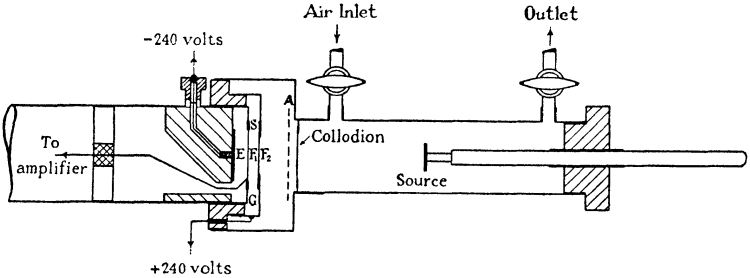

Figure 7.1: A diagram of the apparatus used by Rutherford, Ward and Lewis in their analysis of alpha-particle groups. It shows the design of the double ionization chamber and the source holder. The front of the first ionization chamber was covered, not as was usual with sputtered mica, but with a thin gold foil F_2, in order to reduce as far as possible the amount of absorbing material between source and counter. The collecting electrode consisted of a thin brass ring over which was stretched a thin gold foil, F_1, of stopping power about 0.4 mm in air for alpha particles. This also formed the front of the second ionization chamber, which was closed at the back by the brass disc E. With these thin foils charged to high potentials and close to the free grid system, the counter behaved as a condenser-microphone, and the apparatus was exceedingly sensitive to sound waves. It was quite impossible to work with it in the open air, and the two ionization chambers had to be enclosed in a gas-tight metal box. Source: Rutherford, Ward, and Lewis, "Analysis" (note 9), p. 688.

Number of group	Range cm of air	Energy in eV$\times 10^{-6}$	Relative number
	6.95	7.683	10^6
1	7.87	8.303	0.49
2	9.13	9.117	16.7
3	9.60	9.412	0.53
4	9.88	9.585	0.93
5	10.31	9.843	0.60
6	10.56	9.992	0.56
7	10.94	10.215	1.26
8	11.37	10.466	0.67
9	11.64	10.623	0.21

Table 7.2: The nine groups of homogeneous long-range alpha particles from radium C, observed by Rutherford, Ward, and Lewis. Source: Rutherford, Ward, and Lewis, "Analysis" (note 9), p. 693.

number having ranges between, say, x and $x + \Delta x$, where Δx was of the order of a few millimeters. The front foil F_2 of chamber one was charged to a potential of +240 volts, the back plate E of chamber two to −240 volts. When a particle entered, the positive ions formed in the first chamber and the negative ions formed in the second were driven to the collecting electrode F_1. Therefore, as experiments showed, only particles that had been stopped between F_2 and E would produce a positive net charge; and if only positive kicks of more than 70 percent of the maximum charge were counted, these would correspond to alpha particles that had been stopped within a region 2 mm. By using this method, Rutherford, Francis Ward and Wilfrid Lewis were able to show that the alpha particles from actinium C were complex, and that the long-range particles from radium C consisted of no less than nine groups of homogeneous alpha particles (see Table 7.2). Moreover, the energy differences fitted well with the energies of the high-frequency gamma rays from radium C. Rutherford and his collaborators did not hesitate to conclude that:

> Considering the evidence as a whole, there can be no doubt that there is a close connection between the emission of long-range α-particles and the emission of the high frequency γ-rays from radium C.[9]

This connection between alpha and gamma rays was corroborated by experiments in Paris, where the installation of a new and strong electromagnet in the Office National des Recherches et Inventions made it possible to employ the semicircular focusing method, so much used in the study of beta-particle spectra, on alpha particles as well. This method was superior to that applied in Cambridge in that it had a greater resolving power, and with it Salomon Rosenblum succeeded in separating the alpha particles from thorium C into four, and later five, very close groups (see Table 7.3).[10] Hendrik Kramers, who was in Paris at the time, was impressed by Rosenblum's experiments, writing to Bohr in June 1930:

> The most beautiful I have seen in Paris were the new experiments by Rosenblum. With [Aimé] Cotton's large magnet he has obtained a spectrum of the α-rays from RaC, in the same way as it is being done for β rays, and he has found that the Ra α rays *quite clearly* possess 4 different ranges, two intensive ones quite close to each other and two weaker ones a little further out.[11]

Accordingly, the Paris magnetic-deflection experiments, as well as the Cambridge investigations on long-range alpha particles, pointed strongly to a close connection between alpha and gamma rays. But how was this connection to be understood? Rutherford and Ellis advanced a hypothesis based upon their long-

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[9]E. Rutherford, F.A.B. Ward, and W.B. Lewis, "Analysis of the Long Range α-Particles from Radium C," *Proc. Roy. Soc. A131* (1931), 684–703, p. 702.

[10]S. Rosenblum, "Structure fine du spectre magnétique des rayons α," *C.R. 190* (1930), 1124–1127; *idem.*, "Progrès récents dans l'étude du spectre magnétique des rayons α," *J. de Phys. 12* (1930), 438–444.

[11]Kramers to Bohr, 18 June 1930, *BSC* (22.3). The translation is my own.

Name of group	Energy differences $(E_0 - E_n) \times 10^{-6}$ volt	Relative Intensity
0	–	1
1	0.0406	3.3
2	0.328	0.1
3	0.462	0.02
4	0.483	0.07

Table 7.3: The five groups of homogeneous alpha rays from thorium C, observed by Rosenblum. Source: Rosenblum, "Structure" (note 10), p. 1126.

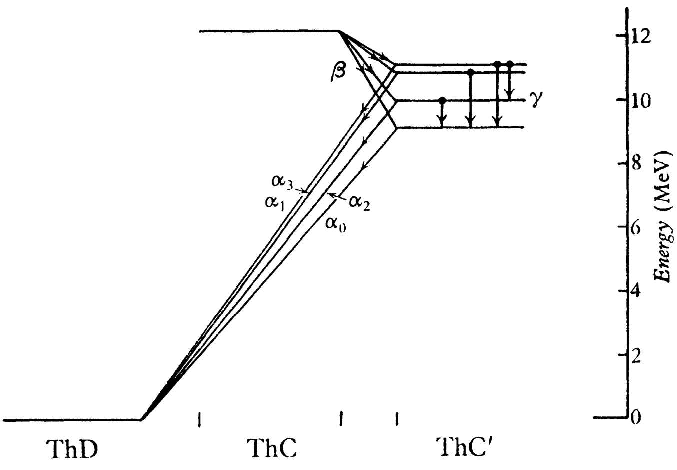


Figure 7.2: Long-range alpha particles in the ThC$\xrightarrow{\beta}$ThC′ $\xrightarrow{\alpha}$ThD transition. Source: W.E. Burcham, *Nuclear Physics: An Introduction* (London: Longmans Green and Co. Ltd., 1965), p. 583.

range alpha-particle experiments.[12] After a beta decay, they argued, the nucleus was left in an excited state. If a number r of the total number n of alpha particles in the nucleus were excited to a higher level, then – according to whether 1, 2, 3, ..., r alpha particles made a simultaneous transition to a lower level – 1, 2, 3, ..., r different gamma rays should be emitted. Owing to interaction effects, which made such multiple transitions possible, the frequencies of these gamma rays would not be multiples of each other. For a similar reason, a transition of, say, one alpha particle from among the number r in the upper state, would produce a gamma ray of slightly different frequency than if there were $r - 1$ in the upper state. Such a system, according to Rutherford and Ellis, would be characterized by the frequencies of the gamma rays being given by the expression $pE_1 - qE_2$, where p and q are integers and the energy E_1 corresponds to the difference in energy of the two states, whereas the energy E_2 is mainly determined by the interaction energy and is expected to be small compared to E_1. An alpha particle in an excited state would have a small probability of escaping from the nucleus instead of returning to the ground state, and these escaping alpha particles then constituted the long-range groups. The modern diagram shown in Figure 7.2 illustrates this phenomenon.

