THE RADIOACTIVISTS

Community, Controversy and the Rise of Nuclear Physics

Jeffrey Alan Hughes

Corpus Christi College
University of Cambridge

A Dissertation submitted in partial fulfilment of the requirements for the degree of Doctor of Philosophy

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Summary

This dissertation is a social and technical history of radioactivity research in the 1920s, and of the emergence of nuclear physics in the 1930s. It is concerned with the production, circulation and certification of practice and knowledge in these fields of scientific research.

By 1914, the study of radioactivity was confined to a few centres – Paris, Berlin, Manchester and Vienna – possessing relatively large quantities of radium. The politics and organisation of this relatively closed network were irrevocably altered by the First World War. The election of Ernest Rutherford to the Cavendish Chair of Experimental Physics at Cambridge in 1919 brought radioactivity research, and a programme of Imperial physics, to the Cavendish Laboratory. Rutherford's programme of research, based on his speculative nuclear model of the atom (1911), sought to map the internal topography of the atomic nucleus by means of scintillation counting experiments. Rutherford's work on artificial disintegration, combined with F.W. Aston's elucidation of the isotopes of the light elements by means of the mass-spectrograph, brought about a profound change in physicists' and chemists' views of atomic architecture.

In the early 1920s, as laboratories in Europe recovered from the war, the work of the Cavendish Laboratory was unchallenged. During the 1920s, as other laboratories entered the field of nuclear research, however, a series of controversies brought into question the reliability of the scintillation technique and the integrity of all experimental results based upon it. The foundational data yielded by the mass-spectrograph, too, were contested, occasioning a 'crisis of certitude' in radioactivity research, and prompting a redistribution of trust into alternative sources of experimental evidence – electronic (Geiger) counters and cloud chambers. The crediting of these techniques (which proved to be as problematic as those they ostensibly replaced) opened up new kinds of problems to experimental investigation.

In virtue of the new kinds of skills now required in the laboratory, a re-definition of the investigative community accompanied technical innovation. In the wake of a prolonged controversy between Cambridge and Vienna, a conference was convened at the Cavendish Laboratory in 1928, as a direct result of which researchers in several other European laboratories (including Maurice de Broglie and the Joliot-Curies in Paris, Bothe in Berlin and Pose at Halle) entered the field of nuclear research, multiplying the number of sites at which the new techniques were deployed. Theoretical physicists like George Gamow, too, began to apply the novel methods of wave mechanics to nuclear problems, gradually transforming the bounds of the possible and the plausible in nuclear research.

A reconfigured network of embodied practice gradually crystallised around the development of these material and conceptual technologies. This network - including laboratories and researchers in Cambridge, Paris, Berlin, Rome, Vienna, New York, Berkeley and Washington D.C. - embodied the emergent discipline of 'nuclear physics.' Chadwick's disclosure of the 'neutron' in 1932 using the new experimental techniques ratified this social and technical re-alignment. The emergence of Nuclear Physics as a recognised discipline by 1934 was thus the simultaneous certification of a new regime of practice, a new socio-political network of laboratories and a new ontology.

I hereby declare that except as specified in appropriate notes this dissertation is the result of my own independent research, and includes nothing which is the outcome of work done in collaboration. I also declare that it does not exceed 100,000 words in length.

Acknowledgements

During the four years in which this thesis has been in the making, I have derived much benefit from conversations with the following colleagues and friends, whose goodwill and support I am glad to acknowledge: Finn Aaserud, Richard Ashcroft, William and Liza Ashworth, Michael Bravo, John Bradley, Andrew Brown, Michael Dennis, David Dewhirst, Isobel Falconer, Neil Gascoigne, "Neil Gascoigne", Yves Gingras, Graeme Gooday, Arne Hessenbruch, Gill Hudson, Rob Iliffe, Myles Jackson, Frank James, Adrian Johns, Thomas Kaiserfeld, Sue Morgan, Jack Morrell, Iwan Rhys Morus, Richard Noakes, Dominique Pestre, Andy Pickering, Xavier Roqué, Otto Sibum, Tsvete Sofronieva, Richard Staley, Roger Stuewer, Jennifer Tucker, Andy Warwick, Thomas John Williams, Alison Winter, and colleagues in the Department of History and Philosophy of Science, Cambridge. In the late stages of writing, I also benefited greatly from conversations with Prof. Ernest Pollard, who was kind enough to share with me his memories and impressions of the Cavendish Laboratory in the late 1920s.

I have received financial support from the T.J. Jones Memorial Fund, the British Academy, Corpus Christi College, Cambridge, and my family. I am grateful to them all.

Inevitably, in writing a dissertation which seeks to characterise and delineate the emergence of an international physics research community, I have relied to an unusually large extent upon archives and libraries in several countries. For their patience and good humour, I must first thank Godfrey Waller and the staff of the Manuscripts Reading Room, Cambridge University Library, and the staff of the Whipple Library, Cambridge. They helped make my research not only easy but enjoyable. For access to archival and other source material, I am grateful to Jim Bennett, Curator of the Whipple Museum, Cambridge; John Deakin, Secretary of the Cavendish Laboratory, Cambridge; Montague Cohen of the Rutherford Museum, McGill University, Montreal; Phoebe Chartrand of the McGill University Archives; Monique Bordry of the Institut Curie, Paris; Robin Rider, Bancroft Library, Berkeley; Valerie Phillips and Ann Barrett of the 1851 Exhibition and Imperial College archives respectively; Tony Simcock of the Oxford Museum for the History of Science; and Sheila Edwards of the Royal Society Library. Librarians and archivists at the following institutions have also been most helpful and accommodating: Churchill College Archives Centre, Cambridge; the infamous Room 132 (Modern Manuscripts), Bodleian Library, Oxford; Birmingham University Library; Leeds University Library; Nuffield College, Oxford; University College, London; the Royal Institution; the Institution of Electrical Engineers, London; Trinity College, Cambridge; Columbia University Library, New York; Center for the History of Physics, A.I.P., New York; Rockefeller Archives Centre, New York; the Library of Congress, Washington D.C.; Queen's University, Kingston, Ontario.

To Simon Schaffer, whose charge I have been for the past five years, I owe a special debt of thanks for inspiration, encouragement, support and patience. I could not have wished for a better supervisor.

For friendship and for their unfailing faith in me, I at last have the opportunity to thank Joanie Kennedy and my parents, to whom this thesis is dedicated.

More has been written of what the experimental physicist has discovered than of how he has discovered it. Because he has changed the technique of living by his intense curiosity to find out about obscure things, many of his discoveries have become common knowledge. But his method of experimental discovery, how he works and thinks, is much less known.

Blackett (1933), 67.

Solutions to the problem of knowledge are solutions to the problem of social order.

Shapin and Schaffer (1985), 332.

What makes a laboratory difficult to understand is not what is presently going on in it, but what *has been* going on in it and in other labs.

Latour (1987), 91.

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Footnote Convention and Abbreviations

Footnote references to published materials are given in the form [Author(s) (date), page number(s)]. Full references can be found in the appropriate section of the bibliography. Occasionally, where circumstances demand (mainly in the case of published articles with no named author), more complete footnotes are given. I have made extensive use of unpublished correspondence and papers. In addition, therefore, the following abbreviations are used in footnotes to refer to unpublished, archival and standard reference materials:

1. Repositories

BLB	Bancroft Library, University of California, Berkeley.
BLO	Bodleian Library, Oxford.
CCAC	Churchill College Archives Centre, Churchill College, Cambridge.
CUL	Cambridge University Library.
ICL	Imperial College, London.
IEE	Institution of Electrical Engineers, London.
RI	Royal Institution of Great Britain, London.
RSL	Royal Society, London.
TCC	Wren Library, Trinity College, Cambridge.
UCL	University College, London.
WML	Whipple Museum Library, Department of History and Philosophy of Science, Cambridge

2. Manuscript Collections, Collected Works and Standard Works of Reference

AHQP	Archive for the History of Quantum Physics, available on microfilm, Science Museum Library, London, and elsewhere.
ASP	Arthur Schuster papers, RSL.
ASPL	Arthur Smithells papers, Leeds University Library.
BDP	Bergen Davis papers, Columbia University Library, New York.
BFSP	B.F. Schonland papers, CUL.
BSC	Bohr Scientific Correspondence, AHQP.
BWSP	B.W. Sargent papers, Queen's University, Kingston, Ontario.
CAV	Cavendish Laboratory archives, CUL.
CPPK	*Collected Papers of P.L. Kapitza*, ed. D. ter Haar, 3 volumes (Oxford: Pergamon, 1964).
CPR	*The Collected Papers of Lord Rutherford of Nelson*, ed. J. Chadwick, 3 volumes (London: George Allen and Unwin, 1962-1965).
CSIC	Cambridge Scientific Instrument Company archives, CUL.
CWJC	*Frédéric et Irène Joliot-Curie: Oeuvres Scientifiques Complètes* (Paris: Presses Universitaires de France, 1961).
CWNB	*Niels Bohr: Collected Works*, ed. L. Rosenfeld *et al.*, 9 volumes (Amsterdam: North-Holland Publishing Company, 1972-1986).
DSB	*Dictionary of Scientific Biography*, 18 volumes (New York: Charles Scribner's and Sons, 1970-1992).
EOLP	E.O. Lawrence papers, BLB.
ESBP	E.S. Bieler papers, McGill University Archives, Montreal.
FALP	F.A. Lindemann papers, Nuffield College, Oxford.
FSP	Frederick Soddy papers, BLO.
FWAP	F.W. Aston papers, CUL.
GBPP	George B. Pegram papers, Columbia University Library, New York.
GEHP	George E. Hale papers, California Institute of Technology, Pasadena.
GNLP	Gilbert N. Lewis papers, BLB.
GPTP	G.P. Thomson papers, TCC.
IFJCP	Irène and Frédéric Joliot-Curie papers, Laboratoire Curie, Paris.

ILP	Irving Langmuir papers, Library of Congress, Washington D.C.
JAGP	J.A. Gray papers, Queen's University, Kingston, Ontario.
JCP	James Chadwick papers, CCAC.
JDCP	J.D. Cockcroft papers, CCAC.
JJTP	J.J. Thomson papers, CUL.
JLP	Joseph Larmor papers, RSL.
LMP	Lise Meitner papers, CCAC.
MATP	Merle A. Tuve papers, Library of Congress, Washington D.C.
NFP	Norman Feather papers, CCAC.
OLP	Oliver Lodge papers, UCL.
OLPB	Oliver Lodge papers, Birmingham University Library.
PMSBP	P.M.S. Blackett papers, RSL.
RAMP	R. A. Millikan papers, California Institute of Technology, Pasadena.
RP	Rutherford papers, CUL.
RTBP	R.T. Birge papers, BLB.
SMP	Stefan Meyer papers, Institut für Radiumforschung, Vienna.
UA	Cambridge University Archives, CUL.
WBLP	W.B. Lewis papers, Queen's University, Kingston, Ontario.
WHBP	W.H. Bragg papers, RI.

CHAPTER ONE

INTRODUCTION

The Scientific Consequences of the Peace

> Radioactivity, discovered in 1896, came of age during the war, but it was hardly due to the war that the event passed almost unnoticed, though the war interfered with radioactive researches as with all philosophical study. Radioactivity never enjoyed real popularity even in its infancy. There is too little in the radioactive phenomena to catch the popular fancy ... [Even] the most striking phenomena, the scintillation visible in the spinthariscope, can only be watched by one person at a time. ... [Yet] the band of workers in the new field swelled, order was established in the apparent chaos, radioactive phenomena were found to occur with the regularity of astronomical events, and at present radioactivity is generally accepted, though as an obscure oddity rather than perhaps as anything likely to play a part in matters technical and general.[1]

1. Introduction

In November 1918, Ernest Rutherford, Langworthy Professor of Physics at Manchester University, wrote to his colleague Niels Bohr to mark the end of the Great War. "Well," he began, "you can imagine everybody is very pleased and relieved at the dramatic issue of the great struggle and the country has been celebrating the great event in a <decorous> way the past week. ... The whole nation is in a hopeful and energetic state and turning their attention to the problems of peace."[2] News of the armistice a week earlier had brought scenes of unrestrained rejoicing to the streets of Manchester, as to other English towns and cities. Work ceased in shops and offices; crowds surged through the streets; omnibuses were seized for the celebrations. And there were few in the rejoicing crowds who did not hope that the suffering and loss of the war would be redeemed by the construction of a 'society fit for heroes' - a sensibility expressed by Woodrow Wilson who told Mancunians in a December 1918 speech of his conviction that "men are beginning to see, not perhaps

1 "Radioactivity," *The Electrician* **107** (1919), 673-674, 673.
2 Rutherford to Bohr, 17 November 1918, RP.

the golden age, but an age which at any rate is brightening from decade to decade, and will lead us some time to an elevation from which we can see the things for which the heart of mankind is longing."[3]

With a view to the reconstruction of science for the post-war world, Rutherford was already turning his attention to the future development of his Manchester laboratory. Before the war, he had created one of England's foremost research centres in physics at Manchester, and now intended to restore the department to its former glory.[4] He began to lay his plans to "make Physics boom" within days of the armistice, making preparations for the expected influx of new students and telling Bohr that "I am intending to suggest numerous alterations in my Dept. to make it a research centre not only for Modern Physics but General Physics."[5] But this optimism about the future was tempered by a complaint about his own recent researches: "I wish I had you here," he told Bohr, "to discuss the meaning of some of my results on collisions of nuclei. I have got some rather startling results, I think, but it is a heavy and long business getting certain proofs of my deductions."[6]

During the war, although his school had been scattered by the demands of conscription and duty to country,[7] Rutherford had managed to continue his experiments in "the odd half day"[8] between extended periods of work for the Admiralty on submarine detection. Following up some observations made in his laboratory by Ernest Marsden in 1913,[9]

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[3] Quoted in Mowat (1955), 1. For the armistice and British society, see Mowat (1955), 1-78; Taylor (1965), 155-157; Marwick (1967)[1965], 277-287; Peele and Cook (eds.)(1975); Marks (1976); Morgan (1979). See also the excellent study by Hynes (1990), esp. 254-266.

[4] In the absence of any wholly adequate account of Rutherford at Manchester, see Marsden (1950, 1954); Birks (ed.)(1962); Andrade (1963); Feather (1963, 1977); Kay (1963); Wood (1963); Burcham (1964); Badash (ed.)(1969), 157 ff.; Bunge and Shea (eds.)(1979); Wilson (1983), 268-405. See also Heilbron (1968, 1974). For the development of radioactivity before the Great War, see Malley (1976); Jensen (1990), 1-92.

[5] Rutherford to Bohr, 17 November 1918, RP, emphases in original. Rutherford was attempting to woo Bohr to a position in Manchester. For further comments on the importance of the laboratory in post-war science organisation, see Rutherford to Hale, 13 November 1918, GEHP.

[6] Rutherford to Bohr, 17 November 1918, RP.

[7] See, for example, the wartime correspondence from Andrade, Chadwick, Florance, Marsden, Moseley, Pring, Robinson, Wood, all in RP; Rutherford to Bohr, 11 January 1919, RP; Birks (ed.)(1962); Wood (1963); Wilson (1983), 339-385.

[8] Rutherford to Bohr, 9 December 1917, RP.

[9] Marsden (1914). See also Geiger and Marsden (1913); Marsden (1913); Marsden and Lantsberry (1915).

Rutherford and his laboratory steward William Kay were investigating the effects produced by energetic α-particles travelling through gases such as hydrogen and nitrogen. During the course of these experiments, they had obtained evidence to suggest that the atomic nucleus could be disintegrated under bombardment by α-particles from Radium C, a sensational result if it could be proved correct.[10] Rutherford stressed to Bohr, however, that the delicacy of the experiments made him "still uncertain of the true explanation of the anomalies I obtain."[11]

The experiments involved the counting of scintillations, tiny flashes of light produced on a zinc sulphide screen by the impact of charged particles. The scintillation technique had been in use in Rutherford's laboratory for a decade. It was through this method, for example, that the nuclear atom had first been disclosed,[12] and it was by the same means that Rutherford now planned to elucidate the structure of the nucleus. In principle, the technique was straightforward: one need only count scintillations to obtain a clear and direct record of atomic behaviour.[13] In practice, however, deployment of the technique involved the imposition of special conditions to guarantee the trustworthiness and reliability of the results of the "rather difficult and trying task" of systematic scintillation counting.

First, and most important, the experiments had to be carried out in a darkened room, preferably "in a dark room at night."[14] The observers were required "to remain in the dark for about 20 minutes" before beginning to make observations, "so as to allow the eyes to become accustomed to darkness."[15] Because it was "difficult to continue counting for more than two or three minutes at a time, as the eye becomes fatigued,"[16] frequent rest

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10 For comments on these experiments, see Rutherford to Bohr, 9 December 1917; Rutherford to Bohr, 30 June 1918, RP; Kay (1963); Trenn (1974c); Wilson (1983), 386-405, esp. 392 ff.; Stuewer (1986a), 323-324.
11 Rutherford to Bohr, 30 June 1918, RP.
12 See Rutherford (1914a, 1914c).
13 Makower and Geiger (1912), 48-50; Marsden (1913); Rutherford (1913), 133-135.
14 Rutherford and Geiger (1908a)[CPR **2**, 104].
15 Makower and Geiger (1912), 49.
16 Rutherford and Geiger (1908a)[CPR **2**, 105]. Responses clearly varied between observers. If Rutherford himself found scintillation counting an extremely trying activity (Rutherford to Bohr, 17 November 1918, RP), the same was not true for Hans Geiger, who had worked in the Manchester laboratory before the war. Rutherford told Bumstead that "Geiger is a demon at the work of counting scintillations and could count at intervals for a whole night without disturbing his equanimity. I damned vigorously after two minutes and retired from the conflict." See Rutherford to Bumstead, 11 July 1908, RP, also quoted in Wilson (1983), 287.

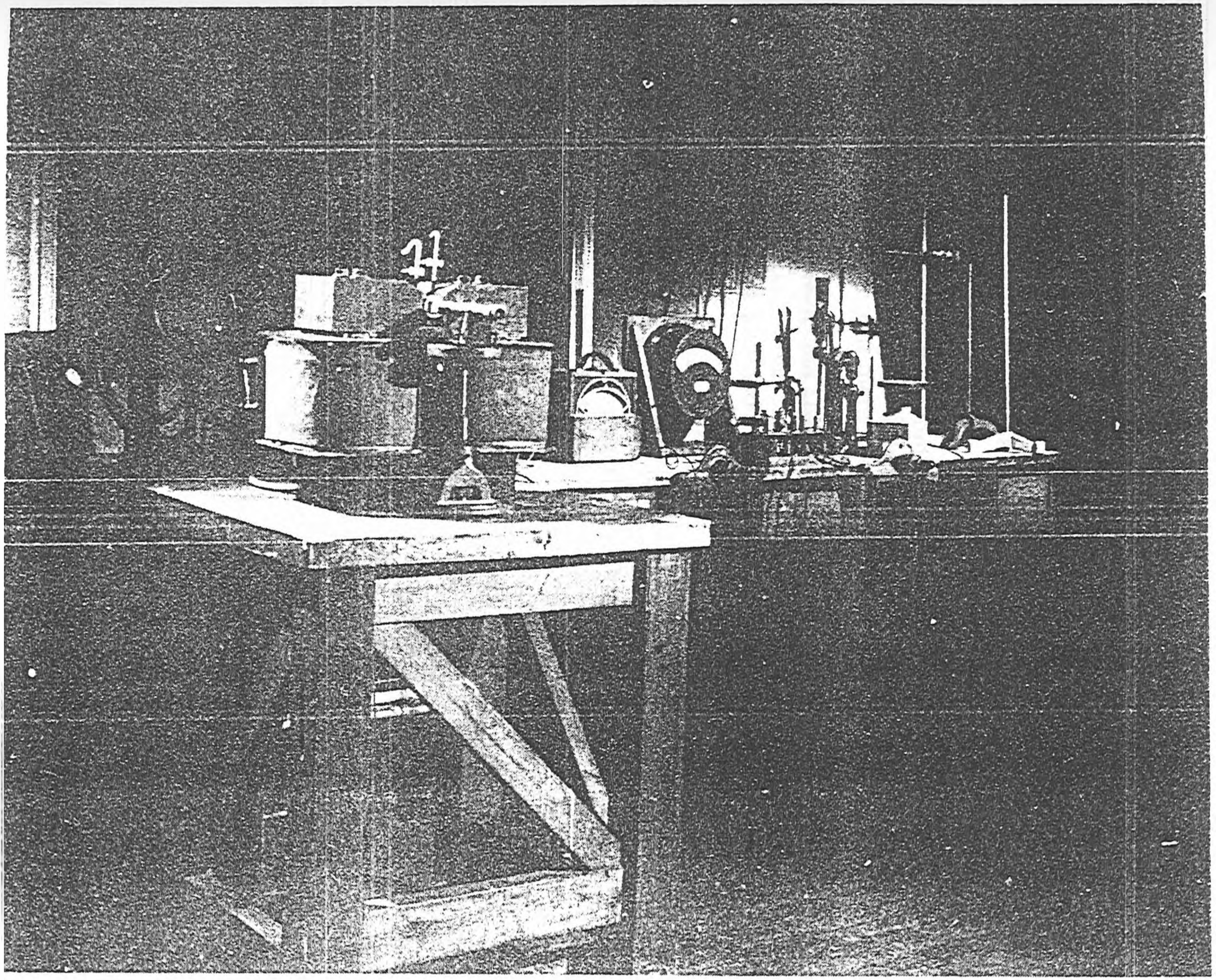

Fig. 1.1 Reconstruction of part of Rutherford's laboratory in Manchester, 1918. On the front table, the microscope for counting scintillations is clearly visible.

Source: Clark (1980).

periods were recommended. And, most importantly, because of the investment of time involved in dark-adaptation of the eye, two workers were required, "one to remove the source of radiation and to make experimental adjustments, and the other to do the counting."[17] With all these precautions and under favourable conditions, reported Rutherford, "counting experiments are quite reliable from day to day."[18]

In terms of the organization of laboratory practice, then, a typical scintillation counting experiment would proceed somewhat as follows:[19]

> There must be two rooms and two workers. One of the rooms must be kept a good deal darker than a photographic dark room, and in it there is one of the men who is to act as the observer ... In this room there is a microscope and scintillation screen, and also whatever may be the set up of radium appropriate to the experiment. In the neighbouring room the other man sits and keeps the record of the count of the scintillations. Thus it may be that what is to be counted is the total number of scintillations made on the screen in two minutes and at the end of it he will write down the number told him by the observer. When the experimental set-up is to be changed, the [observer] must first blind-fold himself, and then the light is put on in the dark room and the other man comes in and resets the instrument. He must turn out the light and go out and shut the door before the observer can uncover his eyes. Altogether it is a laborious business.

Laborious, but worth the effort, for few other techniques could yield comparable information about the internal structure of the atom. So, as he began to reconstruct his department in the winter of 1918, Rutherford continued with the scintillation experiments, hoping to obtain "certain proofs" of his deductions concerning the disintegration of the nucleus (fig. 1.2).[20]

In the first week of March 1919, the scattering experiments were disrupted by the arrival of a letter from Joseph Larmor, effectively inviting Rutherford to apply for the Cavendish Professorship of Experimental Physics in Cambridge. The previous incumbent, J.J.

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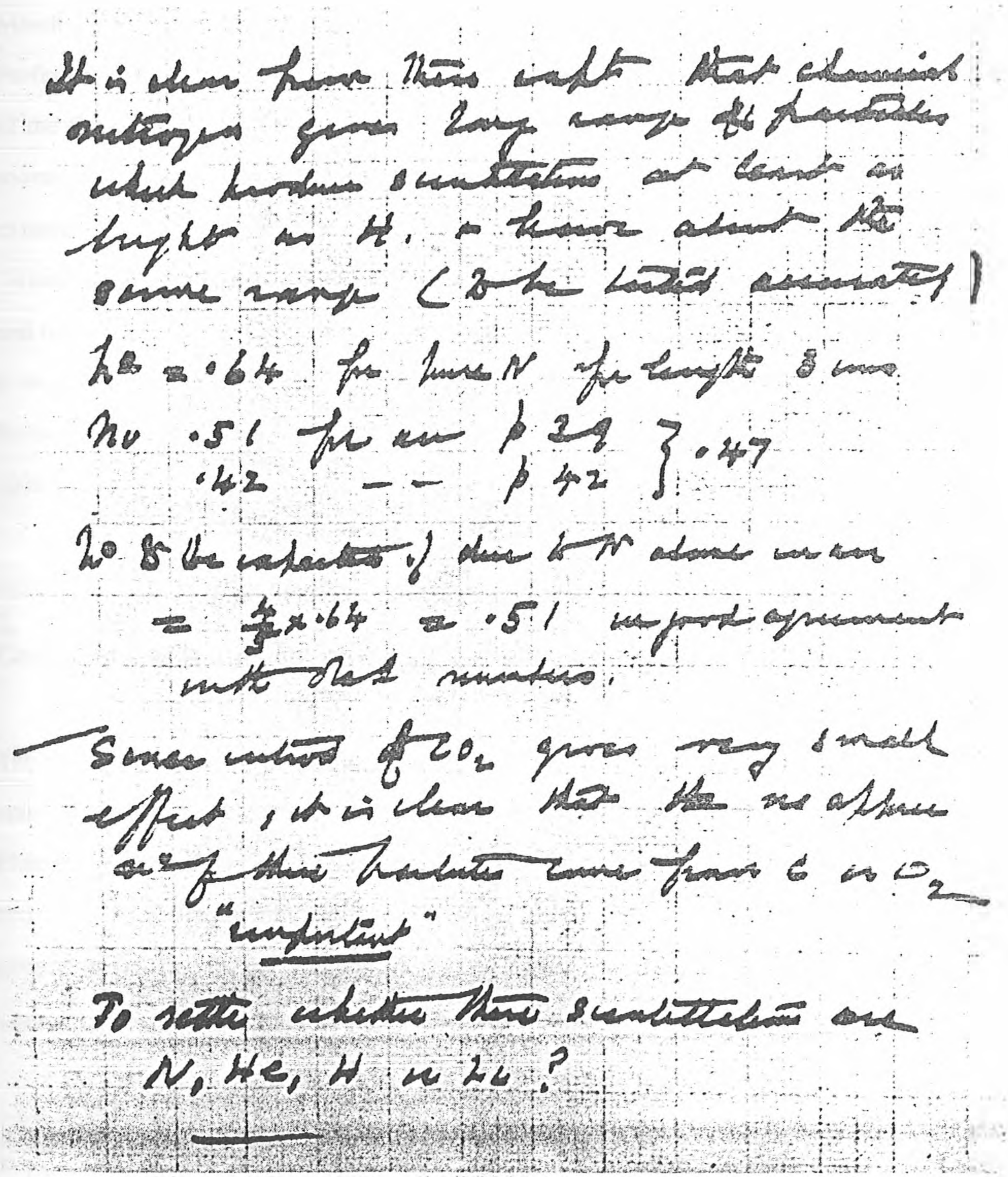
17 Rutherford (1919a)[CPR 2, 551].
18 Rutherford (1919a)[CPR 2, 551].
19 Darwin (1956). For a slight variation, see Rutherford (1919a)[CPR 2, 551].
20 Rutherford told A.B. Wood in December 1918 that he was "very anxious to get someone to "scintillate" [i.e count scintillations] for me early in the [new] year ..." See Rutherford to A.B. Wood, 8 December 1918, 25 January 1919, MS Add. 8404, CUL. See also Rutherford's list of 'Projected Researches, Feb. 18 1919,' in NB 19, RP.



It is clear from these expts that chemical nitrogen gives long range particles which produce scintillations at least as bright as H. & have about the same range (to be tested accurately)

Ratio = ·64 for pure N for length 8 cms

No ·51 for air p 29 } ·47
·42 — — p 42

Ratio to be expected if due to N alone in air
= $\frac{4}{5}$ × ·64 = ·51 in good agreement
with Ratio numbers.

— Since extra of CO_2 gives very small effect, it is clear that the no of these of these particles come from C or O_2
"important"

To settle whether these scintillations are
N, He, H or Li ?

Fig. 1.2 Rutherford, laboratory notebook, 9 November 1917: "It is clear from these expts that chemical nitrogen gives long range particles which produce scintillations at least as bright as H & have about the same range (to be tested accurately). … To settle whether these scintillations are N, He, H or Li ?"

Source: NB 22, RP.

Thomson, had been appointed to the Mastership of Trinity College by Lloyd George in March 1918. For a full year he had held that position in conjunction with the Cavendish Professorship. Early in 1919, however, Thomson had been persuaded to relinquish control of the Cavendish Laboratory in return for a personal professorship, the use of a suite of rooms in the Laboratory and a small sum - £300 a year - to pay for "a mechanic and expenses."[21] Larmor, one of the electors to the Cavendish chair, had long marvelled at the 'White Magic' worked by Rutherford in Manchester. He now sought to bring Rutherford and radioactivity to the Cavendish to help make Cambridge "the Imperial University that it is expected to be in the new scheme of things."[22] After much reflection and negotiation, Rutherford applied for the post.[23] The only candidate, he was duly elected on April 2, 1919.[24]

2. A Discipline Redefined: The Radioactivists and the Post-war Geography of Radioactivity

The year 1919 saw sweeping changes in British university departments, as in most other spheres of life. Frederick Lindemann was appointed to the Chair of Experimental Philosophy at the Clarendon Laboratory in Oxford.[25] W.L. Bragg took over at Manchester, and there were new appointments too at Bristol, Leeds and Sheffield.[26] As part of this general reshuffle, in the summer of 1919, Rutherford moved from Manchester to Cambridge. The institution of which he became director, the Cavendish Laboratory, was

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21 Larmor to Rutherford, 4 March 1919, RP; Wilson (1983), 406-408.
22 Larmor to Rutherford, 4 March 1919, RP. For Larmor's views on Rutherford's work at Manchester, see especially Larmor to Rutherford, 22 July 1908, 26 November 1908, RP.
23 Larmor to Rutherford, 9 March 1919, RP. See also Rutherford to Schuster, 15 March 1919, 23 March 1919, ASP; Schuster to Rutherford, 22 March 1919, RP. See also Larmor to Rutherford, 6 March 1919; Miers to Rutherford, 22 March 1919, RP.
24 *Cambridge University Reporter*, 3 April 1919; Wilson (1983), 406-412.
25 On the early development of Lindemann's regime at the Clarendon, see Morrell (1992).
26 Williamson (ed.)(1987), 3. There is no general history either of British university physics or of the British physics community in the early twentieth century comparable to Nye (1986) (on France) or to Kevles (1987) (on the U.S.A.). For some recent work, however, see Keith (1984); Morrell (1992). Also see the foundational study by Moseley (1977).

one of the world's foremost physical laboratories, having worked productively and successfully in the field of gas discharges and ionic physics for over a quarter of a century. Rutherford would turn it to the study of the speciality he had made his own at Manchester: radioactivity.

Even before Rutherford left Manchester, it was clear that the war had had a profound effect on the disciplinary geography and cultural politics of radioactivity. On the eve of the conflict, non-medical radioactivity research had effectively been confined to four European centres: the Institut Curie in Paris;[27] the Institut für Radiumforschung in Vienna;[28] the Kaiser Wilhelm Institute in Berlin, where Otto Hahn and Lise Meitner worked on chemical and physical aspects of the subject;[29] and, of course, Rutherford's laboratory in Manchester. Operating within what one might call a "political economy of radium," these centres each had the material resources - relatively large amounts of radium and other radioactive substances - and the skills necessary to pursue research in radioactivity.[30] And, by 1913, many of the researchers who worked in them had come to regard themselves as part of a distinct disciplinary community, often referring to themselves only half-jokingly as the 'radioactive people' or the 'radioactivists.'[31]

For this community, the outbreak of war had meant the immediate cancellation of an

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27 The Curie laboratory has typically been seen through the persona of Marie Curie herself. For works in this genre, see Reid (1974); Giroud (1986); Pycior (1987); Pflaum (1989). The most recent such treatment is Pycior (1993). Elements for a reassessment can be found in Weart (1979), 3-36; Pestre (1984); Paul (1985); Shinn (1986). For interesting contemporary comments, see M.S. Leslie to Smithells, 30 November 1909, 10 June 1910, 8 June 1911, MS 416/219/1, MS 416/225/1 and MS 416/234 respectively, ASPL; Marchmay (1921).

28 Meyer (1920a, 1949, 1950); *Festschrift des Institutes für Radiumforschung. Anlässlich seines 40 Jahrigen Bestandes (1910-1950)* (Vienna: Institut für Radiumforschung); Karlik and Schmid (1982). For a contemporary assessment, see also Hevesy to Rutherford, 14 February 1912, 28 February 1913, RP

29 Hahn (1967, 1970); Frisch (1970); Spence (1970); Hahn (ed.)(1979). For recent treatments from biographical and institutional perspectives, see Sime (1986); Kerner (1988); Johnson (1990), 176-177; Rife (1990).

30 For further comments on the various centres and for the politics of this community, see *inter alia* Hevesy to Rutherford, 14 February 1912, 28 February 1913, RP; Boltwood to Rutherford, 12 September [1913], in Badash (ed.)(1969), esp. 285-286; Badash (1978, 1979a). Malley (1976) and the works of Badash and Trenn are the most comprehensive sources for any understanding of radioactivity up to 1914.

31 For such characterisations, see, for example, the extensive correspondence between Rutherford and Boltwood in Badash (ed.)(1969), *passim*.

International Congress on Radioactivity (the third such meeting[32]) which was to have been held in Vienna at the end of June 1915.[33] The peace made matters even more difficult. While Rutherford was quick to re-establish contact with his colleagues in Vienna, Berlin and elsewhere in Europe,[34] the war and its outcome made the pursuit of any kind of research difficult in the defeated counties of the Central Powers.[35] From Rutherford's perspective, however, the difficulties in which foreign workers found themselves had the advantage of leaving a largely uncontested intellectual space for his ongoing experiments. That freedom was enhanced by the absence of another constraint. The retirement in 1912 and death in 1916 of William Ramsay, Rutherford's principal English antagonist and rival before the war, had a profound effect on the geography of the discipline, for it meant that no more 'spurious' radioactivity research would emanate from the chemical laboratories of London's University College, absorbing the time and energy of researchers elsewhere.[36] At

32 For details of the previous gathering, which had been held at Brussels in September 1910, see the programme for the meeting, PA 384, RP; Rutherford to Boltwood, 27 September 1910, in Badash (ed.)(1969), 224-228; Makower (1910).

33 Meyer to Rutherford, 13 June, 20 June 1914; Rutherford to Meyer, 29 June 1914, RP. On the arrangements for the congress, of which Rutherford was to have been President, see Curie to Rutherford [1914], RP. For the effect of the Great War on scientific relations, see Kevles (1971, 1973); Paul (1972); Schroeder-Gudehus (1973, 1978); Badash (1979d); Alter (1980); Wallace (1988).

34 Geiger to Rutherford, 18 May 1919, RP; Rutherford to Geiger, 14 June 1919, in Eve (1939), 271-272; Rutherford to Meyer, 13 January 1920, Meyer to Rutherford, 22 January 1920, RP; Rutherford to Hevesy, 13 January 1920; Hevesy to Rutherford, 26 May 1920, RP. There is no extant correspondence with Hahn for the immediate post-war years.

35 As Arnold Sommerfeld told the Carlsberg Foundation in October 1919 in support of the Bohr Institute in Copenhagen , for example:

> The war burdens and unbearable peace terms have made scientific efforts in Germany impossible for a long time to come. Previously, Germany's numerous universities and institutes of technology were able to further experimental research with good financial support. Together with Germany, almost the whole European continent has become impoverished. But happy Denmark can step into the breach here. ... The Institute of Mr. Bohr should not only serve the upcoming scientific generation of Denmark, it will also be an international place of work for foreign talent whose own countries are no longer in a position to make available the golden freedom of scientific work.

See Sommerfeld to Carlsberg Foundation, October 1919, BSC; "International Science and the War," *Science* **50** (1919), 453-454. See also Forman (1971) and the penetrating study by Gay (1988).

36 For Ramsay's work on radium and radioactivity, see Tilden (1918), 158-171; Travers (1956), 209-230, 265-273; Trenn (1974c). An illuminating insight into the relationship between Rutherford and Ramsay can be gathered from the extensive correspondence between Rutherford and Boltwood, in Badash (ed.)(1969), *passim*. Among workers in radioactivity, Ramsay's pre-war work in radioactivity was allowed to fade gently into obscurity, though puzzlement about his work persisted in other quarters for some years after his death. See, for example, B. Brauner to A. Smithells, 29 June 1921, MS 416/292, ASPL; Berthoud (1924), 183-185.

the same time, however, there was an obvious heir (some would say pretender) to Ramsay's throne. Elected to the Dr. Lee's Professorship of Inorganic and Physical Chemistry at Oxford in 1919, Frederick Soddy was widely seen as Ramsay's scion, in which capacity he was expected to establish a significant school of radioactivity at Oxford.[37] But Rutherford could deal with Soddy, a former co-worker, in a way that he could not with Ramsay. In England at least, radioactivity looked set to forge ahead in the attack on the fundamental problems of 'artificial transmutation' and the structure of the nucleus.[38] Cambridge would be at the forefront of that attack.

Rutherford quickly wrote up his wartime scattering experiments in four comprehensive papers, a summary of the previous three years' work. His tentative conclusions were published in the *Philosophical Magazine* in June 1919.[39] When Radium C α-particles were passed through nitrogen, he reported, scintillations "about equal in brightness to H scintillations" were observed, which he now took to be "probably atoms of hydrogen or atoms of mass 2."[40] From this, Rutherford inferred "that the nitrogen atom is disintegrated under the intense forces developed in a close collision with a swift α particle, and that the hydrogen atom which is liberated formed a constituent part of the nitrogen nucleus."[41]

Moreover, while most of the light elements had an atomic weight of the form $4n$ or $4n+3$ (n an integer), he noticed, nitrogen's was the only weight expressed by $4n+2$. This difference could be made significant and given physical meaning: "We should anticipate from radioactive data that the nitrogen nucleus consists of three helium nuclei each of atomic mass 4 and either two hydrogen nuclei or one of mass 2."[42] If these H nuclei were regarded as relatively loosely-bound "outriders of the main system of mass 12," he reasoned, "it is to be expected that the α particle would only occasionally approach close

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Contemporary reactions to Ramsay's death and its consequences for radioactivity research may be gauged from Collie (1917); Richards (1917); Moore (1918); Tilden (1918); Harrow (1919); Moureu (1919).

37 Soddy had, of course, worked with Rutherford at Montreal. See Trenn (1971a, 1971b, 1977); Malley (1976); Sinclair (1976); Kauffman (ed.)1986). For Soddy's work after 1919, see Soddy to Rutherford, 21 July 1919, 22 May 1921, 10, 26, 31 August 1922, RP; Soddy (1920a, 1920b).

38 Soddy to Rutherford, 21 July 1919, RP.

39 Rutherford (1919a, 1919b, 1919c, 1919d).

40 Rutherford (1919d)[CPR 3, 589].

41 *ibid.*

42 *ibid.*

enough to the H nucleus to give it the maximum velocity, although in many cases it may give it sufficient energy to break its bond with the central mass."[43] Under these favourable conditions, "the H atom would acquire a high velocity and be shot forward like a free hydrogen atom" to be detected on the scintillation screen.[44] The nucleus had a visualisable, detectable structure. Crucially, the scintillation technique was the means by which that structure could be disclosed.

Moving the experiments from Manchester to Cambridge was a great deal more difficult than Rutherford had supposed it would be. He brought his Manchester radium (a loan from the Vienna Academy of Sciences) with him, of course, but there existed as yet few other resources at the Cavendish to sustain his programme of research. Rutherford had hoped to bring the versatile, keen-eyed and dependable Kay to Cambridge with him, but Kay eventually decided to stay in Manchester for personal reasons.[45] So as to mitigate Rutherford's difficulties in setting up his new line of research at the Cavendish, however, Kay visited Cambridge for a few days in July 1919 to help set up a radium room.[46] While this provided the basic necessity for Rutherford's own requirements, however, it was hardly sufficient to meet the needs of a large, modern physical laboratory of the kind that Rutherford had been engaged to develop. In fact, to establish in Cambridge the kind of programme which he had maintained at Manchester required that almost the entire physical, social and intellectual geography of the Cavendish Laboratory be altered to provide a congenial environment for radioactivity research.

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43 *ibid.*; Stuewer (1985), 239-241; Stuewer (1986a), 322-323.
44 Rutherford (1919f), 575 [not in CPR].
45 Kay (1963), 143-145.
46 Rutherford to A.B. Wood, 22 July 1919, MS Add. 8404, CUL.

3. "Importunate Mendicancy" and "Practical Politics": Radioactivity at the Cavendish Laboratory

Bringing about such change was easier at Cambridge than it might have been elsewhere. During the negotiations to bring about Rutherford's appointment to the Cavendish chair, Larmor had assured him that "[t]he fame of the Cavendish ought to make it easy to beg for funds for extension; and Shipley and our other public men are past masters at importunate mendicancy, and would be available."[47] This was just as well, for in the Michaelmas Term of 1919, Rutherford reported that the Cavendish was having "a busy time - nearly 600 people in the laboratory including 50 Naval Officers & as many research people as we can find room for. I am looking for room to extend the old place; we are very congested under present conditions."[48] Soon after his arrival, therefore, Rutherford fired off a memorandum to Shipley, the University's Vice-Chancellor. Entitled "History and Needs of the Cavendish Laboratory, 1919," it gave a brief (and idealised) history of the laboratory, in which he conjured up the image of a successful and vibrant research centre at the heart of an extensive imperial network:[49]

> While the Cavendish Laboratory had, from the beginning, been a focus of research in physics, the beginning of a definite research school dates from the year 1895 when the University opened its doors wide to advanced students from other Universities. This new step led to a rapid increase in the research students in the Cavendish Laboratory ... The output of important work in physics grew rapidly and the laboratory was soon recognised as the chief centre for research activity in physics ... It may safely be said that a large proportion of physicists now holding important scientific positions in the country and also in our Dominions have at one time worked in the Cavendish Laboratory.

What the Cavendish needed now, according to Rutherford, was more money and more space, both to increase the amount of teaching and the volume of research that could be

47 Larmor to Rutherford, 9 March 1919, RP. See also "Cambridge Needs. Opportunities for a Benefaction. The Scope for Expansion," *The Times*, 19 January 1920, 7.
48 Rutherford to Boltwood, 4 December 1919, in Badash (ed.)(1969), 321-322, 322. For contemporary comment, see "Crowded Cambridge. The Spirit of Hard Work," *The Times*, 24 November 1919, 7.
49 Rutherford, "History and Needs of the Cavendish Laboratory, 1919," PA 362, RP.

done. The classrooms and laboratories were "crowded to excess," and although a small extension was being built to relieve some of the pressure, "still further increase in space and increase in teaching power will be required if the laboratory is to be brought up to date as a great centre of teaching and research in physics."[50]

What this amounted to in terms of hard cash was that the Cavendish needed £75,000 for a major new building, plus a further £125,000 endowment to provide for maintenance, purchase of equipment and salaries of new teaching staff. Rutherford demanded three new, well-equipped teaching laboratories for applied physics, optics and something he referred to as 'General Properties of Matter.' More space would be necessary to accommodate research into the problems of radio signalling, especially "if we are to play our part in the researches required by the state and in providing well-qualified research men for various branches of industry and for the scientific departments of the state."[51] Towards the same end, he also asked for three new University lectureships and a new Professorship in Theoretical Physics, for which he had Bohr in mind.[52]

So much for the physical geography of the laboratory. The social and intellectual geography were slightly more intractable matters. Recent scholarship has drawn attention to the negotiation and conflict over space engendered by conflicting intellectual programmes.[53] For Rutherford, the challenge came from Thomson who had, after all, been Cavendish professor for over thirty years. Even during the negotiations to bring him to Cambridge, Rutherford had worried about Thomson's continuing presence in the laboratory, and had written privately to him to clarify matters relating to organization and the division of supervisory labour.[54] Thomson had made soothing noises, and had arranged a Fellowship at Trinity for Rutherford; yet even after his election, the spectre of an

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50 *ibid.*

51 *ibid.* He also noted that it was now "the declared policy of our government to farm out to the universities the pure researches required for the army, navy and aviation."

52 On Rutherford's attempts to obtain more money and space for the Cavendish, see Rutherford to Boltwood, 19 August 1920, in Badash (ed.)(1969), 329-331, on 329; Wilson (1983), 415-421. None of these demands were ultimately met, although limited funds were available for apparatus.

53 Ophir and Shapin (1991), 15.

54 Rutherford to Thomson, 7 March 1919, RP. See also Rayleigh (1942), 215-216.

omnipresent "J.J." still weighed heavily in Rutherford's mind.[55] After discussions between the two men, therefore, a semi-formal treaty was concluded establishing the terms of a working relationship and guaranteeing certain rights and privileges on each side. A document was drafted, re-drafted (enough "to make a lawyer weep"[56]), and eventually initialled by Thomson and Rutherford.[57] Containing a series of separate clauses under the heads "Space, Apparatus and Students," "Technical Staff and Workshop" and "Finance," the document codified a precise division of laboratory space between Rutherford and Thomson, and a corresponding index of credits and entitlements. Even the disposition of the laboratory servants' time and labour was carefully apportioned in an "internal market" so as to maintain the delicate relationship between the two men and their laboratories.