Another hypothesis on the origin of the gamma rays was advanced by George Gamow in 1930.[13] To explain the emission of longe-range alpha particles, he pointed out, the above-mentioned model was suitable; but a serious difficulty arose when it was applied to the fine-structure of the thorium C alpha particles observed by Rosenblum. The relative number of alpha particles in a long-range group, Gamow argued, must be given by

$$N = p\,\frac{\lambda}{\theta},$$

where p is the relative number of nuclei in an excited state, λ the corresponding decay constant, and θ the probability that the excited nucleus will make a transition to a lower state with the emission of energy in the form of gamma radiation. Since the decay constant λ for an excited thorium C nucleus is very small, very small transition probabilities also had to be assumed, which was unreasonable.

Gamow therefore proposed an alternative explanation of the fine-structure groups observed by Rosenblum. He imagined that two (or more) alpha particles in the thorium C nucleus were in the ground state and suggested that, after one had escaped, the nucleus was left in an excited state, that is, the remaining alpha particles were excited to a level above the ground state. The excited nucleus (thorium C$''$) could then return to its ground state by emitting a gamma-ray quantum. The modern diagram shown in Figure 7.3 illustrates Gamow's explanation.

[12] E. Rutherford and C.D. Ellis, "The Origin of the γ-Rays," *Proc. Roy. Soc. A132* (1931), 667–688.

[13] G. Gamow, "Fine Structure of α-Rays," *Nature 126* (1930), 397. See also G. Gamow, *Constitution of Atomic Nuclei and Radioactivity* (Oxford: Clarendon Press, 1931).

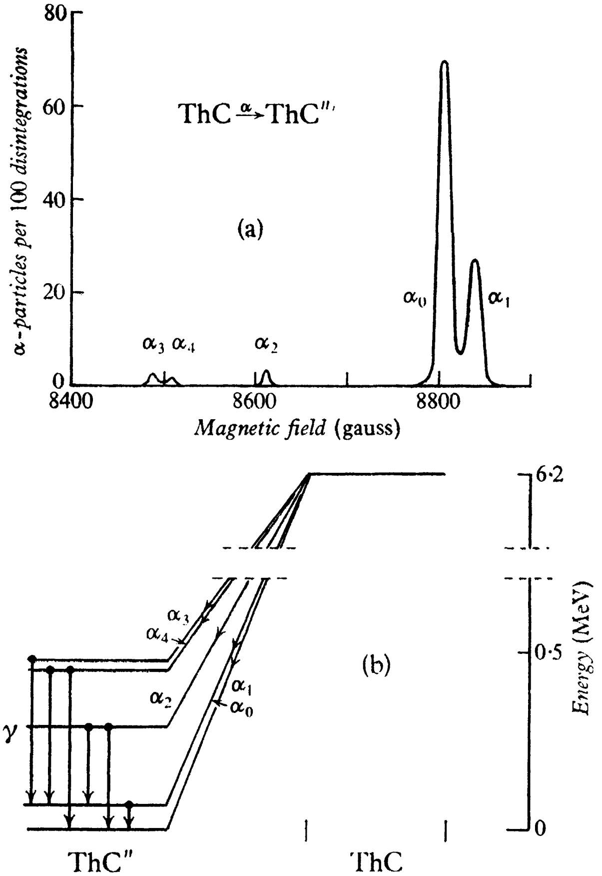


Figure 7.3: Fine-structure of alpha-particle spectra. (a) Magnetic analysis of groups from ThC⟶ThC″ transitions. (b) Level scheme, showing gamma-ray transitions in the final nucleus. Source: Burcham, *Nuclear Physics* (Figure 7.2), p. 581.

Crucial to Gamow's hypothesis was that the gamma rays were emitted immediately after the alpha-particle disintegration of thorium C, and definitely not after the beta decay of thorium C″. Accordingly, his hypothesis could be tested, and since ThC″ could be obtained in a pure state from ThC by recoil, the most obvious way to do so was to look for the beta-ray lines corresponding to the very intense gamma ray of 40.8 keV from pure ThC″.

Such experiments were carried out both in Cambridge and in Berlin, and once again prompted disagreement between Ellis and Meitner. As Ellis wrote to Bohr on 11 January 1932:

> I have been carrying out experiments to photograph the β-ray spectra from Thorium C″ prepared by recoil, and have satisfied myself that the β-rays associated with the γ-rays in question are not found from a source of Thorium C″, and therefore come after the disintegration of Thorium C as required by Gamow's hypothesis. My experiments seem quite definite, and, of course, Gamow's theory is very plausible on general grounds, and I should feel no doubt at all about the whole problem were it not for a short note by Meitner in the Naturwissenschaften. She appears to have been carrying out similar experiments and claims to have found that the crucial γ-rays are emitted by Thorium C″, in disagreement with Gamow's views. She said nothing about these experiments in Rome, and I presume that she has started them subsequently, and I cannot understand the result.[14]

Ellis was here referring to a short note of 30 November 1931, in which Meitner and Kurt Philipp mentioned the result of a very recent investigation that indeed did seem to disagree with Ellis's:

> We have confirmed the existence of both of the ThC″ lines (transition ThC″ $\xrightarrow{\beta}$ ThPb), and more precisely the 40.8 kV line by 3 corresponding β-ray groups and the 279 keV line by 2 β groups. This finding contradicts Gamow's theory of the α fine structure.[15]

Meitner and Philipp repeated their experiment and submitted a detailed account of it to the *Zeitschrift für Physik* late in 1932. They confirmed their previous result, but offered a different interpretation of it:

> Now Rosenblum's very accurate fine-structure measurements have demonstrated that the strong 279 kV line, attributed by Gamow to the fine structure, actually has to be replaced by the weaker 292 kV γ line; and, furthermore, by means of his extremely high magnetic field

[14] Ellis to Bohr, 11 January 1932, *BSC* (19.1).

[15] L. Meitner and K. Philipp, "Das γ-Spektrum von ThC″ und die Gamowsche Theorie der α-Feinstruktur," *Naturwiss.* *19* (1931), 1007.

> Rosenblum has been able to show that the 40.8 kV γ line is a very narrow doublet.[16]

Accordingly, what Meitner earlier considered to be grounds to reject Gamow's hypothesis now turned out to confirm it. Further support appeared in the early 1930s. Rosenblum and Marie Curie, for example, observed six alpha-particle groups from RdAc and found their energy differences to be in good agreement with the gamma-ray spectrum.[17] Gamow's theory thus became generally accepted, and the correct connection between the emission of alpha and gamma rays was established.

7.3 The radiation hypothesis proves insufficient to explain internal conversion of gamma rays

As we have seen, Smekal introduced the idea of unified quantum transitions in the early 1920s. Very few physicists took them seriously at that time; nevertheless, Smekal maintained his view, and in 1926 raised philosophical as well as experimental objections to the photoelectric-effect interpretation of internal conversion. Smekal argued that, "It is awkward to postulate the existence of a radiation process that takes place entirely inside the atomic system and thus is *unobservable in principle*."[18] This positivistic attitude towards theory construction was widespread on the Continent at the time. For example, already in 1919 Wolfgang Pauli liked "to insist that only quantities which are observable in principle should be introduced in physics,"[19] and Werner Heisenberg's matrix mechanics was precisely the outcome of elaborate efforts to create a theory exclusively founded upon fundamentally observable quantities.

Smekal's experimental objections concerned gamma-ray absorption coefficients. When gamma rays ejected electrons from non-radioactive metals, say Pb, their absorption coefficients indicated a regularity very similar to that of X rays, that is, a λ^3 dependence. Internal-conversion coefficients, however, showed no systematic dependence on the wavelength λ. Moreover, the internal-absorption coefficients a^* were usually several orders of magnitude larger than the external-absorption coefficients a. As a conspicuous example, Smekal noted that for one of the gamma rays of radium D ($\lambda = 264$ X.U.) a^* was 0.67 whereas a was only 3×10^{-21}. Finally, as the French physicist Jean Thibaud first demonstrated, some

[16] L. Meitner and K. Philipp, "Die γ-Strahlen von ThC und ThC$''$ und die Feinstruktur der α-Strahlen," *Z. Phys. 80* (1932), 277–284, pp. 279–280.

[17] S. Rosenblum and M. Curie, "Spectre magnétique des rayons α du dépôt actif de l'actinium," *J. de Phys. 2* (1931) 309–311; reprinted in *Oeuvres de Salomon Rosenblum* (Paris: Gauthier-Villars, 1969).

[18] A.G.S. Smekal, "Über spontane 'strahlungslose' Quantenvorgänge," *Ann. d. Phys. 81* (1926), 391–406, p. 393; *idem.*, "Über spontane 'strahlungslose' Quantenvorgänge," *Phys. Zeit. 27* (1926), 831–833.

[19] W. Pauli, "Merkurperihelbewegung und Strahlenablenkung in Weyls Gravitationstheorie," *Verh. d. D. Phys. Ges. 21* (1919), 742–750, pp. 749–750.

strong groups in natural beta-ray spectra did not have counterparts in the corresponding corpuscular spectra excited in non-radioactive metals.[20] As an example, he observed that the RaC gamma ray of wavelength 8.66 X.U. was not able to excite beta rays in Pb, Pt, and U, even though its beta-ray line in the natural spectrum was of very high intensity. According to Smekal, these discrepancies proved that internal and external absorption of gamma rays could not be explained in the same way. It was necessary, he argued, to adopt the idea of "an in principle inseparable quantum-like cooperation between nucleus and electron shell."[21] How was Smekal's view received?

In Cambridge, a new program to investigate beta-ray intensities had been initiated in the mid-1920s. Studies on the energy of homogeneous beta-particle groups had led to many important results. For example, the newly deduced energy of gamma rays strongly indicated the existence of nuclear-level systems. But current measurements were not sufficiently accurate to provide a unique determination of these level systems, and no immediate prospect of achieving a significant increase in accuracy seemed to be present. This negative outlook made Ellis turn to what he considered to be a much more promising method of obtaining information about the nucleus, namely, intensity measurements. He emphasized that it would be valuable to have even approximate values of beta-ray line intensities.

The first results of this new program of research were submitted to the *Proceedings* of the Royal Society in January 1927.[22] Ellis and Wooster had investigated the relative intensities of the beta-ray groups of RaB and RaC, and fastened upon the similarities between internal and external absorption. In both cases, they observed a constancy of the L/K absorption ratio over a wide range of frequencies and found good agreement between the numerical values. On the whole, their experiments indicated a high similarity between the two types of absorption. They admitted that there was too little evidence to speak with certainty on this point, but that did not prevent them from concluding that:

> It is our opinion that in the case of γ-rays it is both useful and probably correct to consider the β-ray groups as due to a true internal absorption, and that this absorption is similar to ordinary external absorption when the special conditions under which it occurs are taken into account.[23]

Thus, in 1927, the new Cambridge program offered no support to Smekal's view.

Later that year, Bertha Swirles, also a Cambridge physicist, presented a quantum-mechanical treatment of the internal absorption of gamma rays in the K

[20] J. Thibaud, *Thèse. La spectroscopie des rayons γ. Spectre β secondaires et diffraction cristalline* (Paris, 1925). See also J. Thibaud, "Les spectres secondaires de rayons gamma: Sur l'origine du fond continu et la variation d'intensité relative des raies," *J. de Phys. 6* (1925), 334–336.

[21] Smekal, "Über spontane Quantenvorgänge " (note 18), p. 406.

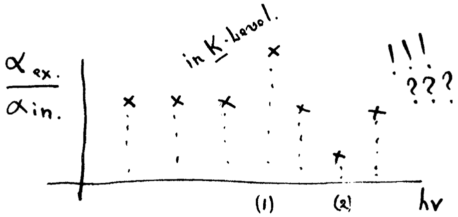
[22] C.D. Ellis and W.A. Wooster, "The Relative Intensities of the Groups in the Magnetic β-Ray Spectra of Radium B and C," *Proc. Roy. Soc. A117* (1927), 276–288.

[23] *Ibid.*, p. 285.

level.[24] She conceived the problem as one involving the perturbation of a heavy atom by a Hertzian doublet placed at its center, assuming that the characteristic functions corresponded to the electron orbits of a hydrogen-like atom. In this way, she obtained expressions for the coefficients of absorption in the K level. The values proved to be about one tenth of the experimental ones, a discrepancy that was ascribed to the neglect of the screening by the other electrons. Swirles also calculated the L-level absorption, confirming her results for the K level: the calculated coefficients of absorption were of the order of ten times too small. Nevertheless, her calculations of the variation of the absorption coefficients with the wavelength of the gamma rays – as well as of the ratios, first, of the absorption between the K and L_{I} levels, and, second, among the three L levels – proved to be in reasonable agreement with experiment. Accordingly, the discrepancies between theory and experiment were not sufficiently marked to undercut the radiation hypothesis seriously.

Ellis continued his own program of research, and during the next two years obtained decisive results. Gamow learned about some of them early in 1929, when he visited Cambridge, and in a letter of 21 January, he informed Bohr:

> The intensity measurements of the γ rays from RaC in Ellis's previous work are not correct! In the new experiments, he measured "ratio of coefficients of external/internal conversions". It "must" be constant, but it is not so:



> It means that α_{internal} (which Ellis previously thought varied as λ^{-3}) has a certain anomaly (quite incomprehensible) at some certain places; it also means that another intensity has to be ascribed to the two lines. For one of them the γ-excitation energy has been very bad! Now it is much better, but still not quite good! The work is probably not yet finished, and he has hardly accurate quantitative results.[25]

Shortly thereafter, on 7 February, Ellis gave an address before the Royal Society in which he reported his remarkable results and the consequences that had to be drawn from them. In the words of the reporter:

[24] B. Swirles, "The Internal Conversion of γ-Rays," *Proc. Roy. Soc. A116* (1927), 491–500; *idem.* "... Part II," *Proc. Roy. Soc. A121* (1928), 447–456.

[25] Gamow to Bohr, 21 January 1929, *BSC* (11.1). Gamow wrote this letter in his typical mixture of Danish and German. The translation is my own, whereas the drawing is taken from the original letter.

> They [Geoffrey Aston and Ellis] find evidence of a coupling between the nucleus and the electronic system which may be illustrated in the following way. If the γ-rays from a radioactive body fall on a sheet of lead the intensities of the photoelectric groups will depend jointly on the intensities of the γ-rays and the coefficients of absorption. Similar photoelectric groups are found to come from the radioactive atom and might be explained as a partial internal absorption of the γ-rays. If the nucleus and the electronic system were as distinct as the nucleus and the sheet of lead, then a parallelism would be expected between the intensities of the photoelectric groups from the lead and from the radioactive atom. Actually the reverse is found, and, although the experiments are not yet complete, it seems certain that remarkable oscillations occur in the ratio of the internal to external absorption as the scale of frequencies is ascended, and this must be taken to show a coupling.[26]

The Cambridge program on beta-ray intensity measurements, initiated and directed by Ellis, had thus led to the recognition that the radiation hypothesis was insufficient to explain internal conversion.