So, by the end of 1919, two distinct regimes co-existed in the Cavendish Laboratory. In the larger part of the laboratory, Rutherford began to introduce radioactivity research, turning increasing numbers of graduate students over to the subject. In the ground floor rooms allotted to Thomson, a few researchers and Thomson himself continued their work on gas discharges and positive rays. The two regimes were indeed "as independent as if their laboratories were in separate buildings."[58] It is ironic, then, that some of the most innovative and fundamental work of the 1920s - work which was to be absolutely fundamental to Rutherford's emergent programme - was to emerge from the two cellar-like rooms at the far end of J.J. Thomson's laboratory.

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55 As Feather (1940), 157, eloquently (and diplomatically) put it: "[Rutherford] returned to Cambridge not without a certain suspicion of the friendliness of the old order towards the advance of his science, but he was not thereby deterred from the bold statement of his requirements for its efficient prosecution." On the Trinity Fellowship, see Thomson to Rutherford, 22 and 25 April 1919, RP.

56 Eve (1939), 273.

57 "Working arrangement for consideration and revision (Aug. 5 1919)," initialled 15 August. See Thomson to Rutherford, 5 August 1919, RP; Wilson (1983), 412-413.

58 Thomson to Rutherford, 23 March 1919, RP.

4. Re-Writing the Constitution of Matter: Positive Rays, Isotopes and the 'Mass-Spectrograph'

Francis William Aston had first arrived in Cambridge in 1910 to take up a post as assistant to J.J. Thomson.[59] Following his discovery of the negative corpuscle of electricity in 1897, Thomson was then engaged on a series of experiments on positive rays in gas discharges, designed to elucidate the nature and character of positive electricity.[60] In practice, however, the experiments were deeply troublesome. Aston, who had studied physics and chemistry at Birmingham's Mason College with Poynting, Tilden and Frankland, was a self-taught glassblower and an "accomplished, if self-taught experimenter."[61] His arrival led to a conspicuous change of approach in Thomson's experiments. The hallmark of Aston's work was perseverance and the systematic, goal-directed modification of a single experimental design or piece of apparatus in order to obtain definite, stable and reproducible effects.[62] As part of this trial-and-error approach to experimentation, Aston introduced a series of modifications to Thomson's apparatus, allowing characteristic positive rays to be elicited for every atomic species in the discharge bulb.[63] Following this achievement, Thomson began to develop the positive ray technique as a method of chemical analysis, for Thomson saw in the slender photographic traces which could now be obtained "a valuable means of analysing the gases in the tube and determining their atomic weights."[64]

In 1912, using this technique, Aston and Thomson found that neon, the rare gas

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59 Poynting to Thomson, 23 January 1910, JJTP; G.P. Thomson (1946); Hevesy (1948); Feather (1959); Brock (1972). See also Aston to Thomson, 13 June 1907, JJTP.

60 Thomson (1909a, 1909b). For recent discussions of the negative corpuscle and the 'discovery' of the electron, see Malley (1971); Turpin (1980); Falconer (1987); Feffer (1989); Chayut (1991). Falconer (1985), 120-338, gives a clear and comprehensive account of Thomson's early work on gaseous discharges.

61 Feather (1959), 24.

62 For an excellent analysis of Aston's experimental style and the effect of his arrival on Thomson's experiments, see Falconer (1985), 106-113.

63 Thomson, laboratory notebooks, MS Add. 8326, CUL; Thomson (1910a, 1910b, 1911a).

64 Thomson (1910b), 758. On the introduction of the photographic method of detection, see Thomson (1911a). See also Thomson (1911b, 1911c, 1913b, 1913d); Aston (1912). On Thomson and chemistry, see Chayut (1991).

discovered only a decade or so earlier by William Ramsay, yielded not one but two positive ray traces, corresponding to atomic weights of 20 and 22.[65] Thinking that they had discovered a new element, which they christened 'meta-neon,' Aston embarked upon a series of attempts to separate the new element from neon.[66] At the same time, the neon results were appropriated by Frederick Soddy, then Lecturer in Radioactivity at Glasgow University and increasingly a populariser and essayist on matters radioactive, in order to substantiate his hypothesis of "isotopes," chemically indistinguishable radio-elements which were supposed to occupy the same place in the periodic table.[67] Elected Clerk Maxwell Scholar in 1913, Aston set out to investigate the question further by improving the separation of neon and meta-neon. He constructed an elaborate diffusion apparatus, which was in operation early in 1914 (fig. 1.3).[68] In the long summer of that year, it seemed only a matter of time before Aston would be able to make definitive measurements of the masses

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65 On the background to the discovery of the rare gases, see Travers (1956); Hirsh (1981). For contemporary comments, see Tilden (1910, 1916, 1918). It is interesting to note that in his attempts to interpret the disintegration experiments in 1916, Rutherford considered the possibility that he might have discovered a previously unknown light gas. See Wilson (1983), 395-6.

66 Aston, laboratory notebooks 1910-1911; "On the Homogeneity of Atmospheric Neon," unpublished and undated typescript, almost certainly Aston's paper to the British Association for the Advancement of Science, 1913, FWAP. Aston offered a revealing explanation for the decision to interpret the '22' line as a new element. In a note appended to an unpublished paper, he remarked that:

> In 1908 appeared a publication by Mrs Besant and Mr. Leadbeater entitled 'Occult Chemistry.' In this the aut[h]ors, by theosophic means entirely unintelligible to the mere student of physics, claimed to have determined the atomic weights of all the elements known, and several unknown at the time. Among the latter occurs one to which they ascribe an atomic weight of 22.33 (H=1) and which they call 'Meta Neon.'
>
> As this name seems to suit as well as any other, what little we know of the properties of the new gas, I have used it in this paper.

In 1908 Annie Besant, the infamous theosophist, and Charles Leadbeater, missionary, seer and alleged pederast, had published *Occult Chemistry: A Series of Clairvoyant Observations on the Chemical Elements*, which described Leadbeater's attempts to use "astral vision" to study and describe the structure of various kinds of atoms. His speculations, originally published in 1895 in the theosophical journal *Lucifer*, were copiously illustrated with beautiful sketches of atomic architecture. While much of the 'scientific' content of the book came directly or indirectly from William Crookes, Leadbeater made some entirely original observations of his own on the rare gases: "Ten [elements] have been observed, five pairs in which the second member differs but slightly from the first; they are: Neon, Meta-Neon; Argon, Metargon; Krypton, Meta-Krypton; Xenon, Meta-Xenon; Kalon, Meta-Kalon; the last pair and the meta forms are not yet discovered by chemists." See Besant and Leadbeater (1908), 83. On Besant, see Dinnage (1986), Taylor (1992). For Leadbeater, see Tillett (1982). For more general background, see Inglis (1984), 17-66; Oppenheim (1985), 195, 440 n.119; Rose (1986), 4-12.

67 Soddy (1913); Kauffman (1986), esp. 68-70.

68 Aston to Larmor, 22 October 1913, JLP; Aston (1914).

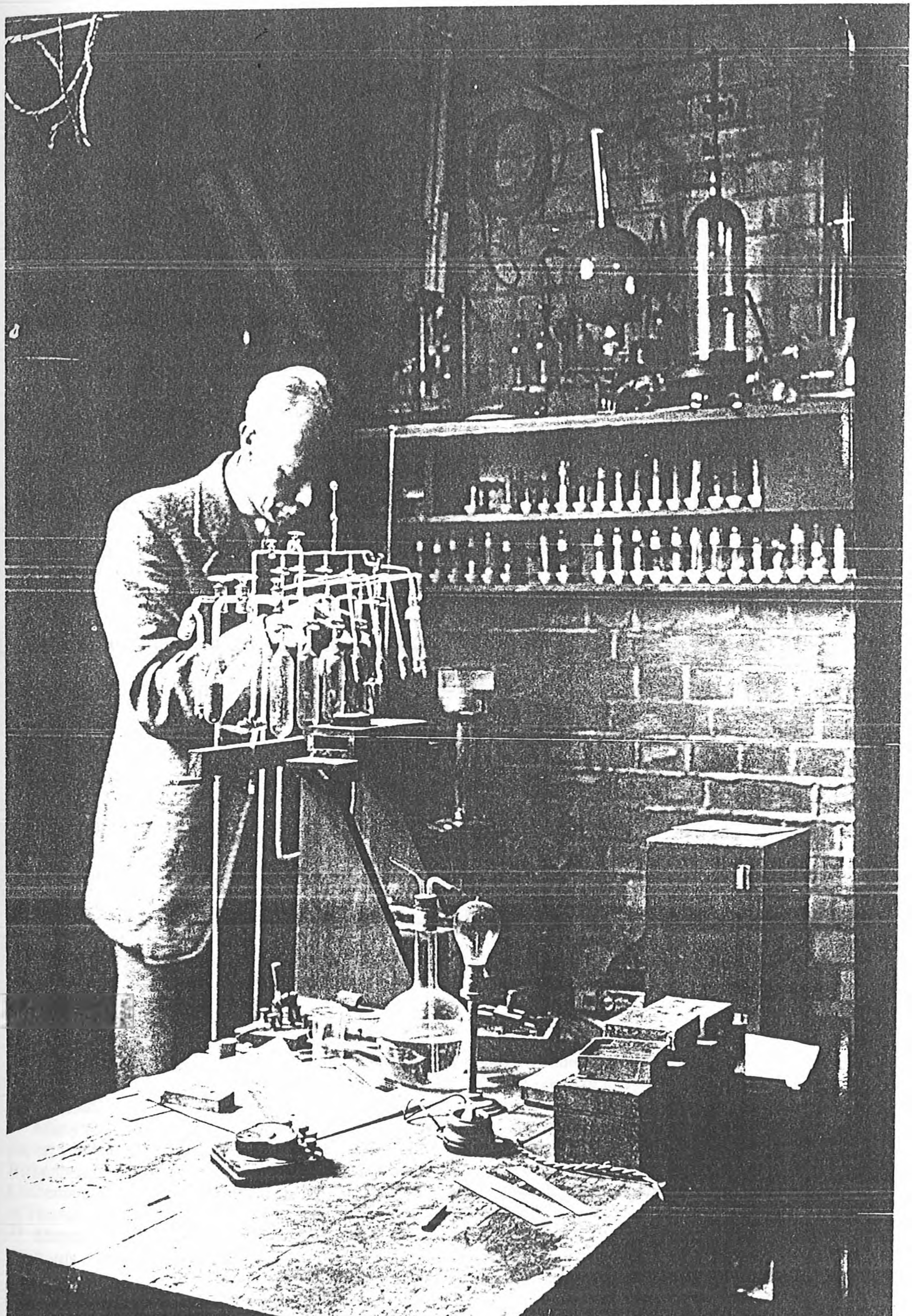

Fig. 1.3 F.W. Aston with diffusion apparatus for separation of neon and meta-neon, Cavendish Laboratory, c.1914.

Source: Cavendish Laboratory.

of the two elements in order to clarify the identity of the heavier element. Such, at least, was the plan.

After the outbreak of war, with research in Cambridge interrupted for the duration, Aston, like several other Cambridge scientists, went to the Royal Aircraft Factory at Farnborough to undertake research for the nascent Royal Air Force.[69] Billetted with other scientists at a house known as Chudleigh Mess, he had frequent scientific discussions with his colleagues, particularly Frederick Lindemann, who had recently returned from Nernst's laboratory in Berlin.[70] By April 1919, when Aston returned to Cambridge, the isotope hypothesis had become significantly more plausible through the work of Hönigschmid and others on the atomic weight of lead from radioactive and non-radioactive sources.[71] Nevertheless, the applicability of the isotope interpretation to the light elements remained debatable.

Resuming his research on neon and meta-neon, Aston began work on a mechanically-operated diffusion apparatus, with the aim of effecting a more complete separation of the two gases.[72] The device proved unsatisfactory, however, so in the summer of 1919, financed by a small sum from the Royal Society's Government Grant Committee, Aston conceived and began to develop another modification of the positive ray apparatus, still in the hope of clarifying the neon issue.[73] With a sufficiently powerful and discriminatory device, he believed he could determine whether the lighter of the two neon parabolas had an atomic weight as high as 20.2 (thereby corresponding to neon's accepted atomic weight),

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69 Crowther (1974), 169-172; Thomson (1946); Hevesy (1948); Feather (1959); Edgerton (1991), 5. For Aston's war-work as a Technical Assistant at Farnborough, see the series of technical reports in FWAP; Aston (1920a).

70 Many of these discussions concerned the plausibility of the isotope hypothesis, and resulted in a joint paper, Lindemann and Aston (1919). On Aston, Lindemann and the Chudleigh Mess, see Harrod (1959), 1-2; Birkenhead (1961), 59-80; Thomson (1964), 137; Wood (1966); Fage (1966); Edgerton (1991), 5. On Lindemann and Nernst, see Birkenhead (1961), 31-58; Mendelssohn (1973), 66-76. For a more recent study of Nernst and the quantum theory of specific heats, see Staley (1992), 221-233.

71 Meyer to Rutherford, 12 March 1915, RP; Richards and Lembert (1914); Richards and Hall (1917); Richards (1919); Hönigschmid (1917, 1918); Hönigschmid and Horovitz (1914); Soddy (1917a, 1917b, 1917c, 1917d). See also "Work of the Vienna Radium Institute," *Scientific American Supplement* **77** (1914), 229, but cf. Freundlich, Neumann and Kaempfer (1914); Shelton (1917). Kauffman (1986) gives a reliable account of the work on the weight of lead and its context. On Richards, see Ihde (1969); Servos (1990), 78-82.

72 Aston to Lindemann, 25 April 1919, FALP.

73 Aston to Lindemann, 14 June 1919, FALP; Lindemann and Aston (1919).

and hence shed light on the character of the two elements. Drawing upon a modified method of positive-ray analysis developed during the war by Arthur J. Dempster, a Canadian postdoctoral physicist at the University of Chicago,[74] Aston designed a sensitive focussing system with separate electric and magnetic fields, which he hoped would serve to settle the neon question beyond dispute.[75] By mid-November he had completed the apparatus (figs. 1.4-1.5) and had made a series of measurements with neon in the discharge tube. Comparing the masses of the two neon lines with established hydrocarbon calibration lines at masses 12, 13, 14, 15 and 16, Aston found, to his surprise, that the neon lines corresponded almost exactly to masses 20.00 and 22.00. Following his conversations with Lindemann and the trend of the previous few years' work in radioactivity, he now unequivocally interpreted meta-neon as an isotope of neon.[76]

A brief note in *Nature* on 27 November announced his preliminary findings.[77] It was followed three weeks later by details of more "remarkable results."[78] After his success with neon, Aston had set out to analyse a few other elements. When chlorine was admitted to the machine, the photographic plates obtained showed "at least two isotopes of atomic weights 35 and 37 ... [whose] ... elemental nature is confirmed by lines corresponding to double charges at 17.50 and 18.50, and [is] further supported by lines coresponding to the compounds HCl at 36 and 38 ..."[79] Carbon and oxygen appeared to be "pure," while mercury, like neon and chlorine, was of "mixed" character. Elated by this burst of revelations, Aston dashed off a letter to Lindemann:[80]

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[74] Dempster (1918). Dempster had graduated from Toronto, and had then won an 1851 Exhibition Scholarship to work with Wien, an expert on positive rays, at Wurzburg. He had managed to leave Germany just in time to escape internment in 1914, and had completed his work in Millikan's laboratory at Chicago, graduating *summa cum laude* in 1916 with a thesis on "The Properties of Slow Canal Rays." See Allison (1952); Dempster file i/370, 1851 Exhibition Archives, ICL.

[75] Aston (1919c, 1919d, 1920c).

[76] There was even the possibility of a third isotope of mass 21, though the line was extremely faint: Aston (1920e), 455.

[77] Aston (1919c). A more comprehensive account was given in Aston (1920e), dated December 1919.

[78] Aston (1919e).

[79] Aston (1919e).

[80] Aston to Lindemann, 13 December 1919, FALP. See also G.P. Thomson to Lindemann, 12 December 1920, FALP.

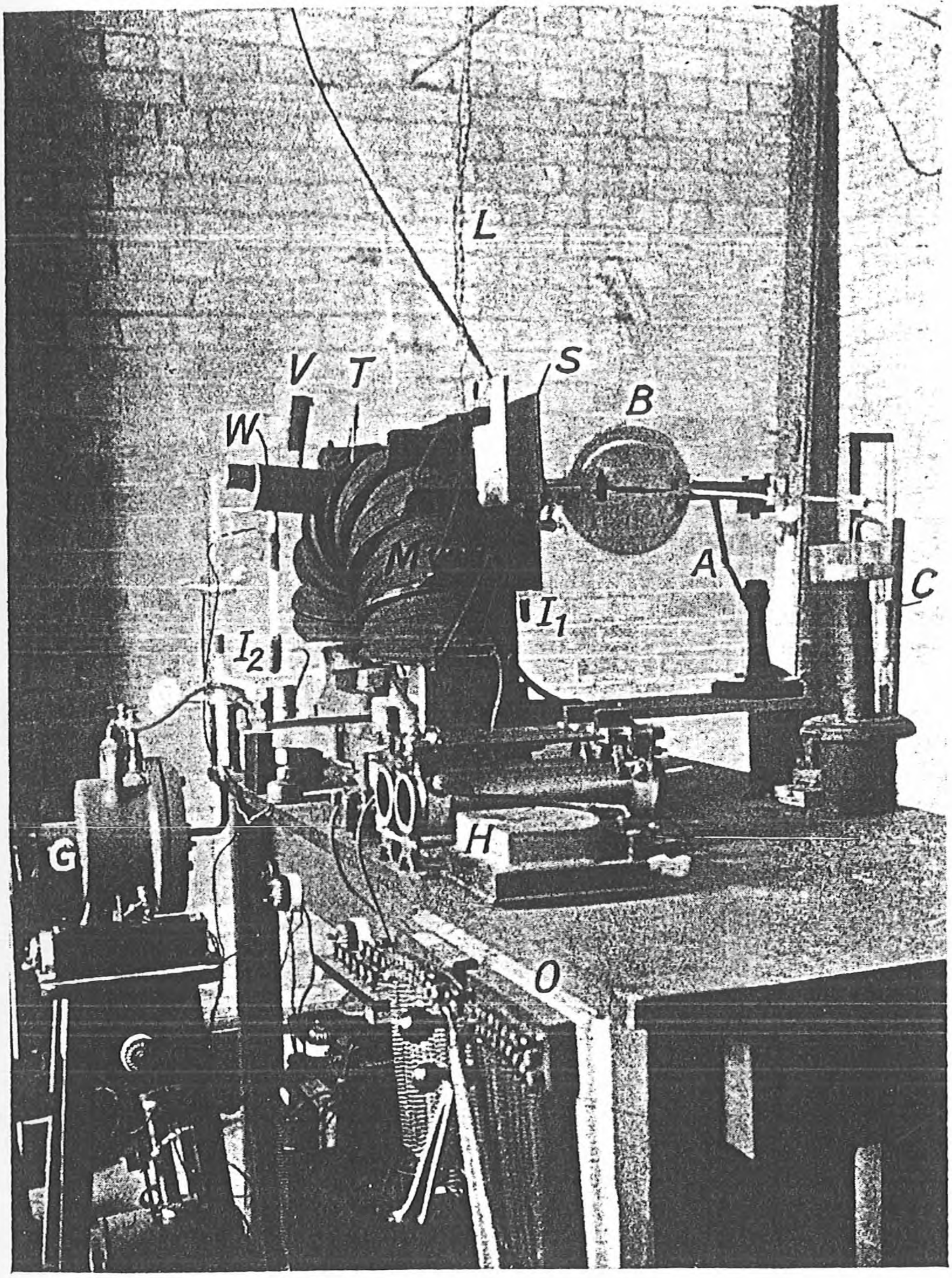

Fig. 1.4 Aston's mass-spectrograph, 1919. B is the discharge tube, M a Du Bois electromagnet, A the anode (connected to an induction coil under the table). G is a Gaede rotating mercury pump, W the camera. Compare fig. 1.5.

Source: Aston (1922a), facing 46.

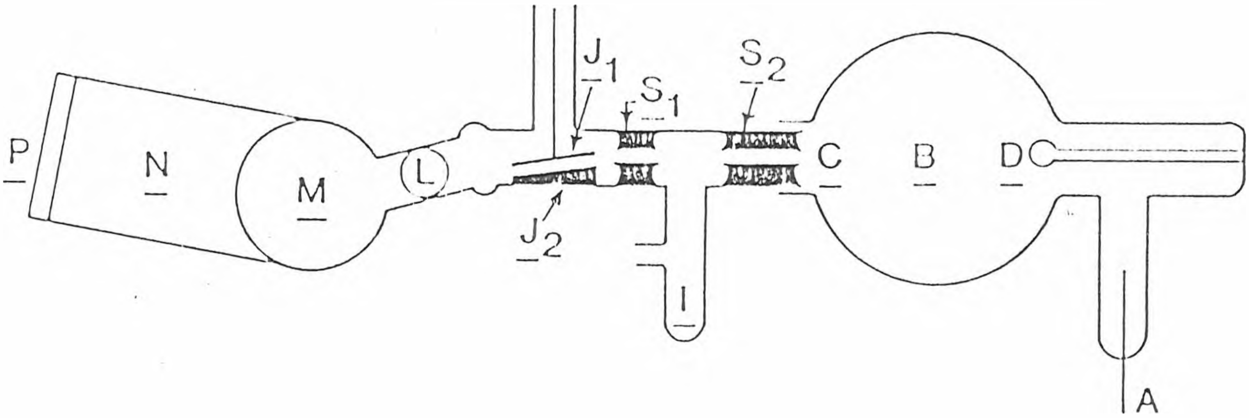

Fig. 1.5 Operating principles of Aston's mass-spectrograph (compare fig. 1.4). Positive rays produced by the discharge in B and collimated by slits S are spread by electrodes J, then magnetically focussed onto photographic plate P by magnetic field M.

Source: Aston (1920f), 612.

> You will probably have seen ere this that my apparatus has been productive of some most astonishing results. I have been living in a state of wild excitement ... By next week I hope *Nature* will publish a letter in which I announce the mixed isotopic nature of Cl and Hg and most important of all the fact that every single mass yet measured with certainty falls exactly on a whole number.

Aston's surprise is delightful - and informative. He had hoped definitively to settle the neon question - that, after all, had been the reason for the construction of the new spectrograph. But the wider applicability of the method came as a genuine revelation to him.

The adoption of the isotope interpretation for neon, the ground for which had been prepared by Soddy,[81] encouraged Aston to extend the concept immediately to the other novel species disclosed by the new machine. In January 1920, helium and hydrogen were submitted to analysis, yielding yet more "very interesting" results. Helium appeared to be a "pure" element of mass 4.00, but hydrogen gave a mass of 1.008 in approximate agreement with that accepted by chemists.[82] By March 1920, a substantial number of the light elements had been successfully analysed.[83] "My apparatus," Aston told Lindemann jubilantly, "is a daisy at isotope production"[84] (fig. 1.6).

In virtue of the revised arrangement of electric and magnetic fields in the modified apparatus, Aston coined the term *mass-spectrograph* for his new device. Although it was a term which he deployed self-consciously and policed carefully in an attempt to distance himself from Thomson's sphere of influence and the older "positive-ray spectrograph," it was not one whose force was immediately apparent to others, who typically saw Aston's system as a 'mere' refinement of the older positive ray method.[85] Aston's attempts to stress

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81 See Bruzzaniti and Robotti (1989).
82 Aston (1920d).
83 Aston (1920f, 1920g, 1920h, 1920i, 1920j, 1920k).
84 Aston to Lindemann, 21 February 1920, FALP.
85 For such a characterisation, see, for example, Loring (1921), 9. A decade later, in an attempt to police the 'proper' usage of the term, Aston was forced to articulate the rationale behind the coinage: "The word [mass-spectrograph] I coined in 1920 to describe an instrument which by its peculiar sequence of electric and magnetic fields eliminated the effect of varying velocity and gave a spectrum dependent on mass alone. Dempster's apparatus, described two years earlier, is essentially an application to the analysis of positive rays of the well-known and widely-used principle of semi-circular focussing. Such an instrument gives a magnetic spectrum which depends upon momentum and not upon mass per se ... *The use of the word mass-spectrograph, unqualified in any way, to an apparatus not using in any manner the principle implied in it,*

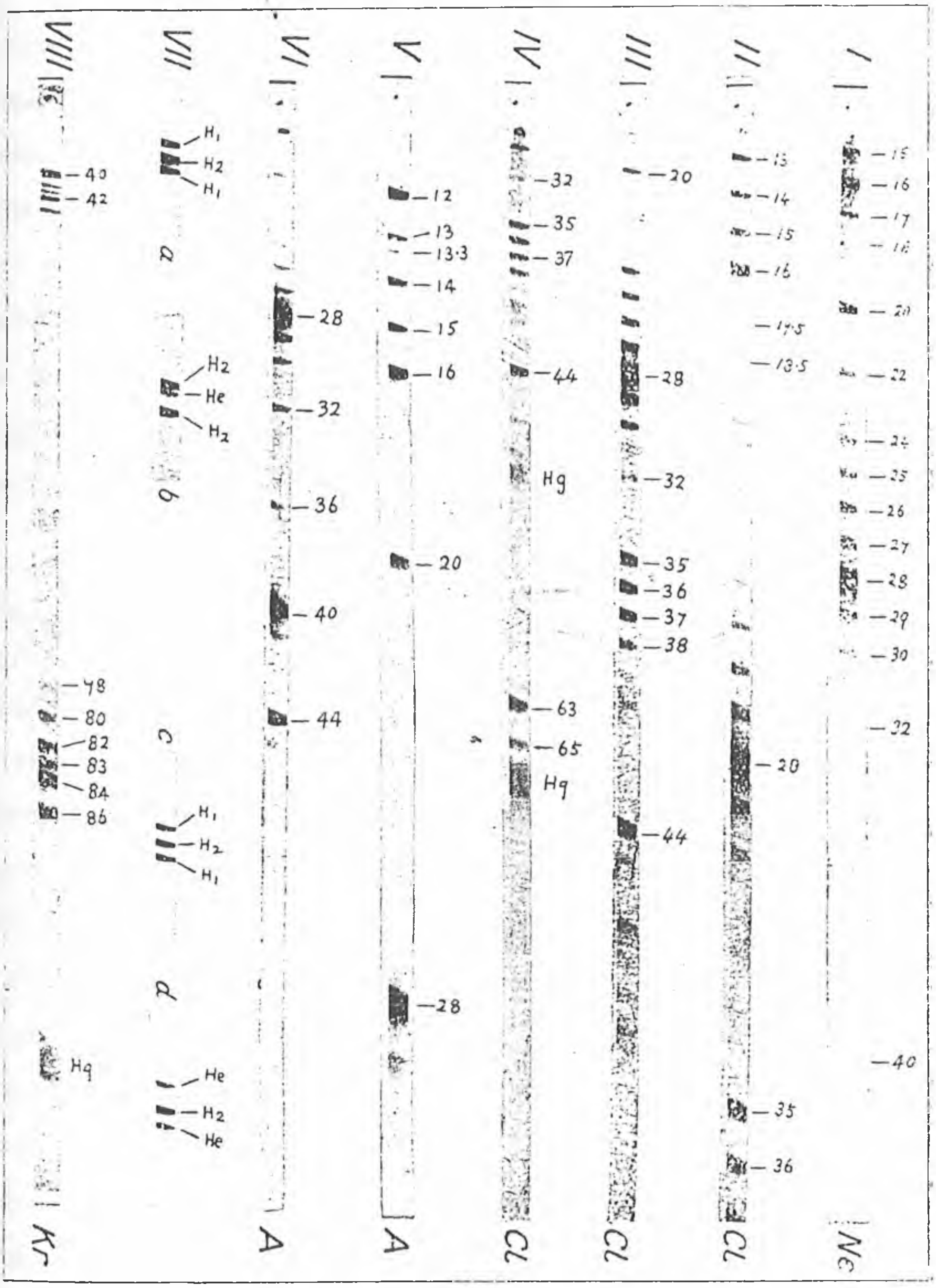

Fig. 1.6 Typical 'mass-spectra,' as produced by Aston's mass-spectrograph. Example I shows the case of neon, with distinct lines at masses 20 and 22, together with a range of hydrocarbon lines.

Source: Aston (1922a), facing 66.

the *differences* between his new technique and the older method were, in part, a response to Thomson's reaction to the flood of new results. Aston told Lindemann, for example, that "Rutherford is most encouraging and so is everyone else except JJT who is apparently extremely annoyed with the whole thing and will hardly look at my negatives at all."[86] As these remarks suggest, Aston could scarcely have wished for a more sympathetic environment in which to pursue this work than Rutherford's Cavendish Laboratory. There was good reason for Rutherford's benevolence. At precisely this moment, he was struggling to interpret his experiments on the 'disintegration' of nitrogen. Aston's work gave him the interpretative resources he needed to do so.

5. "The Nuclear Constitution of Atoms" and a Programme for Research: Rutherford's Bakerian Lecture, 1920

As he sought to make social and intellectual order in the Cavendish Laboratory in the summer and autumn of 1919, Rutherford also sought to make sense of the scintillation experiments and, through them, the nucleus.[87] It had become clear that the geography of the atom's core was to be mapped using the familiar technology of scintillation counting. In the 'disintegration' experiments, however, this delicate and subtle technique left something to be desired, for it was difficult to distinguish between and identify with certainty the various possible kinds of particle arriving at the scintillation screen. Matters were made worse by the fact that Rutherford himself had little patience in the counting of feeble flashes. At Manchester, as we have seen, he had delegated the arduous counting work to Hans Geiger, Marsden and, latterly, Kay. Having failed to persude Kay to come to Cambridge, Rutherford spent some time training another "private assistant" (probably George Crowe) for his work at the Cavendish. But while Crowe acted mainly as

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appears to me a misleading and undesirable practice" (my emphasis): Aston (1931b). See also Dempster to Aston, 3 September 1921, FWAP.

86 Aston to Lindemann, 21 February 1920, FALP.

87 Rutherford laboratory notebooks NB 26-31, RP. For some apposite remarks, see Bohr to Rutherford, 20 October 1919, RC; Stuewer (1986a), 324. The experiments are also discussed briefly by Wilson (1983), 446-447.

preparateur and amanuensis, the actual counting was done by two research students, Yoshio Ishida and the 28-year-old James Chadwick.[88]

Chadwick had trained under Rutherford at Manchester. In 1913 he had taken an 1851 Exhibition Scholarship to work in Geiger's laboratory at the Physikalische-Technische Reischsanstalt in Berlin on the magnetic spectrum of β-rays.[89] Trapped in Germany by the outbreak of war, Chadwick was interned for the duration. Returning to Manchester after the armistice, he moved to Cambridge with Rutherford to complete the tenure of his Scholarship.[90] Towards the end of 1919 he was invited to join the disintegration experiments, principally as a scintillation counter but also as a makeshift chemist. Aside from the difficulties of scintillation counting, the experimental results were constantly under suspicion, for what seemed to be disintegration hydrogen atoms might in fact be due to hydrogen contamination in the apparatus or the target element.[91] Having been trained in Manchester, and therefore grounded in the laboratory practice of radioactivity, Chadwick also served as a useful assistant in the management of the protocols and the general running of the counting experiments. It had been extremely difficult for Rutherford to import radioactivity practice into the Cavendish Laboratory: Chadwick, with his Manchester credentials, made the task a great deal easier.[92]

While the elaborate social organization developed at Manchester, now duplicated at Cambridge, helped to make certitude in the scintillation experiments, there was still the problem that the effects being sought - scintillations due to disintegration protons - were

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88 Rutherford to Bohr, 18 February 1920; Rutherford to Fajans, 17 March 1920, RP. For Crowe, see "George Crowe. Best-known lab. assistant of his time," *New Scientist* 6 (1959), 516-517; Wilson (1983), 569-570. On Ishida, see Millikan to Rutherford, 3 February 1920, RAMP. On Chadwick's early career, see Massey and Feather (1976), 11-15, 47-51; Hendry (1990a); Jensen (1990), 64-70; Brown (forthcoming).

89 Chadwick (1914). For Geiger and the P.T.R. see Cahan (1989), 200-202.

90 Chadwick to E.S. Shaw [Secretary to the Trustees of the Exhibition of 1851], 29 June 1919; Shaw to Chadwick, 2 July 1919, file i/385, 1851 Exhibition Archives, ICL.

91 As Rutherford confided to Boltwood, "I wish I had a live chemist tied up to this work who could guarantee on his life that substances were free from hydrogen. With this little detail set on one side, I believe I could prove very quickly which of the lighter elements give out hydrogen, but it is very difficult to do so without the chemical certainty as the effect is so very small" (Rutherford to Boltwood, 19 August 1920, in Badash (ed.)(1969), 329-331, on 329-330).

92 Chadwick (1969), 35; Brown (forthcoming). A semi-formal training programme for radioactivity research had been developed at Manchester, much of which was codified in a textbook, Makower and Geiger (1912). As I shall show in the next chapter, such a programme was rapidly established at Cambridge. For indications of Rutherford's continuing difficulties in establishing Manchester research practice at the Cavendish Laboratory, see Rutherford to Bohr, 18 February 1920; Rutherford to Fajans, 17 March 1920, RP.

extremely feeble in comparison with other effects which might intrude. In order to make the task of scintillation counting easier, and to aid in the identification of the disintegration products, a new microscope objective of wide aperture was introduced, increasing the brilliancy of the scintillations.[93] By the end of March 1920 Rutherford and his collaborators had obtained what he finally considered to be "conclusive proof that the long-range disintegration products from N = Hs," vindicating his earlier speculation.[94] In the course of his investigations, he had also found numerous short-range particles, which could be obtained from oxygen, as well as nitrogen. Comparing their deflection in a magnetic field with the behaviour of α- and H-particles under the same conditions, he found that the short-range atoms from nitrogen were "not N atoms but particles more bent than α [particles] under [the] same conditions ... [and] ... not bent like H atoms."[95] In fact, the short-range disintegration products behaved like particles with a double positive charge and a mass of 3. He tentatively designated them 'X_3^{++}.' A full fortnight was spent checking this result, for such a particle corresponded to no known atomic species. On April 8th, Rutherford sat down at his desk and brought together in his laboratory notebook three pieces of evidence: data from magnetic field deflections, from collision experiments and from range considerations, all of which relied upon scintillation counting. All these data strongly suggested that the short-range disintegration products indeed had a mass of 3.[96]

This caused immediate interpretative difficulties. While it seemed natural to suppose that the mass-3 atoms were "independent units" in the nuclear structure of both nitrogen and oxygen, their identity remained a mystery. A possible solution to the difficulty soon presented itself, however. In February 1920, Aston had given Cavendish researchers an account of his recent positive ray work. Although the paper met with hostility from J.J. Thomson - Rutherford archly told Bohr that "Aston gave a paper on isotopes in the

[93] Another way in which certainty might be increased was to increase the magnitude of the effect to be observed by increasing the quantity of radium used in the experiments, to which end a number of trials were made "to obtain more powerful sources of radiation with the radium at my command." Such attempts were soon abandoned, however, probably because the β- and γ- rays from the stronger sources tended to confuse rather than clarify the counting experiments. See Rutherford (1920a), 38 [CPR 3, 20].
[94] Rutherford notebook NB 26, RP, p.50.
[95] *ibid.*, p.51 (27 March 1920).
[96] *ibid.*, p.53 (8 April 1920).

laboratory the other day and J.J.T. said he did not believe his results about chlorine," adding "You can imagine that I enjoyed myself thoroughly between the two"! - Rutherford himself had "little doubt that Aston is quite correct," for he found Aston's photographic plates "exceedingly clear and convincing."[97] More to the point, Aston's work opened up a new interpretative possibility for Rutherford's own results, for in the Royal Society's prestigious Bakerian Lecture on 3 June, he unequivocally identified X_3^{++} as an *isotope of helium.*[98]

Rutherford's appropriation of Aston's work to account for the strange new particle solved the immediate difficulty of identifying X_3^{++}, but it raised a series of new problems. In particular, Aston's mass-spectrographic analysis of helium had seemed to indicate that the element was "pure" - that it contained only atoms of mass 4.00.[99] Rutherford solved this apparent contradiction - and saved his interpretation - with the claim that "[s]ince, probably, most of the helium in use is derived, either directly or indirectly, from the transformation of radio-active materials, and these, as far as we know, always give rise to helium of mass 4, the presence of an isotope of helium of mass 3 is not likely to be detected in such sources." Brushing all further difficulties aside, he ventured to speculate on the internal constitution of the new particle and its role in the nuclei of other elements. Although his experiments to date had been "unable to settle whether the new atom has a mass exactly 3" - the element of doubt again - it seemed likely, he told his audience, that "from the analogy with helium we may expect the nucleus of the new atom to consist of three H nuclei and one electron, and to have a mass more nearly 3 than the sum of the individual masses in the free state."[100]

Rutherford followed the analogy through to its logical conclusion:[101]

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[97] Rutherford to Bohr, 18 February 1920, RP. The friction between Aston and Thomson is also evident in Aston to Thomson, 14 March 1920, JJTP.
[98] Rutherford (1920a)[CPR 3, 32-33].
[99] Aston (1920f), 621-622.
[100] Rutherford (1920a), 396 [CPR 3, 34].
[101] *ibid.*

> If we are correct in this assumption it seems very likely that one electron can also bind two H nuclei and possibly also one H nucleus. In the one case this entails the possible existence of an atom of mass nearly 2 carrying one charge, which is to be regarded as an isotope of hydrogen. In the other case it involves the idea of the possible existence of an atom of mass 1 which has zero nucleus charge. Such an atomic structure seems by no means impossible.

Indeed, he continued, such neutral particles seemed "almost necessary" to explain the building up of the heavier elements. Now it bears stressing that all these hypothetical entities and the cosmological conclusions based upon them were speculative isotopic forms based on extrapolation from the results of Aston's mass-spectrographic data. In appropriating Aston's work for his own ends, Rutherford ensured that his experimental and conceptual programme and the mass-spectrograph were inextricably linked.

If Rutherford's adoption of the terminology of isotopes in his account of X_3^{++} gave the particle a legitimate existence, it also made space, as it were, for further speculation about the internal constitution of nuclei. Stressing the preliminary and tentative character of his remarks, Rutherford produced diagrams to illustrate possible structures for the nuclei of lithium, carbon, oxygen and nitrogen, "based on the view that probably in many cases a helium nucleus of mass 4 may be substituted for the corresponding nucleus of mass 3 without seriously interfering with the stability of the system" (fig. 1.7). Because X_3^{++} was observed as a disintegration product from nitrogen and oxygen, Rutherford assumed that it was a constituent of at least these nuclei, where it jostled with hydrogen ions - which he christened *protons* at the Cardiff meeting of the B.A.A.S. in 1920[102] - and electrons. As fig. 1.7 shows, both carbon and oxygen required X_3^{++} as a major constituent, nicely "explaining" the observed disintegrations, while nitrogen, the only element from which disintegration protons had been obtained, obligingly required two protons in its schematic constitution.[103]

Stressing again that the constitutional formulae he had suggested were "purely

102 Rutherford (1920b).
103 Rutherford (1920a), 397-400 [CPR 3, 36-38]. See also Stuewer (1986a), 326.

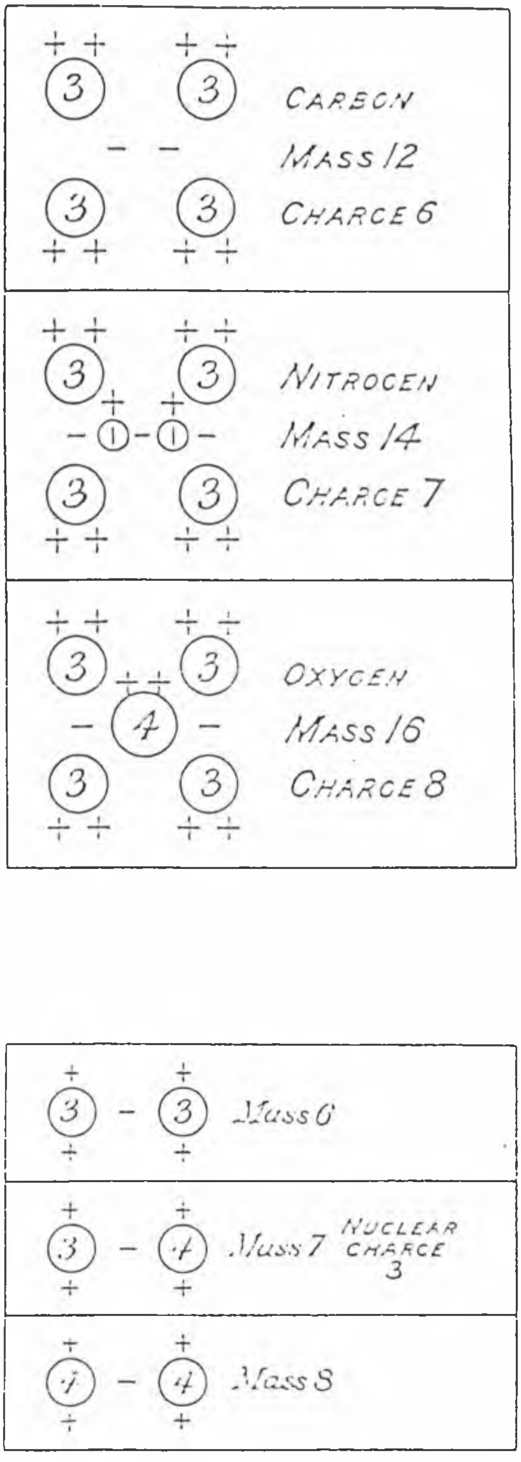

Fig. 1.7 Rutherford's 1920 speculations concerning the nuclear structure of three elements (top), and of the isotopes of lithium (bottom). Note the use of X_3^{++} as a major constituent, together with helium nuclei, protons and electrons.

Source: Rutherford (1920a), 398-399 [CPR 3, 35, 37].

illustrative," with "no importance attached to the particular arrangement employed," Rutherford drew attention in his peroration to some of the problems to be addressed by future research:

> With the exception of a few elements which can exist in the gaseous state, the possible isotopes of the elements have not been settled. When further information is available as to the disintegration of other elements than the two so far examined, and more complete data have been obtained as to the number and mass of the isotopes, it may be possible to deduce approximate rules which may serve as a guide to the mode in which the nuclei are built up from simpler units. ... It is intended to continue experiments, to test whether any evidence can be obtained of disintegration of other light atoms besides nitrogen and oxygen.[104]

As well as the disintegration experiments, in which he thought further progress was "not likely to be rapid," isotopes, the law of force near the nucleus and the possible existence of neutral particles all provided key sites for future research. Rutherford had presented a tantalising series of speculations and a manifesto for research at the Cavendish Laboratory. The enactment of that manifesto and its fate are the subject of this dissertation.

6. Prospectus: The Basis and Structure of the Argument

In 1920 the nuclear atom and Rutherford's nascent conceptions of nuclear structure stood tentatively on the fragile evidence provided by the mass-spectrograph concerning the isotopes of the light elements, and on the results of the delicate scintillation counting experiments. The increasingly strained economic and political situation in Europe made it difficult for laboratories there to join the new field of artificial disintegration. While the laboratories which had worked in radioactivity before the war - the Vienna Institut für Radiumforschung and the laboratories of Curie and of Hahn and Meitner - continued with

104 Rutherford (1920a), 397, 400 [CPR 3, 35, 38].

their traditional lines of research, the Cavendish Laboratory stood alone in its experimental work on artificial disintegration and in its use of the quantitative scintillation technique.

By 1930, that situation had changed radically. At least half a dozen laboratories in Europe and the United States were working on artificial disintegration and the structure and properties of the atomic nucleus. A trans-national nuclear research community was beginning to emerge. Why such a major change in the space of only ten years? And what forces shaped the rise and development of that community and its science? In part, of course, the change reflected the reconstruction of European science and the rehabilitation of international scientific relations in the mid-1920s. But there were other changes - changes which penetrated to the heart of research practice, and which cannot be explained by so simple an account. By 1930, for example, the scintillation method, so essential to Rutherford's disintegration experiments, had been replaced by electrical methods of particle detection (still familiar today in the clicking Geiger counter). Where experiment predominated in 1920, mathematical theory had come to assume a significant, if not a dominant role in nuclear research by 1930. A host of new particles reconfigured the nucleus. And the field of 'artificial disintegration' was becoming 'nuclear physics.' In the space of just a decade, deep structural changes had taken place both in the content of nuclear research and in the form and character of the community producing it. I want to understand those changes and the processes which shaped them.

One powerful and stimulating way of understanding such episodes of socio-technical change is to look at the resources - material, social, discursive and conceptual - used to carry out particular kinds of scientific work, and at the ways in which the character and distribution of those resources (and the knowledge produced with them) vary over place or time.[105] How exactly is it, for example, that one set of instruments and practices ever comes to replace another? This process can best be analysed, I suggest, by examining the complex cultural processes by which instruments (or, for that matter, theories) are attributed the capacity to yield *evidence*, and at the ways in which that capacity shifts from

[105] For such a resource model, see Shapin (1984); Shapin and Schaffer (1985); Warwick (1989). Compare also Callon (1986a, 1986b); Latour (1987); Callon and Law (1989).

one set of instruments and practices to another - by looking, in other words, at what one might call the historical sociology of the agonistic field.[106] To attribute to an instrument the capacity to yield evidence in a particular evidential context[107] is simultaneously to warrant a particular set of observations or data. But it is also to redefine the evidential context itself, for to make such an attribution is also to specify a community of investigators - those with the relevant instrument and its associated practical competences. And it was through just such a shift in evidential context, I shall argue, that 'nuclear physics' emerged as a concerted regime of practice and as a disciplinary category.