Ellis and Aston continued their experiments, and in November Gamow, again in Cambridge, reported new and interesting results to Meitner:

> There is a good deal of experimental news here – but as to theory and understanding the situation is still rather poor. Ellis has now found that the γ lines (from RaC) can be separated into two classes. Some exhibit a normal "internal conversion," the others one about 10 times larger. It seems that also the energy-level diagram falls into two parts. What does this mean? Does it mean that here we have to do with two completely different nuclei – or are both classes of lines something which, so to speak, exist during – (or before –) and after – the decay processes?[27]

Two days earlier Gamow had written to Bohr on the same subject:

> Here are some news about nuclear questions. Ellis has found that (for RaC) all the γ-lines, and it seems also the γ-levels, can be divided in two classes. First class has the ordinary internal conversion coefficient, the second ten or twenty times larger;

[26] "Discussion on the Structure of Atomic Nuclei," *Proc. Roy. Soc. A123* (1929), 373–390, pp. 385–386.

[27] Gamow to Meitner, 27 November 1929, MTNR 5/6.

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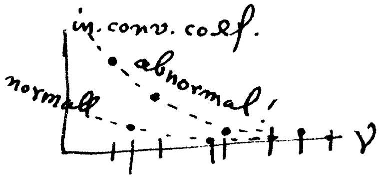

What could it all mean?[28]

Ellis and Aston's paper was long in preparation, but late in June 1930 it was submitted to the *Proceedings* of the Royal Society.[29] The method they used was simple in principle. It consisted of taking photographs of the natural beta spectrum of radium B + C and of the corresponding spectrum excited in platinum, under exactly known conditions, and then determining the relative intensities of corresponding beta-ray groups by a photometric study of the plates. Considering the rth gamma ray, the intensity of the RaB + C beta line of energy $h\nu_r - E_K$ is given by

$$N \cdot p_r \cdot {}_K\alpha_r,$$

where N is the number of disintegrations, p_r the probability that the energy $h\nu_r$ is emitted from the atom in one form or another, and ${}_K\alpha_r$ the probability that this energy will appear in the form of a photoelectron of energy $h\nu_r - E_K$, that is, corresponding to the internal-absorption coefficient for the K level. The intensity of the corresponding beta line in the Pt spectrum is

$$N \cdot p_r \cdot (1 - \alpha_r) \cdot \tau_K,$$

where $\alpha_r = {}_K\alpha_r + {}_{L_\mathrm{I}}\alpha_r + \cdots$, and τ_K represents the part of the normal photoelectric-absorption coefficient that corresponds to conversion in the K level. Experiments of this type thus would lead to values of the quantity

$$\frac{{}_K\alpha_r}{1 - \alpha_r} \cdot \frac{1}{\tau_K}, \tag{7.1}$$

and since τ_K could be determined from the empirical rule

$$\log_{10} \tau = a + b \cdot \log_{10} \lambda + c \cdot (\log_{10} \lambda)^2,$$

where a, b, and c are known constants, and $\alpha_r \approx 1.2\ {}_K\alpha_r$, an approximate value of ${}_K\alpha_r$ could be obtained.

[28]Gamow to Bohr, 25 November 1929, *BSC* (11.1). The drawing is taken from the original letter.

[29]C.D. Ellis and G.H. Aston, "The Absolute Intensities and Internal Conversion Coefficients of the γ-Rays of Radium B and Radium C," *Proc. Roy. Soc. A129* (1930), 180–207.

In practice, a determination of the quantity (7.1) was difficult, and only after a detailed and complicated analysis of disturbing effects did Ellis and Aston arrive at the following relation:

$$\frac{{}_K\alpha_r}{1-\alpha_r} \cdot \frac{1}{{}_K\tau_r} = \frac{L_r}{G_r} \cdot \frac{H\rho \cdot \beta^3 f(\beta)}{B} \cdot S \cdot A,$$

where L_r and G_r are the peak intensities of the natural beta-ray line and the corresponding excited line, respectively, A is a numerical factor depending upon the dimensions of the source and the apparatus, S arises from the straggling of the electrons in emerging from the lower levels of the platinum tube, and $\beta^3 f(\beta)$ is the corresponding loss in velocity. The term $f(\beta)$ is an approximate correction factor taken from Bohr's theory of the penetration of charged particles through matter; B is a constant depending upon the relative intensities of the natural and excited lines, which could be determined experimentally.

The results of Ellis and Aston's investigation are shown in Table 7.4. In accordance with previous measurements, the values of $\frac{{}_K\alpha_r}{1-\alpha_r}$ for radium B decreased with increasing frequency, but those for radium C showed quite a different behavior. The first five gamma rays appeared to have practically equal values for the internal-conversion coefficients; the same held true for the remaining three, but their coefficients were much smaller in absolute magnitude.

In addition, for the gamma ray of energy 14.26×10^5 electron volts, Ellis and Aston observed three lines in the natural spectrum, corresponding to internal conversion in the K, L_{I}, and M_{I} states, respectively, but they found no line in the beta spectrum of Pt that could be associated with this gamma-ray line. They thus confirmed Thibaud's result of 1925, showing the absence of this gamma ray.

The conclusion from this substantial investigation was obvious. "In the face of these facts," Ellis and Aston stated, "we have no hesitation in abandoning the hypothesis of only radiative coupling between the nucleus and electronic system, in fact our results point directly to a direct action of the nucleus on the electronic structure."[30] This statement signaled the start of intensive efforts to obtain a quantum-mechanical understanding of internal conversion.

7.4 A theory of internal conversion is developed

Ralph Fowler, the senior theorist in Cambridge, was among the physicists who were attracted to the internal-conversion problem. "I have tried very hard lately to understand internal conversion coefficients for γ-rays by playing with fantastic model nuclei," he wrote to Bohr on 3 February 1930, and continued:

[30] *Ibid.*, p. 195.

$h\nu$ of γ-ray in volts $\times 10^{-5}$	photoelectric absorption coefficient in K level of platinum (τ_K)	$\dfrac{{}_K\alpha_r}{1-\alpha_r}$
	Radium B	
2.43	9.00	0.364
2.97	5.35	0.186
3.54	3.45	0.117
	Radium C	
6.12	0.940	0.0061
7.73	0.568	0.0048
9.41	0.370	0.0061
11.30	0.256	0.0062
12.48	0.210	0.0057
13.90	0.170	0.0014
14.26	0.162	–
17.78	0.108	0.0016
22.19	0.073	0.0013

Table 7.4: The result of Ellis and Aston's measurements of $\dfrac{{}_K\alpha_r}{1-\alpha_r}$. The radium B values behaved as expected: they decreased with increasing frequency. The radium C gamma rays, however, formed two groups, each consisting of rays with almost equal internal-conversion coefficients. Source: Ellis and Aston, "Absolute Intensities" (note 29), p. 190.

> I came across one obvious point of which I was ignorant, & that is that the transition
>
> $$\left.\begin{array}{c}\text{Excited nucleus}\\ K\text{-electron}\end{array}\right\} \longrightarrow \left\{\begin{array}{c}\text{deexcited nucleus}\\ \text{Free electron}\end{array}\right.$$
>
> which can be regarded as a two stage
>
> $$\left.\begin{array}{l}\text{emission}\\ \text{of } \gamma\text{-ray}\\ \text{by nucleus}\end{array}\right\} + \left\{\begin{array}{c}\text{reabsorption of}\\ \gamma\text{-ray}\\ \text{by } K\text{-electron}\end{array}\right.$$
>
> can occur when no γ-ray can be emitted – that is there can be true collision processes at work.[31]

[31]Fowler to Bohr, 3 February 1930, *BSC* (19.2).