Turning to the chapters which follow, then, let me briefly outline the substantive content and overall structure of my argument. In the following chapter, I begin by presenting a detailed analysis of the material, social and intellectual foundations of the Cavendish Laboratory's experimental work on radioactivity and the structure of the nucleus in the early 1920s. Much of this work was based upon the speculations developed in Rutherford's Bakerian Lecture, and relied, as we have already seen, upon the scintillation method. Stressing the details of laboratory organisation, training and discipline, I describe the practical strategies used to make certitude in the scintillation-counting experiments in several fundamental sets of investigations. The important result which emerges from such an analysis is that the confidence implicit in the papers and public pronouncements on artificial disintegration issuing from the Cavendish Laboratory in the early 1920s was belied *within* the laboratory by a nagging scepticism concerning the scintillation technique, as I shall demonstrate through a detailed analysis of contemporary laboratory notebooks. There was, in other words, a strategically maintained and carefully managed disjunction

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106 On the concept of 'agonistic,' see Latour and Woolgar (1986)[1979], *passim*, but esp. 237. My emphasis on evidence and the capacity of instruments (or theories) to yield it is informed by the important work of Pinch (1981, 1985, 1986). I also draw on recent studies of sociotechnical change by Bijker, Hughes and Pinch (eds.)(1987) and Bijker and Law (eds.)(1992). For some of the ramifications of these analytical approaches, see Law (1991b); Star (1991).
107 Pinch (1985); Morus (1988).

between the public, published, *constitutive* facade of confidence and certainty, and the private face of ambivalence, tentativeness and persistent doubt.[108]

While the Cavendish had the field to itself, that disjunction had no serious consequences, and Rutherford's laboratory could regard and represent itself as *the* authority in radioactivity and nuclear research. In 1923, however, Cambridge's hegemony was challenged by the entry of a second laboratory into the field of disintegration research. Deploying the scintillation method, two novice researchers at Vienna's Institut für Radiumforschung began to produce results which directly contradicted those published by Rutherford and Chadwick. A long, increasingly acrimonious (and increasingly *public*) dispute ensued, during the course of which the carefully-established protocols underlying and sustaining the scintillation method, and the competence and reliability of each set of researchers, were systematically brought into question. By 1926, as I show in Chapter Three, the dispute had reached deadlock, each claim from one side being met with a persuasive counter-claim from the other.

Hoping to resolve some of the contested issues, James Chadwick visited Vienna in December 1927. By physically taking control of his opponents' scintillation-counting experiments, and by successfully imposing the counting protocols customarily employed in Cambridge, Chadwick was able to locate the differences between the results obtained in the two laboratories in the Vienna workers' use of 'incorrect' counting procedures. I argue that Chadwick's 'proof' of the artifactuality of the Viennese results was a contingent one, reliying upon mutual agreement about his demonstration of the human agency involved in their production. In an attempt to maintain the disjuncture between public certitude and private doubt, however, Chadwick and his Vienna hosts undertook not to disclose details of Chadwick's visit to researchers elsewhere. This strategic decision, made in the name of scientific propriety, had a series of important, if unintended consequences. It meant, first and foremost, that it was possible for researchers elsewhere to continue to regard the

[108] For the useful distinction between the 'constitutive' and 'contingent' registers of scientific discourse, see Gilbert and Mulkay (1984a, 1984b). For helpful remarks on the creation and maintenance of the distinction between the realms of 'public' and 'private' see Morus (1992b).

Cambridge-Vienna controversy as ongoing and in need of decisive resolution. It also meant that while researchers in Cambridge and Vienna were beginning to develop alternative methods of measurement - cloud chambers and electrical counting methods - workers elsewhere could continue to see the scintillation technique as viable, unproblematic and trustworthy. The balance of the dissertation is devoted to an exploration of those facts and their consequences.

There is now a considerable body of work in the history and sociology of scientific knowledge based upon the study of controversy, for it is in the course of controversy that scientists' implicit assumptions about their apparatus, their interpretative framework and about nature are articulated and made explicit.[109] While such studies usefully draw our attention to the contingency embodied in particular knowledge claims and forms of practice, however, I want to go beyond the conventional controversy study to show how controversies - or the *perception* of controversy - may themselves act as foci for constructive technical innovation and disciplinary development. The Cambridge-Vienna controversy, I argue in Chapters Four and Five, defined the context in which several other groups of researchers entered the field of experimental nuclear research in the late 1920s.[110] These groups entered the field of nuclear research hoping to shed light on *what they saw as* the ongoing controversy between Cambridge and Vienna.

Several of those who sought to resolve the artificial disintegration controversy attempted to do so using electrical counting methods, drawing upon recent innovations in the radio and valve industries and upon widely distributed electronics skills. Researchers like Walther Bothe, Heinz Pose, Frédéric and Irène Joliot-Curie and Maurice, duc de Broglie entered the field of experimental nuclear research, several of them as a direct result of attendance at a conference held at the Cavendish Laboratory in 1928. In many ways,

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109 The *locus classicus* for such studies is Collins (ed.)(1981a). For other important examples in the genre, see Biagioli (1990); Collins (1985); Franks (1981); Gieryn (1992); Harvey (1981); MacKenzie (1981, 1990); Schaffer (1989); Shapin and Schaffer (1985). Shapin (1982), Golinski (1990) and Collins and Pinch (1993) provide a convenient and accessible summary of earlier work.

110 At the same time, the nature of that 'context' was itself open to redescription by the participants. 'Context,' in other words, served as an flexible interpretative resource in terms of which participants could situate themselves, their research and their claims. Compare Sahlins (1987); Callon and Law (1989); Johns (1992).

however, the new electrical counting methods were as problematic as the scintillation method they ostensibly replaced, a state of affairs made clear in 1929 and 1930 in an episode involving researchers in Cambridge and at New York's Columbia University. As I show in Chapter Four, it was *this* controversy, closed by an almost exact repetition of Chadwick's visit to Vienna, which finally made explicit and public the difficulties associated with the scintillation technique, for in this case there was no conspiracy of silence. Almost overnight, the scintillation method became obsolete.

While several groups of experimentalists entered nuclear research in response to their assessment of the state of play in the artificial disintegration controversy, theoretical physicists, too, structured their contributions to the emerging wave-mechanical description of the nucleus in terms of the Cambridge-Vienna debate. In so doing, and in being accepted as serious contributors by established researchers, the newcomers, experimentalists and theoreticians alike, implicitly redefined the character and the social and intellectual geography of the investigative community.[111] And conversely, just as the Cambridge-Vienna controversy defined the context in which theoreticians codified their contributions to the study of the nucleus, it also helped shape nuclear experimentalists' responses to the new developments in mathematical physics, as I show in detail in Chapter Five. In the wake of what I term the 'crisis of certitude' resulting from Chadwick's visit to Vienna, Cambridge experimentalists, previously hostile to theory-led physics, took an unusually accommodating stance towards George Gamow's wave-mechanical description of nuclear processes. The concept of tunnelling, in particular, provided a timely new way of thinking about nuclear phenomena, transforming the nature of the relationship between experimentalists and theoreticians, and opening new avenues of research to the expanding experimental community. One such avenue was the study of the γ-rays excited during the process of artificial disintegration, a line of investigation which became particularly

[111] A term chosen here for its resonance with Fish's notion of 'interpretive community.' See Fish (1980), esp. 338-371. I shall be concerned in Chapter Five to show that the very meanings of the terms 'experimentalist' and 'theoretician' changed during the course of (and perhaps partly as a result of) the episodes to be described in this dissertation.

significant in 1930 and 1931. It was out of such studies that the neutron, a new neutral nuclear particle, emerged.

Postulated by James Chadwick in February 1932 to explain a series of anomalous observations made elsewhere, the neutron was quickly taken up in the laboratories which had entered nuclear research as a result of the Cambridge-Vienna and Cambridge-New York controversies. In that sense, the emergent community was defined by those places where neutrons could be produced, studied and manipulated. Chapter Six, the concluding chapter, is accordingly devoted to a review of the early career of the neutron, for as well as ratifying the emergent network of laboratories, the new particle provided a convenient point of convergence for experimentalists and theoreticians.

The entry of a number of workers into the field of nuclear research, bringing with them new experimental and mathematical techniques, different ideals of physics practice and an outlook unburdened by shared tradition or by commitment to particular sets of past results led to a gradual expansion and redefinition of the nuclear research community and to complex changes in the character of its science. Where researchers in Cambridge and Vienna had seen themselves unambiguously as contributing to the discipline of radioactivity in which they shared a history, the newcomers - experimentalists and theoreticians alike - largely belonged to no such tradition, and sought their disciplinary identity elsewhere. They established that identity and constituted the emergent community in an enterprise which came to be called 'nuclear physics.'

CHAPTER TWO

"ATOM VIRUMQUE"

Radioactivity, the Cavendish Laboratory and the 'Stupendous Possibilities of the Atom'

> Atoms today may be described without exaggeration as familiar objects in any laboratory, and the more lively ones may be said to perform their antics visibly before the eye of idle enquiry at scientific conversaziones. The limit of delicacy indicated by Mr. Kipling in his "apparatus for slicing into fractional millimetres the left eye of the female mosquito" has been incredibly exceeded, the millimetres have become Angstrom units and the subject of the operation the atom itself. The causes of this remarkable advance are to be ascribed in general to the steady and continuous improvement in technical methods.[1]

1. Introduction

In 1920, as peace settled slowly across Europe and reparations were much discussed, attitudes towards science were also beginning to change, not least because of the increasing visibility of the products of science and technology. Radio, electric light and the automobile were beginning to impact on people's lives. Although "it has always been our habit to view political and religious changes as matters of greatest moment," wrote one populist author in the wake of what would become known as the 'chemists' war,' "these are of small consequence compared with the vital revolutions in our mode of living caused by the new technical knowledge. Science is the master of law and is the true agent of social change."[2] To many observers, scientist and non-scientist alike, it was a commonplace that such social change was to be brought about by releasing the energy of the atom.

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1 Aston, original MS introduction to *Isotopes* (1922a), Aston MSS drafts, TCC.
2 Parsons (1921), 374. But cf. "Dubious Benefits of Science," *Literary Digest* **60** (1919), 27. Studies emphasising the wider cultural significance of science and technology after World War 1 include Marwick (1967)[1965], 244 ff.; Maier (1970); Forman (1971, 1973, 1978); K. MacLeod and R. MacLeod (1976); Cameron (1983); Cock (1983); Chant (ed.)(1989); Hughes (1989); Edgerton (1991). Also see Kent (1989).

While by the 1920s it had become a Wellsian cliché to say that the energy in a gramme of radium would propel an ocean liner to the United States and back,[3] such utopian language referred to the energy immanent in *radioactive* elements. And while they continued to grip the popular imagination, such speculations were more than mere tropes in the post-war world. During the war coal had been in such short supply that mines had been worked close to the Front and American citizens were advised to keep their homes chilly. In the early 1920s, strikes in the British coalfields showed the vulnerable dependence of society on one source of energy, and statements to the effect that "the coalfields of the British Isles are approaching exhaustion"[4] were often heard, even more so than they had been a decade earlier.

Equally worrying, however, was the supply of oil, which was beginning to replace coal as the world's chief fuel. Debating the regulation of the oil industry in the early 1920s, some commentators warned that oil shortages might provoke renewed conflict in the future.[5] In December 1919 Sir Oliver Lodge, paterfamilias of British science, lectured to the Royal Society of Arts on "Sources of Power Known and Unknown," invoking the "constitutional energy" of the atom, "the energy which makes it what it is"[6] as a potential source of useful power. In rebuff a few days later, however, 'Studiosus' warned readers of the *Times* not to take Lodge's promisory note too seriously: "It is to be hoped that the wasters of our coal deposits will not be further encouraged by the frequent assertions that we may be on the eve of the discovery of an unlimited supply of energy from radium or other sources. In any practical sense there is about the same chance of our being able to import coal from the moon."[7]

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[3] Soddy (1909); Wells (1914); Soddy (1915), 581; Weart (1988), 8-10.
[4] Lotka (1920), 687. For apposite remarks, see also Allen (1939)[1931], 281-284; Marwick (1967)[1965], 244-258; K. MacLeod and R. MacLeod (1976).
[5] "When fuel gives out," *Literary Digest* (May 1919), 100-103; J.K. Barnes (1921). For contemporary accounts of science and the war, see Soddy (1920b); M. Curie (1921); Moureu (1924). See also Graves (1991)[1940], 91-98; K. MacLeod and R. MacLeod (1976); Hartcup (1988).
[6] Lodge (1919a), 68-69. See also "Disintegration of atoms and atomic energy," *Science Monthly* 9 (1919), 587-589.
[7] 'Studiosus,' "The Waste of Mechanical Power," letter to *The Times*, 13 December 1919, 10.

Yet an unlimited supply of energy was precisely what Soddy, Wells and others were promising. In his public lectures at Glasgow, Soddy had told enthusiastic audiences that "the energy which we require for our very existence and which Nature supplies us with but grudgingly and in none too generous measure for our needs, is in reality locked up in immense stores in the matter all around us, but the power to control and use it is not yet ours."[8] In the post-war world, some thought it about time that scientists acquired that control, for the moral and political well-being of the civilised world depended upon it:[9]

> It is no exaggeration to say that the whole course of human life in the future depends largely upon the development of knowledge concerning the atom. Recently the press of the world has been filled with news about the efforts of the Allies to make Germany pay the debt fixed on her by the Reparations Commission. If some German scientist should happen to discover a way artificially to break up an atom, and if this new found power were to be employed by the Teutons to destroy their conquerors, there would be a new set of victors and a new Treaty to fulfil. Although such a development is hardly probable, it is possible, and this forcibly calls attention to the political and economic uncertainties that surround us, due to the marvellous advances of science.

The point was that the power of the atom, if it could be harnessed, could work for man's benefit, perhaps even preventing a repetition of the carnage of the previous five years: "When we have discovered the secret of the atom and can control its force, it is likely that all nations will be ready and willing to lay down their arms and abolish their armies and navies. Statesmen will be glad to sit down and compromise their differences without any talk of force, for a power will be available in the world so mighty in its potentialities that no person would dare consider its use except for some constructive purpose."[10]

In the new political and economic order, Soddy warmed to his favourite theme. The

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[8] Soddy (1909), 239-240; Wells (1914).

[9] Parsons (1921), 374. For scientific fears about the possibility of unleashing uncontrollable amounts of energy in the disintegration experiments, see Rutherford to Smithells, 26 January 1922, ASPL: "You need not be alarmed about any possibility of atomic disintegration; if it had been feasible it would have happened long ago on this ancient planet. I sleep soundly in my bed at night !"

[10] Parsons (1921), 379.

fourth edition of his ever-popular book *The Interpretation of Radium* (1920), gave him the opportunity to summarise recent developments in characteristic style: "The problem of transmutation and the liberation of atomic energy to carry on the labours of the world is no longer surrounded with mystery and ignorance," he wrote, "but is daily being reduced to a form capable of exact quantitative reasoning. It may be that it will remain for ever unsolved. But we are advancing along the only road likely to bring success ... As suddenly and unexpectedly as the discovery of radioactivity itself, at any moment some fortunate one among the little group of researchers engrossed in these enquiries might find the clue and follow it up."[11] The bulk of this work was being carried out, of course, at Rutherford's Cavendish Laboratory.

Rutherford had recommenced his own disintegration work soon after he had arrived in Cambridge, but preparing the Cavendish for a more comprehensive programme of work on radioactivity and the nucleus was not a straightforward task. At Manchester, he had developed an elaborate machinery for radioactivity research. Men, materials and money had been devoted to the elucidation of the radioactive elements and their properties. As I began to show in the previous Chapter, recreating such a programme in Cambridge demanded that the entire physical, social and intellectual geography of the Laboratory be engineered to provide favourable conditions for work in radioactivity. It is to the construction and development of the research machinery of Cambridge and the Cavendish Laboratory in the early 1920s that I now turn, beginning with Rutherford's attempts to acquire the material central to any research programme in radioactivity: radium.

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[11] Soddy (1920a), 251

2. Physics for the Empire: The Social Reconstruction of Science

2. 1 The Spoils of War: 'Radium for England'

In the shadow of Versailles, the relationships between the various centres of radioactivity research were significantly affected by the outcome of the war. In particular the Vienna Radium Institute, like other Austrian and German institutions, emerged from the war into a hostile peace. During the conflict, Meyer had managed to maintain research at the Institute, with Godlewski, Loria, Hönigschmid, Hevesy, Hess and Paneth among those who continued to work there.[12] In 1916, with his colleague Egon von Schweidler, Meyer had published *Radioaktivität*, a comprehensive account of the subject which became a standard reference work among radioactivity researchers.[13] After Versailles, however, raging inflation and a bitter peace settlement made conditions in post-war Vienna more difficult than they had been during the war itself.[14]

Rutherford re-established contact with Meyer in 1920. He brought his friend and coll-eague up to date with the latest results from the Cavendish Laboratory:[15]

> You will have heard that I am now transferred to the Cavendish ... I brought down your radium with me and have been able to start my investigations again on the nitrogen problems. You will appreciate that it is very difficult work, but I am hopeful that I will be able to settle the question definitely before long. If the atom is not disintegrated by alpha particles I am of opinion it will not be done at all in our time. You will have seen in "Nature" about Aston's work on the isotopic nature of neon, chlorine and mercury. He has greatly developed the positive ray method and I have great confidence in his conclusions. He is a very skilful experimenter and has had much experience with positive rays. You will appreciate what a large field of work this will open up and we may hope before long to decide which elements contain isotopes.

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[12] Meyer to Rutherford, 3 September 1915, RP; Karlik and Schmid (1982), 110-111; Stuewer (1985), 246.
[13] Meyer and Schweidler (1916). See also Meyer (1920a, 1950); Przibram (1950).
[14] For comments on the difficulty of sustaining research in the hostile post-war economic climate, see Sommerfeld to the Carlsberg Foundation, October 1919, BSC.
[15] Rutherford to Meyer, 13 January 1920, RP.

Much as he might admire Aston's ingenuity and workmanship, however, Meyer had more pressing problems. Economic conditions in Vienna, he reported, were much worse than Rutherford could imagine. It was barely possible to find life's necessities, let alone English scientific journals.[16] In 1921, Meyer put Rutherford in a rather awkward position: "I would ask you if perhaps it would be possible for you to get the funds for your laboratory to buy a [part] of the radium we have lent you. The [price] of 1mg Ra is now about 120 Dollars. You have a big quantity and a small one from us. If you cannot get the big sum for the first, perhaps you could get the smaller for the second and we would find in this way the means to keep our institute going for some time."[17] Rutherford's work, both at Manchester and at Cambridge, had literally been made possible only by the loan of the Vienna radium. Meyer was now calling in the debt. Rutherford therefore arranged to buy part of the radium he had been lent in 1908, a transaction which helped to save Meyer's Institute from penury, but which also gave the Cavendish Laboratory its own, secure supply of radium.[18]

While Rutherford's radium deal cut both ways, it does display quite nicely the sense in which the old pre-war network of radioactivity workers remained cohesive in the politically troubled years after the war.[19] In 1921, for example, English radioactivity worker Robert W. Lawson, who had spent the war trapped in Vienna but who had been accorded every facility by Meyer for the continuation of his work at the Institut für Radiumforschung, published an article stressing precisely the international character of the discipline of radioactivity.[20] At the same time, however, things could not be as they had been before the

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[16] Meyer to Rutherford, 22 January 1920, RP.
[17] Meyer to Rutherford, 8 February 1921, RP.
[18] Meyer to Rutherford, 8 February, 24 February, 28 April, 23 June 1921; Rutherford to Meyer, 19 February, 14 April, 16 June, 25 July 1921, RP.
[19] See, for example, "International Science and the War," *Science* **50** (1919), 453-454.
[20] Lawson (1921), esp. 269. Lawson made an estimate of "the numbers of authors in each country who have contributed four or more original papers" on radioactivity: "British Empire 45 (171); Germany 28 (210); France 18 (70); Austria 10 (76); America 9 (89); Poland 4 (14); Switzerland 3 (19); Sweden 3 (9); Italy 2 (21); Norway 2 (20); Holland 2 (12); Hungary 2 (7); Russia 1 (13); Japan 1 (12); Denmark 1 (4); Roumania 0 (4); Spain 0 (1)" (numbers in parentheses refer to "the total numbers of authors who have made any noteworthy original contribution to radioactivity"). Total: 131 (748). On Lawson's enforced sojourn in Vienna, see Lawson (1919); Soddy to Rutherford, 7, 15 April 1919; Rutherford to Meyer, 13 January 1920, RP; Soddy to Meyer, 21 November 1927, SMP.

war. Radium and other radioactive resources, still precious (and to the radioactivists indispensable) commodities, occupied an unusual strategic position in this changed political and disciplinary space.[21] Crucially, Austria had lost control of the Joachimsthal uranuim mines, which now came within the orbit of the ascendant British Empire.[22]

In the summer of 1921, Frederick Soddy and his wife travelled to Czecho-Slovakia. Soddy had been engaged as 'expert scientific adviser' to the Imperial and Foreign Corporation of London, a company which had been formed to procure a fifteen-year contract for radium with the Joachimsthal mines. It was arranged that after his holiday, Soddy should visit the mines and return to England with the first consignment of 2 grams of radium. The mission was a semi-official one, for the British Minister in Prague provided Soddy with diplomatic bags for safe transit of the precious material, and he travelled as King's Messenger.[23] After a journey blighted only marginally by the experience of having a bullet whistle past his head while the train was stationary at Munich,[24] the intrepid Soddy, looking "tired and anxious," arrived safely back at London's Victoria station with his cargo (fig. 2.1). Refusing a porter's offer of help, he took up the heavy bags - the 2g of radium were contained in nine glass phials, packed in a lead case 3 inches thick weighing about 70 pounds - and drove off with them. This was the largest quantity of radium - some £70,000-worth - ever brought into England. "I am sure," Soddy told waiting newsmen,

> that this radium will be an enormous help to British science and medicine. It is of exceptionally pure quality. The cry of the medical profession has hitherto been, 'We cannot get enough.' The greatest amount I have so far ever had to work with has been 30 milligrams. There will be more shipments of radium from Czechoslovakia, but not necessarily to England.[25]

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[21] See, for example, "Radium After the War," *Literary Digest* **61** (1919), 119-123; "Concerning Radium," *Literary Digest* **65** (1920), 114-117; "Radium is becoming of Ordinary Household and Industrial Use," *Current Opinion* **69** (1920), 537-538.

[22] Meyer to Rutherford, 22 January 1920, RP. The Joachimsthal mines produced much of the pitchblende from which radium was derived. See Meyer (1950).

[23] Documents relating to Soddy's involvement with the Imperial and Foreign Corporation of London are in MS Eng. Misc. 187/20, FSP; details of the Czechoslovakia trip are in MS Eng. Misc. 186/193, FSP. See also MS Eng. Misc. 170/16, FSP; "Radium for England," *Science* **54** (1921), 373-374; Howorth (1958), 214.

[24] Howorth (1958), 214.

[25] "Radium for England," 373-374.

Fig. 2.1 Radium for England: Frederick Soddy arriving at Victoria Station, London, with £70,000-worth of radium, 1921.

Source: Howorth (1958).

The radium was deposited at the Foreign Office until its future was decided. It was to be used primarily for medical purposes, but also "for the production and sale of radio-active water in bottles, for use at radio-sanitoria, the production and sale of radio-active fertilizers, and for its by-products such as polonium."[26] Soddy hoped that some of the radium would find its way to Oxford - unfounded rumours soon circulated to the effect that a special laboratory was to be set up there under Soddy's direction - but he was to be disappointed. Nevertheless, Soddy stressed the great importance of Britain's newly-acquired monopoly on the Joachimsthal products "for radium, apart from its curative possibilities, is essential to the study of the composition and decomposition of the atom, a study which may well revolutionise the work of the world by supplying it with a new source of power."[27]

Other sources of radium were also becoming available to physicists. In 1920 Walter Morley Fletcher, Secretary of the Medical Research Council, was approached by J.J. Thomson, who pointed out that at least five grams of radium from the government's Surplus Property Disposal Board were controlled by various medical authorities in Britain, and requested that a gram or so of this radium be released for physical research.[28] The plea was taken up by Rutherford and W.H. Bragg, the latter having become Quain Professor at University College, London, in 1915.[29] Having secured ownership of the material and the expert advice of the Middlesex Hospital's Sidney Russ, the M.R.C. established a scheme whereby the radium could be deployed for both physical *and* medical research.[30] For the Cavendish Laboratory, Larmor's would-be centre of "Imperial Physics," Rutherford managed to secure 493 milligrams of radium from the Middlesex Hospital.[31] So keen was Rutherford to acquire some of the 'Government' radium, in fact, that he even complied with the demand of Russ - his ex-demonstrator - that he travel to London to collect his

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26 *ibid*, 374.
27 Quoted in "Precious Material," *Westminster Gazette*, 26 September 1921, copy in MS Eng. Misc. 170/16, FSP.
28 On the Medical Research Council and its precursors, see Alter (1987), 172 ff.
29 Caroe (1978), 79-92.
30 Murphy (1986), 5.4 ff. Russ had worked with Rutherford at Manchester. See Eve (1939), 188.
31 Rutherford to R.W. Boyle, 23 April 1921, published in Eve (1939), 283-285, on 284.

allocation in person.[32] So, with the M.R.C. material and the Vienna radium, at least part of which the Cavendish Laboratory now owned outright, Rutherford's radioactive wants were amply satisfied.[33]

2.2 The Idea of a University and the Importance of Research

The University, too, assumed a new significance in the post-war world. The eclipse of Germany and the development of a new political order in Europe created opportunities for the promotion of science and research of which Rutherford and like-minded reformers were quick to take advantage.[34] Rutherford argued that it was crucial for the universities to establish themselves as places for the production of new knowledge as well as the dissemination of old, for "on [the] question of Research in general, and the lead that the Universities can give, will largely depend the reputation of this country as an intellectual centre."[35] With William Pope, Professor of Chemistry at Cambridge, he established an agenda for reform. They argued for the full admission of women into the University, for example,[36] and agitated for adequate provision and, equally important, recognition and credit, for research. In the future, surmised Rutherford in 1921, "the Universities will be judged not so much by the number of their undergraduates or by the extent of their endowments, as by the magnitude of their contributions to knowledge."[37] Bearing in mind Larmor's injunction about the place of Cambridge in the new scheme of things, he went on to articulate a vision of Imperial intellectual domination based on leadership in research:[38]

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[32] Murphy (1986), 5.7; Wilson (1983), 480. Russ also imposed the condition that the radium be returnable upon six months'notice. The radium thus acquired was transferred to the Department of Scientific and Industrial Research in 1928.
[33] Rutherford continued to acquire radioactive materials as occasion allowed and as the ongoing research programme demanded. See, for example, Rutherford to Boltwood, 20 February 1924, Boltwood to Rutherford, 7 June 1924, in Badash (ed.)(1969), 352-353 and 354-356 respectively.
[34] See, for example, Arthur (1919); Soddy (1920b); J.W. Evans (1921); Smithells (1921). Cf. also "Longing of Scientists to Remain Useless," *Current Opinion* 71 (1921), 89-90.
[35] Rutherford, in Hill (ed.)(1921), 371.
[36] W.J. Pope and E. Rutherford, "Women at Cambridge. A National Enlargement," letter to *The Times*, 8 December 1920, 8.
[37] Rutherford, in Hill (ed.)(1921), 371.
[38] *ibid.*

> It appears to me that we are at the moment in a very important stage in our University history. We have to play our part in developing the newer knowledge, not only for this country for ourselves, but also for our Dominions, and at the same time take our proper share in the higher education of that great flood of post-graduate students which is now in movement and which will grow rapidly in the near future. In the past, Germany diverted a large part of this stream, and quite rightly, because she provided for it. She had a very large number of teachers, enthusiastic and devoted to their subjects, who welcomed the foreign student. If we are to take our proper place as an intellectual nation, we must put our University house in order so that we may be in a position to play our part in this great intellectual revival.

One of the key means by which this great revival could be brought about was through the introduction of the Ph.D. degree.

Although Cambridge had long offered the degree of Sc.D., the campaign to institute the Ph.D. at Cambridge had begun only in 1916, when J.J. Thomson had thought it "probable that after the war many students from neutral countries will be unwilling to go to Germany for their postgraduate studies, and would much prefer to come to England." If the doctorate were not obtainable in Cambridge, such students "would not be likely to come in any considerable numbers."[39] Against opposing claims that the Ph.D. was "a piece of goods made in Germany,"[40] the degree was established by Royal Patent in May 1920. In the wake of that decision, Rutherford and his allies also argued for the introduction of 'lesser' postgraduate degrees such as the M. Litt. and M.Sc. for those who wished to follow a course of research, but who did not necessarily want to work for three years towards a Ph.D.[41]

In 1920, Rutherford and Pope submitted a comprehensive memorandum on the 'Administration and Control of Scientific Departments' to the post-war Royal Commission on Oxford and Cambridge Universities.[42] The document was supplemented by oral

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[39] J.J. Thomson in discussion of the General Board of Studies, 7 June 1916, quoted in Wilson (1983), 418.

[40] Hobson, in *ibid.* Simpson (1983), 140-147, gives a good comparative account of the establishment of the Cambridge Ph.D.

[41] Wilson (1983), 419. For Rutherford's comments on the advantages of the Ph.D. degree over the older Cambridge D.Sc., see Rutherford to Laby, 5 December 1921, RP.

[42] W. Pope and E. Rutherford, "Administration and Control of Scientific Departments - A Memorandum Addressed to the Royal Commission (1920) on the Universities of Oxford and Cambridge," Cam.a.922.5, CUL. On Pope, see Moody and Mills (1947); Mann (1975).

testimony to the Commission, during which Rutherford argued that, in his opinion, "the aim of the [scientific] Department[s] should be to develop a really effective system of post-graduate work ... [including] ... advanced lectures and training in technique and methods of research."[43] In particular, he stressed, "care ought to be taken to secure a correlation between the detailed research of an individual student with the general advance in scientific research and discovery." Collaboration between neighbouring subjects was desirable, for it was often at the borders between one science and others that "the most rapid progress" was to be made, a conclusion exemplified by "the phenomenal developments now occurring upon the fringe which separates Chemistry and Physics."[44]

At the same time, however, he warned of the danger of "hangers on," men who became unproductive after years of teaching. Such men should be "encouraged to migrate and to gain experience of other places," the "great desideratum" being to "scatter" men across the network of Imperial universities. The best of them should then return to Cambridge to take their place in the reproduction of the system. This educational philosophy of training, circulation and return characterised precisely the system which he would create at the Cavendish Laboratory over the next few years. The path to the institution of such a system was already well marked out. The '1851 Exhibition' awards for science scholars in the Empire existed precisely to facilitate such circulation. Rutherford himself had been an 1851 Scholar. Many of those who came to work with him at the Cavendish Laboratory in the 1920s and 1930s were from the Dominions - Australia, New Zealand, Canada, India - and had been forwarded by men who had themselves worked in Cambridge or who had worked with Rutherford at Montreal or Manchester. As Appendix 1 shows, Joseph A. Gray at Kingston, Ontario, Laby at Melbourne, Eve at McGill and McLennan at Toronto were all regular channels through whom aspiring researchers might obtain 1851 awards to work at the Cavendish Laboratory.[45]

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43 Rutherford, "Oral Evidence Given Before the Commission," Cam.a.922.9, 14, CUL.
44 W. Pope and E. Rutherford, "Administration and Control of Scientific Departments," 7.
45 For apposite comments on Rutherford's support of "Dominion Students," see Rutherford to Laby, 19 May 1922, RP. The importance of the '1851 Exhibition' awards is emphasised in Thomson (1931), 24; Picken (1948); Lewis (1967); Gingras (1991), 41-45.

The emergent research programme at the Cavendish was also sustained by the provision, for the first time, of government-funded research grants. The creation of the Department of Scientific and Industrial Research (D.S.I.R.) after the war significantly changed the prospects for young researchers. In 1914, other than the highly competitive '1851 Exhibition' awards, there were only 214 full time postgraduate awards open to British graduates, for which there were long waiting lists.[46] During the war, however, "the Professor, the Lecturer, the Research Assistant and the Research Student" had become "powerful assets to the nation." According to H.A.L. Fisher, "[w]hatever university you may choose to visit you will find it to be the scene of delicate and recondite investigations, resulting here in a more deadly explosive, there in a stronger army boot."[47] Having proved their usefulness to the state, scientists now reaped their rewards. The first 159 D.S.I.R. awards were made in 1919-1920, establishing a major source of support for university research.[48]

These developments were contingent on domestic political and economic developments, however. After the post-armistice euphoria, the decline of Britain's international markets and the ensuing instability in world trade set the country into a deflationary spiral of depressed wages and unemployment. By mid-1921, hyper-inflation was raging in Europe and industrial recession had dashed plans of reconstruction in Britain. One direct consequence was the 'Geddes axe' of 1922 in which expenditure on education was cut by £6.5 million.[49] Nevertheless, in the summer of 1922 Rutherford was able to report satisfactory progress to George Hevesy. He had, he said, "got the laboratory in pretty good order for an old fashioned Institution."[50] Over the next fifteen years, the D.S.I.R. awards, together with the '1851 Exhibition' scholarships and various other research grants would be

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[46] K. MacLeod and R. MacLeod (1976), 306. For contemporary remarks, see Thomson (1909a), 4-6; Soddy (1920b), 49-64, esp. 57-63. Compare Thomson (1931).
[47] Fisher (1917), xx.
[48] On the formation, development and importance of the D.S.I.R., see "Organization of Scientific Research under the British Government," *Scientific Monthly* **11** (1920), 571-572; "State Grants for Scientific Investigators in England," *Science* **51** (1920), 559-562; Melville (1962); MacLeod and Andrews (1970, 1971); R. MacLeod and K. MacLeod (1979); Moseley (1977).
[49] The effects of the Geddes axe on education are dealt with in Lowndes (1969), 124-125.
[50] Rutherford to Hevesy, 13 June 1922, RP.

put to good use in the Cavendish, with no less than 104 research students submitting dissertations in experimental physics (Appendix 2).[51] Situated at the centre of a network of imperial universities feeding it the brightest and best of the Dominions' students, the Cavendish Laboratory would match, even exceed, Larmor's expectations of it.

2.3 "Brains in their Fingertips": Training for Research at the Cavendish Laboratory

Having crossed the first two hurdles by securing a place and a research grant, what, then, could the newly-arrived Ph.D. student expect when he[52] walked into the Cavendish Laboratory? Many, if not most, of the physics graduates who came to Cambridge in the 1920s did so specifically to work with Rutherford in radioactivity, the subject he had made his own. These newcomers were expected to join graduates of the Cavendish in completing a preliminary course of training in measurement and manipulation techniques.[53] Organised and run by Chadwick,[54] the induction course took place in an attic room known as the 'Nursery.' In the early 1920s, the course lasted a month or so, though by the end of the

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[51] In 1922 the Royal Commission for the Exhibition of 1851 inaugurated a new category of scholarship for 'Senior' students - researchers who had already completed a Ph.D. and wished to continue with research for a few more years. Again, the Cavendish put these postdoctoral awards to good use (Appendix 2). See *Record of the Science Research Scholars of the Royal Commission for the Exhibition of 1851, 1891-1950* (London: Royal Commission for the Exhibition of 1851, 1951), 74 ff.

[52] Gender used advisedly.

[53] Although practical training started at the undergraduate level - Cavendish students were expected to complete courses in the practical laboratories - none of the undergraduate practicals involved radioactivity. In 1926, T.G. Bedford codified and published much of the Cavendish Laboratory's Part I undergraduate practical curriculum in textbook form. See Bedford (1926); Searle (1934); Woodall and Hawkins (1969); Bullard (1974); Ward (1987).

[54] Holding a studentship at Gonville and Caius College and, later, the Clerk Maxwell Scholarship in succession to Aston, Chadwick quickly consolidated his position in the laboratory. In 1921 he took his Ph.D. and also signed the preface to *Radioactivity and Radioactive Substances*, a short monograph in the Pitman's Technical Primer series. In the same year he was elected to a research fellowship at Caius. A year or so later he was officially appointed Assistant Director of Radioactive Research, and from 1924 his salary was paid by the Department of Scientific and Industrial Research. In this capacity, he became responsible not only for the running of the disintegration work, but also for the assignment of projects to research students, for ordering supplies and for matters relating to "mechanicians and other laboratory servants." See Chadwick to Rutherford, 30 May 1920, RP; "University and Educational Intelligence," *Nature* **105** (1920), 601; Chadwick (1921); Massey and Feather (1976), 15-17. Robinson (1962), 73-74, implies that Geiger had fulfilled much the same function at Manchester. On Rutherford's assessment of Chadwick, see "Memorandum of Conversation with Sir Ernest Rutherford. Extract from Doctor Trowbridge's Log of Visit to Cambridge, England, 17 April 1926," Box 29 f410, International Education Board archives: "R. stated that he had one man who acted as a sort of assisitant director, a man who he thought had no ambition ever to become a director."

decade its duration had been extended slightly to six weeks. The curriculum varied slightly from year to year, depending on the resources available, but there was a fixed core of experiments which formed a rite of passage into the world of research, an initiation into the intricacies of radioactive technique.[55] In the 1922 Nursery course, for example, the following exercises were carried out:[56]

Construction of the leaf system for an α-ray electroscope	1 day
Setting up α-ray electroscope for use: natural leak, uniform part of scale, test for saturation	1 day
Range, by ionization methods, of α-rays from Radium F.	
Ranges when aluminium foils were interposed between source and electroscope	1 day
Absorption coefficient of β-rays from Radium E in aluminium	1 day
Decay of actinium active deposit (long exposure) by α-rays	1 day
Decay of actinium active deposit (short exposure) by α-rays Decay of actinium C measured by α-rays	1 day
Decay of actinium D	1 day
Preliminary adjustments of γ-ray electroscope	1 day
Absorption of γ-rays from radium in lead	2 days
Construction of shallow ionisation chamber and α-ray electroscope for determining ranges of α-particles	1 day
Range of the α-particles from radium F	1 day
Decay of thorium emanation (by α-rays)	4 days
α-ray scintillations from radium C and radium F	3 days
Ranges of α-rays from thorium C + C' (by scintillations)	2 days
Number of α-particles given off by a source of radium F	2 days

[55] For accounts of the 'Nursery' and its importance in training new researchers, see *inter alia* Feather (1960b), 600; Devons (1971); Sargent (1985), 209.
[56] J.S. Rogers, Report of "Preliminary Work in Radioactivity," file ii/6, 1851 Archives, ICL.

The intensive regime of the 'Nursery' was designed not only to give the young researchers a thorough grounding in the technique of radioactivity. General laboratory practice, too, was accorded considerable importance. Vacuum technique was a crucially important part of the Nursery course, for in radioactivity experiments much depended on the strength and durability of the vacuum attained. In the early 1920s rough vacua were produced with a water pump, 'high' vacua with a Töpler pump.[57] Students who persevered with the Töpler pump, which took about $1\frac{1}{2}$ hours to reach a pressure of 10^{-3} mm Hg and required manual operation, might then be given the opportunity to use a Gaede pump, a mechanised form of pump which could be left to its own devices for an hour or so.[58] In the period after 1924, a number of innovations were introduced. First came the Fleuss pump, a piston pump with leather valves lubricated with grease. Circa 1926 rotary oil pumps were introduced, and were found useful because they could be left pumping overnight (subject to the danger of oil flooding back into the vacuum in the event of belt breakage or a power cut). Such devices produced vacua of about 3×10^{-3} mm Hg. These were followed by the mercury diffusion pump, which gave a substantial improvement both in speed of operation and in the vacuum ultimately obtainable.[59] Clearly, then, vacuum technics were central to both training and research, and had a significant impact on the kinds of experiments which would become possible.[60]

Practical technique was at least as important as hardware. In May 1920, for example, Aston gave a series of lectures on the design of apparatus and vacuum practice. He offered his students the benefit of his twenty years' experience with discharge technology and the production and manipulation of vacua, covering selection of materials, joints, seals, taps and pumps in considerable detail.[61] Aston's hints and tips were supplemented by practical exercises in the Nursery, where a typical task might be to measure the vapour pressure of

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57 Ditchburn (1977), 566. On the Töpler pump, see Kaye (1927), 12-15.
58 Ditchburn (1977), 566-567. For the Gaede rotary oil pump, see Kaye (1927), 76-78.
59 Ditchburn (1977), 567.
60 For the constraints imposed by vacuum technology on experimental work, see Blackett (1933), 71-72; Oliphant (1972a); Price (1984).
61 "Design of Apparatus etc.," Notebook B33, GPTP. For another contemporary survey of vacuum technique, see Kaye (1927), 31-70.

mercury and two vacuum greases at known temperatures - reflecting Chadwick's interest in finding improved vacuum greases and sealing compounds for vacuum work.[62] In 1925, having "seen some of the efforts in the workshop," Chadwick also organized a class of instruction in the operation of lathes and other machine tools to encourage manual dexterity among his flock.[63]

Having completed the introductory course, the novice research student would be set his first problem. Under Chadwick's careful surveillance, each individual's performance in the Nursery was monitored, and his strengths and weaknesses noted.[64] The research problem subsequently allotted to the student would depend quite heavily on his performance in the attic. At the same time, Rutherford and Chadwick were constrained in the way they chose to allocate research problems by the need for a student to be able to complete a Ph.D. within three years. Thus the pattern of research in the laboratory at any given time would depend quite heavily on the contingencies of particular indivduals' capabilities, the apparatus available and the foreshortened timescale of a Ph.D. research project.[65] Within those constraints, however, the instruction of graduate students, the Cavendish programme of nuclear studies and Rutherford's personal research were closely linked. In the early 1920s, as I shall shortly show, many of the research students were set experimental problems in radioactivity which had been left unresolved from Rutherford's Manchester period, as well as the new experiments suggested by the analysis developed in the Bakerian Lecture.[66] With an unprecedented array of junior and senior research grants, an in-house

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[62] Sargent (1985), 209.
[63] Chadwick to Rutherford, 21 November [1925], RP. The class was taken by George Crowe and "Bert" [?].
[64] For a powerful analysis of the role of discipline in the pedagogical regime, see Foucault (1977)[1975], 170 ff. See also Schaffer (1988); Gooday (1990), 39-43; Gooday (1991).
[65] Research students were expected to devote most of their time to research during the three terms of the academic year. The residence requirement for the Ph.D. was normally nine terms. The final examination was a 1-hour viva on the thesis and the principles of physics. Difficulties could often arise if the Ph.D. project developed in such a way that it could not be completed within the statutory time. See Sargent (1985), 210; Ward (1987). Gingras and Trépanier (1993) stress the importance of social and organisational constraints in the knowledge-making process.
[66] Later in the decade, however, as more laboratories (in Europe and elsewhere) began to be productive in experimental research and as claims about new phenomena became increasingly frequent, the organisation and division of graduate labour - even what it *meant* to be a research student - changed in significant ways. By the late 1920s, as we shall see in later chapters, young Cavendish graduates would often cut their research teeth by replicating experiments carried out elsewhere.

regime of technical training and a developing career structure in the expanding network of universities in Britain and in her Dominions, the Cavendish Laboratory quickly became a powerful machine for social and professional mobility.[67] Radioactivity, the stock-in-trade of the Cavendish in the 1920s, truly was a physics for the empire.

3. Making Isotopes Matter: The First Mass-Spectrograph

3.1 The Whole Number Rule and the 'Stupendous Possibilities of the Atom'

While Rutherford's engineering of the social and material environment of the laboratory was crucial to the enactment of his programme, that programme also relied heavily upon conceptual resources drawn from the wider culture of Cambridge physics. Throughout the 1920s, one of the elements central to the development of Rutherford's understanding of the nucleus and its structure was the mass-spectrograph. In December 1919, when he first announced the mass-spectrographic analyses of several elements, Aston, presumably at Rutherford's prompting, noted an unusual numerological relation: "A fact of the greatest theoretical interest appears to underlie these results, namely, that of more than forty different values of atomic and molecular mass so far measured all, without a single exception, fall on whole numbers, carbon and oxygen being taken as 12 and 16 exactly ... Should this integer relation prove general, it should do much to elucidate the ultimate structure of matter."[68] By February 1920, Aston's apparatus was producing results at an astonishing rate. In the space of two days, eleven elements fell to mass-spectrographic analysis. Aston had told the Cambridge Philosophical Society a few weeks earlier that while helium appeared to be a "pure" element of mass 4.00, hydrogen was "very definitely heavier than unity (O=16)," thereby constituting the single exception "proving" what he

67 Werskey (1978), 20-26; Wilson (1983), 544.
68 Aston (1919c).

came to call the "whole number rule."[69] Aston archly offered Lindemann "the latest official quotations for elemental stocks, fractions barred except in the case of Hydrogen,"[70] a witticism doubtless savoured by the aristocratic Oxford don.

The importance of the whole-number rule was two-fold. On the one hand it introduced a "very desirable simplification into the theoretical aspects of mass."[71] On the other, it opened up a new discourse of nuclear energy which was closely linked to Rutherford's account of the constitution of the nucleus. This link with nuclear constitution and Rutherford's speculations about isotopes allowed Aston to "explain" why hydrogen had to be an exception to the whole-number rule, since "on the Rutherford 'nucleus' theory the hydrogen atom is the only one not containing any negative electricity in its nucleus."[72] In fact:[73]

> The case of the element hydrogen is unique, for its atom appears to consist of a single proton as nucleus with one planetary electron. It is the only atom in which the nucleus is not composed of a number of protons and electrons packed exceedingly close together. Theory indicates that when such close packing takes place the effective mass will be reduced, so that when 4 protons are placed together with two electrons to form the helium nucleus, they will have a weight rather less than four times that of the hydrogen nucleus, which is actually the case.

Where Rutherford had appropriated Aston's work to sustain his interpretation of the disintegration experiments, Aston now repaid the compliment by using the nuclear hypothesis as an interpretative scheme within which to situate and make sense of his results. As Rutherford had done in his Bakerian Lecture, Aston constructed models of the nuclei of various isotopes. While Rutherford had used protons, electrons and helium nuclei of mass 3 and 4, however, Aston used only protons and electrons, which he referred to as the "standard bricks" of matter.[74] These bricks were so arranged that "*[i]n the nuclei of*

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69 "Cambridge Philosophical Society," *Nature* **104** (1920), 714. The term "whole number rule" to express the numerical relationship between isotopic masses was in use by March 1920: see Aston (1920d).
70 Aston to Lindemann, 21 February 1920, FALP; Aston (1920a, 1920b, 1920c).
71 Aston (1920h), 619.
72 Aston (1920h), 619.
73 Aston (1921a), 341.
74 Aston (1922a), 97.

normal atoms the packing of the electrons and protons is so close that the additive law of mass will not hold and the mass of the nucleus will be less than the sum of the masses of its constituent charges."[75] Aston constructed diagrammatic representations (fig. 2.2) to indicate "the sort of arrangements which may take place in atoms," and constructed a table of the "stable systems of protons and electrons known to occur" (fig. 2.3).[76]

There was a disciplinary pay-off to this reductionist programme. Aston's results, he told a Royal Institution audience, "lie on the border line of physics and chemistry, and although as a chemist I view with some dismay the possibility of eighteen different mercuric chlorides, as a physicist it is a great relief to find that Nature employs at least approximately standard bricks in her operations of element building."[77] Crucially, the isotope interpretation of matter called for "a drastic revision of conventional ideas regarding the elements."[78] The fractional weights which had been found by chemists for many of the elements were now to be explained away as "fortuitous statistical effects due to the relative quantities of the isotopic constituents"[79] - what Arthur Smithells, Professor of Chemistry at Leeds called "the tidying up of the atomic weights," in which Aston "brushes all the nasty fractions up and puts them into the wastepaper basket afforded by the atom of hydrogen."[80]

Such an interpretation threatened to undermine decades of careful and painstaking work by atomic weight chemists.[81] But there was another point to the model-building. By 1921 it was evident that the non-integral mass of hydrogen and the possibility of a "packing effect" in the formation of "stable assemblages" meant that the energy of the heavier nuclei

75 Aston (1922a), 101, emphasis in original. See also Siegel (1978).
76 Aston (1922a), 96-97. Compare Bohr (1922). On the importance of nuclear electrons in this scheme, see Stuewer (1983).
77 Aston (1921a), 342.
78 "Chemistry at the British Association," *Nature* **106** (1920), 358-359, 358.
79 Aston (1920f), 624.
80 Smithells to T.W. Richards, 12 November 1920, ASPL.
81 At the same time, however, isotopes found a large constituency among chemists who attempted to separate and characterise the new species, thereby embodying isotopes in chemical practice (George Hevesy, for example, joked to a friend that he had joined "die Sekte der Isotopentrenner" - the 'sect of the isotope-separators: see Levi (1985), 49). For examples of the work on isotope separation, see Merton (1915, 1920); Brønsted and Hevesy (1921, 1922); D.L. Chapman (1920a, 1920b); Hartley, Ponder, Bowen and Merton (1922); Joly and Poole (1920); Laby and Mepham (1922). An important centre of such work was Chicago, where R.S. Mulliken constructed an automated isotope separator. See Harkins and Mulliken (1921); Mulliken (1922, 1923); Mulliken and Harkins (1922); Mulliken (1989), 28-38. Gleditsch (1925), 35, points explicitly to the importance of such work in embedding isotopes in chemical practice.

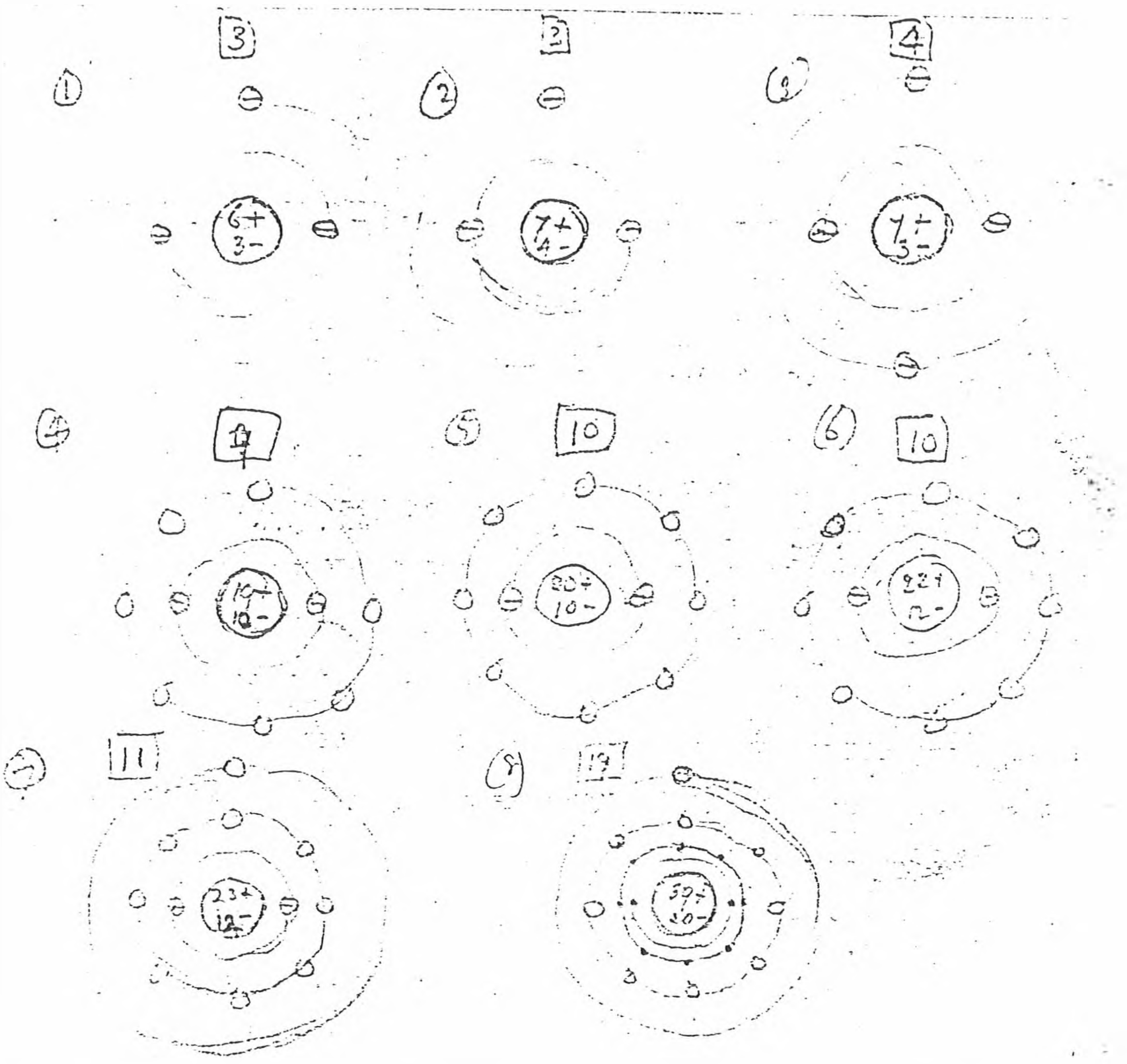
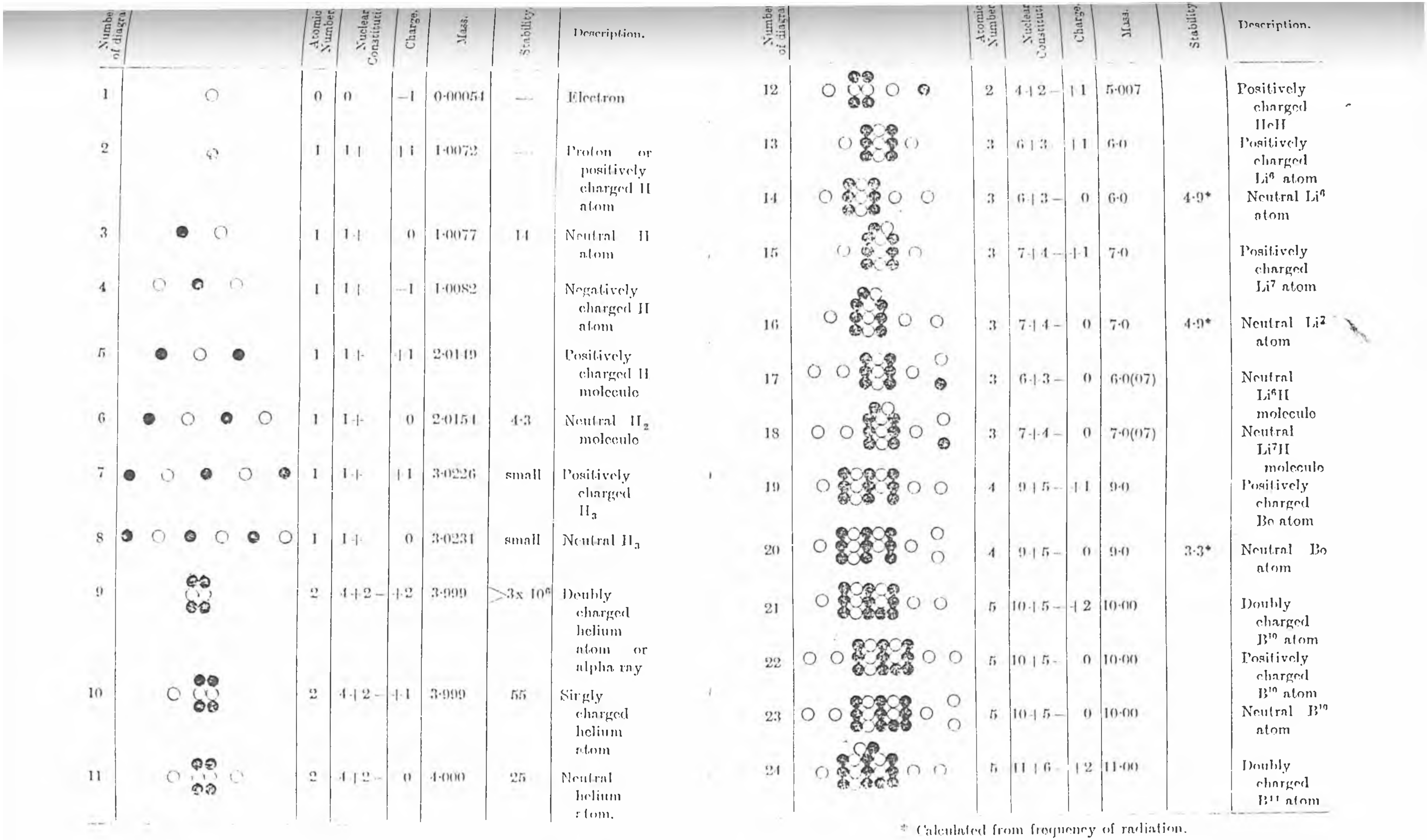


Fig. 2.2 Isotopes and the Nucleus: F.W. Aston, draft diagrammatic representations of isotopes and nuclear atoms, including the case of neon (examples (5) and (6)). Beyond the indication of the number of protons (+) and electrons (-) in each nucleus, there is no indication of nuclear structure.

Source: Aston MSS, TCC.



Number of diagram		Atomic Number	Nuclear Constitution	Charge	Mass	Stability	Description
1		0	0	−1	0·00054	—	Electron
2		1	1+	+1	1·0072	—	Proton or positively charged H atom
3		1	1+	0	1·0077	14	Neutral H atom
4		1	1+	−1	1·0082		Negatively charged H atom
5		1	1+	+1	2·0149		Positively charged H molecule
6		1	1+	0	2·0154	4·3	Neutral H_2 molecule
7		1	1+	+1	3·0226	small	Positively charged H_3
8		1	1+	0	3·0231	small	Neutral H_3
9		2	4+2−	+2	3·999	$>3 \times 10^6$	Doubly charged helium atom or alpha ray
10		2	4+2−	+1	3·999	55	Singly charged helium atom
11		2	4+2−	0	4·000	25	Neutral helium atom.
12		2	4+2−	+1	5·007		Positively charged HeH
13		3	6+3	+1	6·0		Positively charged Li^6 atom
14		3	6+3−	0	6·0	4·9*	Neutral Li^6 atom
15		3	7+4−	+1	7·0		Positively charged Li^7 atom
16		3	7+4−	0	7·0	4·9*	Neutral Li^7 atom
17		3	6+3−	0	6·0(07)		Neutral Li^6H molecule
18		3	7+4−	0	7·0(07)		Neutral Li^7H molecule
19		4	9+5−	+1	9·0		Positively charged Be atom
20		4	9+5−	0	9·0	3·3*	Neutral Be atom
21		5	10+5−	+2	10·00		Doubly charged B^{10} atom
22		5	10+5−	0	10·00		Positively charged B^{10} atom
23		5	10+5−	0	10·00		Neutral B^{10} atom
24		5	11+6−	+2	11·00		Doubly charged B^{11} atom

* Calculated from frequency of radiation.

Fig. 2.3 F.W. Aston, table showing isotopes and nuclear constitution of the atoms of the light elements in terms of protons and electrons, as published in *Isotopes* (1922).

Source: Aston (1922a), 106-107.

could be taken as a key indicator of their constitution. Rutherford's most recent experiments had led him to re-conceptualise the nucleus in terms of a 'core' of tightly-bound α-particles surrounded at a distance by hydrogen outriders or 'satellites.' Because they were less tightly bound, these satellite protons should increase the mass of the atom slightly, and "we should expect that the whole-number rule found by Aston, which appears to hold for atomic masses to about 1 in 1000, would be departed from if measurements could be made with yet greater accuracy."[82] In virtue of this it was "of the greatest importance to push the accuracy of methods of atomic weighing as far as possible, for variations from the whole number rule, if they could be determined with precision, would give us some hope of laying bare the innermost of secrets, the actual configuration of charges in the nucleus.[83] Aston's thoughts began to turn to the design and construction of a second, more powerful machine.

3.2 Atomic Energy, Nuclear Constitution and Cosmological Speculation

Through the mass-spectrograph, then, energy was to be a key indicator of nuclear structure. And where Soddy and Wells fantasised about controlling the energy of the radioactive elements, Aston's work made it possible to speak of unleashing the energy contained in ordinary, everyday substances. Aston saw, and was quick to capitalise on, the rhetorical, as well as the scientific, possibilities of his work. Aping Soddy's well-known example, he claimed that "if we could transmute the hydrogen contained in one pint of water the energy so liberated would be sufficient to propel the Mauretania across the Atlantic and back at full speed. With such a vast store of energy at our disposal there would be literally no limit to the material achievements of the human race."[84] Like Soddy and Wells, Aston could affect

[82] Rutherford (1922a), 413; Siegel (1978). On the genesis of the satellite model of the nucleus, see Stuewer (1986a).
[83] Aston (1921a), 341-342.
[84] Aston (1922h), 705.

a prophetic, even apocalyptic, tone when the occasion arose. Of the constitutional energy of the nuclei of the light elements he wrote:[85]

> Should the research worker of the future discover some means of releasing this energy in a form which could be employed, the human race will have at its command powers beyond the dreams of scientific fiction; but the remote possibility must always be considered that the energy one liberated will be completely uncontrollable and by its intense violence detonate all neighbouring substances. In this event the whole of the hydrogen on earth might be transformed at once and the success of the experiment published at large to the universe as a new star.

It was powerful imagery.

Citing Einstein's Theory of Relativity, Aston thought it "absolutely certain that if hydrogen is transformed into helium a certain quantity of mass must be annihilated in the process," a conclusion of "profound cosmical importance."[86] The idea had already been put forward by Arthur Eddington, the Cambridge mathematician and astronomer. During the war, Eddington had begun to consider the question of stellar constitution and relativity in the light of an anomaly in the theory of Kelvin and Helmholtz, according to which a star's energy arose from its gravitational contraction.[87] This gave unaccountably short stellar lifetimes, however. Radioactivity had early been considered as an alternative energy source, but this also turned out to be insufficient. In 1920, Eddington drew upon the combined resources afforded by Aston's mass-spectrograph and Rutherford's disintegration experiments to construct an entirely new interpretation of the problem of stellar energy and evolution. Putting the continued existence of the contraction theory, an "unburied corpse," down to "inertia of tradition," Eddington argued that some other account was needed of

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[85] Aston (1922a), 104. Compare Parsons (1921). For a discussion of the relationship between science and science fiction in this period, see Haynes (1980); Weart (1988); Lambourne, Shallis and Shortland(1990), 1-33.
[86] Aston (1922a), 103.
[87] On Eddington, see Douglas (1956); Huffbauer (1981), esp. 299-302. See also Hendry (1987). For Eddington and relativity in the context of Cambridge mathematics, see the exemplary study by Warwick (1989), Chapter 7.

the "vast reservoir of energy" available to the stars. He told Section A of the British Association in 1920 that this energy had to be sub-atomic:[88]

> F.W. Aston's experiments seem to leave no room for doubt that all the elements are constituted out of hydrogen atoms bound together with negative electrons. But Aston has further shown conclusively that the mass of the helium atom is less than the sum of the masses of the 4 hydrogen atoms which enter into it. There is a loss of mass in the synthesis amounting to 1 part in 120 ... Now mass cannot be annihilated, and the deficit can only represent the mass of the electrical energy set free in the transmutation. We can therefore at once calculate the quantity of energy liberated when helium is made out of hydrogen. If 5 per cent of a star's mass consists initially of hydrogen atoms, which are gradually being combined to form more complex elements, the total heat liberated will more than suffice for our demands, and we need look no further for the source of a star's energy.

Such a claim, he acknowledged, was "difficult to assert but perhaps rather more difficult to deny." Rutherford's disintegration experiments showed the new domains which had been opened up to investigation, and "what is possible in the Cavendish Laboratory may not be too difficult in the sun ..."[89] - a locution of which Wells himself could have been proud!

3.3 Dissent and Disproof: The Mass Spectrograph and its Critics

In 1921, contrary to his usual practice, Aston began a collaboration with G.P. Thomson (son of J.J.) in an attempt to manifest the isotopes of some of the metallic elements which had hitherto eluded analysis. Another wartime member of Farnborough's Chudleigh Mess, Thomson junior had started research on positive rays under J.J. in 1914. It was this technique to which he now returned. After a series of trials with various arrangements of the parabola apparatus, Thomson and Aston produced isotopes of atomic weights 6 and 7 for lithium - one of the examples Rutherford had speculatively illustrated in his Bakerian

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88 Eddington (1920), 34. See also Aston (1922a), 103-104.
89 Eddington (1920), 34.

Lecture the year before.[90] Rutherford was naturally delighted.[91] J.J. Thomson, however, was not. On 3 March 1921 the Royal Society held a "Discussion on Isotopes" under the chairmanship of its President - J.J. Thomson. Rutherford waggishly reported the highlight of the meeting to Bohr: "There was a discussion on isotopes at the R[oyal] S[ociety] yesterday. JJT led off followed by Aston, Soddy &c. I believe the former rather threw doubt on isotopes in a vague way because they did not fit well with his conceptions of atoms and the forces therein ..."[92]

Thomson had been a sceptic about isotopes from the outset. And while his dissension from the Rutherford-Aston account of isotopes and the nucleus surely reflected his own prejudices and interests (in this case, as Rutherford noted, his own model of the atom), it also emphasises the contingency and contestability of the new interpretation. Early in 1920, for example, Rutherford had told Bohr that "Aston gave a paper on isotopes in the laboratory the other day and J.J.T[homson] said he did not believe his results about chlorine. You can imagine that I enjoyed myself thoroughly between the two ..."[93] It was the case of chlorine upon which Thomson now chose to focus in an attempt to articulate his dissent.

By making a careful distinction between the processes possible inside the discharge and those conventionally observed outside it, Thomson attempted to provide an alternative account of Aston's results, using the interpretative resources he had developed a decade earlier to explain the results of the positive ray experiments:[94]

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[90] Thomson to Lindemann, 21 September [1920], 29 September [1920], 23 February [1921], 11 May [1921], FALP; G.P. Thomson notebooks B31, B33, GPTP; Aston and Thomson (1921). For Thomson's work on positive rays, see Moon (1977), 532-533; Hendry (1990b), 909.

[91] See Rutherford to Boltwood, 28 February 1921, in Badash (ed.)(1969), 341-344.

[92] Rutherford to Bohr, 4 March 1921, RP. There is no adequate study of Thomson's work in the 1920s (see, however, Thomson (1921a, 1923); Rayleigh (1942), 215-230). For Thomson's model in the pre-war period, see Heilbron (1977); Falconer (1985).

[93] Rutherford to Bohr, 18 February 1920, RP.

[94] Thomson (1921b), 88, my emphasis. See also Thomson (1921a), and compare Pinch (1985).

> If we take ... two substances found by Mr Aston - 35 and 37 - we might imagine that the 37 was a compound of chlorine with two atoms of hydrogen. I know that a chemist would treat with derision a compound with that composition; so should I, if the hydride was formed under normal circumstances, but what I think is not sufficiently recognised is that chemical properties of charged atoms - *and it is charged atoms that occur in the discharge tube* - are very different from those of uncharged ones. For example we regard the normal chlorine atom as having seven electrons in the outer layer, and as being able, therefore, to take one hydrogen before it becomes saturated. Now if the chlorine atom becomes positively charged, it loses an electron, and has only six in the outer layer; it is now analogous to the normal atom of oxygen, and can take up two atoms of hydrogen before being saturated, and form the compound H_2Cl.

As another example of this kind of combination, Thomson cited the case of the compound H_3O whose line at mass 19 had often been found on positive ray photographs. Likewise it was "by no means impossible that the inert gases might, in the discharge tube be able to form compounds."[95] For good measure, Thomson also queried the variations in intensity of certain "isotopic" lines which Aston had seemed to gloss over, and questioned the accuracy of Aston's measurement technique and data reduction process:[96]

> [F]rom some points of view, the focus method which Mr. Aston uses is, I think, more liable to error than the older method. One reason for this is that the quantity which Mr. Aston has to measure is the position of the edge and not the middle of the line; and one feature of the photographs of the positive rays is that the lines broaden when the exposure is increased. A line which looks like a spider line for a short exposure may under exactly similar circumstances become quite thick when the time of exposure is increased.

Thomson's comments were not entirely negative, however. In an astute manoeuvre, he appropriated one of Rutherford's more recent hypotheses to give an alternative account of the neon "isotope" of mass 22. If Rutherford could fabricate a close combination of a proton and an electron which did not combine closely enough to form hydrogen (the speculative "neutron"), then Thomson could use exactly the same argument to account for the 22 line:[97]

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[95] Thomson (1921b), 90.
[96] *ibid.*, 92.
[97] Thomson (1921a), 215-216.

On the [Rutherford-Aston] view that the atoms of all the different chemical elements are built up of the same constituents, say atoms of hydrogen and helium, the atom of 22 would be that of 20 with the addition of a molecule of hydrogen, in this sense it might be called a compound of 20 and hydrogen, but whereas in ordinary chemical compounds the atoms of the different elements are separated by distances comparable with 10^{-8} cm, in "22" the 20 and H_2 are only separated by a very minute fraction of that distance.

Over the next few years, Thomson continued to advance his alternative interpretation of the 22 and 37 lines, while claiming that the similarity of isotopes and the difficulties in the way of their separation had been greatly exaggerated.[98] In that, at least, he was correct, as the work of many chemists would subsequently show.

Having been savaged in public by the President of the Royal Society, Aston was forced to attend to Thomson's criticisms in order to maintain the credibility of the mass-spectrograph. In order to rebut Thomson's analysis, but lacking the mathematical and analytical machinery to do so, he called upon the expert assistance of Ralph Fowler, Cambridge mathematical physicist and Rutherford's son-in-law. Fowler parried Thomson's attack by questioning the assumptions underlying his criticisms:[99]

[Thomson] discusses the focussing effect of the electric and magnetic fields deflecting in opposite directions, and assumes an ideal arrangement practically identical with the existing instrument. he points out that the emergent rays for each value of e/m must have a caustic, but that when (as here) rays of constant kinetic energy are selected only certain portions of the caustic will be touched by the existing rays, and the photographic plate must be placed so that it passes through the existing element of each caustic ... The order of approximation, however, which he uses is, we think, inadequate for the purpose ... so that criticism of the results based on this non-linearity cannot be regarded as of great weight.

Aston and Fowler believed that they had successfully seen off Thomson's challenge: "These considerations show that the theory and practice of this form of mass spectrograph

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98 See, for example, Thomson (1921a); Thomson (1923), esp. 14-17.
99 Aston and Fowler (1922), 519-520.

are in very satisfactory agreement, and present no anomalous and disturbing discordances."[100]

Nevertheless, Aston continued to modify various elements of the mass-spectrograph and his interpretative practice. In June 1922, for example, Rutherford told Bohr that "[t]he laboratory has been in a state of great excitement the last week, due to trying certain new kinds of photographic plates, which one makes in the laboratory. Aston finds them about six times faster and very much clearer for his positive rays."[101] Photographic plates proved to be crucial to what had become Aston's project: a systematic survey of the isotopes of the elements. After the first flush of success in 1920, a minor change in the manufacturing process by the Paget Plate Co., proprietary suppliers of photographic apparatus, produced greatly inferior mass-spectra, to Aston's intense dismay. He even commissioned a special batch of plates of the old design from the company so as to be able to continue his investigations in the style to which he had become accustomed. He also began a series of trials of his own on photographic plates. Trial-and-error produced some surprises. Using the specially treated plates Rutherford mentioned - ordinary plates converted into Schumann plates by dissolving away the gelatine - Aston found the results "successful beyond all expectations," revealing new isotopes of tin for the first time - a "lucky accident," as he put it.[102]

Bolstered by constant modifications in technique and improvements to the photographic detection apparatus, Aston and the mass-spectrograph maintained a continuous flow of results in the early 1920s. And while Thomson remained sceptical, Rutherford and other audiences - chemists, spectroscopists, astronomers - appropriated Aston's results for their own ends. Thomson was fighting a losing battle. In November 1922 it was announced that both Aston and Soddy had been awarded Nobel Prizes for Chemistry, Soddy receiving the reserved 1921 award and Aston the prize for 1922. At the same Stockholm ceremony, the 1921 and 1922 Physics Prizes were collected by Einstein and Bohr respectively.

[100] Aston and Fowler (1922), 521. Fowler received fulsome praise in the second (1924) edition of Aston's book *Isotopes*. See Aston (1924a), vi.
[101] Rutherford to Bohr, 5 June 1922, RP.
[102] Aston (1922e, 1922f, 1922g); Aston (1925b), 550.

Following the award of the Nobel Prize, Aston was celebrated with an even more prestigious accolade: a song, specially written and performed his honour at the annual Cavendish dinner in December:[103]

Since J.J. on the game began,
By analysing Neon,
Many a speculative man
Had isotopic thoughts which ran
Beyond a paper's rightful span,
So all did this agree on -
It needs a man both strong and stout
These isotopes to sever;
Of this there is no possible doubt,
No probable, possible shadow of doubt,
No possible doubt whatever.

So Aston made a cute 'machine'
For atom separations.
The atoms passed through fields serene,
Magnetic poles they went between,
And made some marks upon a screen,
Apart from their relations
The numbers whole were soon made out
By methods neat and clever;
Of this there was no manner of doubt,
No probable, possible shadow of doubt,
No possible doubt whatever.

Lyrics: E.C. S[toner]
Music: Gilbert and Sullivan's "The Highly Respectable Gondolier."

103 Reprinted in Cockburn and Ellyard (1981), 40.

4. Mapping the Geography of the Atom: Radioactivity at the Cavendish Laboratory, 1919-23

In the previous chapter, I indicated that Rutherford's Bakerian Lecture of 1920 was more than a mere summary of his own research. Concluding as it did with an extended series of speculations on the structure of the nucleus, based on information from disintegration experiments, the mass-spectrograph, the cloud chamber and other techniques, it was also a statement of his programme of research for the Cavendish Laboratory, a manifesto for radioactivity. Having undertaken an archaeology of the social, material and conceptual basis underlying research at the Cavendish in the immediate post-war years, it is to the practical elaboration of that programme, to an analysis of the ways in which those resources were put to work, that I now turn.

4.1 Experimental Searches for 'Neutrons'

One of the tentative speculations arising from Rutherford's discussion of the existence and structure of the new X_3^{++} disintegration particle had been the suggestion by analogy that a single electron might be capable of binding three, two or perhaps even one proton. While the hydrogen atom was pictured as consisting of a single proton and an electron, Rutherford had something a little different in mind, a sort of imploded hydrogen atom:[104]

> On present views, the neutral hydrogen atom is regarded as a nucleus of unit charge with an electron attached at a distance, and the spectrum of hydrogen is ascribed to the movements of this distant electron. Under some conditions, however, it may be possible for an electron to combine much more closely with the H nucleus, forming a kind of neutral doublet. ... The existence of such nuclei may not be confined to mass 1 but may be possible for masses 2, 3, or 4, depending on the possibility of combination between the doublets.

[104] Rutherford (1920a), 397 [CPR 3, 34].

As to the properties of such neutral doublets and the possibility of their detection, Rutherford was able to speculate very precisely in terms of contemporary laboratory technique:[105]

> Such an atom would have very novel properties. Its external field would be practically zero, except very close to the nucleus, and in consequence it should be able to move freely through matter. Its presence would probably be difficult to detect by the spectroscope, and it may be impossible to contain it in a sealed vessel. On the other hand, it should enter readily into the structure of atoms, and may either unite with the nucleus or be disintegrated by its intense field, resulting possibly in the escape of a charged H atom or an electron or both.
>
> If the existence of such atoms be possible, it is to be expected that they may be produced, but probably only in very small numbers, in the electric discharge through hydrogen, where both electrons and H nuclei are present in considerable numbers.

The first graduate student to be set the task of searching for evidence of these neutral doublets was J.L. Glasson, an Australian who had come to Cambridge before the war to work with Thomson. If the neutron were supposed to be a proton and an electron in close combination, reasoned Glasson and Rutherford, then the most likely place to search for them was in the positive rays in a discharge tube, since such a tube would contain "a plentiful supply both of free hydrogen nuclei and of electrons."[106]

The detection of such 'neutrons' was more of a problem. They would most likely be very stable. Because of their small size and "restricted external field" they would be extremely penetrating. The method employed by Glasson was therefore to allow any putative neutrons to fall on mercury and lead atoms, then attempt to detect any secondary ionization. Three arrangements were developed. The first used a mercury-filled ionisation chamber, but this was found to be insufficiently sensitive. The second method was "an ordinary α-ray electroscope with thick lead lining provided with a thin window either of lead or of platinum." The third arrangement consisted of a zinc-sulphide screen "such as is used for the detection of α-particles." Despite many long runs with the tube at all stages of

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105 *ibid.*
106 Glasson (1921), 597.

hardness, however, Glasson reported defeat at the end of the year: "no evidence was obtained, by any of the three methods, of a radiation capable of penetrating .005 cm of lead. The present experiments," he concluded, "have not given any evidence of the existence of particles *of the nature anticipated.*"[107]

The next academic year saw another attempt by a graduate student to detect 'neutrons.' J.K. Roberts was an 1851 Scholar of the University of Melbourne. He had been awarded the Scholarship in 1920, and had come to Cambridge specifically to work with Rutherford.[108] Rather than attempt to duplicate Glasson's approach, Roberts placed the discharge tube inside a calorimeter, and compared the heat produced against the energy supplied, expecting to find an excess of the former over the latter if exothermic combination of protons and electrons took place. He found, however, that the two quantities agreed "to less than ½ per cent, which was within the error of the experiment."[109] He raised the possibility that "if the close combination of an electron and a hydrogen nucleus did occur, the energy evolved might not appear directly as heat, but might come out in the form of a very penetrating γ-ray, which would escape detection in ordinary experiments."[110] This suggestion was taken up by Chadwick in 1923 in yet another attempt to detect collapsed hydrogen atoms. This effort, however, met with the same failure as its predecessors.[111] Undeterred - the neutron had been a speculative entity, after all - Chadwick turned all his energies to the disintegration experiments.

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107 *ibid.*, 600, my emphasis.
108 On Roberts, see Rutherford to Laby, 2 November 1920, RP: "I have arranged for [Roberts] to start what I consider an important research in which his former experience with you in heat measurement will be useful. As a preliminary I have got him to take a course in radio-activity, so that he will not leave the laboratory without some practical information on that subject." Also see Rideal (1944).
109 Roberts (1922), 73. In view of the negative result in terms of the stated aim of the experiment, it is of interest to note that Roberts made the best of the exercise by turning into a verification of the conservation of energy through a systematic analysis of possible sources of error.
110 Roberts (1922), 74.
111 Chadwick tried to detect the emission of gamma radiation from neutron formation in a mass of hydrogenous material. He persisted with variations on this theme for a year or more, but finally gave up when the disintegration experiments he was carrying out with Rutherford (see below) became too pressing on his time. See Chadwick (1969), 35 ff.

4.2 Disintegration Experiments, Laws of Force and the Mysterious X_3^{++}

In the summer of 1920 Rutherford recapitulated his recent work for Boltwood: "I have evidence that oxygen and nitrogen are disintegrated by collisions with α-particles giving rise to an atom of mass 3 and carrying two charges, which should be an isotope of helium. Nitrogen also breaks up another way with the emission of hydrogen atoms. Recently I believe I have found that carbon and possibly fluorine break up the same way as oxygen, but it will take a great deal of experiment to prove this definitely."[112] It was to the disintegration experiments that Rutherford - and Chadwick - now turned their attention.

The experiments relied upon the scintillation technique. While in principle the method was a simple one - the observer had simply to count disintegration protons as they fluoresced on a zinc sulphide screen seen through a magnifying system - the work was in practice beset with difficulties. The results of the delicate counting experiments were liable to vitiated by the presence of hydrogen impurities, or, worse, by radioactive contamination, which could throw a whole day's work into doubt. Nor was the scintillation counting itself a straightforward task, for consistency and reliability in the counting of weak flashes were difficult to achieve. As figs. 2.4 and 2.5 show, for example, the counting experiments often produced inconsistent and indecipherable results. Wild variations in the reported counts could sometimes be ascribed to radioactive contamination of the counting apparatus, in which case experiments would cease and the day's work would be lost. The extant notebooks are full of entries, almost every other page, like "Contamination ... Results not definitive ... No detectable H particles ... The screen was very bright - certainly brighter than we should use in disintegration counting ... Doubtful," and so on.[113] At other times, however, it was the observers who seemed to be at fault. In December 1921, for example, the laboratory notebook contains the curtly dismissive remark "Ellis's counts most uncertain and rejected" (fig. 2.5).[114]

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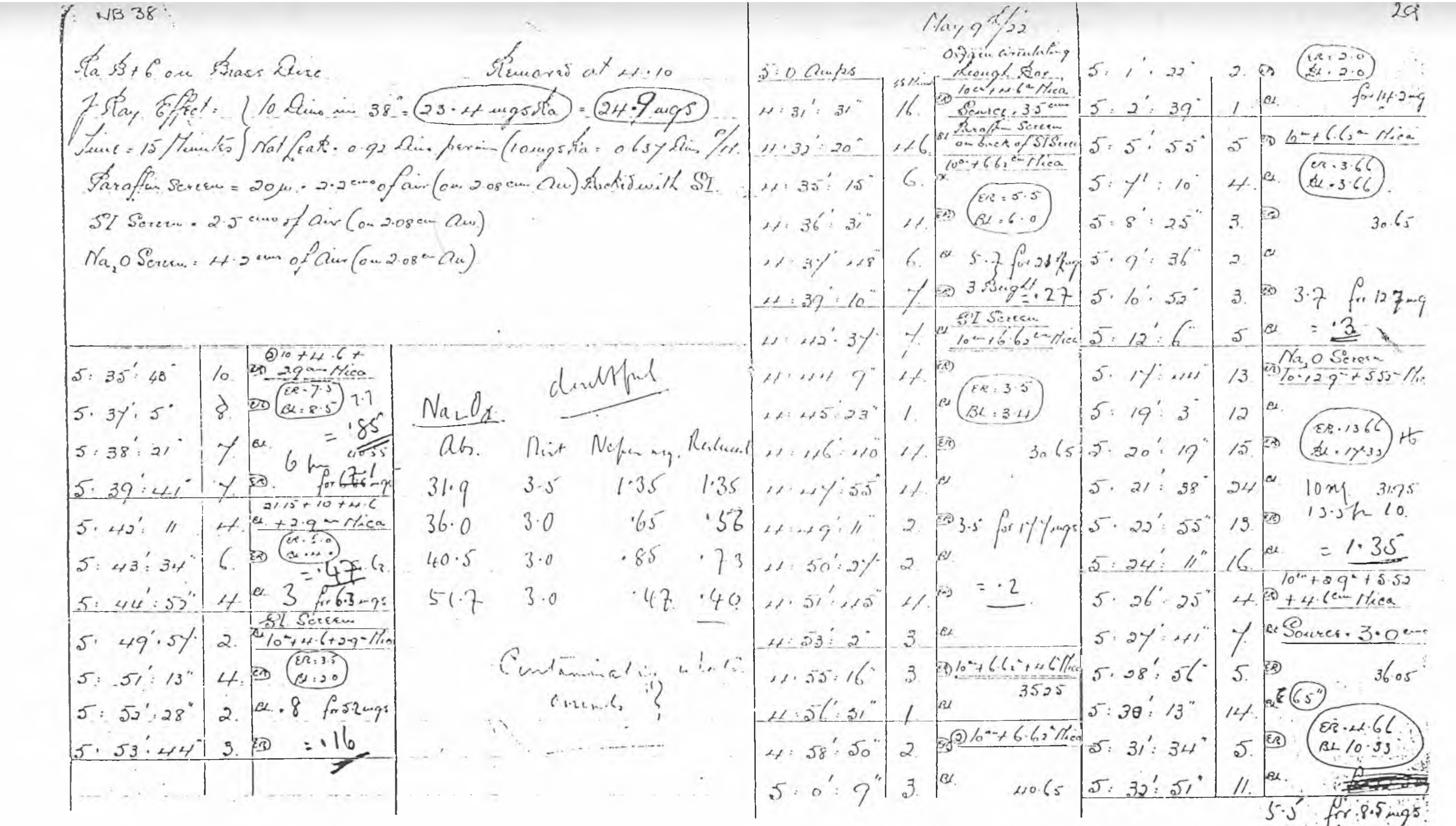
112 Rutherford to Boltwood, 19 August 1920, in Badash (ed.)(1969), 329-331, 329.
113 See, for example, laboratory notebook NB 38 (May 9, 1922), RP.
114 Laboratory notebook NB 37 (December 19, 1921), RP. Ellis' crime here was his consistent tendency to produce significantly lower counts than Chadwick in a trial where they alternated as counters. On the other


NB 38
29
May 9th/22
Ra B+C on Brass Disc Removed at 4.10
γ Ray Effect: (10 Divs in 38" = (23.4 mgs Ra) = (24.9 mgs)
Time = 15 Minutes) Nat Leak = 0.92 Div per min (10 mgs Ra = 0.637 Div/m.
Paraffin Screen = 20μ = 2.2 cms of air (on 2.08 cm Au) Backed with SI.
SI Screen = 2.5 cms of Air (on 2.08 cm Au)
Na2O Screen = 4.2 cms of Air (on 2.08 cm Au)

5:35'48" 10 ⓒ 10+4.6+29 cm Mica ER 7.5 BL 8.5 7.7
5.37'5" 8 ER
5.38'21" 7 BL = .85 6 for 7.1 mgs
5.39'41" 7 ER
5:42'11" 4 BL 21.15+10+4.6+2.9 cm Mica ER 5.0
5:43'34" 6 ER = .47
5:44'53" 4 BL 3 for 6.3 mgs
5:49'57" 2 BL SI Screen 10+4.6+29 cm Mica
5:51'13" 4 ER ER 3.5 BL 2.0
5:52'28" 2 BL 8 for 5.2 mgs
5:53'44" 3 ER = .16

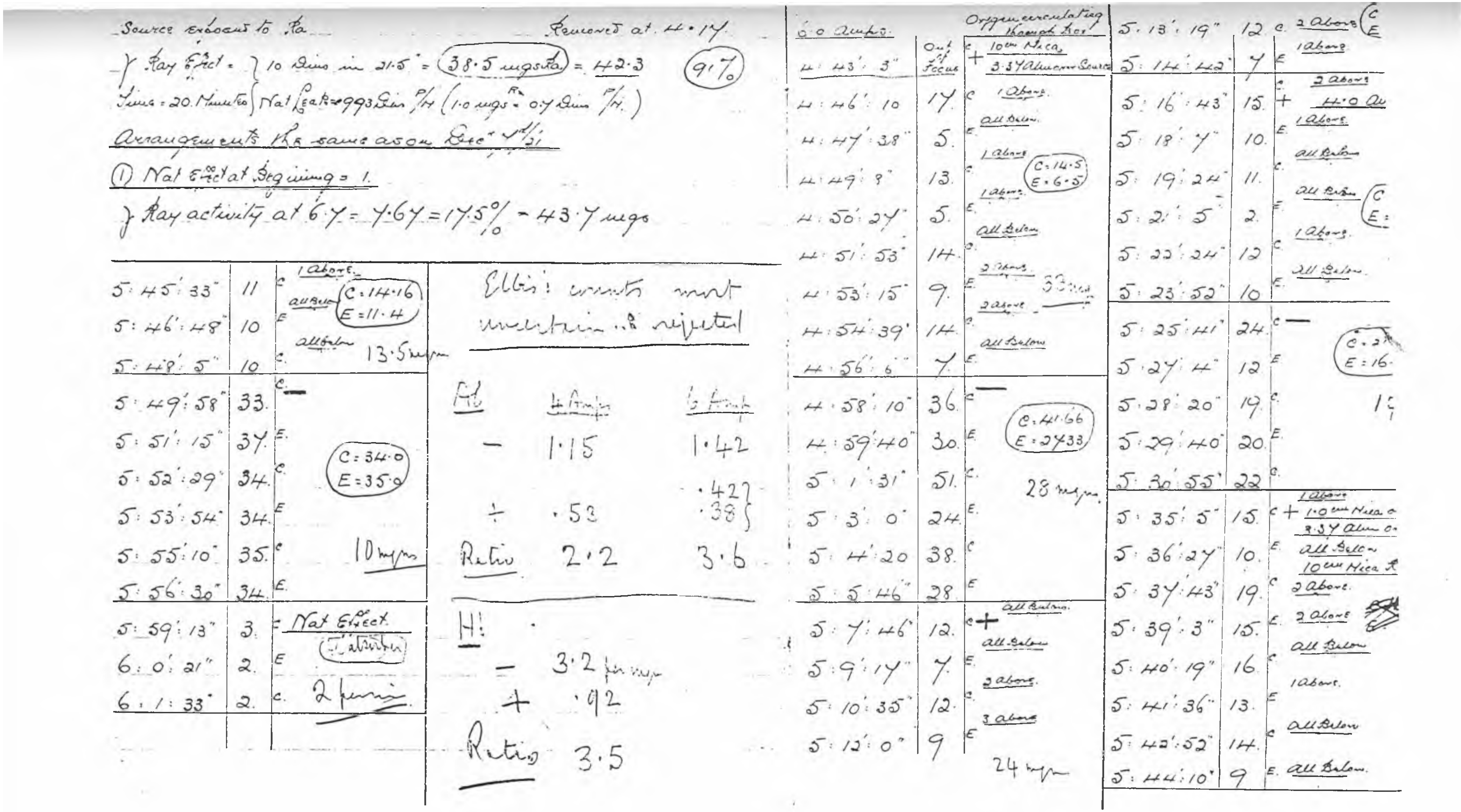
Na2Ox doubtful
Ab. Nat Ne per mg. Reduced
31.9 3.5 1.35 1.35
36.0 3.0 .65 .56
40.5 3.0 .85 .73
51.7 3.0 .47 .40
Contamination in later counts?

5.0 Amps
4:31'31" 16 ER Oxygen circulating through Box. 10 cm + 4.6 cm Mica Source 3.5 cm
4:32'20" 16 BL Paraffin Screen on back of SI Screen 10 cm + 6.62 cm Mica
4:35'15" 6 BL
4:36'31" 11 ER ER 5.5 BL 6.0
4:37'48" 6 BL 5.7 for 28 mgs
4:39'10" 7 ER 3 Bugs = .27
4:42'37" 7 BL SI Screen 10 cm + 6.62 cm Mica
4:44'9" 14 ER
4:45'23" 1 BL ER 3.5 BL 3.4
4:46'40" 4 ER 30.65
4:47'55" 4 BL
4:49'11" 2 ER 3.5 for 17 mgs
4:50'27" 2 BL
4:51'45" 4 ER = .2
4:53'2" 3 BL
4:55'16" 3 ER 10+6.62+4.6 Mica 35.25
4:56'31" 1 BL
4:58'50" 2 ER ⓒ 10+6.62 Mica
5:0'9" 3 BL 40.65

5:1'22" 2 ER ER 2.0 BL 2.0
5:2'39" 1 BL for 4.2 mg
5:5'55" 5 ER 10+6.62 Mica
5:7'10" 4 BL ER 3.66 BL 3.66
5:8'25" 3 ER 30.65
5:9'36" 2 BL
5:10'52" 3 ER 3.7 for 12.7 mg
5:12'6" 5 BL = .3
5:17'44" 13 ER Na2O Screen 10+2.9+5.52 Mica
5:19'3" 12 BL
5:20'19" 15 ER ER 13.66 BL 17.33 H
5:21'38" 24 BL 10 mg 31.75
5:22'55" 13 ER 13.5 for 10.
5:24'11" 16 BL = 1.35
5:26'25" 4 ER 10+2.9+5.52+4.6 cm Mica
5:27'41" 7 BL Source 3.0 cms
5:28'56" 5 ER 36.05
5:30'13" 14 BL E 6.5"
5:31'34" 5 ER ER 4.66 BL 10.33
5:32'51" 11 BL
5.5 for 8.5 mgs = .65

Fig. 2.4 Rutherford, laboratory notebook, 9 May 1922. Scintillation-counting experiment to determine effect of Radium B+C α-particles on sodium. Timings refer to time at beginning of 30-second counting periods. Counters Rutherford and [?]Blackett. Annotated [in Chadwick's hand?] 'Contamination in later counts?' and 'doubtful.'