In June 1930, Fowler submitted the results of his speculations to the *Proceedings* of the Royal Society.[32] Actually, Fowler and Ellis intended to publish jointly the outcome of repeated discussions, but for some reason they did not.

The internal-conversion coefficient for the K shell, α_K, is defined as the ratio

$$\alpha_K = \frac{b}{p},$$

where b is the probability per unit time that an electron is ejected from the K shell, and p is the probability per unit time that the nucleus makes a transition followed by the emission of either a gamma-ray quantum or an extra-nuclear electron. Since p could be calculated easily by evaluating the rate at which energy was radiated by the field of the oscillating structure in the nucleus, the difficulty arose in calculating the probability b.

As we have seen, Swirles succeeded in calculating some internal-conversion coefficients for RaB and RaC by assuming the presence of a Hertzian doublet at the center of the atom, but her results were generally about ten times too small. The reason for this discrepancy, Fowler suggested in his paper, might be that she did not include the effect of collisions between the nucleus and the K electrons. Even without any specific knowledge of the nuclear wave function, the angular-momentum selection rule for the emission of an extra-nuclear electron due to collision could be shown to be $\Delta j = 0, \pm 1$, that is, the same as that for radiative transitions *except* that $j = 0 \to j = 0$ was allowed. Consequently, beta-ray transitions could actually occur even though the corresponding gamma rays could never be emitted. An obvious example, Fowler stated, was the strong, abnormally converted beta ray of energy 14.26×10^5 electron volts, which probably originated from an excited nuclear level from which a radiative transition was impossible.

In general, to calculate the contribution to the internal-conversion coefficient due to collision, a detailed knowledge of the nucleus was required, and Fowler tried to use various speculative nuclear structures. He had no success, however, and finally had to conclude that "detailed investigation of finite barriers as a possible source for the required correction factor of 10 in Miss Swirles's calculations is not likely to be profitable."[33] The values of the internal-conversion coefficients, and their variations, were still a riddle.

In 1931, the Dutch physicist Hendrik Casimir made another attempt to improve on Swirles's results.[34] Instead of Erwin Schrödinger's equation, he used Paul Dirac's relativistic equation, but found results no closer to the experimental values. He calculated the internal-conversion coefficients for very hard gamma rays only, that is, for $h\nu \gg mc^2$, and only used the asymptotic expansion of the free-electron wave function. Still, the conclusion seemed obvious:

[32] R.H. Fowler, "Speculations Concerning the α-, β- and γ-Rays of RaB, C, C′ – Part I. A Revised Theory of the Internal Absorption Coefficient," *Proc. Roy. Soc. A129* (1930), 1–24.
[33] *Ibid.*, p. 24.
[34] H.B.G. Casimir, "Innerer und äusserer Photoeffekt," *Phys. Zeit. 32* (1931), 665–667.

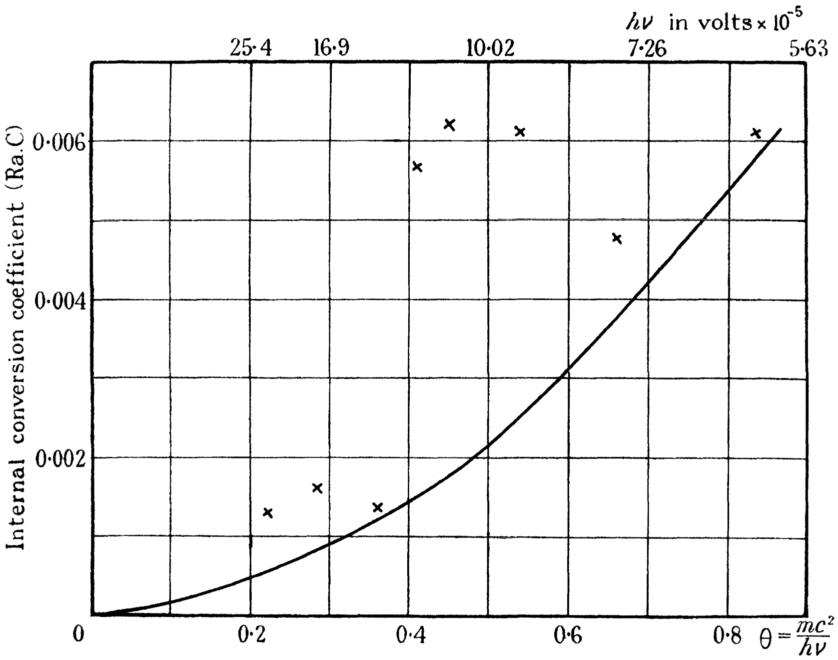

Figure 7.4: The curve shows the values of the internal-conversion coefficients for the K shell of RaC, as calculated by Hulme. The points marked with crosses are the experimental values determined by Ellis and Aston. Source: Hulme, "Internal Conversion" (note 36), p. 662.

> The experimental values cannot be interpreted as due to an inner photoeffect; the "internal conversion" is caused by the unknown interaction in the vicinity of the nucleus.[35]

Paul Ehrenfest, Casimir's teacher, asked to have this important conclusion checked by an independent calculation, and Fowler gave this task to Henry Rainsford Hulme.[36] Hulme realized that the relativistic calculation could be carried through for *all* values of $h\nu/mc^2$. He thus went beyond Casimir's calculations and used the actual wave functions instead of asymptotic expansions. The part of the wave function near the nucleus could well be important, Hulme argued, and this turned out to be correct. The internal-conversion coefficients for the K shell of RaC were calculated for four values of $h\nu$, as shown in the curve in Figure 7.4, where the

[35] *Ibid.*, p. 666.

[36] H.R. Hulme, "The Internal Conversion Coefficient for Radium C," *Proc. Roy. Soc. A138* (1932), 643–664.

experimental values are marked with crosses. The results of theory and experiment seemed to be of the same order of magnitude if the three points between $\theta = 0.4$ and $\theta = 0.6$ were excluded. Accordingly, Hulme's relativistic calculations greatly improved the agreement between the theoretical and experimental values for some internal-conversion coefficients, while for other rays his calculations did not fit the experimental points at all. Internal conversion in radium B, for example, proved impossible to explain on the basis of the present dipole theory.

Immediately following Hulme's paper in the *Proceedings* of the Royal Society was one by Henry Taylor and Nevill Mott that focused on the internal-conversion coefficients that had not yet been explained satisfactorily.[37] Taylor and Mott proposed a reason for the discrepancy. At least for points within the nucleus, they argued, the assumption of dipole radiation could not be correct. They therefore repeated Hulme's calculations on the assumption that the nuclear field was that of a quadrupole, instead of a dipole. A radiating system emitted a quadrupole field during transitions in which the angular-momentum quantum number j changed by two or, in certain cases, by zero units. In atomic systems, such transitions were much less frequent than dipole transitions. Gamow and Max Delbrück had recently pointed out, however, that in a nucleus built up of alpha particles only, or of any *one* kind of particles, the centers of mass and of electrical charge would coincide, and the dipole moment would therefore vanish.[38] In this case, quadrupole transitions would be much more probable than dipole transitions, and it therefore seemed reasonable to assume, Taylor and Mott argued, that a gamma ray would have either a quadrupole or a dipole field associated with it.