Source: NB 38, RP.



Source exposed to Ra Removed at 4.17

γ Ray Effect = 10 Divs in 21.5" = 38.5 mgs Ra = 42.3 9%

Time = 20 Minutes Nat Leak = 993 Div P/H (1.0 mgs = 0.7 Div P/H)

Arrangements the same as on Dec 7/21

(1) Nat Effect at Beginning = 1.

γ Ray activity at 6.7 = 7.67 = 17.5% = 43.7 mgs

Ellis: counts most uncertain & rejected

Al	4 Amps	6 Amps
−	1.15	1.42
÷	.53	.427 .38
Ratio	2.2	3.6

H!

− 3.2 per mg

+ .92

Ratio 3.5

Fig. 2.5 Rutherford laboratory notebook, 9 December 1921. Scintillation-counting experiment, counters Chadwick and Ellis. Ellis produces a series of counts radically different from those of Chadwick (circled total figures in results tables). The day's summary is annotated [by Chadwick]: "Ellis' counts most uncertain [and] rejected."

Source: NB 37, RP.

So rarely was consistency achieved, in fact, that a "good agreement with previous results" was an achievement worthy of special note (fig. 2.6).[115] It was largely to assist in the practical management of these difficulties that Rutherford had co-opted Chadwick into the disintegration experiments early in 1920. Now, with a clear programme of work ahead, Chadwick and Rutherford commissioned a new optical system from Hilger & Co. in order to improve the detection of weak scintillations due to particles near the end of their range. This had the desired effect of making the counting of scintillations "much easier and more certain."[116] In addition to the new microscope, Chadwick introduced additional measures in an attempt to improve the reliability of the scintillation method. He imposed a strict discipline to make each observer "count with consistency." The discipline consisted in "(1) suitable dark adaptation of the eye (2) counting for 1 minute only at a time (3) with at least 1 minute's rest between counts, and in addition complete concentration during the 1 minute of counting."[117] The most consistent results seemed to be obtained with the apparatus arranged to yield about 40 scintillations per minute. More than about 80 or less than about 10 made the counting especially "troublesome and uncertain." With a strictly enforced discipline of counting and the new microscopes, Rutherford and Chadwick were able to report, "we have found the counting results much more concordant than with the old, and observations taken at six months interval have been found in good agreement."[118]

Certitude was also deemed to rest on the number of particles counted: the more counts one made, the more certain one could be of the results. Rutherford reasoned that "[o]n account of the probability variations in the number of particles falling on the zinc sulphide

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hand, Rutherford's scintillation counts were "often much higher than those of other observers." See Osgood and Hirst (1964), 682; Pollard (1991), 31.

[115] Laboratory notebooks NB 36 (8 August 1921); NB 38 (16 March 1922), RP. Compare the discussion of 'data archives' in Lynch (1991b), esp. 102-103.

[116] Or, at least, could be presented as such in the public domain. See Rutherford and Chadwick (1921b), 809 [CPR 3, 48].

[117] Chadwick to R.G. Stansfield, 5 August 1972, MISC 47, CCAC.

[118] The counting discipline was also closely linked to the strength of the radioactive source which could be employed in the experiments. Rutherford and Chadwick recorded that "In order to reduce the luminosity of the zinc-sulphide screen due to the γ rays , it is important to employ a thin and finely powdered layer. With the use of a strong magnetic field to turn aside the β rays, we have found it feasible to count the scintillations with a source of radium C of activity equivalent to 20 mgs." Rutherford and Chadwick (1921b), 810-811 [CPR 3, 49].

NB 38

March 16/22

Radium B + C on 4.15 am Ag — Removed at 4.11

γ Ray Effect = } 10 Div in 42" = (20.8 mgs Ra) = 22.5 mgs

Time 14 Minutes { Nat Leak = 1.0 Div, " 1.0 mgs Ra = 0.637 Div, " }

Expt with C_2N_2 — C_2N_2 Screen = 7.25 mgm per sq cm

5.0 Amps			
4 : 34' : 30"	23.	C	Oxygen circulating through Box. 2.9 cm Mica. Source = 3.5 cms. C_2N_2 Screen Backed
4 : 35' : 52"	21.	E	
4 : 37' : 19"	28.	ER	ER = 26.33, C = 26.0, E = 20.33
4 : 38' : 33"	27	C	
4 : 39' : 48"	19.	E	22.4 for 13.5
4 : 41' : 5"	23	ER	
4 : 42' : 19"	23.	C	= 1.
4 : 43' : 34"	21.	E	
4 : 44' : 54"	28.	ER	
4 : 47' : 33"	9.	C	C_2N_2 Removed. All else the same.
4 : 48' : 56"	10.	E	
4 : 50' : 11"	9.	ER	ER = 10.0, C = 8.66, E = 9.33
4 : 51' : 24"	12.	C	
4 : 52' : 41"	11.	E	9 for 15.5
4 : 53' : 54"	14	ER	= .49
4 : 55' : 10"	5.	C	
4 : 56' : 24"	37	E	
4 : 57' : 43"	7.	ER	

5 : 1' : 37"	6.
5 : 2' : 53"	5
5 : 4' : 9"	10
5 : 5' : 25"	9.
5 : 6' : 43"	7
5 : 7' : 57"	3.
5 : 10' : 32"	11.
5 : 11' : 47"	14.
5 : 13' : 0"	10.
5 : 15' : 56"	8
5 : 17' : 12"	10
5 : 18' : 23"	7.
5 : 21' : 47"	21.
5 : 23' : 7"	20

Good agreement with previous results.

C_2N_2

Absorptn	No per mg at 3.5
13.6	1.35
14.3	1.2
Naturals.	
14.3	.49.
13.6	.4
Brass.	
14.3	.4
13.6	.58

Fig. 2.6 Rutherford laboratory notebook, 16 March 1922. Scintillation-counting experiment with three counters: Rutherford, Chadwick and Ellis. Annotated [by Chadwick] 'Good agreement with previous results.'

Source: NB 38, RP.

screen, a large number of scintillations must be counted to obtain the true average. For example, if 400 scintillations are counted in all, the average proability error is √400, or 5 per cent of the total number. We cannot hope in the experiments ... to obtain results of an accuracy of more than 5 or 10 per cent. unless a very large amount of time and energy is spent in counting a great number of scintillations."[119] Chadwick therefore introduced a third innovation: "In order to count as many particles as possible during an experiment, two counters were always used who counted alternately for a period of one minute each. An additional observer made the necessary adjustments and recorded the data."[120] The introduction of a second counter working in parallel with the first effectively doubled the number of runs which could be carried out in a counting session, and, perhaps more importantly, enabled each counter to act as a 'check' on the other's results. Based on their performance in trials arranged as part of the 'attic' course, research students were enrolled to serve as counters under Chadwick's direction. In the year 1921-2, for example, the two counters were Etienne Bieler and Charles Ellis. When Bieler returned to Canada in 1922, he was replaced by P.M.S. Blackett. These keen-eyed young researchers undertook the counting experiments in addition to their own researches, and were usually rewarded with a cursory note of thanks in Rutherford and Chadwick's publications.[121]

How, then, were the counting experiments carried out in practice? How were doubt and confusion managed?[122] There were two pre-arranged sessions a week, from about four to six or six-thirty in the afternoon. Nothing was allowed to interfere with these sessions.[123] By four o'clock in the afternoon, when the performers - sometimes joined by Ralph Fowler

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119 Rutherford (1922a), 404.
120 Rutherford and Chadwick (1921b), 810 [CPR 3, 49].
121 Osgood and Hirst (1964), 682. See, for example, Rutherford (1920a), 400 [CPR 3, 38]; Rutherford and Chadwick (1921b), 825 [CPR 3, 62]. George Crowe, too, would be acknowledged for "preparing the radioactive sources." See "Profile. George Crowe. Best-known lab. assistant of his time," *New Scientist* 6 (24 September), 516-517, and compare Shapin (1989). A problem with these arrangements arose in 1921 when press stories described the dangers of X- and radium rays. In order to safeguard the counting experiments, Rutherford arranged for "all the men who do much work in this direction to have regular blood counts, so that they will not get unduly alarmed when they feel under the weather." Rutherford to R.W. Boyle, 23 April 1921, published in Eve (1939), 283-285, on 284. For press reports of the dangers of radiation, see Caufield (1990), 16-17, 29ff.
122 Compare Woolgar (1988), 30-35.
123 Osgood and Hirst (1964), 683.

- assembled in Rutherford's research room, Crowe would have set up the apparatus, fixed the shutters on the windows and adjusted the electromagnet to deflect unwanted β-rays. While the radioactive source was being prepared by Chadwick elsewhere in the laboratory, tea and buns would be served by Rolfe, a laboratory assistant. Electric lights were then extinguished and a gas-burner near the door lit to provide faint background illumination. Ten or fifteen minutes would be allowed for dark-adaptation of the eyes, after which Chadwick would arrive with the freshly-prepared source. All was now ready for counting to begin.

Intense concentration was required for counting. To keep the eye focussed during periods of counting, a faint adjustable lamp was used to illuminate the screen. The students counted alternately for a minute at a time, the intervals being called by Chadwick (later a bell was rung automatically at one-minute intervals).[124] The counters were "seldom" informed about the nature of the particular experiment in hand, although they "naturally knew in general what was happening." As a further precaution, individual counts were not revealed until the end of a short series in an effort to avoid 'bias,' though Rutherford and Chadwick "always had a good idea of the order of magnitude of the counts to be expected in any particular experiment." In the event that some planned alteration to the experimental arrangement required that the lights be turned on, the counters retired to a small wooden closet on one side of the room in order to protect "the investment of time that had already been made in getting our eyes adapted to the dark." They would emerge only after the alterations had been made and the lights extinguished once again.

With these elaborate precautions (which quickly became routinised and taken for granted within the Cavendish), Rutherford and Chadwick became more confident of the results of the counting experiments. Assured of their ability to produce consistent and reliable results with the modified and disciplined scintillation technique, they announced a series of new results in the November 1921 number of the *Philosophical Magazine*. Taking special precautions to exclude the possibility of "natural" H-particles reaching the scintillation

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124 Blackett (1933), 77.

screen, they had found that Radium C α-particles could expel long-range disintegration particles from a further five elements: boron, fluorine, sodium, aluminium and phosphorus.[125] A further sixteen elements, including carbon and oxygen, showed no such effect. Although they had not yet been able to confirm it, they assumed that these disintegration products were H-particles, as in the case of nitrogen. While such particles were expelled predominantly in the forward direction, in the case of aluminium the spatial distribution of the disintegration particles appeared "to a large extent independent of the direction of the impinging α particles," although the backwards-directed protons seemed to have a shorter range than the forward-directed ones.[126]

A significant feature of these results, they pointed out, was that all the disintegrable elements had an odd atomic number and an atomic weight of the form $4n+2$ or $4n+3$, where n is an integer - an observation which made sense if one considered Rutherford's speculative nuclear models, in which atomic nuclei consisted of helium nuclei of mass 4 and hydrogen nuclei.[127] Noting, lastly, that the energy of escape of the disintegration H-particle was nearly proportional to the energy of the incident α-particle, Rutherford sought to provide a model which would account for all the facts. He arrived at a scheme in which the helium nuclei constituted a central core, around which the H-particles circled as more distant satellites (fig. 2.7).[128] The expulsion of an H-particle could now be thought of as resulting from a collision between the impinging α-particle and one of these satellites, and the observed distribution and energy characteristics of the disintegration particles could also, it seemed, be accounted for.

Two consequences followed from the adoption of such a model. The existence of satellites of the kind postulated would require firstly "that positively charged bodies attract one another at the very small distances involved ... in order to hold the ordinary composite nucleus in equilibrium," implying a change of sign of the force acting on the α-particle near

125 Rutherford and Chadwick (1921a, 1921b).
126 Rutherford and Chadwick (1921b), 816-819 [CPR 3, 54-56].
127 *ibid.*, 819-821 [CPR 3, 56-59].
128 *ibid.*, 821-824 [CPR 3, 59-61]. For an excellent account of the genesis and development of Rutherford's satellite model, see Stuewer (1986a).

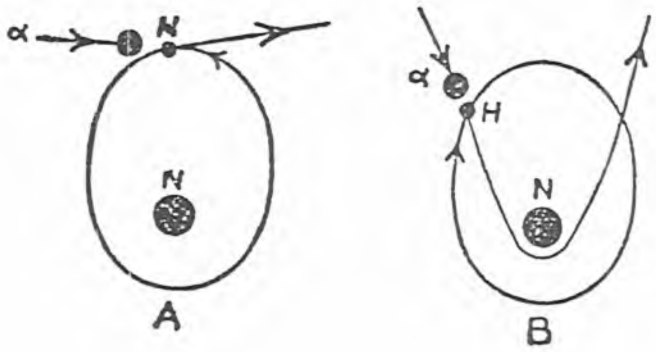

Fig. 2.7 Rutherford's satellite model of the nucleus, 1921, illustrating a mechanism for artificial disintegration of nitrogen nucleus. H is a proton orbiting N, the nitrogen nucleus, α the impinging α-particle. Impact at different points of the proton's orbit produces disintegration particles with particular spatial and velocity characteristics.

Source: Rutherford and Chadwick (1921b), 822 [CPR 3, 60].

the nucleus.[129] Secondly, the mass of the H satellite should be close to that of the free H nucleus. Aston and his mass-spectrograph again became crucial elements in the interpretative strategy. Supposing the nitrogen nucleus to be formed from that of carbon plus two H satellites and one electron, "it is to be anticipated that the mass of the nitrogen atom should be 14.016 nearly, assuming C=12.00, H=1.008 in terms of O=16." This opened up the prospect of an independent test of the satellite model, for "[b]y accurate experiment with positive rays by Aston's method, it should be possible to decide whether the atomic mass of nitrogen is nearer this calculated value than the whole number 14."[130] Unfortunately, as I showed above, the mass-spectrograph was incapable of producing measurements of sufficient accuracy to be of use. While Aston began to turn his thoughts to the construction of a more powerful machine capable of yielding the kind of information his patron required, Rutherford and Chadwick moved on to the question of X_3^{++}.

When it had first seemed as if he had discovered a new disintegration particle, Rutherford began a search for X_3^{++} from radioactive sources in the hope of obtaining "a more direct method of determining the mass of the new atoms with accuracy, since they would be emitted in number from the radioactive source instead of from the volume of the gas bombarded by the α rays."[131] A prime site for the search seemed to be the anomalous long-range particles from thorium C. In 1914, Rutherford and Alex Wood had found that in addition to the α-particles of range 5.0 and 8.6 cm, thorium C also emitted a small number of long-range particles which travelled 11.3 cm. Rutherford now wondered whether, in virtue of their long range, these long-range particles were in fact the new helium nuclei of mass 3. While Wood repeated and confirmed the original investigation at Rutherford's request,[132] Rutherford and his team set out to examine the deflection of the long-range thorium C particles in a magnetic field by the scintillation method.

They faced an immediate constraint in the shortage of appropriate radioactive materials. At the end of his first term in Cambridge, Rutherford had written to Boltwood that he was

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129 Rutherford and Chadwick (1921b), 824-825 [CPR 3, 61].
130 *ibid.*, 825 [CPR 3, 62]; Rutherford (1922a), 412-413.
131 Rutherford (1921b), 570 [CPR 3, 43]. See also Stuewer (1986a), 327.
132 Wood (1921); Smekal (1921).

"very anxious to get a reasonable quantity of Meso-Thorium of the order of 10-20 milligrammes."[133] He had already tried to tap Geiger for material, but he had been told that there was "nothing to be got in Berlin." Boltwood offered to sound out his friends in industry, and clearly performed the task well, for Rutherford was able to choose between two offers. At first he ordered £400-worth of Mesothorium from H.S. Miner, chief chemist of the Welsbach Light Company in New Jersey and a close friend of Boltwood.[134] Within days, however, he had cancelled the order in favour of an offer from his colleague H.N. McCoy, who "generously offered 10 milligrammes of Mesothorium" at two-thirds the price demanded by the Welsbach Co.[135] McCoy proved to be a valuable connection. After some delay, in September 1920, he forwarded the mesothorium and a quantity of radiothorium with "a Gamma ray activity equal to two or three mgs. of radium element," refusing to accept payment, and insisting that Rutherford "consider the material I am sending you as a gift for the good of the cause, since I am sure that you can use the funds for the purchase of apparatus."[136]

Using the materials supplied *gratis* by McCoy and worked up by Chadwick, Rutherford's group compared the deflection of the long-range particles with that suffered by α-particles. They found that the long-range particles behaved exactly like ordinary helium nuclei. Rutherford was forced to concede that the experiments "negative the idea that that particles of mass 3 are ejected from thorium C."[137] There was also a much more serious problem. By repeating the magnetic deflection experiments using "a more direct and simpler method," Rutherford had "convinced [himself] that, at any rate in the case of oxygen, the [long-range particles of mass 3] have their origin in the radioactive source and not in the volume of the surrounding gas." Under these conditions, he confessed, "the comparative method of estimating the mass of the particles is no longer trustworthy."[138]

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133 Rutherford to Boltwood, 4 December 1919, in Badash (ed.)(1969), 321-322, 321.
134 Rutherford to Boltwood, 26 January 1920, *ibid.*, 321.
135 Rutherford to Boltwood, 5 February 1920, *ibid.*, 326. Rutherford also decided to spend only £200 on the material.
136 McCoy to Rutherford, 11 September 1920, CUL.
137 Rutherford (1921b), 573 [CPR 3, 46].
138 Rutherford (1922a), 413 [not in CPR].

X_3^{++} could no longer be regarded as a nuclear constituent. The models Rutherford had presented in his Bakerian Lecture were incorrect.

The search for particles of mass 3 ended for the moment, but the episode described here is emblematic of one of the most significant problems facing Rutherford in the interpretation of his experiments. It was extremely difficult to distinguish between particles from the radioactive source, especially if they had an abnormally long range, and the disintegration particles which were the ostensible object of the experiments.[139] Postponing further work on the long-range particles to "a more convenient season,"[140] Rutherford and Chadwick returned to the disintegration experiments. In order to distinguish the wheat from the chaff, however, they needed to know with certainty where the long-range particles actually came from. In the autumn of 1922 two research students, L.F. Bates and J.S. Rogers, were therefore set the task of studying the long-range particles from Radium C, the source most commonly used in disintegration work.[141] Using the scintillation method, and taking advantage of the improved optical arrangements introduced by Chadwick, Bates and Rogers found a large number of previously undetected long-range particles, including particles of ranges 11.2 and 13.3 cm from radium C, of 15.0 and 18.4 cm from Thorium C.[142]

But doubt persisted in Rutherford's mind. The scintillation experiments were designed to detect disintegration particles with ranges much larger than those of the primary α-particles. If a "massive particle" were liberated in a disintegration, its range would quite likely be shorter than that of the primary α-particle, and it would therefore escape detection by conventional methods. Fortunately, other techniques might be pressed into service. "To examine cases of this kind," Rutherford told the Chemical Society in February 1922, "we can utilize the beautiful method developed by Mr. C.T.R. Wilson for showing the trails of

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139 Cf. Pinch (1985, 1986).
140 Rutherford and Chadwick (1924c), 511 [CPR 3, 122].
141 On Bates, see Kurti (1983). On Rogers, see File ii/6, 1851 Exhibition Archives, ICL. After his time at the Cavendish, Rogers returned to Melbourne, where he became Senior Lecturer in Natural Philosophy and Physics (1924-40), while Bates took posts at University College, London (1924-36) and University College, Nottingham (1936-1964).
142 Bates and Rogers (1923, 1924).

ionizing particles."[143] As so often in the Cavendish, where one method proved capricious or unreliable, another could provide a fresh line of attack.

4.3 The Shimizu Reciprocating Cloud Chamber

One of Rutherford's first research students at the Cavendish was Takeo Shimizu, a Japanese who had come from Harvard, where he had worked on lead isotopes with Duane.[144] In Cambridge, Shimizu had started work on the cloud chamber technique in an effort to obtain "a very sensitive method of detecting ionising rays, such as X-rays and rays from radio-active substances ... [so as] to attack certain problems relating to the structure of aetherial waves and other delicate questions."[145] Rutherford redirected his research towards the problem of the anomalous collisions between α-particles and nitrogen.[146]

The cloud chamber, invented by C.T.R. Wilson at the Cavendish some years earlier, had quickly become an important tool for the study of ionising radiations.[147] In his influential 1913 treatise *Radioactive Substances and their Radiations*, for example, Rutherford had described the expansion method as "one of remarkable power," promising to "throw much light on the distribution and nature of the ionisation produced by the radiations."[148] In particular, its capacity to yield publishable photographs did much to establish it as a promising tool. From 1913, moreover, the device had been marketed by the Cambridge Scientific Instrument Company as the 'Wilson Expansion Apparatus,'[149] soon finding use in several laboratories across the globe.

Despite this commercial success, however, the method was essentially an elaborate and

143 Rutherford (1922a), 413.
144 Duane and Shimizu (1919). The relationships between Japanese, American and European scienctific traditions are discussed by Bartholomew (1989), esp. 238 ff. See also Hirosige (1964); Koizumi (1975).
145 Shimizu (1921b), 435.
146 Blackett (1969), xxxiv.
147 Wilson (1913); Cattermole and Wolfe (1987), 249 ff.; Galison and Assmus (1989).
148 Rutherford (1913), 663.
149 'The Wilson Expansion Apparatus,' Cambridge Scientific Instrument Co. Catalogues 1912-15, List No.217 [August 1913], 1-3, CUL.

time-consuming one, demanding much labour for little reward. In an investigation such as that now proposed by Rutherford, it was fairly clear that large numbers of photographs would have to be taken to capture so rare a process as disintegrative collision. Shimizu therefore designed a reciprocating expansion chamber capable of 50-200 expansions per minute. The apparatus was similar in design to Wilson's original, differing chiefly in the operation of the piston. In the Wilson chamber, the position of the plunger was adjusted before each expansion according to the expansion ratio desired, and the air pressure on the plunger allowed to press it down suddenly. In the Shimizu apparatus, a mechanical connection was made from a "prime mover" to the piston, which now made a reciprocating motion between two definite positions. A commutator regulated an electric field between the upper and lower surfaces of the chamber "in synchronism with the piston."[150] Shimizu found the new reciprocating apparatus "very convenient for taking a large number of photographs within a reasonable time" - just as well, since, according to Rutherford, only one in a hundred thousand α-particles from radium C passing through air would undergo a close nuclear collision, producing a disintegration product.[151] Taking a large number of photographs of α-ray tracks might therefore produce "some evidence to indicate the disruption of atoms by the α-particles."[152]

The first results had been available in time for the Bakerian Lecture. Rutherford reported that "both Shimizu and myself saw on several occasions what appeared to be branching trails of an α-particle in which the lengths of the two tracks were comparable." Such "eye observations" were "too uncertain to regard them with much confidence," however, so "[a]rrangements were made to devise a suitable method of photographing such tracks and to show their orientation in space."[153] Peering into a cloud chamber for long periods of time would quickly induce fatigue. If a reliable photographic record could be obtained, it could be taken away from the instrument and analysed at leisure. And as with the mass-

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[150] Shimizu (1921a), 426 ff.
[151] Shimizu (1921b), 432; Rutherford (1920a), 385 [CPR 3, 24].
[152] Shimizu (1921b), 432.
[153] Rutherford (1920a), 393 [CPR 3, 31]. For cogent discussion of the construction and objectification of emergent phenomena through negotiations between witnesses around an instrument, see Garfinkel, Lynch and Livingston (1981); Lynch (1985a), 202-273; Amann and Knorr-Cetina (1989, 1990); Woolgar (1990).

spectrograph, such a record could also be demonstrated - and published - in a way that direct output from the instrument could not.[154] A special camera was therefore designed to make exposures "automatically and synchronously with the apparatus." Cinematographic film was used, together with an optical system which allowed two mutually perpendicular images of the object to be photographed simultaneously - a variation on "an ordinary range finder."[155] The two mutually perpendicular images were placed side by side on the negative. Under favourable conditions, Shimizu reckoned it possible "to be able to get a thousand or more exposures in an hour." In practice, however, owing to "various difficulties" only 200 feet of film had been exposed after the first series of trials, and no disintegration reactions had been observed in the 3000 α-ray tracks thus photographed.[156]

Like Aston's mass-spectra plates, the cloud chamber photographs which graced the pages of the *Proceedings of the Royal Society* and the other scientific journals in the 1920s were heavily mediated representations of cloud chamber phenomena. Much work had to be done to present those particular facets of the phenomena which the reader was meant to 'see' as interesting. Shimizu explained in 1921 that in one of his illustrations,

> the two photographs are mutually perpendicular views of the same track of an α-ray emitted by polonium ... In this reproduction the end parts of the track were enlarged 13.2 times from the negative, cut out, and brought together to a convenient distance. The image on the negative itself was 0.42 times the size of the object, so that the true magnification of the reproduction is about 5.5.[157]

Judiciously selected and artfully assembled as they were, however, the tracks still lacked the persuasive force to ensure that they conveyed a single, self-evident message. In the early 1920s at least one publication used a marked-up tracing paper overlay in conjunction with a complex cloud chamber photograph as a means of displaying to the reader exactly

[154] Cf. Latour (1987), 64-74; Latour and Woolgar (1986)[1979], 45-53.
[155] Shimizu (1921b), 432; Rutherford (1920a), 393 [CPR 3, 31].
[156] Shimizu (1921b), 435.
[157] Shimizu (1921b), 434. On photography and the representation of phenomena, see Amann and Knorr-Cetina (1990); Lynch (1985a,1985b); Lynch and Edgerton (1988). Scharf (1974), Bolton (ed.)(1989), Tagg (1988, 1992) and Goldberg (1991) provide interesting points of departure for comparative analysis.

which tracks were to be noticed as significant. Through such artifices, the disengaged viewer was taught which tracks to see as important.[158]

Such strategies quickly became important, for, notwithstanding the difficulties of eliciting photographs of nuclear disintegrations, Shimizu's reciprocating chamber soon attracted interest outside the laboratory. The Cambridge Scientific Instrument Company helped Shimizu patent his design, and afterwards manufactured it under a royalty agreement with him, marketing the device as the 'Wilson-Shimizu Ray-Track Apparatus.'[159] In the C.S.I.C. production model of the Shimizu apparatus, the expansion chamber itself was very small in comparison to the earlier model - only 5.5 cm in diameter and 1 cm high. The advertised purpose of the new model was to display the tracks of ionising particles, a requirement which was built into the new design (fig. 2.8).[160] A stereoscopic camera was supplied as an optional accessory. In 1926, the company designed and marketed a simplified version of the Shimizu chamber for schools and colleges (fig. 2.9).[161] The C.S.I.C./Shimizu chamber proved to be a popular instrument both in schools and in laboratories. Twelve were constructed in March 1922, a further twelve in July 1923, another dozen the following April and a batch of twenty in 1925.[162] Within four years, over fifty chambers had been sold to researchers in Paris, Berlin and elsewhere.[163]

In 1921 Shimizu returned to Japan, apparently for personal reasons.[164] After his departure, Rutherford himself briefly took up the cloud chamber experiments, anxious to find

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158 For a particularly fine example, see Kinoshita, Ikeuti and Akiyama (1921). On the notion of witnessing at a distance see Shapin (1984); Shapin and Schaffer (1985).

159 Much as they had originally done with Wilson. See Barron (1952), 8-9; Cattermole and Wolfe (1987), 255-257; British patent 177,353, "Improvements in and relating to expansion apparatus for rendering visible the paths of ionizing particles." Also see 'Ray-Track Apparatus,' Cambridge Scientific Instrument Company catalogues 1921-1926, list no. 106 [1921], C.S.I.C. Archives, CUL.

160 Cattermole and Wolfe (1987), 256.

161 'Ray-Track Apparatus,' Cambridge Scientific Instrument Co. brochure [ca. 1927], C.S.I.C. Archives, CUL; Cattermole and Wolfe (1987), 257.

162 Cambridge Scientific Instrument Co. Serial Number Books, Nos. 21732-21743, 23 March 1922; 37336-37347, 11 July 1923; 50156-50167, 29 April 1924; 72740-72759, 13 October 1925, WML.

163 Among the investigations carried out on cloud chambers in the early 1920s were Akiyama (1923, 1924); Auger (1923, 1924, 1925a); Auger and Perrin (1922); Blackett (1922, 1923a, 1923b); Bose (1922); Bose and Ghosh (1923); Harkins and Ryan (1923a, 1923b); Harkins and Shadduck (1926a, 1926b). Working in the Cambridge Solar Physics Observatory, C.T.R. Wilson himself also investigated atomic rays with his original apparatus. See Wilson (1923a, 1923b, 1923c, 1925); Stratton (1949); Blackett (1960b), 289-291.

164 Blackett (1969), xxxiv.

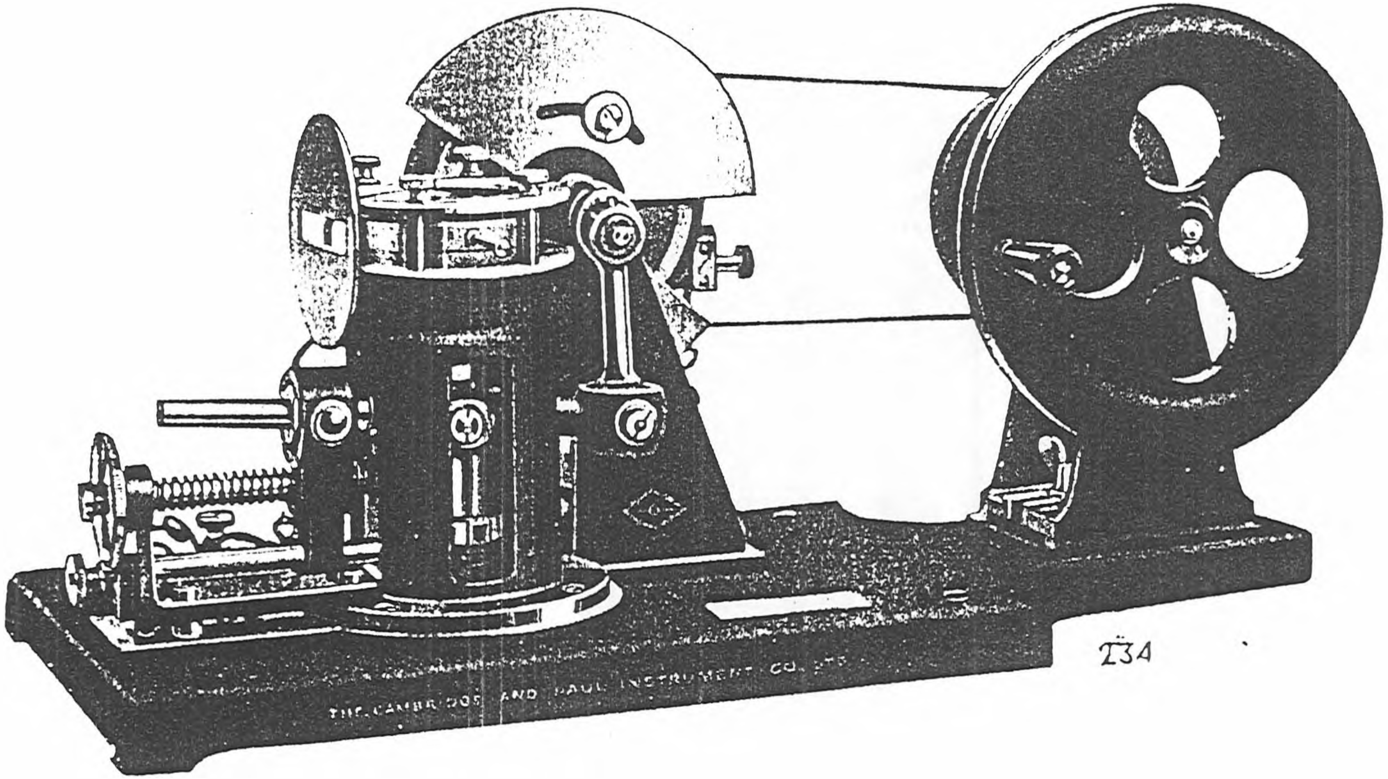

Fig. 2.8 Cambridge Scientific Instrument Company proprietary Shimizu cloud chamber, marketed as 'Ray-track apparatus,' 1923.

Source: CSIC Catalogue 106, CUL.

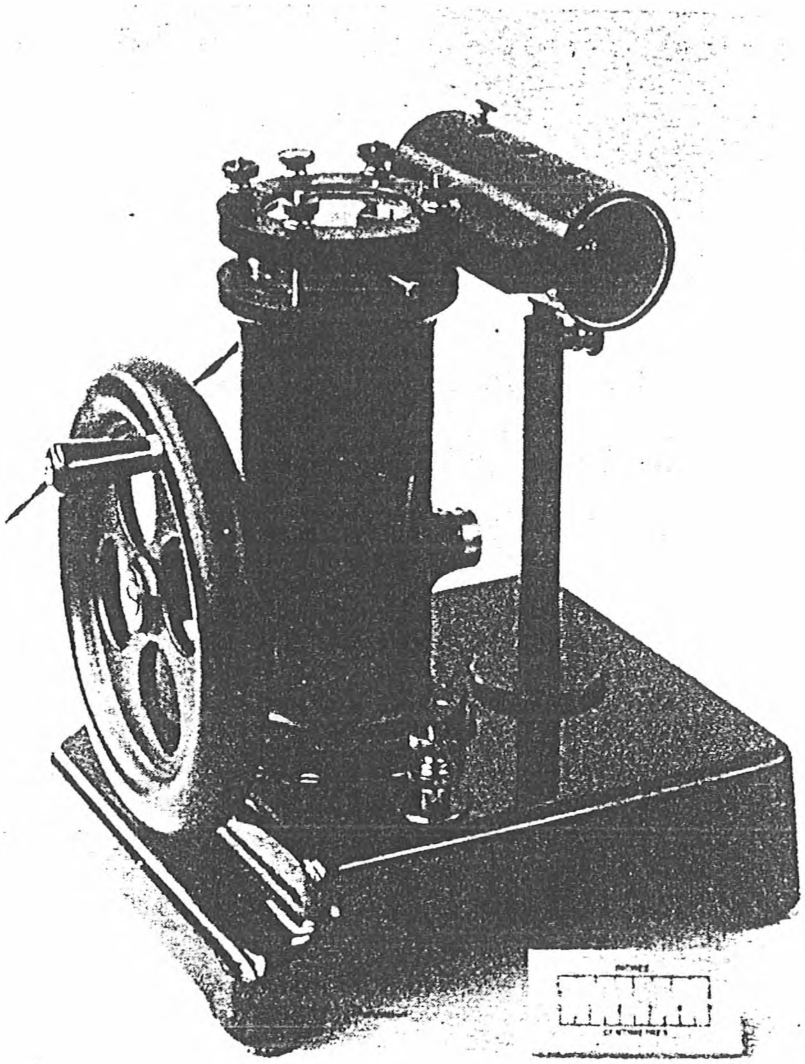

Fig. 2.9 Cambridge Scientific Instrument Company schools' cloud chamber, marketed from 1926.

Source: Barron (1952), 11.

evidence of atomic collisions.[165] In the summer of 1921, however, with the scintillation counting experiments increasingly occupying his attention, Rutherford asked another research student to take over Shimizu's experiments. Patrick Blackett had arrived in Cambridge in January 1919 as part of a contingent of 400 young Naval officers. He studied for Part I of the Mathematics Tripos, then switched to Physics, obtaining a First in Part II in 1921. He was elected to a Bye-Fellowship at Magdalene College the following October and found himself "with a few bits of [Shimizu's] apparatus in an otherwise empty research room and told to get on with it."[166]

Blackett replaced the reciprocating mechanism of Shimizu's prototype chamber with a spring action, enabling him to obtain very sudden expansions as frequently as desired.[167] The speed and amount of expansion could be controlled by adjustment of the spring. A mechanical arrangement ensured that α-particles only entered the chamber near the bottom of the stroke, keeping the chamber clear of all but sharp tracks. With the same aim in view, Blackett also modified Shimizu's stereoscopic camera arrangement by linking the shutter to the expansion mechanism, so that the photograph was taken just as the expansion was completed.[168] Large numbers of clear photographs could therefore be taken. They would be examined, "and whenever a track is seen to make a sudden bend, the two images on the photograph are measured up by means of a low-power microscope fitted with an eye-piece and cross-wire."[169]

By the summer of 1922, Blackett had achieved a number of results, including the disclosure of α-particles of much lower velocity than any previously observed down to $^1/_{20}$ of the velocity of α-particles from radium C. And he had acquired a great deal of experience in the production and analysis of α-ray photographs.[170] But he had not found evidence of

165 Rutherford, "Projected Researches, June 1921," NB 48, RP. See also Wilson (1983), 448.
166 Blackett (1969), xxxiv; Lovell (1975), 5.
167 Blackett (1922), 295.
168 Blackett (1922), 297; Lovell (1975), 6.
169 Blackett (1922), 299.
170 Blackett (1922, 1923a, 1923b); Lovell (1975), 6-7. Blackett (1933), 73, stresses the importance of 'knowing the materials' and of extended experience with apparatus.

nuclear disintegration. In keeping with a dictum of Aston's, more, more and yet more photographs would have to be taken.

4.4 The Natural History of the α-Particle: Experiments on Electron Capture

While Blackett continued to refine the cloud chamber technique, other experiments in progress in the laboratory sought to characterise the behaviour of α-particles during their all-too-brief lifetimes - a subject crucial to the interpretation of the insubstantial cloud chamber tracks and also, implicitly, to the integrity of the scintillation technique.[171] As with the artificial disintegration experiments, these experiments were a continuation of work begun at Manchester, where Marsden and Taylor had found in 1912-1913 that swiftly-moving α-particles begin to take up electrons when their velocity decreases to 0.4 of its initial value ($V_1=0.4V_0$).[172] Now, at Cambridge, Rutherford could devolve the work to another trustworthy research student.

George H. Henderson, an 1851 Exhibitioner from Dalhousie University, had arrived at the Cavendish in 1919. His earliest work on radioactivity pre-dated his arrival in Cambridge, but, as one might expect, it had been performed under the direction of a Rutherford pupil. At Dalhousie he had produced an undergraduate thesis on "The Distribution of the Active Deposit of Thorium in an Electric Field" under the supervision of Howard L. Bronson (who had himself been one of Rutherford's demonstrators at McGill).[173] Following graduation in 1914 Henderson became a Demonstrator in Physics, two years later submitting an M.Sc. thesis on "The Distribution of the Active Deposit of Radium in an Electric Field." After the war, during which he obtained a commission as

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171 Blackett's statistical investigations of cloud chamber tracks were "based on the assumption that an α-particle of given velocity will travel a given distance." If this assumption were not valid - say if the α-particle were to lose some of its charge towards the end of its range - then Blackett's calculations and results would be void. Blackett pointed out that such an analysis "must certainly instil caution against attaching undue weight to the exact form discovered for the relation between the velocity and the remaining range." See Blackett (1922), 316.

172 Marsden and Taylor (1913).

173 On Bronson at McGill, see Heighton (1990); for Henderson, see Lewis (1951) and Heighton (1990), 350-353.

Lieutenant in the Royal Canadian Engineers, he won a National Research Council scholarship to work with L.V. King (another Rutherford student) at McGill on the thermal conductivity of gases. His early researches on radioactivity determined him to work with Rutherford, an ambition realised when he was subsequently awarded an 1851 Exhibiton.

On arrival in Cambridge, Henderson was set to work on an investigation of the ionisation curves of Radium C, Thorium C_1 and Thorium C_2 in air, with the object of studying the processes involved in the passage of α-particles through matter. For these investigations he used a conventional ionization chamber and a sensitive Compton quadrant electrometer capable of yielding 5,000 divisions per volt, the "great advantage" of this arrangement being "that the measurements were direct."[174] His study revealed that straggling - the tendency of α-particle tracks to suffer deflections near the end of their range - was much more prevalent than had previously been supposed, calling into question the whole notion of a definite "range" (the quantity in terms of which α-particle energies were usually measured and expressed).[175]

These investigations were a continuation of the Manchester tradition of physical investigation in radioactivity being, as it were, phenomenological studies of the behaviour of radiation. In the same vein, for example, Henderson also carried out work on the direction of emission and other properties of α-particles.[176] But there was a second, more significant aspect to his researches. One of the chief lines of evidence for the definiteness of nuclear binding energies had been the homogeneity of the energies of the groups of α-particles emitted during radioactive decay by heavy nuclei. Henderson now brought this evidence into question. Furthermore, the phenomenon of electron capture represented a potential threat to the integrity of the scintillation technique. The scintillation method relied upon the assumption that the particles reaching the screen were a faithful representation of the particles emerging from the source.[177] This fundamental assumption underpinned the

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[174] Henderson (1921), 540, 541.
[175] And therefore calling into question the assumption underlying many of the results derived from scintillation counting and other experiments, as I show below. See Henderson (1921), 551; Henderson (1922c, 1922d).
[176] Henderson (1922a, 1922b).
[177] For the importance of this assumption, cf. Pinch (1985).

conclusions drawn from scintillation counts about the geography of the atom, and therefore the credibility of the information which could be derived from the method. The phenomenon of electron capture challenged the integrity of this evidential chain and therefore, implicitly, the viability of the technique (Lindemann, for example, used precisely this argument to query the integrity of Rutherford's results on the artificial disintegration of nitrogen).[178]

In order to continue his investigations, Henderson applied for an extension to his 1851 Scholarship, an application strongly supported by Rutherford.[179] With a third year's money thus secured, Henderson began a comprehensive investigation of the changes in charge of an α-particle as it passed through matter so as to map more completely its behaviour during its brief trajectory through space. Three key elements of Cavendish technique were essential to his enterprise. Rejecting the scintillation technique on account of the dual possibilities that "if scintillations were produced by particles of [low] velocity they would fail to be detected by the eye" and that "the particles might not be able to penetrate sufficiently far into the zinc sulphide to stimulate an appreciable proportion of scintillations,"[180] Henderson implicitly recognised that the scintillation method was itself in question, and adopted instead a photographic method. Because the particles to be detected were presumed to have a low penetrating-power, he chose to use plates of the Schumann type, supplied by Adam Hilger and advertised as being among the most sensitive available.[181] The choice was a good one, for the plates proved "more sensitive even than the plates used in positive ray work"[182] - in other words, more responsive even than the plates used by Aston to manifest the delicate and subtle indications of the isotopic nature of the elements. Secondly, a high vacuum (of the order of <0.01mm Hg) was required,

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[178] Lindemann to Rutherford, 9 June 1919, RP. On the important notions of 'evidential chains,' inference from observational data to the external world and the place of the scientific instrument in this process, see Pinch (1985, 1986).

[179] Rutherford to the Commissioners for the Exhibition of 1851, 14 June 1921, file i/428, 1851 Archives, ICL.

[180] Henderson (1923), 497.

[181] See, for example, the Hilger catalogue, file 'Optical Apparatus,' Box 344, MATP.

[182] Henderson (1923), 497. For the importance of photographic plates in positive ray work, see Aston (1925b).

necessitating the use of a Gaede pump - rare indeed for the Cavendish, but, as Rutherford put it, "[f]or the success of these experiments it is essential that the apparatus in which the deflexion is observed should be exhausted to a very low pressure, corresponding to that required for a good X-ray tube."[183] The third technology required was a strong magnetic field with which to deflect the α-particles in the hope of revealing something of their charge-behaviour. This was a key piece of ionist technology, and was the technique used by Rutherford to identify the products of his artificial disintegration experiments in 1919. Henderson employed a field of 4100 Gauss, with the pole-pieces of the electro-magnet arranged to give a uniform field across the space between source and plate. The radioactive source itself consisted of "an α-ray tube containing two or three millicuries of radium emanation."[184]

Using this complex combination of apparatus, Henderson found that when α-particles from the source were passed through absorbing sheets of mica equivalent to 1.42 cm of air and deflected by the magnetic field, a new band appeared on the photographic plate between the undeflected (magnetic field off) and deflected (field on) bands. This he ascribed to α-particles which had gained an electron, and which were therefore singly charged. Such a band, still visible at velocities down to $0.25V_0$ had escaped detection by the scintillation method, vindicating Henderson's decision to use the photographic technique. Under certain conditions, moreover, another band appeared which was undeflected in the magnetic field and which Henderson ascribed to the presence of neutral helium atoms. These results, suggested Henderson, were sufficient to account for the excess straggling he had observed in his earlier investigations.

Henderson's investigations, based on the most sensitive photographic plates, the highest feasible vacuum and the strongest possible magnetic field, thus resulted in a reconceptualisation of the processes occurring during the flight of an α-particle. A complex economy of ionization and electron capture, a breathless "interplay of charges,"

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183 Rutherford (1923c), 306. On Cavendish attitudes towards vacuum pumps and their distribution, see Ditchburn (1977).
184 Henderson (1923), 497.

was summoned up, in which an α-particle "might become, several times in its career, a doubly-charged, a singly-charged, and even an uncharged particle."[185] The "life history" of the α-particle had been traced out in detail. Henderson took his Ph.D. in June 1922 and, having come to the end of his Scholarship, returned to Canada. He continued the investigations for some time at Saskatchewan, where, as Assistant Professor, he persuaded the University to buy 25 mg of radium at the new, lower prices in the wake of new discoveries in the Belgian Congo. Lacking the state-of-the-art technology available only in the Cavendish Laboratory, he used the old ionisation method to complete his investigations, before returning to Dalhousie in 1924 as Professor of Mathematical Physics.[186]

While the majority of research papers emerging from the Cavendish Laboratory attracted little explicit technical comment from outside the narrow and specialist confines of the radioactivity community, Henderson's report in the *Proceedings of the Royal Society* was unusual in that it excited interest in an unusual quarter. Linking Henderson's work with the "beautiful" evidence then emerging from the mists of the cloud chamber regarding the ionisation produced by α-particles, Bergen Davis, Professor of Physics at New York's Columbia University, found it "a matter of some surprise" that in the act of ionising a molecule, or immediately after, the α particle does not attach one or more of the free electrons to itself,"[187] since the α-particle had a double charge and passed so close to many electrons. Suggesting that this might be due to the high velocity of the alpha-particles, Davis calculated the limiting parabolic velocity for an electron falling into the K ring of (1) a doubly charged, and (2) a singly charged alpha- particle, based on the premiss that a free electron would move towards a swift α-particle during its passage through matter, but, "[i]f the [α-particle] is moving with a velocity greater than the velocity of fall (parabolic velocity) of an electron into the K ring, the electron will fail to reach the K ring and effect a combination." Since the radius of the K ring a=$h^2/4\pi^2 meE$, where E=excess nuclear charge and m=mass of the nucleus, the velocity v of the electron in the orbit is given by $\frac{1}{2}mv^2$

185 Henderson (1923), 503.
186 See Henderson to Rutherford, 12 January 1923, RP; Henderson (1925); Lewis (1951), 158.
187 Davis (1923), 706.

=Ee/*a*. The calculated velocities for the first and second captures then become $v_1=6.2 \times 10^8$ cm/sec, $v_2=3.2 \times 10^8$ cm/sec, these figures being in "sufficiently close" agreement with Henderson's results to suggest "that this may be the proper explanation of the action." Viewed in such a way, Davis pointed out, it should be the case that "all α-rays, of whatever initial velocity, should capture the first and second electrons at the same velocit[ies]," a "matter of sufficient importance to determine experimentally."[188]

The suggestion was ignored, in typical Cavendish fashion. An outsider with none of the relevant material resources or practical know-how, Davis himself was in no position to undertake such an intricate series of experiments, and his suggestion came to naught. Henderson's "striking" researches, on the other hand, were taken up by Peter Kapitza, the Russian engineer who had arrived in Cambridge in the summer of 1921. After the obligatory course in the attic (from which he was exempted after two weeks), Kapitza was set to work on the loss of energy by α-particles towards the end of their range, using a Boys radiomicrometer to measure the energy of a collimated beam of particles.[189] For this research, Kapitza built a device which, he said, he had "perfected to such a degree that it can detect the flame of a candle placed one and a half miles away."[190] After Henderson's departure in the summer of 1922, Rutherford encouraged Kapitza to continue the magnetic deflection experiments, in collaboration with Patrick Blackett.[191] Kapitza, applying his engineering skills to the problem, and with the input of considerable financial and material resources from Rutherford and the assistance of Emil Yanovitch Laurmann, an Estonian mechanic with whom he had worked in Petrograd,[192] was able to develop the astonishing magnetic field of 80,000 Gauss for very short periods of time. The feat was not a trivial one; Kapitza attested to the labour required to produce results, and to the trial-and-error quality of much Cavendish work:[193]

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188 *ibid.*
189 Kapitza (1922); Boag, Rubinin and Shoenberg (eds.)(1990), 10-11.
190 Kapitza to his mother, 22 December 1921, in Boag, Rubinin and Shoenberg (eds.)(1990), 139-140.
191 Kapitza to his mother, 15 June, 19 June 1922, *ibid.*, 150-152.
192 Shoenberg (1954); Boag, Rubinin and Shoenberg (eds.)(1990), 14.
193 Kapitza to his mother, 3 November 1922, in Boag, Rubinin and Shoenberg (eds.)(1990), 159-160.

> I am working on the production of magnetic fields of great intensity. These are needed to study certain phenomena in radioactivity. ... I proposed three methods for obtaining such fields. The first of these had to be rejected on theoretical grounds, leaving two others. We began work on the second method and straight away struck almost insurmountable technical difficulties. While my collaborator continued with this approach I tried the third method and almost immediately obtained positive results. After that Laurmann, Blackett and I worked on this third method for six weeks and succeeded in firmly establishing its suitability. It was then only necessary to go from the small-scale experiment to a larger one.

Combining the high magnetic field with a cloud chamber of the Shimizu pattern, Kapitza was able to report in November 1922:[194]

> For me this day is somewhat historic, for today I obtained the result I had been hoping for. In front of me is a photograph on which there are just three curved lines. But these three curved lines are the paths of α-particles in a magnetic field of enormous strength. These three lines have cost Professor Rutherford £150 and myself and Laurmann three and a half months of very hard work. But here they are and everyone in the University is talking about them. Strange!

Strange indeed. Kapitza had found his métier, however, and would devote the rest of his time at the Cavendish to applications of the impulsive field technique and to the production of ever higher magnetic fields.[195] Blackett, meanwhile, returned to his own attempts to photograph artificial disintegration, a goal which he would achieve in 1924.

Rutherford himself, while he had devolved part of the α-particle work to Kapitza and Blackett, also took up Henderson's researches with the aim of verifying his main results and conclusions. Whereas Henderson had used the photographic method of detection, however, Rutherford imported what was, for him, a tried and trusted technique, the scintillation method, in which "the energy of the α particles can be estimated by the brightness of the scintillations and their number determined by direct counting."[196] This also meant the use of the complicated protocols for scintillation counting which had been put in place to guarantee certitude in the disintegration experiments. In practice, this

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194 Kapitza to his mother, 29 November 1922, *ibid.*, 160-161, on 160; Kapitza (1923a, 1923b).
195 For the remainder of Kapitza's Cambridge career, see Boag, Rubinin and Shoenberg (eds.)(1990); Wilson (1983), 496-537; Badash (1985).
196 Rutherford (1923b), 504 [CPR 3, 81].

amounted to running the electron capture experiments in parallel with the continuing disintegration work, enabling the same counters to be employed for both.

Rutherford's electron capture apparatus is shown schematically in fig. 2.10. Rays from the radioactive source W passed through the slit S and fell on the zinc sulphide screen inside the evacuated box. The necessary vacuum was attained by the initial application of a Gaede pump followed by a Langmuir diffusion pump. A magnetic field of 6,000 Gauss served to deflect the rays, and the scintillations were viewed through a microscope, which was so arranged as to have "a vertical motion so that any part of the screen could be brought into view."[197] With Blackett and Ellis (and later A.W. Barton) as scintillation counters, Rutherford extended Henderson's researches to count He^{++}, He^{+} and neutral helium particles individually after deflection in a magnetic field. The results seemed to verify the earlier work,[198] opening up a new and interesting field of inquiry" - always a good thing, as far as Rutherford was concerned.[199] As he told Bohr in January 1923, "I have confirmed [Henderson's] conclusions completely and am myself making an investigation of the laws controlling the capture and loss of electrons, and have already broken the back of the work."[200]

The investigations occupied the spring and summer of 1923, still being pursued in parallel with his continuing work with Chadwick on the artificial disintegration of the light elements.[201] Rutherford developed a model of the complex behaviour of the α-particle, though the mathematical elaboration of such a model eluded him. His work was therefore backed up with a series of papers by Fowler articulating and developing the mathematical theory of the motion of α-particles through matter.[202] Bohr, visiting Cambridge in June, also contributed to discussions on electron capture, and continued to acted as a consultant to Rutherford on theoretical matters.[203] On June 15, Rutherford lectured on his latest results

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197 Rutherford (1923b, 1924a).
198 See Rutherford to R.W. Boyle, quoted in Eve (1939), 292.
199 Rutherford (1923b), 510 [CPR 3, 87].
200 Rutherford to Bohr, 8 January 1923, RP.
201 See laboratory notebooks NB 39-44, RP. The electron capture experiments seem to have come to a close at the end of May 1923, although Rutherford (1924a) is dated December 1923.
202 Fowler (1923a, 1923b, 1924a, 1924b, 1925).
203 Bohr to Rutherford, 23 June 1923, RP.

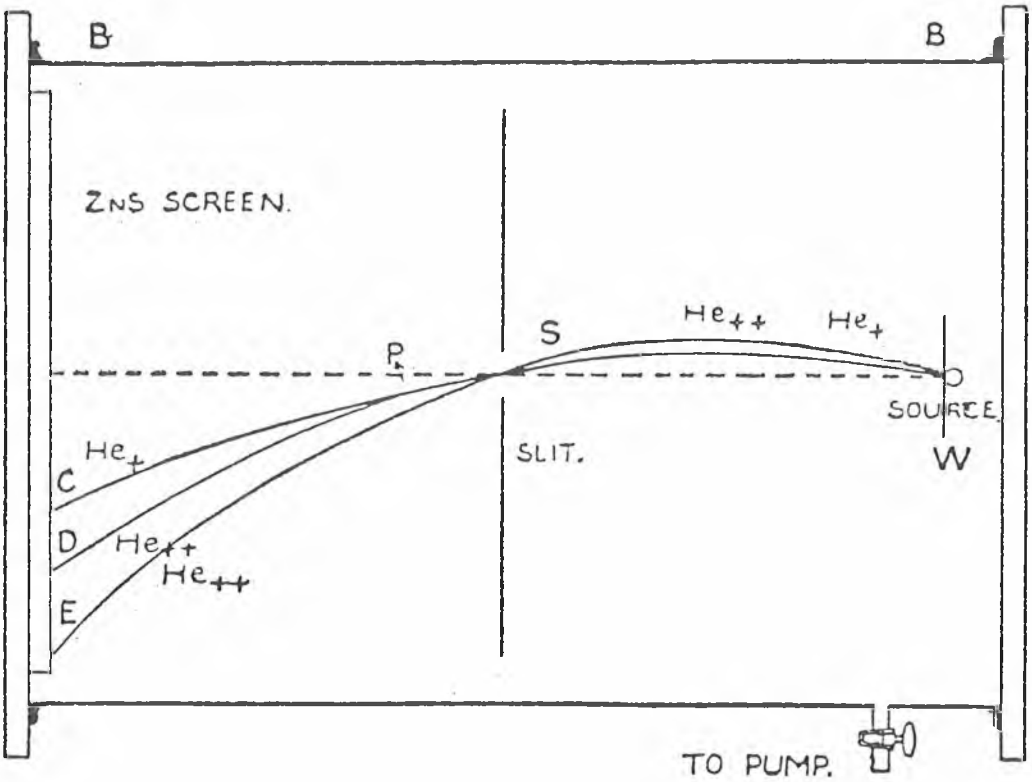

Fig. 2.10 Rutherford's apparatus for electron capture experiments. The radioactive source W is a fine platinum wire coated with Radium B+C, placed in an exhausted box B. The trajectories shown are those of helium atoms with various charges under the action of a magnetic field of 6,000 gauss. A microscope able to move in the vertical plane allows any part of the ZnS scintillation screen to be observed.

Source: Rutherford (1924a), 278 [CPR 3, 89].

at the Royal Institution. He found a topical example to make his research significant: "Large quantities of helium, sufficient to fill a large airship, have ... been isolated from the natural gases which escape so freely from the earth in various parts of Canada and the United States. It is a striking fact that every single atom of this material has in all probability had the life history here described."[204] He conjured up a similarly spectacular cosmological vision for another audience:[205]

> We can follow in imagination the long life of the alpha particle - on an average many thousands of millions of years - as an integral and orderly part of the structure of the nucleus of uranium, or its descendants; the sudden cataclysm in the atom leading to its violent expulsion; its brief but exciting career of about a hundred-millionth of a second plunging through the atoms in its path, its long imprisonment in the mineral, and its release ... to show its brilliant effects when an electric discharge passes through it. I hope you will agree with me that it is a fascinating story of a single atom of matter which, in its chequered career, had undergone so many vicissitudes.

For Rutherford himself, though, the real importance of the electron capture experiments was that they clarified the behaviour of what was the most effective and most readily available laboratory tool for the investigation of the nuclear structure. "This flying atomic nucleus," as he put it, "is not only the most energetic projectile known to us, but it is also an agent of great power in probing the structure of atoms."[206] More to the point, with the processes of electron capture well understood and codified, Rutherford could continue to treat the scintillation method as a reliable and 'transparent' indicator of the numbers and trajectories of the particles produced in his experiments.[207] Confident in this, he told his Royal Institution audience that "[w]e are enabled, particularly by the scintillation method, to count the individual particles, and thus we have at our command *a method of great delicacy* for studying the effects produced by the passage of α-particles through matter."[208] He stressed the point again before the British Association in September. The scintillation

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[204] Rutherford (1923c), 311.
[205] Quoted in Eve (1939), 294.
[206] Rutherford (1923c), 305.
[207] On the notion of 'transparency,' see Schaffer (1989).
[208] Rutherford (1923c), 305, my emphasis.

method had proved "invaluable in many researches, for it gives us a method of unequalled delicacy for studying the effects of single atoms."[209] With over a decade of positive results to its credit, including those which yielded the nuclear atom and the artificial disintegration of nitrogen, the scintillation method had been, and promised to remain, the key technology with which to delineate and conquer the "unexplored territory" of the atomic nucleus.[210]

5. Conclusion: Counting, Confidence and the Public Face of Cavendish Physics

President of the British Association at its Liverpool meeting in 1923, Rutherford took the opportunity to make a rare statement of his metaphysics. He lavished praise on the D.S.I.R. which had "made a generous provision of grants to train young men of promise in research methods in our scientific institutions, and [had] aided special fundamental researches which are clearly beyond the capacity of a laboratory to finance from its own funds." He pressed the point home:[211]

> In order to obtain the best results ... [it] is necessary that our universities and other specific institutions should be liberally supported, so as not only to be in a position to train adequately young investigators of promise, but also to serve themselves as active centres of research. At the same time there must be a reasonable competence for those who have shown a capacity for original investigation. Not least, peace throughout the civilised world is as important for rapid scientific progress as for general commercial prosperity. Indeed, science is truly international, and for progress in many directions the co-operation of nations is as essential as the co-operation of individuals. Science, no less than industry, desires a stability not yet achieved in world conditions.

With a regime of training in place, a steady supply of accomplished students from the Imperial university network, a career structure and an ethos of circulation and return, the

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209 Rutherford (1923d), 6.
210 *ibid.*
211 *ibid.*, 3, 24.

Cavendish Laboratory embodied Rutherford's scientific philosophy of empire and experiment.

Dedicated to the elucidation of nuclear structure, the Cavendish was, too, charting a new terrain. Rutherford could have pointed to the cloud chamber, the mass-spectrograph and isotopes, or indeed the nucleus and his own disintegration experiments when he announced that:[212]

> From time to time there arises an illuminating conception, based on accumulated knowledge, which lights up a large region and shows the connection between these individual efforts, so that a general advance follows. The attack begins anew on a wider front, and often with improved technical weapons. The conception which led to this advance often appears simple and obvious when once it has been put forward. This is a common experience, and the scientific man often feels a sense of disappointment that he himself had not foreseen a development which ultimately seems so clear and invitable.

But it was in and through the delicate scintillation-counting experiments that Rutherford and his close co-workers chiefly sought to map the structure of the nucleus. In a sense, however, those experiments operated in two distinct registers. There was the public, constitutive register, in which Rutherford could make confident pronouncements about the delicacy and transparency of the technique. And, on the other hand, there was the register of the darkroom, in which the experiments were surrounded with doubt, always provisional, always liable to reinterpretation. Within the laboratory, certitude was always contingent, bound up with a complex social organisation of experimental practice. At the B.A.A.S., Rutherford chose, unsurprisingly, to stress the former, noting that the "rapidity and certitude of advance" of the previous few years had depended "on the fact that it has been possible to devise experiments so that few variables were involved." That public face of confidence and certitude was short-lived.

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[212] *ibid.*, 23-24.

Fig. 2.11 Coat of arms presented to Rutherford by the 'jackals' of the Red Lions Dining Club at the meeting of the British Association for the Advancement of Science, Liverpool, 1923.

Source: Eve (1939), facing 296.

CHAPTER THREE

DISCIPLINE AND DISSENT

The Dark Side of Radiant Physics

1. Introduction

Standing before the British Association for the Advancement of Science in September 1923, Rutherford triumphantly articulated the renewed optimism of British physicists:[1]

> There has never been a time when the enthusiasm of the scientific workers was greater or when there was a more hopeful feeling that great advances were imminent. This feeling is no doubt in part due to the great improvement during this epoch of the technical methods of attack, for problems that at one time seemed unattackable are now seen likely to fall before the new methods. In the main the epoch under consideration has been one of experiment, where the experimenter has been the pioneer in the attack on new problems. ... I feel it is a great privilege to have witnessed this period, which may almost be termed the Renaissance of Physics.

It was a remarkable statement, backed by a strong record. In the five years after the war to end wars, Rutherford's Cavendish Laboratory had managed to make significant headway in the attack on the structure of matter. The combined social, technical and conceptual practice of Cavendish physics - scintillation counting, cloud chambers, the mass-spectrograph, nuclei, isotopes, grants, fellowships - seemed unassailable, underpinning Rutherford's breezy optimism and promising yet further glories. The award of Nobel Prizes to Aston and Soddy in December 1922 had only served to ratify and further entrench the isotope interpretation of matter and, implicitly, Rutherford's nuclear hypothesis. Indeed, it was largely as a result of the programme of Imperial physics at the Cavendish Laboratory that the question of atomic structure was increasingly coming to be seen as "the

[1] Rutherford (1923d), 23.

most attractive problem in physics." With the 'improved methods of attack' developed over the previous few years, Cambridge physics stood at the forefront of the assault on the atom.

By Rutherford's reckoning, that assault must be by experiment.[2] And in the experimental field, the Cavendish Laboratory still had the nucleus largely to itself in 1923, for no other laboratory had yet ventured to undertake disintegration experiments, leaving the Cambridge work and its legitimacy effectively unchallenged. Yet the nucleus was not entirely uncontested territory. The very success of the Cavendish programme of experimental investigations into the physical basis of radioactivity and the structure of matter made opportunities and provided the data for others to speculate and theorise about matters nuclear without themselves venturing into the dangerous and difficult world of the laboratory. From the Cambridge perspective, radioactivity was a discipline whose boundaries had to be carefully demarcated and defended against the incursions of such amateurs, dilettantes and wild speculators. And in the early 1920s, as I shall show in this chapter, Rutherford and his collaborators could claim some success in maintaining those boundaries and in keeping the speculators at bay.[3] The ascendancy of experiment pointed the way ahead, for "[e]xperiment, directed by the disciplined imagination either of an individual, or still better of a group of individuals of varied mental outlook, is able to achieve results which far transcend the imagination alone of the greatest natural philosopher."[4]

In 1924, however, a new problem emerged. Cambridge's domination in experiment was broken. Controversy suddenly engulfed the disintegration experiments, destroying the taken-for-grantedness of many of the experimental and conceptual techniques which had

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[2] Commenting on popular reaction to Einstein's work in 1920, for example, Rutherford told G.E. Hale that "[t]he interest of the general public in this work is most remarkable and almost unexampled. I think it is due to the fact that no one can give an intelligent explanation of the same to the average man and this excites his curiosity. While I personally have not much doubt about the accuracy of Einstein's conclusions and consider it a great piece of work, I am a little afraid it will have the tendency to ruin many scientific men in drawing them away from the field of experiment to the broad road of metaphysical conceptions. We already have plenty of that type in this country and we do not want to have any more if Science is to go ahead." See Rutherford to Hale, 13 January 1920, GEHP.

[3] Or at least in making their speculations irrelevant to the practice of 'proper' physics.

[4] Rutherford (1923d), 23.

been developed at the Cavendish Laboratory over the previous few years. The challenge was an unexpected one. Two young workers at the Vienna Radium Institute, another key centre of radioactivity research and an important repository of radioactive substances and technique, entered the field of artificial disintegration and began systematically to repeat the work already carried out in Cambridge. Using the scintillation method, the expansion chamber and other methods developed and deployed at the Cavendish, the Viennese workers established a set of results at variance with those found in Cambridge and, with them, an alternative interpretation of the nucleus. The Viennese researchers prosecuted their case vigorously, contesting not only the substantive results obtained in Cambridge, but also, in the end, the adequacy of the instruments and techniques by which those results had been obtained - thereby challenging much of the warrant for Rutherford's confidence and undermining the accustomed certitude of the scintillation technique. For it was the elaborate set of rituals underpinning that certitude, and ultimately the authority of experiment itself, which the controversy called into question.

2. On Speculation: The 'Almighty Atom' and the 'Renaissance of Physics' in the 1920s

Describing "A Layman's Odyssey around the Scientific Centres of Europe" in search of the "Almighty Atom" in the mid-1920s, author C.E. Bechhofer Roberts perceived a distinct convergence of scientific forces: "No one can tell today where physics ends and chemistry begins. ... One might say that there is now a single science in which physics, chemistry and mathematics are combined, its purpose being the study of atomic structure."[5] The atom, indeed, figured large on the scientific agenda of the 1920s. Even an important area of study like acoustics was "a field regarded by many as possessing but little interest"[6] relative to the excitement and exoticism of investigations into the ultimate structure of matter. J.A.

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5 Roberts (1925), 195. Of particular interest is Roberts' description of his witnessing of a cloud chamber experiment (*ibid.*, 195-196).

6 Stewart (1923), 4.

Crowther, himself an old Cavendish man, identified one reason for this state of affairs. Although the atom was "a subject on which no worker in physics or chemistry dare allow his knowledge to become out-of-date, and in which other scientific workers take an interest which is by no means entirely extraneous," the "distinction and lucidity of some of its famous exponents" had aroused "the interest of a wider non-scientific circle." Coupled with the high profile of events like Marie Curie's visit to America in 1921 and the continuing public fascination with radium, it was hardly surprising that the atom had won for itself "a distinctly 'good press'."[7]

In such circumstances, mused Crowther, it was "not surprising that books on the subject, addressed to one or other of these numerous classes of potential readers, should appear at frequent intervals." And so they did. Throughout the 1920s, a flood of popular books and articles catered to the would-be student of the atom and its viscera, provoking a gentle complaint from Crowther (again) three years later:[8]

> We have books on the atom ... by chemists, by mathematicians, by technicians, and by journalists, and addressed to all sorts and conditions of readers. Thus we have "Atoms for Amateurs," "Atoms for Adepts," "Atoms for Adolescents," "Atoms for Archdeacons," "All about Atoms for Anybody" - these are not the exact titles, but they indicate the scope of the volumes well enough - in fact there seems to be a determination that no class of reader shall be left without an exposition of the subject suited to his condition and attainments ... If we add to these the enormous output of serious scientific contributions from the many laboratories engaged in investigating the structure and properties of the atom, it is clear that this infinitesimal particle exerts an attraction unique in the history of science over the minds and imaginations of many types of men.

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[7] J.A. Crowther (1923), 232. Weart (1988), 3-74, gives a brief (but implicitly teleological) cultural history of the atom before World War II.

[8] J.A. Crowther (1926). See also Forman (1978). Some of the books and articles bringing atomic physics and the reductionist programme to a wider English-speaking audience were: "Triumphs of the Nucleus Type of Atom. How Rutherford has made Radio-activity the most Progressive Department of Physics," *Current Opinion* **67** (1919), 33-34; "At the Rock Bottom of Matter," *Current Opinion* **69** (1920), 72-73; "Impending Subjugation of Nature," *Current Opinion* **70** (1921), 369-370; "Magnificent Complexity of the Atom," *Literary Digest* **81** (1924), 23-24; "Whose Atom, the Chemists' or the Physicists'?" *Outlook* **137** (1924), 258-259; "Shattering the Atom," *Literary Digest* **84** (1925), 23; "Smashing an Atom," *Literary Digest* **91** (1926), 22; Abbott (1922); Andrade (1923, 1927a, 1927b, 1930); Berthoud (1924); Chadwick (1921); Clark (1926); J.A. Crowther (1927); J.G. Crowther (1928a, 1928b); Darwin (1931); Kendall (1929); Kramers and Holst (1923); Lawson (1921); Lemon (1923); Lodge (1923, 1924, 1927); Lotka (1920); Menzies (1922); Mills (ca. 1923); Parsons (1921); Roberts (1925); B. Russell (1923, 1924); Shearcroft (1925); Slosson (1922, 1924); Sullivan (1923b); G.P. Thomson (1930); Verschoyle (1925).

Most of these expository texts gave accounts of the Rutherford-Bohr atom, with its distinct division between the nucleus and the extra-nuclear shells of electrons. And most pointed out that as far as the central core of the atom was concerned, experimental studies of the nucleus were effectively confined to the Cavendish Laboratory.

Though no other laboratory had yet ventured into this most esoteric of scientific fields, Rutherford's disclosures and his ongoing conjectures as to the constitution and structure of the nucleus nevertheless opened a space (and provided the experimental data) for speculation, numerological and otherwise, about the internal constitution of the nucleus. Following his 1920 Bakerian Lecture, several authors set their imaginations loose on the new terrain. As Roger Stuewer has documented, for example, nuclear models of various degrees of internal definition and rigidity were constructed by J.C. McLennan (fig. 3.1),[9] E. Gehrcke (fig. 3.2),[10] E. Kohlweiler (fig. 3.3)[11] and others.[12] While it usually took its cue from Rutherford's own musings, such rampant speculation was heartily deplored by Rutherford's follower E.N. da C. Andrade, who noted that the atomic nucleus had offered "a vast field for what the Germans call *Arithmetische spielereien*, which serves rather to entertain the players than to advance knowledge."[13]

Rutherford himself complained to Boltwood about another speculation-monger, William Draper Harkins, who:

> writes at great length on every topic but as a whole is moderately sound from my point of view. Actually, however, most of the ideas on which [his speculations] are based have been common property in this country and especially to myself for the last five years. It is exceedingly easy to write about these matters but exceedingly difficult to get experimental evidence to form a correct decision. ... Harkins is a man of intelligence, but I wish he did more experimenting and spent less time in theorising and in endeavouring to cover every possible idea.[14]

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9 McLennan (1922), 231. See Stuewer (1983), 23-32. For a contemporary evaluation of the status of speculations as to nuclear structure, see Kovarik and McKeehan (1925), 124-125.
10 Gehrcke (1921).
11 Kohlweiler (1920, 1921).
12 See, for example "Hypothetical Constitution of Atomic Nuclei by 'R.M.D.' of 67, Priory Road, Kew," [1919], PA 324, RP.
13 Andrade (1923), 111.
14 Rutherford to Boltwood, 28 February 1921, in Badash (ed.)(1969), 341-344, on 342-343.

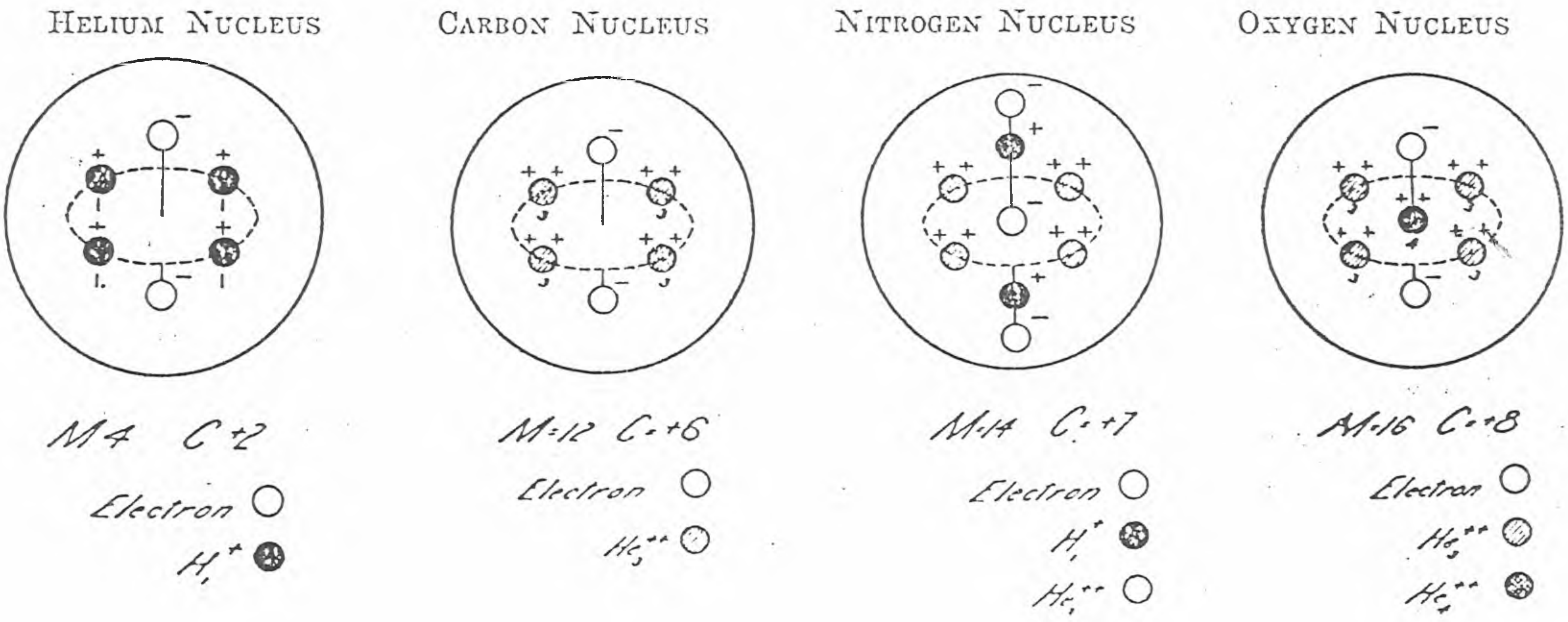

Fig. 3.1 J.C. McLennan's elaborately structured models of nuclear constitution (1921), based on protons, electrons and helium nuclei of mass 3 and 4 as constituents.

Source: McLennan (1922), 231.

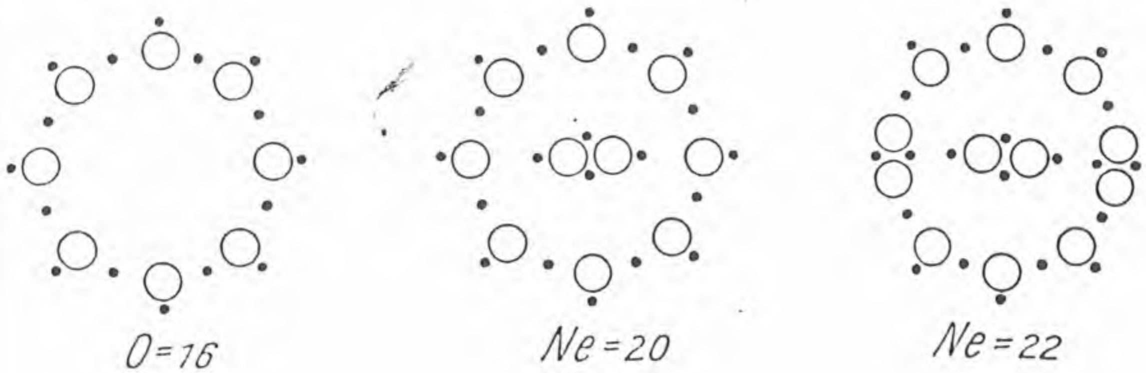

Fig. 3.2 E. Gehrcke's loosely-structured models for the nucleus of oxygen and for the isotopic nuclei of neon (compare figs. 3.1 and 3.3), based on the work of Rutherford and Aston. Circles represent electrons, dots protons.

Source: Gehrcke (1921), 151.

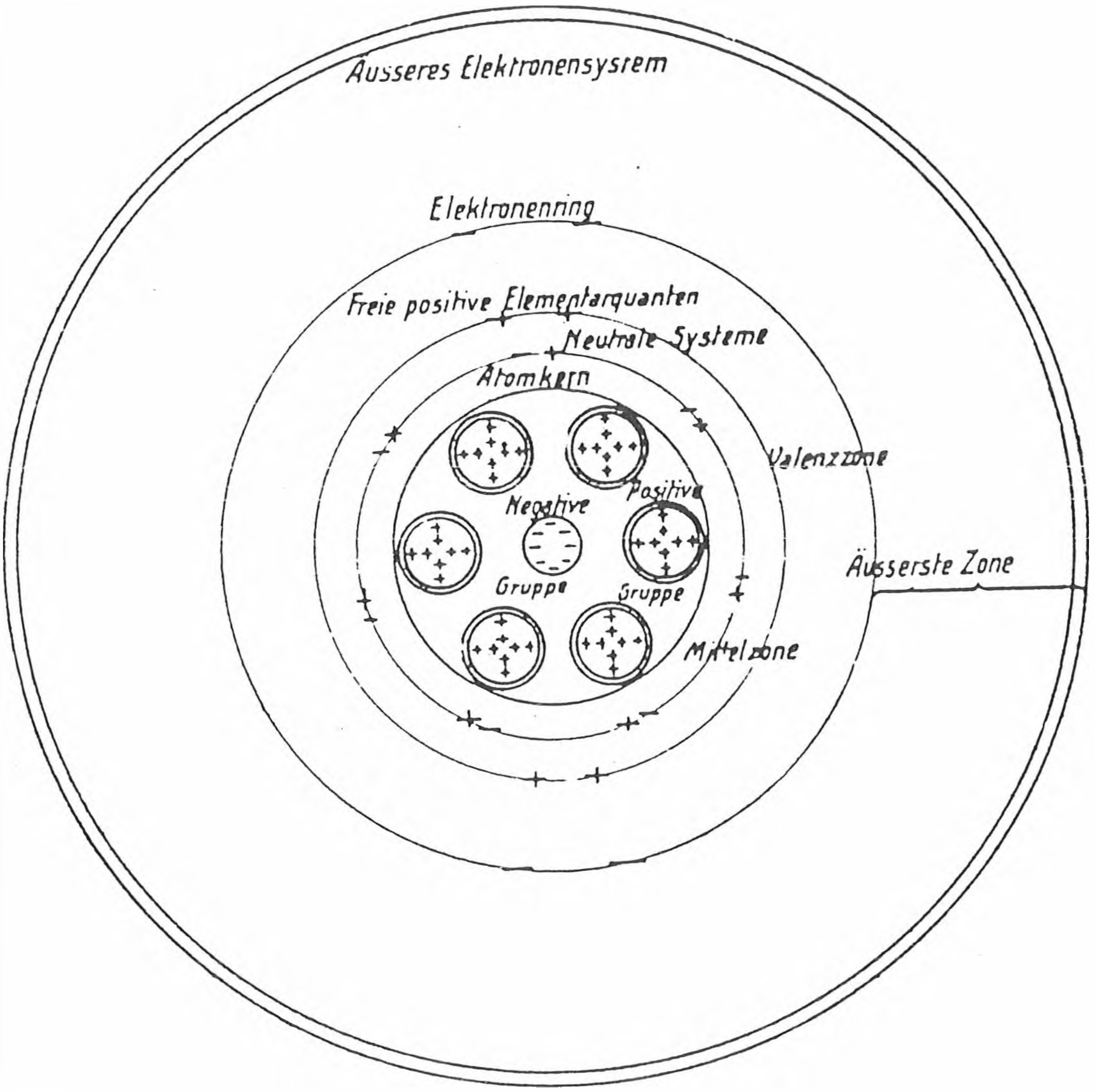

Fig. 3.3 Kohlweiler's detailed and complex atomic structure for element 44. In the nucleus, a group of negative charges is surrounded by groups of positive charges. Around the nucleus are, first, a neutral ring of positive and negative systems, then two further rings, one of free positive and the other of free negative charges. Outside these is the external zone of electrons.

One of the most prolific speculators on nuclear structure in the 1920s, Harkins, a physical chemist at the University of Chicago, had undertaken a systematic survey of nuclear constitution, arriving independently at many of the conceptions developed in the Cavendish Laboratory - including the neutral doublet and the notion of the "packing effect."[15] But Rutherford's charge was unfair, for Harkins and his students were the only Americans to engage in experimental studies on the nucleus until the late 1920s, and were (as we have seen) among the first to attempt to separate the isotopes of chlorine and mercury.[16] Later, Harkins would direct cloud-chamber studies of atomic disintegration to rival those in Cambridge and elsewhere, though he would always remain marginal to the concerns of the Cavendish Laboratory, tarred with the reputation of being a hopeless speculator.[17]

While Harkins engaged in what seemed like pointless nuclear systematics, a keynote speaker told the American Association for the Advancement of Science in 1923 that "we would be willing to sacrifice a great deal to know more of the content and arrangement in nuclei."[18] But the intellectual excitement engendered by the atom was to be tempered by the application of a healthy scepticism to unguarded hypothesis, for while it was "doubtful if ever there has been a more inviting appeal to imaginative reason than can be found at present in atomic structure and radiation theories," the atomic quest was, "in fact, so exciting that we can easily forget the mysteries in our own hypotheses."[19] For Rutherford and like-minded empiricists, Aston's experimental work, and particularly the whole number rule based on it, guarded against such amnesia by basing the construction of "stable assemblages" on concrete experimental evidence.[20] The space inside the nucleus was a

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[15] Harkins to Lewis, 5 February 1916, GNLP; Harkins to Rutherford, 6 December 1920, RP; Harkins (1917, 1920a, 1920b, 1920c, 1921a, 1921b, 1921c); Harkins and Hall (1916); Harkins and Wilson (1915a, 1915b). On Harkins, see Mulliken (1975); Fowkes (1972); Kamen (1985), 48-58; Kauffman (1985a).

[16] Harkins (1921d, 1925); Harkins and Mulliken (1921); Kauffman (1985a), 759.

[17] More to the point, Harkins was not a card-carrying member of the radioactivity community. His interest in radioactivity developed just before the war and he had no personal or professional contact with the more established European workers. For his later work, see Harkins (1928); Harkins and Ryan (1923a, 1923b); Harkins and Shadduck (1926a, 1926b). Despite his disparaging remarks, Rutherford occasionally drew inspiration from Harkins' extensive writings: see Rutherford (1920a), 398 [CPR 3, 36], footnote.

[18] Stewart (1923), 1.

[19] *ibid.*

[20] Andrade (1923), 111. This anti-theory attitude was deplored by Eddington, who quipped that "When an experimenter tries to examine and take into account all perturbing influences or factors (proved or unproved) which are likely to affect the conclusions reached in his work he is said to be taking precautions. When the

terrain to be mapped by exacting experiment and controlled conjecture, not by wild speculation or flights of fancy. Rutherford stressed the point - and the corporate character of the experimental enterprise - at Liverpool in 1923:[21]

> Experiment, directed by the disciplined imagination either of an individual, or still better, of a group of individuals of varied mental outlook, is able to achieve results which far transcend the imagination alone of the greatest natural philosopher. Experiment without imagination, or imagination without recourse to experiment, can accomplish little, but, for effective progress, a happy blend of these two powers is necessary. The unknown appears in a dense mist before the eyes of men. In penetrating this obscurity we cannot invoke the aid of supermen, but must depend on the combined efforts of a number of adequately trained ordinary men of scientific imagination. Each in his own special field of inquiry is enabled by the scientific method to penetrate a short distance, and his work reacts upon and influences the whole body of other workers.

With his usual polemical flourish, Frederick Soddy selected a slightly different target when he deprecated the speculators' incursions into his beloved radioactivity, "one of the few fields in the vast borderland between physics and chemistry, overrun of recent years by an advancing swarm of mathematicians and physicists, armed with all sorts of new-fangled weapons, in which the invaders have found the chemist already in possession."[22] But Soddy was fighting a rearguard action. He had been expected to develop a serious research programme in radioactivity at Oxford, especially in the wake of his 1921 Nobel prize for chemistry.[23] These expectations remained unfulfilled. Conflict within the University - he called himself 'a catfish among the cod'[24] - and his growing interest in economic questions

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theoretical investigator likewise gives consideration to the possible factors which if present might modify his conclusions he is said to be speculating." See Eddington, MS lecture notes, n.d. but [?] late 1920s, Box 1, BFSP.

[21] Rutherford (1923d), 23. Compare also the comments on the "conjectural character" of much nuclear theorising in Aston (1922a), 102.

[22] Soddy (1923), 208. For a rebuff, see Lodge to Soddy, 3 January 1923, OLPB: "It is evident that I take a much higher estimate of the achievement of Mathematical Physicists than you do. Their grasp of principlies and their power of deducing consequences are to me amazing. Experiments are vital of course: but without the guide and the suggestiveness and comprehensive outlook of mathematical theory, the results obtained would be rather poverty-stricken. For instance -- to take one small example, I doubt if Positive Ray Analysis would ever have been attempted, or thought of, or understood. And your Isotopes would not have obtained the importance that they now possess." Soddy's response is not extant.

[23] Cruickshank (1986), 160. Soddy was awarded the (reserved) 1921 Nobel chemistry prize in 1922; see Crawford, Heilbron and Ullrich (1987), 218-225.

[24] Howorth (1958), 227-236; Cruickshank (1986).

and the social relations of science meant that Soddy's polemics were directed elsewhere. He made no further contributions to the experimental study of the atom's constitution, although he remained a trenchant and widely-read commentator on matters atomic.[25] Soddy's own research in the 1920s centred, in fact, on the extraction of thorium from Travancore monazite sand, a problem which had occupied his attention since the acute radium shortage of 1909. Economic affairs and the redevelopment of the Oxford chemical laboratories also began to occupy more and more of his time, so that by the 1930s, he and Oxford were no longer active in radioactivity.[26] With Soddy's energies directed elsewhere, British physicists, it seemed, had every right to regard the atom as their "protégé."[27]

They were not slow in exploiting their charge. The imperial atom and its self-appointed spokesmen featured prominently in an exhibition organised by the Royal Society at the British Empire Exhibition in 1924,[28] at which more than a hundred displays of scientific apparatus, many of them accompanied by technical demonstrations, helped establish an extremely positive image of atomic physics and its place (at least as the scientists saw it) in the Imperial scheme of things.[29] So pervasive was the atomic theme, in fact, that even laboratories having no direct connection with atomic research offered exhibits on the subject. From Oxford's Clarendon Laboratory, Lindemann, T.C. Keeley and E. Bolton King exhibited 'A Method of Making Audible the Movement of α- and β-Particles in an Electric Field.' Using a commercial amplifier and loud-speaker supplied by Messrs. S.G. Brown Ltd. and valves and high tension batteries supplied by the Metropolitan-Vickers Electrical Co. and Siemens Bros., the Oxford workers contrived an ingenious device to

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25 Soddy's ever-popular utopian tract *The Interpretation of Radium* went into a fourth edition in August 1920, and was again reprinted in May 1922. The book was completely revised in 1932, appearing as *The Interpretation of the Atom* (Soddy (1932)). See Fleck (1957), 210-213; Trenn (1979); Cruickshank (1986); Daly (1986); Davies (1992), 356-357.

26 Note, however, the presence of radio-chemist Alexander Russell at Christ Church. See A.S. Russell (1922, 1926). Russell suceeded Soddy and Aston as compiler of the Chemical Society's "Annual Report on Sub-Atomic Phenomena and Radioactivity" (A. Russell (1931a)).

27 J.A. Crowther (1926), 365.

28 On the 1924 British Empire Exhibition, see Graves and Hodge (1991)[1940], 177-178. The science exhibition is described in the official handbook, *Phases of Modern Science* (London: The Royal Society, 1924).

29 See, for example, the correspondence between the Royal Society and O.W. Richardson, March 1925, Reel 18, AHQP/RDN.

make the small ionisation current produced by the passage of ionising particles through a chamber operate a loud speaker instead of a galvanometer or string electrometer, thereby making the atoms audible instead of visible - flippant, perhaps, but a nicely modern touch.[30]

As one might expect, Cambridge past and present was particularly well represented, though with a display of rather more conventional equipment. The apparatus with which J.J. Thomson had discovered the electron, Aston's mass-spectrograph, the Cambridge Scientific Instrument Company's Shimizu expansion chamber, dozens of positive ray photographs and much of Rutherford's apparatus graced the demonstration stands for the edification of the public. The centrepiece of the Cambridge exhibit, though, was a demonstration of the scintillation method, darkened room and all, helping to convey the directness and simplicity of the technique.[31] Unbeknown to the crowds who took their turn sitting in the darkened room to witness the feeble flashes signifying the transmutation of atoms, however, the Cambridge scintillation-counting experiments were in trouble. The first glimmers of doubt had become public the previous September during the Liverpool meeting of the British Association. Halfway through the week's procedings, Rutherford's breezy optimism regarding the "rapidity and certitude of advance in this epoch"[32] had been challenged by a surprising letter in *Nature*. Two researchers at Stefan Meyer's Institut für Radiumforschung in Vienna claimed to have detected the disintegration of several elements which had been found by Rutherford and Chadwick to be impervious to attack. Cavendish physics was suddenly called to account by an institution with the substance to make its challenge matter.