The internal-conversion coefficients for RaC, calculated for quadrupole and dipole radiation, respectively, are shown in Figure 7.5, where for comparison Ellis and Aston's experimental values are marked with crosses. The agreement was as good as could be expected, and Taylor and Mott concluded that "although the experimental results are not yet sufficiently advanced to give a definite proof to the theory, they are certainly consistent with it, and the theory does give a reasonable explanation of the fact that the internal conversion coefficient is not a smooth function of the frequency ν."[39]

Thus, with a few exceptions, it looked as though internal conversion could be regarded as an internal photoelectric effect. However, Taylor and Mott stated in a

[37] H.M. Taylor and N.F. Mott, "A Theory of the Internal Conversion of γ-Rays," *Proc. Roy. Soc. A138* (1932), 665–695.

[38] The same argument followed from the nuclear model recently put forward by Heisenberg, as Taylor and Mott proved in their paper. Later on, however, it was realized that their proof was based on a misconception of Heisenberg's theory, and accordingly the present ideas of nuclear constitution lent no support to the view of a vanishing dipole moment. See H.R. Hulme, N.F. Mott, Frank Oppenheimer, and H.M. Taylor, "The Internal Conversion Coefficient for γ-Rays," *Proc. Roy. Soc. A155* (1936), 315–330.

[39] Taylor and Mott, "Theory" (note 37), pp. 667–669.

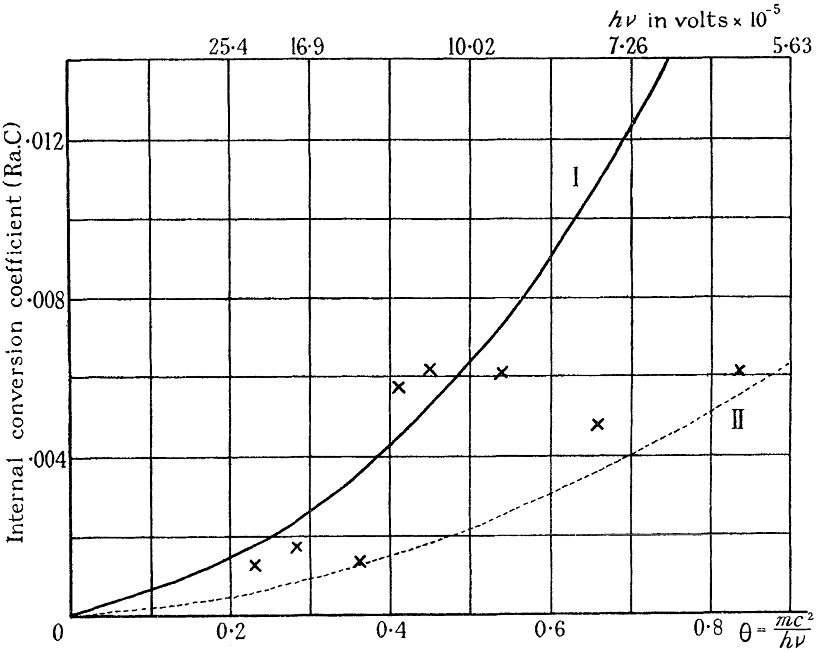

Figure 7.5: The internal-conversion coefficients for the K shell of RaC. Curve I is for quadrupole radiation as calculated by Taylor and Mott. Curve II is for dipole radiation as calculated by Hulme. The points marked with crosses are the experimental values determined by Ellis and Aston. Source: Taylor and Mott, "Theory" (note 37), p. 668.

subsequent paper that this view could not be upheld.[40] Until then, the probability p had been calculated on the assumption that it was the same as it would be if the K electrons were not present, and this was not correct. The presence of the K electrons perturbed the nucleus, and accordingly p would be larger than if the electrons were not there. The emission of an extra-nuclear electron must then be thought of as partly due to the absorption of a gamma ray and partly to the direct interaction between the nucleus and the electron, but the two effects were impossible to distinguish.

Taylor and Mott calculated the probability per unit time that a gamma-ray quantum was emitted from the complete system consisting of the nucleus coupled to the two K electrons, and they found that it was nearly always the same as it

[40]H.M. Taylor and N.F. Mott, "The Internal Conversion of γ-Rays. – II," *Proc. Roy. Soc. A142* (1933), 215–236. See also N.F. Mott, "Théorie de l'absorption interne des rayons γ," *Annales de l'Institut Henri Poincaré 4* (1933), 207–220.

would be if the K electrons were not present. Consequently, the internal-conversion coefficients calculated were not, as was thought, a measure of the ratio

$$\frac{\text{Number of } \beta \text{ particles emitted per unit time}}{\text{Number of } \gamma \text{ quanta leaving the nucleus per unit time}}.$$

They proved, instead, to be a measure of the ratio

$$\frac{\text{Number of } \beta \text{ particles emitted per unit time}}{\text{Number of } \gamma \text{ quanta escaping from the system per unit time}},$$

which was the quantity directly measured in experiments. The term "internal-conversion coefficient" had thus lost its meaning.

As to agreement with experiment, Taylor and Mott's modifications of the theory were of little importance. In the radium B region the agreement was slightly poorer than before, and for radium C the difference was negligible. Theoretically, however, their modifications were essential, and the idea that the atom had to be considered as a whole, put forward by Smekal already in 1922, had finally found full recognition.

The theory developed by Hulme and by Taylor and Mott met with success in explaining the observed values of the internal-conversion coefficients for RaC and ThC. It was not, however, in good agreement with observations on softer gamma rays, such as those emitted from RaB and ThB, where the experimental results were about twice as large as the theoretical values.[41] How could this discrepancy be explained?

In early 1934, Taylor and James Fisk pointed out that electrical dipole and quadrupole fields were not the only possible fields.[42] Electric multipoles of higher order were improbable, as had been shown, but there were still those associated with oscillating *magnetic* multipoles. In the optical region, the intensity of magnetic radiation was small compared to electric radiation, but this was not necessarily the case in radiation from nuclei. From Taylor and Fisk's calculations, it appeared that the radiation from magnetic multipoles gave rise to coefficients up to fifty times as large as those from the previously considered electric multipole radiation. A calculation of the relative amount of magnetic and electric radiation to be expected from an actual nucleus required a more complete theory of the nucleus, but their result was promising. They concluded:

> If then, in the actual nuclear radiation, a small amount of this "magnetic" radiation accompanies what is in the main an electric dipole or quadripole [*sic*] radiation, it is quite possible that the calculated internal conversion coefficient may be raised sufficiently above its value

[41] As to the experimental values for ThB and ThC, see C.D. Ellis and N.F. Mott, "The Internal Conversion of the γ-Rays and Nuclear Level Systems of the Thorium B and C Bodies," *Proc. Roy. Soc. A139* (1933), 369–379.

[42] J.B. Fisk and H.M. Taylor, "The Internal Conversion of γ-Rays," *Proc. Roy. Soc. A146* (1934), 178–181.

> for pure electric radiation to agree with the experimental values in the RaB region, while still leaving unaffected the agreement in the RaC region.[43]

By a curious coincidence, then, this essential feature of the quantum theory of internal conversion was in place almost at the same time as Enrico Fermi advanced his theory of beta decay. The year 1934 therefore may be viewed as a landmark in the history of the beta spectrum. An exciting epoch had ended and a new one was ready to begin.

[43] *Ibid.*, p. 179.