Towards the end of 1921, Stefan Meyer had received a letter from a young Swedish oceanographer-geophysicist, asking whether he might work for a time at the Vienna Institute for the purpose of making radioactive measurements connected with his work.[33]

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30 *Phases of Modern Science*, 171; "Listening to the Atom," *Literary Digest* **87** (1925), 23. On Lindemann and physics at the Clarendon, see Berman (1987); Jones (1987); Morrell (1992).
31 *Phases of Modern Science*, 165.
32 Rutherford (1923d), 23.
33 Pettersson to Meyer, 28 November 1921, SMP.

Welcomed by Meyer, Hans Pettersson, who had worked with Ramsay in London and Ångstrom in Stockholm, arrived in Vienna in the spring of 1922 to work on the radioactivity of deep-sea sediments. Soon after his arrival, he began a collaboration with Gerhard Kirsch. Kirsch, a native of Vienna, had entered University there in 1911. Completing his studies at Vienna and Uppsala after the war, he had taken his doctorate under Meyer in 1920,[34] and had then begun research at the Institute on radioactive problems suggested by Meyer, mainly concerning the relationships between radioactive decay products,[35] but also including an exploratory foray into the connections between radioactive laws and nuclear structure.[36]

Believing, like most other observers of the scientific scene, that the experiments in progress at the Cavendish Laboratory represented the most abstruse and interesting branch of physics at that moment, it was into this field that Pettersson and Kirsch launched themselves.[37] At the same time, however, *Nature* reminded its readers that it was Ramsay who had been the first to suggest and carry out this kind of experiment, having "held tenaciously to the view that the immensely concentrated energy of the α-particle offered a means of testing [the] apparent simplicity of the chemical elements." Ramsay's experiments, however, "could not at the time be convincing."[38] It was Ramsay's former pupil who now set out to test the authority of the Cavendish Laboratory: from 1923, publications on the subject of artificial disintegration began to appear from Meyer's Institut für Radiumforschung under the names of Gerhard Kirsch and Hans Pettersson.

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34 Stuewer (1985), 247.
35 Kirsch (1920a, 1920b, 1922).
36 Kirsch (1921), citing work by Rutherford, Soddy, Kossel and others.
37 Rutherford had given a series of lectures on 'Radioactivity' at the Royal Institution in March and April 1922. *Nature* thought it "no exaggeration to say that these experiments are some of the most fundamental which have ever been made." Indeed, "so fundamental are the consequences of this discovery [artificial disintegration] that the intellectual world at large must follow with the keenest interest the progress of the experiments associated with the name of Rutherford." See "Contemporary Alchemy," *Nature* **109** (1922), 601-602, 601.
38 *ibid.*, 602. For Ramsay's work on disintegration , see Trenn (1974c).

3. Discipline, Authority and the Management of Dissent: the Cambridge-Vienna Controversy

3.1 "A Valiant Effort ...": Artificial Disintegration in Vienna[39]

Having decided to enter the field of disintegration studies, an entirely new line of work for the Vienna institute, Pettersson and Kirsch began to acquaint themselves with the relevant literature and to work up the necessary apparatus and techniques.[40] As part of this programme, Pettersson developed a new method of preparing Radium C α-particle sources.[41] Finding an unexpected emission of protons from the fused silica capillary tube containing the Radium C source - 'unexpected' because the Cambridge workers had found no disintegration protons from silicon - Pettersson and Kirsch widened their investigations to include "some of the lighter elements found 'non-active' in the experiments of Rutherford and Chadwick."[42] They found, to their surprise, that beryllium, magnesium, lithium and silicon were disintegrable yielding protons with ranges between 10 and 18 cm, the result which they published in *Nature* in September 1923.[43] The article prompted a swift response from L.F. Bates and J.S. Rogers, two Cavendish research students whom we have already come across. Using the improved scintillation technique, they had been investigating the α-particles emitted by Radium C, and had found what seemed to be a series of long-range particles in addition to the well-established particles of range 7 cm. What Kirsch and Pettersson were seeing, suggested the young Cambridge researchers, were these long-range particles from the source, not genuine disintegration protons.[44] This

[39] For an excellent and comprehensive account of the Cambridge-Vienna controversy, see Stuewer (1985). The controversy is also briefly treated, from a rather different perspective, in Mladjenovic (1992), 169-173. My account is indebted to Stuewer, though I wish to modify the level of analysis by framing the controversy in terms of the analysis of certitude-making strategies established in the previous chapter, and by drawing particular attention to the changing notions of evidence and credibility in play during the course of the dispute. I shall also draw somewhat different conclusions about the closure of the controversy.
[40] Kirsch and Pettersson, "Program für weitere Untersuchungen," dated 22 June 1923, SMP.
[41] Pettersson (1923).
[42] Kirsch and Pettersson (1924b), 507.
[43] Kirsch and Pettersson (1923a).
[44] Bates and Rogers (1923); Aston (1925e), 249. Rutherford told Laby that Bates and Rogers had "managed to do an extraordinary amount of counting and I have tried to curtail their activity in that direction." See Rutherford to Laby, 6 October 1923, RP.

suggestion was firmly rebutted by Kirsch and Pettersson. "The difference in brightness between the scintillations from α-particles and from H-particles viewed under identical conditions is so conspicuous," they retorted, "that no mistake is possible. Comparing the former to stars of the first magnitude, the latter would be of about the third magnitude; that is, a ratio in luminosity of about 6 to 1."[45]

Interceding before matters got out of hand, Rutherford wrote to Meyer to check Pettersson and Kirsch's credentials. Though they had managed to produce "a valiant piece of work" in a difficult field, Rutherford impressed upon Meyer his view that Kirsch and Pettersson ought to reconsider their claims in the light of Bates and Rogers' findings.[46] Far from recanting, however, Pettersson continued to challenge Cambridge conventional wisdom, turning his attention now to Rutherford's satellite model of the nucleus. In a paper read before London's Physical Society early in 1924, Pettersson offered "an alternative hypothesis which assumes that the α-particle communicates its energy to the nucleus as a whole, precipitating an explosion which is supposed to have only a limited stability in the case of each of the elements."[47] Pettersson outlined two main lines of attack on Rutherford's model. First, the satellite hypothesis was built upon the presupposition that the small number of elements which were found to be disintegrable all had an atomic weight of the form $4n+3$ or, in the case of nitrogen, $4n+2$, from which it would "seem reasonable to attribute this quality to a peculiar structure of their nuclei." This assumption was undercut, however, by the fact that Pettersson and Kirsch had found disintegration to be "a general property, common to the nuclei of all atoms,"[48] with the consequence that there was no longer any reason to believe that these particular nuclei had a satellite structure. Moreover, the relatively large numbers of disintegration protons counted by the Vienna workers was inconsistent with the low probability of a satellite collision central to Rutherford's model. Secondly, Pettersson attacked the change in sign of Coulomb's law at

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45 Kirsch and Pettersson (1923b).
46 Rutherford to Meyer, 24 November 1923, SMP; Bates and Rogers (1923, 1924); Stuewer (1985), 249-250.
47 Pettersson (1924), 194 (abstract), read to the Society by A.W. Porter; Stuewer (1985), 250; Stuewer (1986a), 332.
48 Pettersson (1924), 199.

short distances inside the nucleus required in order to explain the stability of the satellite system. He cited the experiments of Rutherford, Chadwick and Bieler as showing that the inverse square law held down to at least 3×10^{-12} cm, providing further evidence against the satellite hypothesis.[49] This artful reinterpretation of Cavendish data was one of Pettersson's hallmarks, and one which he consistently used to good effect. But what of Pettersson's alternative hypothesis ?

In the explosion hypothesis, as Pettersson described it, "the impact against a swift α-particle brings the structure of the nucleus to a state of instability resulting in a kind of explosion, at which one or possibly more fragments are expelled." This model meshed well with a more fluid idea of nuclear structure, in which the constituents of the nucleus could be assumed to be "in a state of perpetual and enormously rapid motion" with large local variations in stability over time.[50] As to the actual mechanism of the disintegration, Pettersson assumed that the approach of the α-particle would induce electrostatic repulsion of the positively-charged constituents of the nucleus, and attraction of the nuclear electrons. These relative displacements of the nuclear constituents might be great enough to "endanger the stability of the structure and increase its chances of exploding under the shock it receives *as a whole* from the α-particle."[51]

Pettersson supported his model with a series of suggestions for further experimental research which might throw light on the vexed question of the mechanism of disintegration. First, it was essential that it be decided which elements could be disintegrated "with our present resources, i.e. with α-particles from *Ra-C* and *Th-C*." This investigation should include an attempt "to observe H-particles of *all* ranges and also eventual atomic fragments of greater mass, say X_3-nuclei, *He*-nuclei, or the residual "recoil-nuclei"."[52] This would require considerable technical development: "Working in a vacuum and with a strong magnetic field, it will be possible ... to observe ... particles of still shorter range. Their mass can, of course, best be determined through combined magnetic and electric deflection,

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49 *ibid.*, 198.
50 *ibid.*, 196, 200.
51 *ibid.*, 201, emphasis in original.
52 Pettersson (1924), 201.

say, by an arrangement analogous to that of the mass-spectrograph of Aston."[53] Another element of Cavendish technique could also be appropriated: "To investigate whether one or more H-particles are expelled from the same disintegrating atom ... can probably best be done by means of the cloud method of C.T.R. Wilson, modified by Shimuzu."[54] Innovative and forward-looking, Pettersson was beginning to establish himself as a serious competitor to the Cavendish Laboratory.

In the discussion following Pettersson's paper, Rutherford's pupil - one might even say 'disciple' - Edward Andrade characteristically poured cold water on any results which contradicted those of his master: "I have listened with great interest to Dr. Pettersson's paper, but I think it would have been of greater value if some of the experiments foreshadowed at the end had been performed before the theory was propounded ... The only experimental evidence put forward is not very striking ... The position is much as if a man having measured up a box and guessed from shaking it that it contained pieces of metal were to start speculating on the dates of the coins inside it ..." What little of Pettersson's evidence was genuinely new, complained Andrade, "furnishes a poor basis for this load of speculation."[55] Given the opportunity to reply in print to his critics, however, Pettersson ingeniously turned Andrade's metaphor to his own advantage: "even if we cannot hope to ascertain the date of the coins within the box," he retorted, "our only chance of getting to know anything at all about them seems to lie in shaking the box as thoroughly as possible, both by experiments and by speculation."[56] He also used the opportunity to express publicly his dissatisfaction with a state of affairs in which one laboratory could monopolise an entire field. He had presented his paper "not to prove that the satellite theory is wrong and the explosion theory right, but to show that the second view agrees quite as well as the other, if not better, with the few experimental data available at present." Furthermore, he considered it "not altogether uneccesary to point this out, considering that

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53 *ibid.*
54 *ibid.*
55 Pettersson (1924), Discussion, 202; Stuewer (1985), 250-251. For Andrade's attitude towards Rutherford, see Andrade (1956, 1963, 1964); Cottrell (1972), 2-3; and Andrade's pseudo-archaic dedication to Rutherford in his (1923, 1927a).
56 Pettersson (1924), Discussion, 203.

the satellite theory has already become introduced into textbooks on atomic structure without any atempts at criticism"[57] - a swipe at Andrade's own recent book *The Structure of the Atom*, which gave a rather full and uncritical exposition of the satellite model.[58] With the critics silenced - temporarily, at least - Pettersson and Kirsch continued their investigations, experimental and conceptual.

3.2 Conflicts of Evidence: The Strange Death of X_3^{++} and the Stranger Birth of O^{17}

Rutherford and Chadwick, increasingly under public pressure to justify their previous four years' work, launched a fresh initiative in the light of the results of Pettersson and Kirsch, on the one hand, and of Bates and Rogers on the other. From the autumn of 1923, as Blackett and Ellis devoted more time to their own research careers, a new pair of scintillation counters had been co-opted into the disintegration experiments. Thomas Harris Osgood and Herbert Sim Hirst had both graduated from the University of St. Andrews, and had come to the Cavendish as graduate students. Having undergone the usual evaluation and training course in the Attic, they were surprised to be selected by Chadwick for scintillation counting work. Such value was placed on their observational capacities, in fact, that Hirst continued to be employed as a counter even when, after a term or so, he tranferred to the Chemistry Department.[59]

Because of the existence of the potentially confusing long-range α-particles from Radium C disclosed by the herculean counting efforts of Bates and Rogers, observations made in a direct line from the source could no longer be regarded as reliable. Rutherford and Chadwick therefore modified their apparatus to detect disintegration protons emitted at 90° relative to the direction of the incident Radium C α-particles, enabling them to "observe

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57 *ibid.*

58 Andrade (1923), 78-79.

59 Osgood and Hirst (1964), 686. Hirst, who had technically been one of J.J. Thomson's research students at the Cavendish, subsequently completed a Ph.D. on "The Mechanism of Chemical Reaction" (1926). Clearly, Rutherford and Chadwick were prepared to use any observer they deemed to be reliable.

with certainty" artificial disintegration particles with a range down to 7 cm.[60] With this new arrangement, which was similar to one employed in Vienna, Rutherford and Chadwick found that in addition to the original six elements, a further seven - neon, magnesium, silicon, sulphur, chlorine, argon and potassium - now yielded disintegration protons. Of the light elements from boron to potassium, only carbon and oxygen still resisted α-particle bombardment. A new pattern emerged: a "marked difference between the behaviour of elements of elements of odd atomic number and those of even atomic number," a difference they illustrated in a bar chart (see fig. 3.4). This "striking difference," they noted, "seems to indicate that the nuclei of even atomic number are more firmly built than those of odd atomic number."[61] Parrying Pettersson's attack on the satellite model, they also adduced evidence to support their earlier claims about the field of force around the nucleus. A series of experiments by Etienne Bieler at the Cavendish Laboratory had shown that even at relatively large distances from the nucleus, the force was less than it should be according to the inverse square law. At any rate, they concluded, the satellite hypothesis had "the great merit of simplicity."[62]

Rutherford and Chadwick's new results were published in *Nature* on 29 March, 1924, and, some months later, in the *Proceedings of the Physical Society*, a forum clearly chosen to redress Pettersson's earlier offensive. Welcoming this corroboration of the Vienna work, Pettersson and Kirsch also took the opportunity to announce two significant new results of their own. Carbon and oxygen had yielded to analysis, they reported, the former giving protons of about 6 cm range, the latter giving "α-particles of 9 cm range in the forward direction" - the first time α-particles had been observed as disintegration products.[63] Rutherford and Chadwick, taken by surprise, made a hasty last-minute addition to their Physical Society paper in which they again suggested that Pettersson and Kirsch were in fact seeing long-range α-particles from their Radium C source. In Cambridge, they

60 Rutherford and Chadwick (1924a). The arrangement adopted was rather similar to the one used in Vienna, as Pettersson and Kirsch were quick to point out. See Kirsch and Pettersson (1924a).
61 Rutherford and Chadwick (1924b)[CPR 3, 117, 118].
62 Bieler (1924); Chadwick and Rutherford (1924b)[CPR 3, 119]; Stuewer (1985), 252-254.
63 Kirsch and Pettersson (1924a); Stuewer (1985), 252-253.

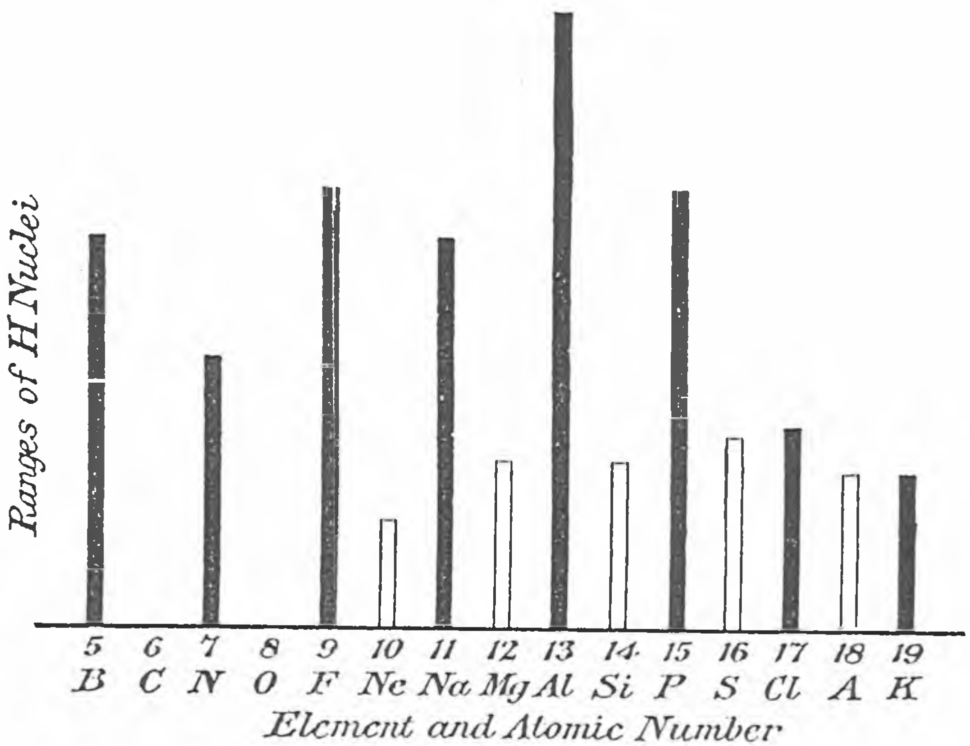

Fig. 3.4 Rutherford and Chadwick's bar-chart, illustrating range of disintegration protons from nuclei of the light elements. Rutherford and Chadwick pointed out the "marked difference between the behaviour of elements of odd atomic number and those of even atomic number," the nuclei of the former being assumed to contain free protons.

Source: Rutherford and Chadwick (1924b)[CPR 3, 117].

reported, they had made a careful study of carbon and had categorically found no disintegration.[64] In order to make safe their argument against the Viennese, however, they returned to a careful study of the long-range particles from Radium C. The results came as something of a shock, for it seemed that while Radium C did indeed emit long-range α-particles of ranges 9.3 and 11.2 cm, as Bates and Rogers had claimed, the particles of range 13 cm were probably *protons* expelled from the mica absorbers conventionally used in the scintillation-counting experiments. This confirmed that Pettersson and Kirsch were probably seeing long-range α-particles from the source, not disintegration protons. But it also meant that Rutherford's X_3^{++} particle of range 9 cm had been a chimera.[65] So the elaborate isotope models of Rutherford's 1920 Bakerian Lecture had been fundamentally flawed. But since they had represented "purely illustrative,"[66] "tentative and highly speculative"[67] remarks, no great harm was done. Rutherford's speculations on nuclear structure had moved on a great deal since 1920.

There was another cause for concern, however: the controversy was beginning to attract attention outwith the two laboratories directly involved. Alarmed at reports that Bohr was expressing a sympathetic interest in the Vienna experiments, Rutherford wrote pre-emptively (and privately) to his former pupil to warn him off the Vienna experiments:[68]

> [Pettersson] seems a clever and ingenious fellow, but with a terrible capacity for getting hold of the wrong end of the stick. From our experiments Chadwick and I are convinced that nearly all his work ... is either demonstrably wrong or wrongly interpreted. For example, he claims to get a large number of particles from carbon. We found practically none at all under the same conditions and we consider that there is no evidence at all of the disintegration of carbon. We have equally failed to observe any effect in lithium or oxygen, and only a slight trace from beryllium possibly due to impurity in the form of fluorine. ... It is a very great pity that he and his collaborators are making such a mess of things, for it is only making confusion in the subject.

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64 Rutherford and Chadwick (1924b)[CPR 3, 115-116]; Stuewer (1985), 253-254.

65 Rutherford and Chadwick (1924c); Stuewer (1985), 254; Stuewer (1986a), 333-334. If the new results were to be trusted, Rutherford had been seeing protons of range 9.3 cm range from the radium C source. As I showed in Chapter Two, doubt had begun to surround the observations on X_3^{++} as early as 1922.

66 Rutherford (1920a), 399 [CPR 3, 37].

67 Rutherford (1920b).

68 Pettersson to Rutherford, 13 July 1924, RP; Rutherford to Bohr, 18 July 1924, BSC.

> All the experiments look easy, when they are really very difficult and full of pitfalls for the inexperienced. So much is this so that I have decided not to get any other work done except under my personal eye.
>
> I am sorry that Pettersson has made such a mess but it looks to me as if he has not done nearly enough experiments on broad experimental lines to make sure of his points, but jumps precipitately to conclusions from rough evidence.

Rutherford also wrote in a similar vein to Meyer. Pettersson, he chided, "seems to me a man of originality and ingenious in his arrangements but I should judge he jumps to conclusions on insecure evidence. The subject of artificial disintegration is full of difficulties and wants investigators who are very careful in experiment and with good judgement."[69]

Rutherford's decision to take personal charge of the disintegration experiments betokens the state of alarm in the Cavendish Laboratory at the challenge from the Viennese neophytes. Not only the substantive results of the disintegration experiments were at stake. The reputation of the Cavendish Laboratory and the 'public face' of radioactivity were also involved. For their part Pettersson and Kirsch would, Meyer assured Rutherford, admit their mistake without compunction if they could be convinced that they were wrong. "But till now," he added, "we do not see what could be wrong with their experiments."[70] Furthermore, the Viennese felt "rather sure that the experiments of Bates and Rogers are not convincing," since neither in Vienna nor, apparently, in the Berlin laboratory of Otto Hahn and Lise Meitner, could any evidence of long-range particles from Radium C be found.[71] In sum, then, Pettersson, Kirsch, with Meyer's full support, were confident of their results, and expressed their continued certainty in a review article in *Die Naturwissenschaften* in June 1924.[72]

Pettersson and Kirsch were constantly modifying and refining their apparatus in an attempt to verify the contentious results. Pettersson wrote to Rutherford about the disintegration of carbon, firstly to reassure him that the H-particles observed in Vienna

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69 Rutherford to Meyer, 19 July 1924, RP. See also Rutherford to Pettersson, 19 July 1924, SMP; Meyer to Rutherford, 24 July 1924, SMP; Stuewer (1985), 255-257.
70 Meyer to Rutherford, 24 July 1924, SMP.
71 Pettersson to Rutherford, 27 July 1924 [misdated 1923], SMP.
72 Kirsch and Pettersson (1924d); Stuewer (1985), 257-260.

could not be from impurities, but also to tell him of the Vienna laboratory's "newest microscope with the scintillation screen directly attached to the front lens." This new microscope was "so superior, with regard to the brilliancy of the scintillations viewed through it, ... that we feel much more confident now, not only in differentiating between scintillations from H- and from α-particles, but also in not overlooking the former even when the particles are relatively near the end of their range."[73] And whatever Rutherford and Chadwick might have thought to the contrary, Pettersson and Kirsch were not naïve in the matter of scintillation counting. Far from it, in fact, for in Vienna as in Cambridge, rigorous protocols had been established to ensure that scintillations were counted properly. In Vienna, as in Cambridge, dark adaptation of the eyes and deep concentration were regarded as indispensible prerequisites for the counting experiments. As in Cambridge, there would be several counters alternating with each other for periods of 20-30 seconds, each set of counts being mediated by a "recorder" whose function was to call out the beginning and end of each counting period and to note down the result. And again, the counters would be required to rest between counts, and the amount of time spent counting per week would be strictly limited "otherwise chronic symtoms of fatigue soon appear and the results are unreliable."[74] The elaborate ritual surrounding the counting of scintillations was thus considered to be as important in Vienna as it was in Cambridge. But, crucially, it was these sets of protocols which would now be brought into question.

While certitude increased in Vienna, the Cavendish Laboratory was more and more on the defensive. Chadwick wrote to the absent Rutherford in the summer of 1924 to appraise him of the latest news:[75]

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[73] Pettersson to Rutherford, 8 August 1924, RP; Stuewer (1985), 259-260.
[74] Pettersson (1929a), 85-86; Stuewer (1985), 304 n.192. The most comprehensive account of the protocols for scintillation-counting in Vienna is given in Pettersson and Kirsch (1926a), 224-227.
[75] Chadwick to Rutherford, [September 1924], RP. Rutherford was attending the meeting of the British Association in Canada.

> I counted the number of disintegration particles from aluminium under as definite conditions as were possible. As near as the experiments allow the number agrees with that calculated on the assumption of an attractive inverse fourth and repulsive inverse square taking (1) zero force at 4×10^{-3} and (2) that the H particle appears when the α disappears. Of course the agreement cannot be very good on account of counting error and error in estimating the solid angle of α's used.

This was not very encouraging. But Chadwick also had more sanguine tidings:[76]

> Blackett has got two more photographs which are somewhat clearer than the others. They show the track of the H particle, the track of the recoil atom but no track for the α (unless it is a very short one or what he calls the recoil track is the α track). If this is true it is a very fine addition to the evidence for the attractive field and fits in very well with our expectations.

In the summer of 1924, after extended trials with the automatic expansion chamber, Blackett had obtained a series of photographs - 24,000 of them - showing some 270,000 α-particles of 8.6 cm range and 145,000 of 5.0 cm range.[77] Of these 400,000-odd tracks, exactly eight showed disintegrations in which the incident α-particle was captured by the target nucleus, expelling a proton and leaving, presumably, a residual nucleus of atomic number 8 and mass 17 - a "hitherto undetected" isotope of oxygen (fig. 3.5). And in the context of Rutherford and Chadwick's "expectations," eight tracks constituted evidence enough.[78]

While Blackett's crop of results was good news for Rutherford and Chadwick, however, it was extremely bad news for Aston. The mass-spectrograph had given no indication of the existence of an oxygen isotope of mass 17. Worse, the results of all the other measurements deriving from mass-spectra depended on the integrity of the standard O=16. A breach of this standard would mean that all the mass values upon which Aston's nuclear energy calculations were based would be invalid. Aware of the consequences of his interpretation and of the conflict of evidence created by the apparent existence of the new

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76 Chadwick to Rutherford, [September 1924], RP; Stuewer (1986a), 334.
77 Blackett laboratory notebooks B2-B5, PMSBP.
78 Blackett (1925); Lovell (1975), 9-10.

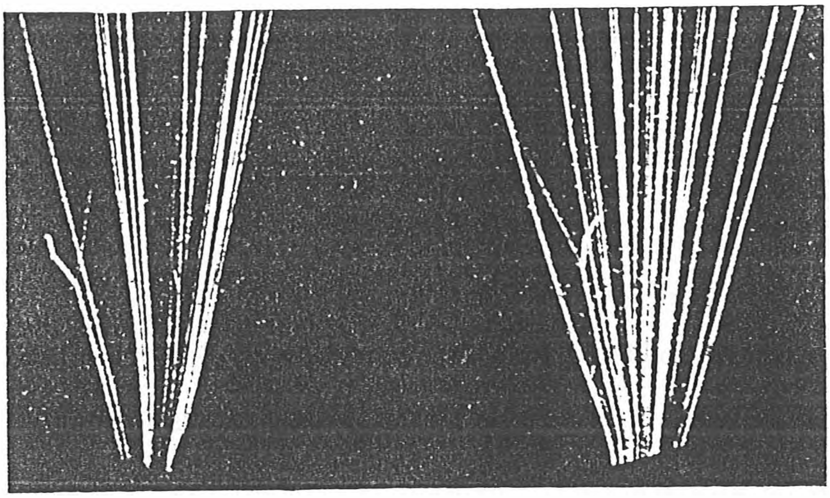

Fig. 3.5 Blackett's stereoscopic cloud chamber photographs of the artificial disintegration of a nitrogen nucleus. Russell (1926), described this photograph as "the famous one in which the newly-born nucleus of atomic number 8 and mass 17 is seen bending thickly round to the right while the ejected hydrogen particle makes a bee-line to north-west almost in line with the α-particle which begat it."

Source: Blackett (1925).

isotope, Blackett undertook some hasty repair-work in an attempt to explain away the dilemma. It might be that the integrated nucleus had a very short life, for example, or, if it were stable, it must exist on the earth "in such small quantities as to escape detection in the mass-spectrograph of Aston, or by its influence on the chemical atomic weight of oxygen."[79]

The plausibility of this somewhat ad hoc defence of the integrity of the mass-spectrograph was compromised by another, equally serious, conflict of evidence. In 1923 Harkins and R.W. Ryan of Chicago had taken about 21,000 photographs of α-particle tracks in air using the Shimizu method. One alone of their photographs showed a collision in which the α-ray track diverged into *three* branches - indicating that the α-particle caused a disintegration without itself being captured, and therefore standing in conflict with Blackett's photographs.[80] Nor was this all. Using a similar method, M. Akiyama in Japan was able to produce a photograph also showing three branches, in which the proton was expelled backwards, while the tracks of the recoiling residual nucleus and α-particle were clearly visible. It was, confessed Rutherford, "difficult to reconcile these photographs with the eight obtained by Blackett in which no third branch has been noted; but it may prove significant that the collisions photographed by Harkins and Akiyama appear to have occurred when the α particle has lost a good deal of its range. It is obvious that there is still much work to be done to clear up these difficulties."[81] For the moment, however, it suited Rutherford to ignore the contrary evidence and to credit Blackett's eight photographs.[82]

While Blackett left to spend the academic year 1924-25 at Göttingen, where he worked

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[79] Blackett (1925), 356, 357. For Aston's own management of the problem, see Aston (1925e), 248.
[80] Harkins and Ryan (1923a, 1923b).
[81] Rutherford (1925b)[CPR 3, 137]; "Notes on the collision of α-pt with light atoms," PA 246, RP.
[82] Rutherford (1925b). A typescript copy of Blackett (1925) with manuscript corrections exists in the Rutherford papers, at PA 26A*, RP. Unfortunately, Blackett's photographs also lent distinct support to Pettersson's mechanism for atomic disintegration, putting Rutherford in an invidious position since he had recently persuaded Pettersson to withdraw from publication a manuscript establishing a claim similar to Blackett's. See Rutherford to Pettersson, 5 March 1925, RP; Kirsch (1925b), 459; Stuewer (1985), 264-266; Stuewer (1986a), 334.

with James Franck on the excitation of hydrogen spectra by electron impact,[83] Rutherford summarised Cambridge views on atomic structure before Philadelphia's Franklin Institute in September 1924.[84] Generalising his model to explain radioactive decay as well as the structure of the light elements, Rutherford supposed that "the α- and β-particles which are liberated from [the radioactive] elements are not built deep into the nuclear structure but exist as *satellites* of a central core ... held in equilibrium by the attractive forces arising from the core."[85] The Bohr model of the extra-nuclear electrons now served as a useful basis for further elaboration, for if a particle were supposed to "occupy in the [nucleus] one of a number of "stationary" positions analogous to the "stationary states" of the electrons in Bohr's theory of the outer atom," the emission of γ-rays could be explained by transitions of α-particles between nuclear levels. With Blackett's results and the development of Rutherford's new model unifying radioactive decay and artificial disintegration in a coherent picture of nuclear structure, confidence began to increase once more in the Cavendish Laboratory.[86] Rutherford left for an extended tour of Australia and New Zealand, certain of his laboratory's capacity to deal with anything that Pettersson and Kirsch might care to throw at it.

3.3 'Experientia Docet': Discipline, Certainty and the Management of Dissent

The tit-for-tat argument between Cambridge and Vienna, each claim from one side being met with a counter-claim from the other, had to come to a head sooner or later. The moment came in the summer of 1925, when Kirsch published another long and comprehensive attack on the Cambridge results. Criticizing Rutherford's "unnecessarily complicated and specialised" disintegration hypothesis, Kirsch put forward a model of the

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[83] Blackett to the Royal Society, 27 February 1924; Jeans to Rutherford, 28 February 1924; Rutherford to Jeans, 4 March 1924; Jeans to Rutherford, 4 April 1924, CD 211-212, RSL; Blackett (1972), 57-58; Lovell (1975), 10-12. Blackett took up the cloud chamber work again on his return to Cambridge, now in collaboration with E.P. Hudson, a first-year graduate student at King's, Blackett's own college. See Blackett (1927a, 1929a, 1929b); Blackett and Hudson (1927).
[84] Rutherford (1924c); Stuewer (1986a), 338-341.
[85] Rutherford (1924c), 732-733, emphasis in original.
[86] Stuewer (1985), 262 ff.; Stuewer (1986a), 341.

nucleus as consisting of alternate levels of "quasi H particles and free electrons," a view he supported by an analysis of the absorption curves of the disintegration protons from nitrogen and aluminium. The data from aluminium, in particular, seemed to Kirsch to suggest the existence of two different groups of H-particles, one of much greater range than the other. This implied the existence of two nuclear levels, the long-range group of H-particles being produced from the inner level by the most rapid α-particles.[87] Coupled with an analysis of the energetics of close nuclear collisions, Kirsch's paper represented one of the most comprehensive attacks yet on the work of the Cavendish.

The pointed attack and new round of speculations proved too much for Chadwick, who wrote to keep Rutherford abreast of the latest news:[88]

> Our friend Kirsch has now let himself loose in the Physikalische Zeitschrift. His tone is really impudent, to put it very mildly. He takes our old experiments the very first results and proceeds to show what fools we are to talk of satellites and then tells what clever fellows he and Pettersson are to think of a nucleus which is simply chock full of satellites rings and rings of them. Kirsch & Pettersson seem to be rather above themselves. A good kick from behind would do them a lot of good. The name on the paper is that of Kirsch but the voice is the familiar bleat of Pettersson. I don't know which is the boss but as Mr Johnson said there is no settling a point of precedence between a louse and a flea.

Kirsch's attack would soon be followed by another surprise: the publication of *Atomzertrümmerung: Verwandlung der Elemente durch Bestrahlung mit α-Teilchen*, a full-length monograph in which Pettersson and Kirsch pressed their claims harder and at greater length than ever.[89] It was a remarkable performance. As Roger Stuewer has pointed out, in just three years Pettersson and Kirsch had established the Institut für Radiumforschung as a serious competitor to the Cavendish Laboratory, and were reaching German-speaking

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87 Kirsch (1925b).

88 Chadwick to Rutherford, [July-August 1925], RP; Stuewer (1985), 271. For another example of the problems of evaluating scientific evidence at a distance, see Doel (1992).

89 Pettersson and Kirsch (1926a); Stuewer (1985), 271-272. Perhaps aware of the forthcoming publication from Vienna, Rutherford had decided late in 1924, after his return from Canada and the United States, to embark upon a new edition of his book *Radioactive Substances and their Radiations* (1913), now over a decade old. He co-opted Chadwick and Ellis to assist in the work. See Rutherford to Roberts, 7 December 1924, Cambridge University Press archives, CUL. I am grateful to Dr. E. Leedham-Green for granting me access to these papers.

audiences as effectively as their Cambridge counterparts were reaching English-speaking ones.[90]

Pettersson and Kirsch were also becoming the focus of a group of young researchers whose names became familiar to readers of the *Sitzungsberichte* of the Vienna institute from 1925 on. These co-workers began to copy and develop the array of techniques and practices which sustained the Cambridge programme, and to use those techniques to Vienna's advantage in the ongoing controversy. Ewald Schmidt, for example, modified Pettersson's apparatus to measure disintegration particles from aluminium at an angle of 150° by the scintillation method, and also found that the relatively low-energy 3.9 cm α-particles of polonium were capable of effecting disintegration just as easily as the more energetic 7 cm α-particles of Radium C, and that counting was actually easier using a polonium source because of the absence of background illumination from γ-rays.[91] Elisabeth Kara-Michailova worked with Pettersson to show that α-particle and H-particle scintillations could be distinguished from each other - a crucial plank in the defence of the Vienna results.[92] Gustav Ortner worked on the preparation of sources for the counting experiments,[93] while Marietta Blau attempted to confirm Kirsch and Pettersson's results by developing a method of recording disintegration protons on a photographic film. In her investigations too, polonium became a central resource because of its lack of β- and γ-rays - a problem which made it difficult to use Radium C sources in her experiments.[94] Finally, and perhaps most crucially, Georg Stetter designed and constructed an apparatus similar in its arrangement to Aston's mass-spectrograph. The photographic plate used in Aston's machine was, however, replaced with a zinc sulphide scintillation screen and microscope, enabling disintegration fragments to be separated by mass, detected and counted. The Vienna group were slowly developing exactly the techniques and practices pioneered and

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90 Stuewer (1985), 267-268. Compare Latour (1987), 79-94, esp. 91.
91 Schmidt (1925); Stuewer (1985), 274.
92 Kara-Michailova and Pettersson (1924a, 1924b). One of the charges made by Rutherford and Chadwick against Pettersson and Kirsch was that the Vienna results were due to the counters mistaking α-particles from the source for genuine disintegration particles. See Stuewer (1985), 278-279.
93 Ortner and Pettersson (1924).
94 Blau (1925a, 1925b).

deployed at the Cavendish Laboratory. In many cases, indeed, they were refining and elaborating those techniques to a pitch far beyond that achieved in Cambridge. Small wonder, then, that Chadwick was worried about the Vienna workers getting "above themselves."

These challenges to the authority of the Cavendish Laboratory, while unwelcome in Cambridge, were greeted elsewhere with enthusiasm, perhaps even a tinge of enjoyment at the discomfiture of the Cavedish 'élite.' Sheffield physicist Robert W. Lawson, for example, wrote to Meyer in December 1924 to express his pleasure at the success of Pettersson and Kirsch.[95] In 1927 Lawson wrote an extremely sympathetic review of Pettersson and Kirsch's *Atomzertrümmerung*, in which he pointed out that Pettersson and Kirsch aimed only "to arouse the interest of physicists in this fascinating branch of research and to stimulate others to take an active part in its development," an implicit attack on the complacency of the Cavendish. It was a point which Pettersson himself had stressed in his paper to the Physical Society in 1924. He had been keen, he said, "to direct the attention of other experimenters to some problems which appear to be of considerable importance to our view on nuclear structure," an objective which "may seem to have some justification, considering how surprisingly little experimental work has been done within this most central field of research during the five years which have elapsed since it was first opened by Sir Ernest Rutherford."[96]

Nicely egalitarian sentiments, and it was certainly true that after Ramsay's death and Soddy's effective withdrawal from radioactivity research Rutherford had no effective critic, at least from within his own discipline.[97] But the difficulties and inconsistencies between Cambridge and Vienna nevertheless remained. Matters were aggravated by the fact that during the same period, Charles Ellis of the Cavendish was involved in a second, parallel

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[95] Cited in Stuewer (1985), 267. Recall that Lawson had been trapped in Vienna at the outbreak of the war in 1914, and had been forced to remain there for the duration. His predicament was eased considerably by financial and material support from Meyer.
[96] Pettersson (1924), 203 (discussion).
[97] A point noticed also by Trenn, who argues that "the lack of criticism and independent experimental double check before acceptance [of Rutherford's results on the disintegration of nitrogen] stood in striking contrast to the treatment meted out to Ramsay" (Trenn (1974c), 77).

controversy with Lise Meitner of Berlin's Kaiser Wilhelm Institute over the continuous β-ray spectrum.[98] Like the Vienna controversy, the Cambridge-Berlin dispute involved not simply differences of opinion about the interpretation of experimental facts, but profound disagreements about what the 'experimental facts' *were*.[99]

Rutherford's policy in controversy was to keep dissent to manageable proportions, a stance deliberately designed to minimise public disagreement.[100] As he jocularly put it to Lise Meitner, in the case of disagreements "it is much better to discuss matters in a friendly way without rushing into print ... I now have two grandchildren and in general, take a grandfatherly attitude even in science."[101] *Especially* in science, he might have said. Rutherford's insistence on the containment of dissent and on the proper - and above all *private* - conduct of scientific disputes was part of a deliberate strategy to present the scientific (and especially the experimental) enterprise as a model of calm, rational and reliable decision-making, and thereby to obtain credit for it in the wider culture.[102] As he had put it to Pettersson, "it is better to discuss these divergences of view in private than in print. Workers in this field are too few and too select to misunderstand one another."[103]

While Rutherford refused to engage in public squabbles himself, he seems to have been happy for those not directly involved in the controversy to attempt to exercise some small influence - provided it was in the right direction. In 1922, Aston succeeded Soddy as the

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98 Among the most important papers in the Cambridge-Berlin controversy are Ellis (1921, 1922a, 1922b, 1922c, 1924); Ellis and Aston (1928, 1930); Ellis and Skinner (1924a, 1924b, 1924c); Ellis and Wooster (1925a, 1925b, 1925c, 1927a, 1927b, 1927c, 1927d); Meitner (1922a, 1922b, 1922c, 1922d, 1923b, 1924b, 1928b); Smekal (1922). On Ellis, see Hutchison, Gray and Massey (1981), esp. 204-214. For Meitner, Frisch (1970); Kerner (1988), 60-72; Rife (1990). The Meitner-Ellis controversy is treated cursorily by Watkins (1983) and Mladjenovic (1992), 196-199. Jensen (1990), 93-252, gives a comprehensive account of the debate and its outcome. I thank Roger Stuewer for informing me of, and Finn Aaserud for sending me a copy of Jensen's dissertation.

99 Ellis and Wooster (1925c), 859, noted that the difference of opinion between Cambridge and Vienna was "not only about the interpretation but even about the experimental facts."

100 As we have seen, for example, Rutherford had already conducted an extensive private correspondence with both Pettersson and Meyer in an attempt to settle misunderstandings and reach agreement, albeit on his own terms.

101 Rutherford to Meitner, 21 October 1926, LMP. This had not always been Rutherford's policy: see Rutherford to Boltwood, 10 October, 12 November 1905, in Badash (ed.)(1969), 90-91, 97-99 respectively; Rutherford to W.H. Bragg, 4 November 1905, WHBP; Wilson (1983), 214-215.

102 Cf. Shapin (1984); Shapin and Schaffer (1985), 72-76. For Rutherford's insistence on privacy, see Rutherford to Bohr, 18 July 1924, 8 February 1926, RP. Rutherford's position as President of the Royal Society from 1925 to 1930 only served to exacerbate the embarrassment caused by his involvement in the increasingly acrimonious controversy with Kirsch and Pettersson.

103 Rutherford to Pettersson, 19 July 1924, SMP; Stuewer (1985), 256.

compiler of the Chemical Society's "Annual Report on the Progress of Radioactivity," now re-named the "Annual Review of Sub-Atomic Phenomena and Radioactivity."[104] Under Aston's stewardship, the Report effectively became a vehicle to publicise the latest results from the mass spectrograph and the work of the Cavendish Laboratory in general. Reaching a large audience who looked to it for an authoritative summary of recent developments, it also served as a convenient platform from which to put across the Cavendish position in the controversy with Vienna:[105]

> Using Rutherford's original method [Kirsch and Pettersson] claim to have disintegrated carbon and oxygen and also to have obtained large effects from beryllium. Time will show if these claims can be substantiated, but in the meanwhile, when it is remembered that the technique of the scintillation method is one full of pitfalls only to be avoided by years of research, the balance of the evidence is overwhelmingly on the side of the more experienced investigators.

Aston's remarks, rich in the rhetorical strategies familiar to modern analysts of scientific discourse,[106] demonstrate vividly that the dispute was no longer about disintegration. It was about Pettersson and Kirsch's competence (or lack of it). And it was about Rutherford and Chadwick's experience and authority.

3.4 Observers Observed: Scintillation Counting and its Troubles

Unable merely to assert that authority, however, Rutherford and Chadwick were forced to answer Pettersson and Kirsch's criticisms, the principal of which related to the microscopes used in Cambridge, in an attempt to re-establish the credibility of the Cambridge work. Chadwick commissioned a new microscope from Hilger & Co. which gave a much larger field of vision than that to which the Cambridge counters were accustomed. While scintillations appeared "much brighter" in the new device, according to Chadwick, extensive comparative tests with the old microscope showed that "while the counting was

104 Aston (1923i), 267.
105 Aston (1925e), 247. Compare Andrade (1927a), 94-99.
106 See, *inter alia*, Gilbert and Mulkay (1984a); Woolgar (1988), esp. 69-72; Gross (1990); Myers (1990).

much easier with the new microscope about the same number of scintillations was observed with both." The old optical system could therefore be regarded as "trustworthy."[107]

Chadwick set out to dispose of the remaining Viennese objections, one by one. Having established the trustworthiness of the Cambridge microscopes to his own satisfaction, he now turned to the counters themselves. The Cambridge counters had had "a long and varied experience in counting scintillations," he noted, and the assistants had been "carefully trained" to the point where "[c]omparison of the observations of one counter with those of another ... revealed only small variations in efficiency."[108] He now offered what he hoped would be a conclusive demonstration of this fact. A few years earlier, Geiger and Werner, at the Physikalische-Technische Reichsanstalt, had undertaken a re-evaluation of the number of α-particles emitted by radium, a figure assumed to be one of the fundamental constants of radioactivity since it gave a measure of the lifetime and heat emission of radium. In an effort to improve the certitude of the scintillation counting process, they had developed a counting technique involving the *simultaneous* observation of a scintillation screen by *two* counters using *two* microscopes. Each observer recorded the occurrence of a scintillation by tapping an electrical contact key connected to the moving tape of a chronograph. If N was the total number of scintillations which occurred on the screen, and λ_1 and λ_2 the efficiencies of the two observers, the first observer would record $N_1=\lambda_1 N$ marks on his tape, the second $N_2=\lambda_2 N$. The number of coincidences on the tape (the number of scintillations seen by both observers) would then be $C=\lambda_1\lambda_2 N$, and since N_1, N_2 and C could be determined from the tape, the efficiencies of the observers could be calculated.[109]

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107 Chadwick (1926), 1061. See also Rutherford (1926b), 838. It is perhaps worth remarking that, notwithstanding Chadwick's public remarks, Osgood and Hirst, the student counters, noted that the 1925 Hilger microscope was "a brute," whose field of view was so extensive that the observer was required to rely on peripheral vision, and which ultimately proved so tiring on the eye that it was only used a few times. See Osgood and Hirst (1964), 685.
108 Chadwick (1926), 1061-1062; Stuewer (1985), 270.
109 Geiger and Werner (1924); Aston (1925e), 253; Chadwick (1926), 1062. See also Stuewer (1985), 270. On the notions of disciplining the observer and the 'personal equation,' cf. Schaffer (1988).