Summary and Conclusion

One of the most challenging problems in modern physics was to understand the complex beta spectrum. This book has traced the experimental and theoretical developments that led physicists to its interpretation and explanation, beginning in the early days of radioactivity and ending in 1934 with Enrico Fermi's theory of beta decay and the Taylor–Mott theory of internal conversion. There were three main phases:

(1) the years until around 1920, when the complexity of the beta spectrum was recognized;

(2) the decade 1920–1929, when the focus was on the interpretation of the composite spectrum; and

(3) the years from the late 1920s until 1934, which saw the theoretical explanations of beta decay and internal conversion.

The recognition of the complexity of the beta spectrum in the pre-war years was brought about above all by new developments in experimental method and technique. Apart from some early magnetic-deflection experiments, the first investigations of beta particles were absorption measurements carried out in Germany, which led to a strong belief in a close connection between exponential absorption and homogeneity of the particles. This assumption proved successful; it led to the discovery of several new radioactive sources. Otto Hahn and Lise Meitner therefore found it natural to put forward the following hypothesis: a single radioactive substance emits only one sort of homogeneous particle radiation, alpha or beta. As we have seen, Meitner clung to this analogy between alpha and beta rays for many years, and it strongly guided her efforts in the post-war years to interpret the beta spectrum.

In Britain, the situation was different. Joseph John Thomson, and also William Henry Bragg, questioned the exponential absorption law, and at the suggestion of Ernest Rutherford, William Wilson took up the problem in 1909. He deflected beta rays in a magnetic field before they entered the absorbing material and found that homogeneous particles were absorbed linearly. Furthermore, he argued convincingly that beta rays, in agreement with this linear-absorption law, suffered a decrease in velocity in their passage through matter.

C. Jensen et al. (eds.), *Controversy and Consensus: Nuclear Beta Decay 1911-1934*

Hahn and Meitner ignored Rutherford's appeal for open-mindedness. Though not very convincingly, they defended their single-radiation hypothesis tooth and nail. Eventually they realized, however, that to say anything about the velocity of beta particles, they had to turn to magnetic-deflection experiments, and they started investigating beta rays with an apparatus like the one used previously by Rutherford on alpha rays. Their initial experiments seemed to corroborate their hypothesis, but gradually they recognized that it could not be upheld. In general, beta spectra seemed to be line spectra, and only RaE appeared anomalous from this point of view.

Around 1911, a new experimental technique was introduced by Jean Danysz. He deflected beta rays 180 degrees before they hit a photographic plate and in this way obtained very clear pictures. Rutherford and Harold Robinson improved this method by taking advantage of the semicircular focusing effect, and during the next few years the number of observed lines in the RaB + C spectrum increased from 9 to 64. Qualitatively, however, these investigations changed nothing: beta spectra still seemed to be line spectra.

In 1913, James Chadwick went to Berlin to carry out scattering experiments. Instead of photographic plates, he used Hans Geiger's recently developed point counter to detect beta particles, and obtained a most surprising result: the RaB + C spectrum proved to be essentially continuous; the line spectrum superimposed upon it was of relatively weak intensity. Physicists had been led astray previously by the high sensitivity of photographic plates to even small changes in intensity.

The recognition of the composite beta spectrum was thus reached through a three-stage development in experimental technique: first, absorption measurements; then magnetic-deflection experiments with a photographic plate as detector; and, finally, magnetic-deflection experiments with a point counter as detector.

Although the first two decades of beta-spectrum research were dominated by experimental work, essential theoretical advances were also made. In early 1913, Niels Bohr argued that the disintegration electrons must come from the nucleus, and in 1914 Rutherford suggested that the entire line spectrum was produced by an internal conversion of gamma rays, a view that was taken over by Charles Drummond Ellis in the 1920s. Finally, as a result of experimental progress, Rutherford realized that the wavelengths of gamma rays are in general much shorter than previously assumed. Consequently, he abandoned his idea that gamma rays consist of wave trains and argued instead that gamma-ray energies are transferred to beta particles in single and not multiple quanta. This opened up the possibility of studying gamma rays by means of the magnetic beta line-spectrum.

The next period spans the 1920s and began with attempts in Berlin and Cambridge to interpret the composite beta spectrum. In the wake of these attempts, a long and tenacious controversy developed between Meitner and Ellis, which was not settled definitely until 1929. Actually, there were two controversial questions: the temporal order of beta-ray and gamma-ray emission, and the origin of the continuous beta-ray spectrum.

In Cambridge, the goal was to establish a physical model of the nucleus, and for that purpose information on gamma rays was considered to be of particular interest. Rutherford's work during the war had revealed that such information could be obtained by studying beta rays, and the great interest in Cambridge immediately after the war in beta line-spectra is to be seen in this perspective. A question of high importance, therefore, was whether the beta particles constituting the line spectrum were emitted from the mother or the daughter nucleus. Ellis trusted unreservedly a pre-war investigation carried out by Rutherford and Edward N. da C. Andrade, which led him to adopt the view that gamma rays are emitted before a beta decay.

Meitner was not much concerned with speculations on nuclear structure; she only inferred that in accordance with quantum theory, gamma rays must be emitted as a result of a discontinuous change of the nucleus after an alpha or beta decay. She thus held the view that gamma rays are emitted after a beta decay. This controversy was settled experimentally in 1925. During a relatively short span of time, several investigations, in Cambridge and in Berlin, proved that Meitner was right; and Ellis immediately conceded the point.

The second controversial question concerned the origin of the continuous beta spectrum and was intimately tied to the interpretation of the beta line-spectrum. Both Ellis's and Meitner's views rested on Rutherford's hypothesis of internal conversion. Meitner's old attachment to an analogy between alpha and beta rays was thus revived and served as a guiding principle for her interpretation of the experimental results – namely, that the line of highest beta-ray velocity consists of nuclear electrons, the remaining ones being due to internal conversion. Ellis, in contrast to Meitner, lacked experience in beta-spectrum research and was guided by his nestor, Rutherford. As his first task, he was encouraged to take up again the pre-war experiment of Rutherford, Robinson, and W.F. Rawlinson, and he found that his results were in complete agreement with Rutherford's suggestion of 1914 that the entire line spectrum was due to internal conversion of gamma rays.

As a consequence of this interpretation of the line spectrum, Ellis argued, the continuous part of the spectrum must consist of nuclear electrons; and since its intensity was too high to be explained by secondary effects, its continuity in energy must arise from the nucleus itself, or at least from inside the atomic K shell. Ellis did not go further into the cause of the continuity in energy; but he felt certain that it could be explained in one way or another, for example, by a strong variation in the nuclear field. According to Meitner, the continuous spectrum consisted of nuclear as well as atomic electrons, but its origin must be due to secondary effects. She proposed several possibilities: lack of resolution in Chadwick's experiment, collisions, the nuclear field, a Compton effect. She believed that a combination of these effects could explain the high intensity of the continuous spectrum.

Once again consensus on experimental results resolved the controversy, even though the decision took more than two years. Ellis and William Wooster's *tour de force*, presented in 1927, showed that the amount of energy released in each beta disintegration corresponds to the mean kinetic energy of the beta particles, and not to their maximum energy. Their experiment was considered as crucial in Cambridge, but not on the Continent. Meitner found that any gamma radiation from the nucleus would escape through the calorimeter, and she clung to the hope that such gamma radiation was emitted. Wolfgang Pauli zealously backed her up. He called the Ellis–Wooster experiment "harmless" and fully enjoyed the Indian summer of Meitner's interpretation. In 1929, however, Meitner and Wilhelm Orthmann repeated and confirmed Ellis and Wooster's experiment in an improved version, and even Pauli had to yield to it.

These controversies offer insight into the role of experiment in science. We have seen that the two controversial questions were settled experimentally, even though both Meitner and Ellis were skeptical about each other's experimental results. Decisive for their settlement, however, was experimental consensus. Experiments in both camps led to the same results. Taken together, the two calorimeter experiments may thus be characterized as crucial in the sense that they falsified Meitner's theory, even in the absence of a better theory: Ellis and Wooster's attempt at explaining the experimental results was certainly not taken seriously by Meitner and her Continental colleagues. This circumstance does not seem to be in accord with Imre Lakatos's statement that "no experiment, experimental report, observation statement or well-corroborated low-level falsifying hypothesis alone can lead to falsification. There is no falsification before the emergence of a better theory."[1] However, within the Ellis–Wooster and Meitner–Orthmann framework of relativistic quantum mechanics, the experiments were not crucial. This theory turned the beta spectrum into an anomaly, one among several in the late 1920s.

The first three decades of beta-spectrum research were dominated by groups of experimental physicists. Theorists in general were spectators in the development; exceptions like Adolf Smekal and Svein Rosseland joined the discussions without particular success. A turning point occurred in the late 1920s, however, when the beta-decay problem was recognized as an anomaly in a comprehensive complex of problems, which were centered around relativistic quantum physics, and which attracted the community of theoretical physicists.

In this connection, the continuous part of the beta-ray spectrum became of primary concern. Several attempts were made to explain it, and their diversity reflected an atmosphere of desperation. Johann Kudar, for example, suggested that the continuity in energy stemmed from a variation in the rest mass of the electron as a result of its compression in volume. Cambridge physicists proposed that the principle of identity was not universally valid. But of paramount significance were the two opposing hypotheses advanced by Bohr and Pauli.

Bohr's idea was that a new theory was required in which energy and momentum were not strictly conserved, even statistically. His hypothesis was, I think, a consequence of the great importance he attached to the correspondence principle.

[1] I. Lakatos, "Falsification and the Methodology of Scientific Research Programmes," in I. Lakatos and A. Musgrave (eds.), *Criticism and the Growth of Knowledge* (Cambridge University Press, 1970), 91–195, p. 119.

This mysterious guiding principle, which was like a "magic wand"[2] in Bohr's hand, could not be used within distances less than the radius of the electron. Inside this domain, he emphasized, one should be prepared for surprises.

With his legendary sarcasm, Pauli argued strongly against Bohr's view. His main objection was that it entailed a lack of symmetry between energy and charge. In a talk in Moscow in 1937, he expressed the point as follows:

> For one thing, I believe that the analogy between the conservation laws of energy and electrical charge has a profound significance and can act as a reliable guide. If energy conservation is abandoned, the law of charge conservation can hardly be maintained, and this latter law has until now never led to difficulties. For this reason, I have refused from the very beginning to believe in any violation of energy conservation.[3]

Pauli's words were clear manifestations of what he considered to be of the greatest importance in physics: symmetry principles and conservation laws. In 1964, Ralph de Laer Kronig and Victor Weisskopf wrote:

> For Pauli, the invariants in physics were the symbols of ultimate truth which must be attained by penetrating through the accidental details of things. The search for symmetry and general validity transcended the limits of physics in Pauli's work; it penetrated his thinking and striving throughout all phases of his life, in all fields of philosophy and psychology.[4]

In his anxious efforts to hold on to conservation of energy and momentum, Pauli introduced a new particle into physics, the neutrino. Bohr did not much care for it. It resolved only some of the problems, and he was more ambitious than that. He looked for a solution embracing all of the problems, and, at least until 1932, several theoretical physicists joined him in his quest.

After the miraculous year of 1932, the situation changed. While Bohr was very reluctant to accept the existence of the positron, Pauli was enthusiastic about its discovery, which was precisely what he required to convince himself completely of the correctness of his neutrino hypothesis. If both an electron and a positron exist, he argued, it was not so fantastic to assume the existence of a neutral particle comprising both together. This view soon had to be abandoned, but by then Pauli believed firmly in his neutrino, and in late 1933 Werner Heisenberg also became convinced of its significance.

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[2] A. Sommerfeld, *Atomic Structure and Spectral Lines* (London: Methuen, 1923), p. 583.

[3] W. Pauli, "Die Erhaltungssätze in der Relativitätstheorie und in der Kernphysik," translated into Russian in *Modern Problems of Physical Chemistry and Chemical Technology*, Vol. 2 (Moscow, 1938), and retranslated into German in *Wolfgang Pauli: Das Gewissen der Physik*, C.P. Enz and K.v. Meyenn (eds.) (Braunschweig: Friedr. Vieweg & Sohn, 1988), pp. 439–453, quotation on p. 449.

[4] R. Kronig and V.F. Weisskopf, "Preface," in *Collected Scientific Papers by Wolfgang Pauli*, R. Kronig and V.F. Weisskopf (eds.), Vol. 1 (New York: Wiley, 1964), pp. v–ix, on p. viii. The paragraph containing the quotation is reprinted as an introduction to Pauli, "Erhaltungssätze" (note 3), p. 439.

Bohr stuck to his guns, however. He expressed great sympathy for the Beck–Sitte theory of beta decay, in which energy was not conserved, and took a very reticent attitude towards Fermi's theory when it was announced. Fermi's theory was victorious and Bohr came to accept the neutrino, but only in 1936 did he publicly give his full support to energy conservation.

Lakatos has claimed that Pauli "remained conservative"[5] when he advanced his neutrino hypothesis in order to save the principle of conservation of energy. This characterization is not convincing, I think. In 1930, proposing the existence of a new particle was a very radical step, probably even more so than Bohr's suggestion of non-conservation of energy. Contrary to some other physicists, both Bohr and Pauli were quite aware that they were facing a profound problem, and that radical proposals were necessary to solve it. The difference between their proposals was a manifestation of their different ways of doing physics. Bohr was thinking in correspondence-principle terms, and was much concerned with the limitations of this magic wand. For Pauli, symmetry principles and conservation laws took precedence over everything else.

Experimental work concentrated on the question of upper limits in beta spectra, and several experiments between 1929 and 1932 pointed to their existence. In 1931, Fernand Terroux introduced a flaw into the fabric by announcing that he found a long tail instead of a sharp upper limit. But he was shown to be wrong; in 1932, Ellis informed Bohr that the spectral end points undoubtedly existed. Bernice Sargent's curves indicated that they were highly significant characteristics of the spectra, and by considering the ThC branching problem, Ellis and Nevill Mott showed that energy conservation was valid for the maximum beta-particle energies.

The beta line-spectrum was to some extent overshadowed by these intense discussions about the continuous spectrum, but especially in Cambridge much effort was expended to obtain a better understanding of the origin of the line spectrum. In 1926, Smekal again took up his earlier idea of unified atoms, and raised philosophical as well as experimental objections against the photoelectric-effect explanation of internal conversion. Until 1927, Smekal's view received no support, but in the late 1920s, the Cambridge group found evidence for a direct coupling between the nucleus and the external electronic system, with the result that the long-lasting hypothesis that only a radiative coupling existed had to be abandoned. Theorists then had to find a quantum-mechanical explanation of internal conversion, and Ralph Fowler, Henry Rainsford Hulme, Henry Taylor, Mott and James Fisk demonstrated that Cambridge was not exclusively a center of experimental physics. Around 1934, they developed the essential features of a new quantum-mechanical theory of internal conversion, and with Fermi's concurrent enunciation of his new theory of beta decay, an epoch in the history of the beta spectrum had come to an end.

[5]Lakatos, "Falsification" (note 1), p. 169.

Name Index

Page numbers referring to a page on which the person is mentioned in a footnote (but not in the main text) are followed by the letter "n".
A page number followed by the letter "c" refers to a page where the person is mentioned only in the caption for a figure or a table.