Using the Geiger-Werner method of coincidences, Chadwick systematically tested a number of students as part of the 'Nursery' training course in October 1925.[110] Again, he found the Cambridge counters "trustworthy." All in all, Chadwick concluded, he was "unable to suggest any explanation which will account satisfactorily for the differences between [our] results and those obtained in Vienna."[111] There only remained the possibility that the Viennese were mistaken in believing that they could distinguish between scintillations due to α-particles and those due to H-particles by the difference in brightness, a practice which the Viennese vehemently defended.[112] The two sides had, it seemed, reached deadlock.

Or so it would have seemed to a reader of the open literature. Chadwick's public confidence about the integrity of the materials, methods and manpower employed at the Cavendish Laboratory was in fact belied by a recurrence of his private misgivings about the scintillation technique. The coincidence counting experiments were not to test nature, after all: they were to test the experimenters. Chadwick was forced to return to ever-more basic aspects of the experiments, testing the scintillation screens and counters with scattered hydrogen from paraffin, a surrogate which provided a way of guaranteeing that the scintillations on the screen were due to protons of a reasonably constant velocity.[113] Even under such basic conditions, however, it was necessary to gerrymander the conditions of the experiment in order that the calibration of the counters made any sense at all - a point clearly illustrated by an entry in the laboratory notebook for 29 January 1926, in Chadwick's handwriting:[114]

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110 Chadwick tested the students against each other and against himself (he enjoyed the reputation of possessing phenomenal powers of observation, with an efficiency of about 98% - see Sargent (1980), 97). For an actual example of this method in operation in the 1926 Attic course, see Feather, "Record of Observations, Cavendish Laboratory," 6-19 October 1926, FEAT 13/1, NFP.

111 Chadwick (1926), 1075; Stuewer (1985), 270-272.

112 Chadwick (1926), 1075. See also Rutherford to Bohr, 8 February 1926, RP: "The idea that you can discriminate between slow α particles and H particles by the intensity of the scintillation is probably the cause of [Pettersson and Kirsch] going wrong. Under normal conditions such a discrimination by eye is terribly dangerous."

113 See, for example, laboratory notebook, 1 February, 4 February 1926, CHAD III 2/7, JCP. On the notion of the experimenter's regress and for the use of a surrogate phenomenon in calibration, see Collins (1985), esp. 100-106, 125-127.

114 Rutherford-Chadwick laboratory notebook, 29 January [1926], CHAD III 2/7, JCP, my emphasis.

> Test of counters by Geiger's method of coincidences.
> Objective: .45 16mm with holoscopic eyepiece
> Scintillations α particles at end of range [&] possibly a few Hs
> Counters myself & [M.C.] Henderson.
> The best rate of scintillations is about 10-15 per minute. Not more than 20 *or it is difficult to get coincidence in registration.*

The counters were counting greatly reduced numbers of particles - less than half the number they would be required to count under 'normal' experimental conditions. So when Chadwick affirmed in print that the counters had been found "trustworthy," he was doing little more than presenting a public re-certification of the Cambridge work for the benefit of the Viennese and a defence of it before a wider audience. In the chaotic and private world of Rutherford's research room, however, the calibrations did not seem quite so certain. On February 1st 1926, "the α-ray tube contained too much emanation & the ZnS screen was very bright indeed"; three days later "the screen was still very bright - certainly brighter than we should use in disintegration counting."[115] And so on. Doubt persisted.

3.5 *J' accuse*: Cambridge, Vienna and the Midwife Toad

By mid-1926, then, the possibility of reaching an amicable and early conclusion to the controversy seemed more remote than ever. In Cambridge, the doubt surrounding the scintillation technique was becoming entrenched as the basic assumptions underlying the use of the method were questioned. That doubt arose partly from uncertainty as to the true competences of the Viennese workers and (ironically) in part from the inconclusiveness of the calibration methods employed by Chadwick in an attempt to vindicate the Cambridge work. As with the parallel controversy between Ellis and Meitner, it was becoming increasingly clear that the debate was no longer about results - data, hypothesis, theory. It had become an explicit dispute about experimental and observational technique, involving issues of competence, credibilty, trust and authority.[116]

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[115] Rutherford-Chadwick laboratory notebook, Lent Term [1926], CHAD III 2/7, JCP.
[116] See, for example, the comments in Pettersson to Meitner, 26 May 1926; Stetter to Meitner, 29 September 1926, LMP.

The stalemate seemed set to continue. But the summer of 1926 witnessed a profoundly shocking event which indirectly catalysed attempts to repair scientific relations between Cambridge and Vienna. In many ways, the artificial disintegration controversy bore a distinct resemblance to another dispute, also between laboratories in Cambridge and Vienna, and also centred on the issue of the credibility of a crucial piece of evidence. The earlier dispute, which came to a sudden and violent conclusion just as relations between the Cavendish Laboratory and the Vienna Radium Institute reached their nadir, concerned the work of a young researcher at the Biologische Versuchsanstalt in Vienna, an institution founded and directed by Hans Przibram, brother of Karl, Stefan Meyer's colleague at the nearby Institut für Radiumforschung.[117] The sceptical challenge came from a senior Cambridge academic, William Bateson.[118] Its consequences were to be fatal.

In 1909, Paul Kammerer had published a number of photographs - the results of a series of experiments on the nuptial pads of the midwife toad - showing that acquired characteristics could be inherited, and therefore seeming to support a neo-Lamarckian view. Kammerer's startling claim was contested in 1910 by Bateson, then Professor of Biology at Cambridge. A strong supporter of Mendelism, Bateson refused to credit Kammerer's photographs and visited Vienna in September 1910 to examine Kammerer's evidence for himself.[119] Nothing was settled, however, due to Kammerer's inability to produce the original specimens which had featured in the photographs. Nevertheless, Bateson was so alarmed at Kammerer's ability to produce results which contradicted the Mendelian dogma that he determined to dispose of them once and for all.[120]

The controversy flared up again in 1919 in the columns of *Nature*. Bateson's hostility now extended far beyond distrust in Kammerer's photographic evidence to encompass thinly-veiled accusations of fraud on Kammerer's part. The decisive episode in the

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117 On the Przibram family, see Koestler (1971), 10-11. On Karl Przibram and the Institut für Radiumforschung, see Karlik and Schmid (1982), 153-154.
118 See Koestler (1971) for an account of this controversy and its outcome.
119 Koestler (1971), 40-56, esp. 50-55.
120 Bateson to his wife, 28 September 1910, quoted in Koestler (1971), 54. Doel (1992), gives a similar example, in which a visit was necessary to allow the assessment of the credibility of a singular (and crucial) piece of evidence. The outcome in Doel's example, however, was that the sceptic ultimately credited the evidence.

controversy came in 1923 when Kammerer visited England at the express invitation of the Cambridge Natural History Society. Kammerer's lecture to the Society on 30th April was not attended by his sceptical opponent, but nevertheless caused such a stir among scientists and in the Press that he was invited to repeat the lecture before the Linnean Society in London on 10 May.[121] Although Bateson was present at this meeting, he petulantly refused to examine Kammerer's specimens. Kammerer took advantage of this omission by noting that if Bateson had looked closely at the specimens, "it might have been possible for me to make him see what he did not wish to see."[122] Bateson, on the other hand, compared one of Kammerer's photographs to "spirit photographs like those handed about a few years ago."[123] Again, nothing was resolved. The bad feeling and suspicion continued.

In an attempt to remove the cloud of suspicion surrounding Kammerer's work, Przibram invited Bateson to Vienna. Bateson gracelessly turned down the offer. For a further three years the controversy hung in the air. Then, in 1926, a new scandal erupted. G.K. Noble, Curator of Reptiles at the American Museum of Natural History and an active member of the anti-Kammerer lobby, visited the Biologische Versuchsanstalt in Vienna. With the consent of both Przibram and Kammerer he examined the last surviving specimen of midwife toad, and authoritatively pronounced the 'evidence' to have been forged.[124] Noble made his allegations public in *Nature* on 7 August 1926; six weeks later Kammerer took his own life on a lonely mountain road.

Kammerer's suicide sent shock-waves reverberating around the scientific world. Through *Nature*'s good offices, Hans Przibram cautioned the scientific public about the proprieties of scientific conduct: "This sad end to a precious life may be a warning to those who have impugned the honour of a fellow worker on unproven grounds."[125] And it is

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[121] Koestler (1971), 66-70, 76-77, 84-89. Kammerer was apparently only the second 'enemy' scientist to visit England after the war: Koestler (1971), 75.
[122] Koestler (1971), 79.
[123] Quoted in Koestler (1971), 78.
[124] Noble (1926); Przibram (1926a); Koestler (1971), 94-95. Not all visits need be destructive: see Doel (1992), esp. 260.
[125] Przibram (1926b).

against this tragic background of scepticism and suspicion that scientific relations between Cambridge and Vienna in the mid-1920s must, I think, be understood.

3.6 'The benefit of the doubt ...': Pettersson visits Cambridge

Political conditions were also changing, the Locarno Pact of October 1925 and the admission of Germany to the League of Nations in 1926 signalling a major shift in international relations. This political *rapprochement* was echoed by the International Research Council which lifted its boycott on German scientists in 1926, making full international scientific exchange legitimate once again.[126] In November 1926, only weeks after Przibram's warning, Hans Thirring, Profesor of Physics at Vienna University and a colleague of Meyer, took advantage of the new political climate to visit Cambridge.[127] Plans for an exchange of visits between the belligerents in the disintegration controversy were mooted, but were set aside as impracticable, despite Rutherford's agreement with Meyer that "it is highly important that this whole question should be amicably settled for I myself feel the whole subject of nuclear disintegration must remain in confusion pending a comparative investigation."[128]

While Rutherford welcomed the prospect of "an interchange of visits between [the Cavendish] Laboratory and your Institute, to get at the bottom of the reasons for the differences in results obtained in the two Institutions,"[129] it was to be another year before such a visit would come about. During that year, Pettersson and Kirsch continued to publicise their own work and to attack results emanating from Cambridge. In a comprehensive rebuttal of Chadwick's 1926 paper, for example, they again challenged the efficiency and appropriateness of the microscopes used for counting experiments in Cambridge, and pointed to a series of flaws in Chadwick's deployment of the Geiger-

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126 Cock (1983).
127 Meyer to Rutherford, 17 December 1926, RP; Stuewer (1985), 272. On Thirring, see Karlik and Schmid (1982), 154.
128 Rutherford to Meyer, 23 December 1926, RP.
129 *ibid.*

Werner method. They also suggested that the results of counting experiments depended on the α-particle source used.[130] With Radium C, the source commonly used in Cambridge, the background illumination due to γ-rays from the source gradually diminished during the course of an experiment, with the result that the fainter scintillations then became "apparent against the darker background like the fainter stars coming out against a darkening evening sky." This phenomenon, claimed the Viennese, could be avoided by using a polonium source, which emits no γ-rays. The background would then remain "practically dark in the whole course of the experiment" giving "optimal" conditions for counting.[131]

Pettersson and Kirsch, then, continued to prosecute their case with vigour. And in the wake of the Kammerer affair, the controversy was being played out before a wider audience.[132] In an attempt to clear matters up once and for all, Pettersson finally visited the Cavendish Laboratory after one of his regular trips to Göteborg in the spring of 1927. He was evidently well-received by Rutherford and Chadwick, who treated him to "the usual Anglo-Saxon hospitality." During his few days' stay, Pettersson had extensive discussions with Chadwick, Rutherford, and other members of the laboratory, including Aston and Blackett. All aspects of the Vienna and Cambridge experiments were discussed, from the discrepancies in the results to the detailed protocols of scintillation counting,[133] and Pettersson was shown the apparatus used in the Cavendish Laboratory. Although nothing was resolved during Pettersson's visit, Rutherford told Meyer that the conversations with Pettersson would be "very useful in removing misunderstandings on both sides, even if they do not settle the points at issue."[134] They did not. Arrangements were therefore made for Chadwick to make a return visit to Vienna in December, for after the Cambridge discussions Pettersson felt sure that "he will have to see our experiments himself in order to give a definite judgement on our results."[135]

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130 Kirsch and Pettersson (1927a, 1927b); Pettersson (1927a, 1927b); Pettersson and Kirsch (1927); Stuewer (1985), 274-275.

131 Pettersson and Kirsch (1927), 5-8; Stuewer (1985), 274.

132 See, for example, the remarks in "The New Physics," *Nature* **118** (1926), 865-867, on 866.

133 Pettersson to Meyer, 16 May, 17 May 1927, SMP; Stuewer (1985), 281-282.

134 Rutherford to Meyer, 1 June 1927, SMP. See also Meyer to Rutherford, 25 May 1927, SMP; Pettersson to Rutherford, 31 May 1927, RP; Stuewer (1985), 282.

135 Pettersson to Rutherford, 31 May 1927, RP; Stuewer (1985), 283.

In the autumn of 1927, Chadwick's impending visit to Vienna was given added urgency by the appearance of R.W. Lawson's extremely sympathetic review of Pettersson and Kirsch's *Atomzertrümmerung* in *Nature* - undoubtedly the most influential science journal in the world - on 6 August.[136] Entitled 'Modern Alchemy,' Lawson's piece not only drew public attention explicitly to the controversy, but actually praised Pettersson and Kirsch for venturing into and expanding the horizons of the field of artificial disintegration. Since its inauguration in 1910, after all, the Vienna Institut für Radiumforschung had been "an active centre of radioactive research, and possesses ideal facilities for such work."[137] According to Lawson "the lack of ... radioactive preparations is a real difficulty"[138] to the prosecution of similar researches elsewhere. Indeed, "experiments of this nature require the use of appreciable quantities of radium, [which] undoubtedly accounts for the fact that so fascinating a study has not been taken up in many more laboratories."[139] Moreover, the circle around Pettersson and Kirsch was increasing its output and extending its grip on the techniques being used in Cambridge. Soon after Blackett's photographic demonstration of the capture of the α-particle by the bombarded nucleus, R. Holoubek had joined the circle and began to develop a modified form of the Shimizu cloud chamber to detect and display disintegration fragments. Although the technique was, he acknowledged, more suitable for qualitative than for quantitative work, he considered his early results to confirm the disintegration of carbon by polonium α-particles, thereby supporting Pettersson and Kirsch.[140] Ortner joined forces with Stetter to develop a method of electrical amplification of ionisation currents, based on a method recently proposed by Greinacher.[141] Experimenting with a 3-valve amplifier, they were able to amplify the current produced by H-particles enough to operate a loud-speaker, much as Lindemann had done in 1924. Ortner and Stetter even claimed to be able to distinguish between the clicks produced by α-

136 Lawson (1927). For *Nature* and its influence in the 1920s, see Werskey (1969); MacLeod (1969).
137 Lawson (1927), 178.
138 *ibid.*
139 *ibid.*
140 Holoubek (1927a, 1927b).
141 Ortner and Stetter (1927). Also see Stetter to Meitner, 29 September 1926, MTNR 5/16, LMP.

particles, protons and β- and γ-rays.[142] Stetter also continued to elaborate his mass-spectrograph to determine the masses of the disintegration particles from aluminium, carbon, boron and iron, the results again confirming those of Pettersson and Kirsch.[143]

While the Viennese threw themselves into a programme of technical development (a programme comprehensively reviewed by Stetter in a paper to the Deutsche Physikertag at Kissingen in September 1927[144]), the increasingly public character of the dispute encouraged researchers elsewhere to attempt to settle the controversy. Such attempts were to be crucially important, for they further expanded the field of those engaged in disintegration experiments, introduced new experimental methods and, at times, additional levels of complication and confusion. It is to the first of these attempts and its ramifications that I now turn.

3.7 An Intervention: Geiger, Bothe and Corpuscular Counting

Until 1927, the controversy had been played out exclusively between Cambridge and Vienna. The first intervention from outside those two centres came early in 1927, when Walther Bothe and his assistant H. Fränz of the Physikalische Technische Reichsanstalt bombarded several of the light elements with polonium α-particles in a deliberate effort to throw some fresh light on the four year old Cambridge-Vienna controversy. Bothe had become something of a specialist on the Geiger point counter, or 'Spitzenzähler.' In 1923-1924, for example, he had worked with Geiger in applying two such point counters to test the Bohr-Kramers-Slater theory.[145] When Geiger left Berlin to take up the chair of physics at Kiel in 1925, Bothe continued methodically to develop the Spitzenzähler, finding that its sensitivity could be improved by reversing the polarity of the point.[146] Using this property,

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142 Ortner and Stetter (1927). See also Pettersson and Kirsch (1927), 31-36; Przibram (1950), 30-31; Stuewer (1985), 279-280.
143 Stetter (1927a, 1927c).
144 Stetter (1927b).
145 Bothe and Geiger (1924, 1925a, 1925b); Stuewer (1975), 300; Trenn (1986), 122; Rheingans (1988), 51-53.
146 Bothe (1926); Trenn (1986), 122.

and taking their cue from the latest experiments in Vienna, Bothe and Fränz set out to investigate the artificial disintegration question, using a polonium source to minimise interference from β- and γ-rays. Their detection apparatus consisted of a Geiger point counter connected to an electrometer, whose deflections were (significantly) counted by two observers. The results, they declared summarily, supported the Cambridge workers: of the elements examined, only boron, nitrogen, magnesium and aluminium appeared to show evidence of disintegration.[147]

Pettersson did not take this well. Reasserting the Vienna workers' certainty that they had disintegrated carbon,[148] he criticised Bothe's use of the 'direct' method of observation. A similar arrangement had already been tried in Vienna "some years ago," but had been "abandoned owing to the many drawbacks it presented," such as its incapacity to register very short-range disintegration particles and its inability to distinguish between between impulses due to H- and β-particles. Pettersson noted that these limitations, coupled with Bothe and Fränz's use of a very weak polonium source (equivalent to 0.0025 mg Radium) and the distinct possibility of hydrogen contamination were, "no doubt, responsible for the negative results found with carbon as a bombarded substance."[149] These factors would also explain the Berliners' curious inability to detect disintegration from fluorine, sodium, silicon and other elements which had been found to be disintegrable both in Cambridge and in Vienna. The Viennese defences stood firm. So did the impasse.[150]

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[147] Bothe and Fränz (1927a, 1927b).
[148] Pettersson (1927a, 1927b).
[149] Pettersson (1928b), 5, 6. See also Pettersson and Kirsch (1926a), 86; Kreidl (1927).
[150] Compare Biagioli (1990), esp. 195 ff.

4. The Whole-Number Rule Refuted: Aston's Second Mass-Spectrograph

If Bothe's intervention failed to shed light on the conflicting results obtained in Cambridge and Vienna, Cambridge hopes of a successful resolution of the dispute in their favour were bolstered in 1927 by the inauguration of Aston's second mass-spectrograph. The new machine had been five years in the making. During that time, mass-spectrographs had eventually been built elsewhere, first in Paris, where Joseph L. Costa, a researcher at Jean Perrin's Institute of Physical Chemistry, constructed a machine similar in principle and design to Aston's in 1925,[151] and secondly in Vienna where, as we have seen, Georg Stetter designed a similar instrument in an attempt to verify Pettersson's results on the disintegration of the light elements.

Aston's original mass-spectrograph, dismantled in March 1925, had had a resolving power sufficient to separate mass lines differing by about 1 in 130.[152] When, in the light of Rutherford's 1921 speculations on nuclear constitution, divergences from the whole number 'rule' had become the significant variables, construction of a second instrument was set in train with the aid of a "liberal grant" from the Department of Scientific and Industrial Research.[153] The new instrument had a resolving power five times that of the original machine, "far more than sufficient to separate the mass lines of the heaviest element known," and an accuracy of 1 in 100, 000 - "just sufficient to give rough first order values of the divergences from whole numbers."[154] The increased resolution was achieved by doubling the angles of electric and magnetic deflection, and by sharpening the lines

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151 Costa (1925a, 1925b); Aston (1927a), 487, 509. Using high vacuum technology, a series of accumulators and Schumann plates for the photographs, Costa claimed an accuracy for his instrument of 1 in 3,000. Despite a successful first investigation into the isotopes of lithium, however, Costa's interest in the mass-spectrograph did not last long. Having spent the academic year 1925-6 as a National Research Council Fellow at Princeton with K.T. Compton, he was forced on financial grounds to take a job in industry, retiring permanently from the field of mass-spectrography. See Costa to Aston, 17 December 1926, FWAP; L.B. Loeb to E.E. Hall, 11 December 1926, Box 19, RTBP.

152 Aston explained that the application of the method of accelerated anode rays "led to an unexpected lengthening of the useful life of the original apparatus so that it was considered best to hold up construction of the new one in order that the final design might have the advantage of all accumulated experience" (Aston (1927a), 487). See also Rutherford to Hevesy, 1 June 1926, RP; G.P. Baxter to Aston, 5 October 1927, FWAP.

153 Aston (1927a), 487.

154 Aston (1927a), 488; G.P. Thomson (1946), 291; Hevesy (1948), 642.

through the use of finer slits placed further apart. Like its predecessor, the second mass-spectrograph was largely hand-built by Aston himself, though the large new magnet and some of the metal parts of the instrument were made by Pye and Co. of Cambridge. The increased sensitivity of the new instrument owed much to improvements in the vacuum system, and in order to take full advantage of the dispersion now available, Aston made further modifications to the camera arrangement, enabling it to yield ever-sharper photographs.[155]

After some teething troubles, the new machine entered service in the summer of 1926.[156] Over the next twelve months, Aston systematically re-analysed 18 different elements from hydrogen to mercury, the results flowing as copiously as they had from the first device.[157] As far as choice of a standard in terms of which to express his measurements was concerned, Aston continued to use the conventional O=16. Though he was acutely aware of the possibility raised by Blackett's work that oxygen might in fact be a mixed element, he noted that "[t]he absence of a very small percentage of an isotope is difficult to prove, and in oxygen particularly so, for the neighbouring units 14, 15, 17, 18 are always liable to be present," and concluded that "the evidence on the whole so far is in favour of oxygen being simple," justifying his adherence to O=16.[158] For the moment, this simple act of faith went unchallenged: the mass-spectrograph could be continue to be treated as a reliable indicator of the isotopic constitution of matter.[159]

The detailed results of Aston's analyses made space for an entirely new category of evidence relating to nuclear constitution: the *packing fraction*. Assuming the nucleus to consist of protons and electrons, it had been clear from the data yielded by the first mass-

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155 Aston (1927a), 493-494. The technical details of the second mass-spectrograph are also dealt with in Aston (1933a), 72-82. The ongoing improvements in vacuum technology in the 1920s are considered in some detail by Ditchburn (1977). See also Kaye (1927).
156 Rutherford to Hevesy, 1 June 1926, RP.
157 Aston (1927a). See also Aston (1925d, 1926b).
158 Aston (1927a), 500.
159 Thus the problem posed by the conflict of evidence from the mass-spectrograph and the cloud chamber was managed in practice by transforming it into a problem of 'technical adequacy,' without affecting the capacity of either device to yield further evidence. See Woolgar (1988), 30-37, esp. 34. For a cogent discussion of the way in which cultures are able to sustain apparently contradictory beliefs and practices, see Veyne (1988).

spectrograph that the additive law of mass failed, presumably because "inside the nucleus, the protons and electrons are packed so closely together that their electromagnetic fields interfere and a certain fraction of the combined mass is destroyed."[160] The mass destroyed "corresponds to energy released," and "the greater this is the more tightly are the component charges bound together and the more stable is the nucleus formed." It was for this reason that "measurements of this loss of mass are of such fundamental importance, for by them we may learn something of the actual structure of the nucleus."[161] Hence the new category. Defined as "the mean gain or loss of mass per proton when the nuclear packing is changed from that of oxygen to that of the atom in question," and calculated operationally by dividing an atom's divergence from the whole number rule by its mass number, the packing fraction promised to offer "entirely new information on the nucleus, for it is a measure of the forces binding [the nuclear] protons and electrons together."[162]

Using the results of his new analyses, Aston plotted the packing fractions of the various atoms against their mass numbers (fig. 3.6). The curve obtained showed "a fundamental class difference between elements of odd and elements of even atomic number," the odd elements being apparently much more unstable than the even, implying that "the nuclei of light atoms have a loose, and therefore heavy, external structure of lightly bound protons or neutrons common to them but not possessed by the much more stable and tightly bound atoms of helium, carbon and oxygen." This result, of course, conveniently supported the views of Rutherford and Chadwick against those of Pettersson and Kirsch, and meshed extraordinarily well with Rutherford's latest version of the satellite atomic model of the atom, in which the nucleus consisted of "an inner part of uniform, tightly bound "crystalline" structure, outside which is a looser system of neutrons, protons and electrons which is more complex the heavier the element."[163]

Between March and August of 1927, perhaps encouraged by Aston's latest findings,

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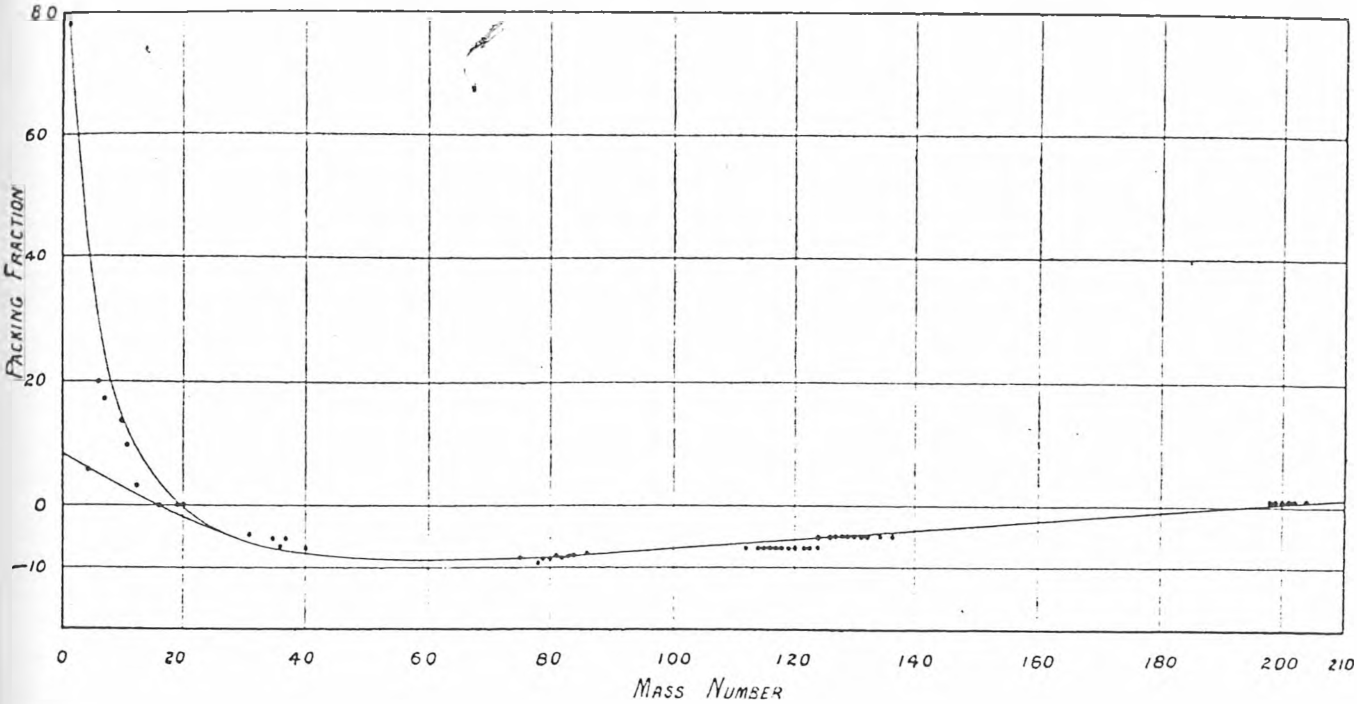
160 Aston (1927a), 501; Siegel (1978).
161 Aston (1927a), 501.
162 *ibid.*, 510.
163 *ibid.*, 513. For Rutherford's atomic model at this time, see Rutherford (1927b, 1927d); Stuewer (1986a), 341-349.



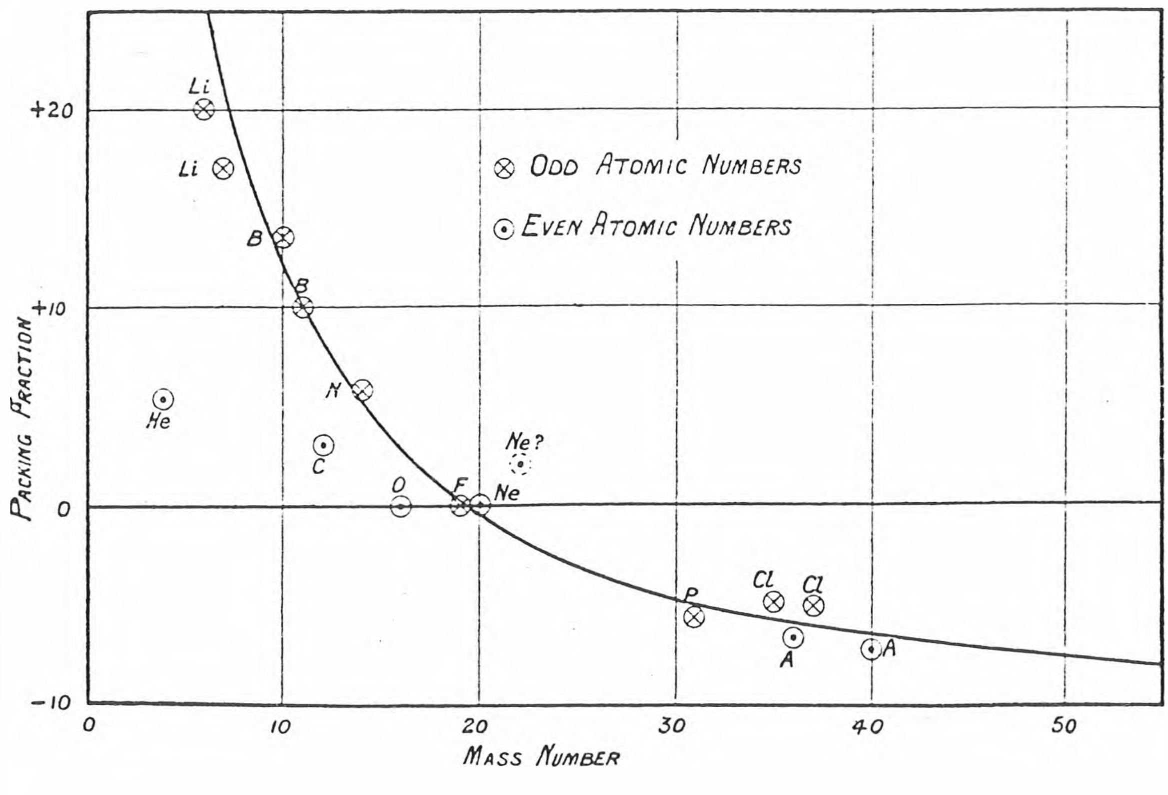


Fig. 3.6 Packing fraction curves, plotting packing fraction against mass number. The lower plot illustrates the packing fractions of the light elements on a larger scale.

Source: Aston (1927a), 511.

Rutherford had been working hard to add a quantitative dimension to his satellite model. Basing his approach on some recent calculations by Peter Debye and his assistant W. Hardmeier,[164] Rutherford derived an expression for the energy of emission E of an α-particle which had started life as a neutral satellite in a quantized nuclear orbit:

$$E = A\sqrt{Z/Z_0} + B(Z_0/Z)^2n^4[1-(Z_0/Z)^2bn^2]$$

where Z is the atomic number of a particular element, Z_0 the atomic number of a reference element (which Rutherford chose to be polonium), n an integer representing the quantized orbit of the neutral α-particle in the nucleus, and A, B and b constants.[165] Comparing the results of the new theory with established experimental data by a trial-and-error evaluation of the constants A, B, b and the quantum numbers n, Rutherford found "a very fair agreement between theory and experiment." There were exceptions, however, leading Rutherford to propose a fundamental re-examination of the ranges and velocities of the α-particles emitted by the radioactive elements so as to enable him to undertake a more comprehensive test of his new model.[166]

The significance attributed by Rutherford to his new conceptual model was reflected in the laboratory's programme of work. For the new academic year, Rutherford drew up his customary 'list of projected researches' for allocation to the new crop of students. Heavily featured was a programme of measurements on the ranges of the α-particles emitted by a variety of radioactive substances. More notably, there was a distinct shift of emphasis away from the scintillation technique and towards the development of other methods. As Rutherford put it, it was "important to develop new methods of measuring the velocity of emission of the α-particles with the greatest possible precision [for] if the relative energies

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164 Debye and Hardmeier (1926); Hardmeier (1926, 1927); Rutherford (1927b, 1927d); Stuewer (1986a), 342. Debye, who had been following Rutherford and Chadwick's publications, had written to Rutherford in 1926 to bring the new calculations to his attention: Debye to Rutherford, 6 February 1926, RP.

165 Rutherford (1927d)[CPR 3, 183-186].

166 *ibid.* [CPR 3, 190]. See also Rutherford to G.H. Briggs, 5 August 1927, MS Add. 8832/2, CUL: "Since you left I have evolved a theory of the origin of the α rays and the structure of the radioactive nucleus ... I have gone into the calculations and have fixed the quantum numbers of the orbits which give rise to the α particles. I came to the conclusion that the range of Th C (4.8 cm) was seriously in error, *for it made much the worst fit in my theory ...*" (my emphasis).

of the main groups of α-particles could be determined with certainty to 1/500 or still better to 1/1000, it would then be possible to test the theory in detail."[167] In keeping with the demand for new techniques, a number of the projected experiments required the use of the cloud chamber, whose potential had been shown by Blackett's work and whose photographic evidence was increasingly preferable to the corrigible records from the scintillation experiments.[168] Graduates Norman Feather and R.R. Nimmo, for example, were given the task of undertaking a cloud chamber study of the long-range α-particles from Radium C and Thorium C, involving the construction and operation of a new apparatus capable of yielding two hundred photographs per day.[169] So important were the cloud chamber experiments considered to be, in fact, that Chadwick even commissioned a special cloud chamber from the Cambridge Scientific Instrument Company to facilitate this work (fig. 3.7).[170]

Success followed success. Rutherford's quantitative model suggested a new approach to the γ-ray problem which lay at the heart of the controversy between Lise Meitner and Charles Ellis. "Since ... we have postulated a number of neutral satellites which circulate in quantum orbits round the nucleus," mused Rutherford, "it is of interest to consider whether the γ-rays have their origin in transitions of the satellite from one quantum level to another."[171] Supposing that "as a result of the violent disturbance which follows the emission of an α or β particle from the nuclear structure, one of the satellites becomes unstable, but insead of being ejected from the nucleus drops from one quantum level to another, radiating during the process the difference in the energies of the satellite in

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[167] Rutherford (1927d)[CPR 3, 190].
[168] "Projected Researches 1927," PA 361, RP. In these notes, Rutherford articulated an implicit theory of the evidential capacities of the cloud chamber technique in terms of *what would count as* satisfactory evidence. One of the proposed researches, for example, was to obtain all the velocities of a set of α-particles "in one exposure"; another was to "determ[ine] relative ranges of ThC and C' (4.8 & 8.6) by active wire (weak) in centre of Wilson chamber. 20 good expansions with 50 on each should suffice for a good measurement. Preferably stereoscopic [ ] for measurement." See also Rutherford to G.H. Briggs, 5 August 1927, MS Add. 8832/3, CUL.
[169] Feather, "Reminiscences of the Cavendish Laboratory, 1926-1937," unpublished typescipt, FEAT 45/7, NFP; Nimmo, "Work carried on at the Cavendish Laboratory, Cambridge," File ii/35, 1851 Exhibition Archives, ICL; Feather and Nimmo (1929).
[170] Nimmo, "Work carried on at the Cavendish Laboratory, Cambridge"; Cambridge Scientific Instrument Company Serial Book, 15 September 1927, WML; Barron (1952), 12.
[171] Rutherford (1927d)[CPR 3, 198].

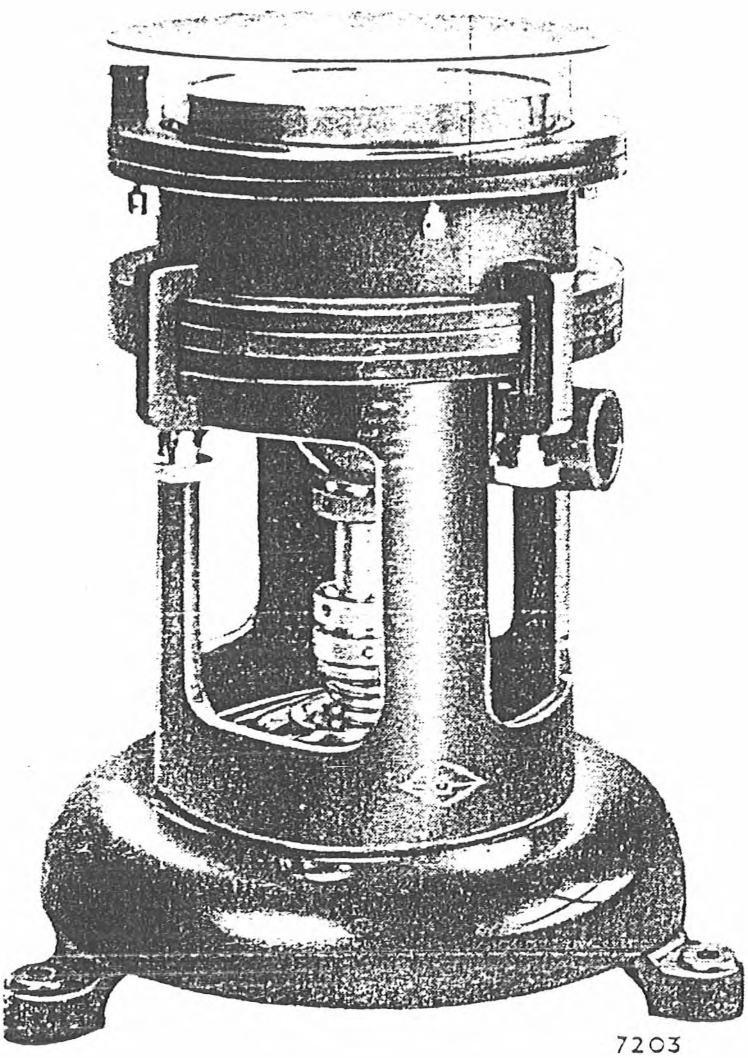

Fig. 3.7 16.6 cm diameter cloud chamber commissioned from the Cambridge Scientific Instrument Company by Chadwick, 1927. It was made "much more rigid than the older patterns," fitting and grinding of the piston being "of a very precise nature." This chamber allowed precision adjustment of the rate of expansion and expansion ratio.

Source: Barron (1952), 12.

equilibrium in these two quantum states," Rutherford again found it possible "to find a scheme of transitions that fits in many cases reasonably well, and in some cases surprisingly well, with existing γ-ray data."[172]

This was all very encouraging. With the more powerful mass-spectrograph, increasing numbers of cloud chambers and the development of a quantitative model of nuclear structure suggesting new lines of work and new levels of analysis, the Cavendish Laboratory was once again beginning to reassert itself in both the experimental and conceptual fields of nuclear research. Notwithstanding the seemingly interminable Vienna and Berlin contoversies, only one cloud now loomed large on the horizon.

5. "Modernists with a Vengeance": Wave Mechanics, Radioactivity and the Autonomy of Experiment

Rutherford's mathematical, quantum-based satellite model of the nucleus was given its first public airing at a physics conference in Como, a small town in northern Italy, in September 1927.[173] It was quite a show of Cavendish strength, for at the same meeting, Aston presented the most recent results from the mass-spectrograph in the hope that they would "form useful data for the theorist in the attack now imminent, on that least understood and most interesting problem of modern physics, the electromagnetic structure of atomic nuclei."[174] The conference had been organised by the local Fascisti, who used the occasion of the centennial of Volta's death for an ostentatious display of scientific nationalism. It was ironic, then, that the Como conference set the seal on the international scientific *rapprochement*, for it was the first such post-war gathering attended by representatives from the countries of the former Central powers - among them Born, Franck, von Laue,

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[172] *ibid.* Rutherford here appropriated the work of W. Kuhn (1927a, 1927b), who had shown that γ-rays could not arise from the movements of nuclear electrons. See Stuewer (1986a), 347-348.
[173] Rutherford (1928c).
[174] Aston (1927a), 513; Aston (1928c).

Pauli and (critically) Planck.[175] The Germans' repudiation of their earlier strategy of isolationism was tempered by the attitude of the French, who, continuing to pursue a more isolationist course than the majority of the International Research Council, sent only a small contingent, including Brillouin, Cotton and Maurice de Broglie.[176] The remaining participants, representing eleven countries, included Bohr, Fermi, Zeeman, Lorentz, Debye of Zurich, Ehrenhaft and Smekal of Vienna, W.L. Bragg, Eddington, C.G. Darwin, A.H. Compton, Langmuir and Millikan, as well as Rutherford and Aston.[177]

Rutherford's new model excited some interest among the participants - Debye raised the question of the structure of the neutral satellites, for example, and Giovanni Gentile of the Scuola Normale in Pisa was stimulated to publish a detailed critque.[178] For many of the delegates, however, the meeting was significant for a different reason: it marked the coming of age of the 'new' quantum mechanics - Heisenberg's matrix mechanics and Schrödinger's wave mechanics. Indeed, Bohr's unveiling of the concept of 'complementarity' at the Como congress and the ensuing discussions of the interpretation of the new mechanics were, in a way, simply a dress rehearsal for the following month's Solvay Conference, to be held in Brussels and devoted to the theme of 'Electrons and Photons.'[179] In this sense, the Como meeting heralded the emergence of a small, cohesive community of theoretical physicists largely (but not exclusively) defined by the twin poles of Bohr's Institute for Theoretical Physics in Copenhagen and Max Born's Physical

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175 Cock (1983); Schroeder-Gudehus (1990), 914-915. Russo (1986) and Galdabini and Guilliani (1988) deal comprehensively with the politics of Italian science in the 1920s. For German attitudes towards the scientific *rapprochement*, see Crawford (1992), 68-78; Forman (1973); Kevles (1973); Paul (1972); Schroeder-Gudehus (1973, 1978). Cf. also Barger (1928); Marks (1976); Maier (1988); Wallace (1988); Crawford, Shinn and Sörlin (1993). On Planck's importance in this context, see Heilbron (1986), esp. 107.
176 Pestre (1984), 149-168, esp. 150-153; Bensaude-Vincent (1987), 96-107.
177 For a complete list of participants, see *Atti del Congresso Internazionale dei Fisici, 11-20 Settembre 1927* (Bologna: Zanichelli, 1928), x.
178 Gentile (1928); Stuewer (1986a), 349.
179 *Électrons et Photons. Rapports et Discussions du Cinquième Conseil de Physique tenu à Bruxelles du 24 au 29 octobre 1927* (Paris: Gauthier-Villars, 1928); Mehra (1975), 133-181. Mehra and Rechenberg (1982-1987) give a comprehensive account of the rise and development of the new mechanics. For the interpretative difficulties which culminated at Como, see Cassidy (1976); Beller (1983); Cassidy (1992), 226-266.

Institute at Göttingen and dedicated to the elucidation of the new mechanics[180] - "modernists with a vengeance," as Oliver Lodge aptly put it.[181]

From the point of view of a traditionalist like Rutherford, who believed that experiment must be the basis of proper physics and who, until now, had succesfully defended that empiricist philosophy, the resurgence of theoretical physics threatened both the disciplinary unity of physics and the ideology of experiment by shifting the intellectual balance of the discipline towards mathematical theory and its geographical centre of gravity towards the continent.[182] The Cambridge-Vienna controversy, doubtless discussed informally at Como, only served to increase the stakes. A speciality like radioactivity was in competition with all the other branches of physics, experimental and theoretical, for increasingly scarce resources. Fellowships, jobs and ultimately the future shape of physics would be affected by the way these resources were distributed.[183] And at Como, it seemed that the theoreticians were in the ascendant.

The conference took its toll: Rutherford, tired and overworked, contracted a stomach upset and a bad cold, and had to return to Cambridge without afterwards visiting Vienna as he had hoped. In an exchange of correspondence with Meyer, arrangements were therefore finalised for Chadwick's visit to Vienna in December.[184] But the difficulties with Vienna and Berlin were now compounded by the implicit challenge from the theoreticians. The establishment of Bohr's Institute for Theoretical Physics in Copenhagen in 1920 had given

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180 The Bohr Institute functioned as a convenient politically 'neutral' venue in the 1920s. This was especially useful to the Germans in view of their stance on international scientific relations. Cf. Sommerfeld to the Carlsberg Foundation, October 1919, BSC: "Just as in the past at the Radium Institute of Vienna, future researchers of all countries should meet one another in Copenhagen for special studies and to pursue common cultural ideals at the Bohr Institute for Atomic Physics." For further comments on Copenhagen and Göttingen, see Pauli to Bohr, 7 July 1922, BSC

181 Lodge to J.A. Hill, 5 March 1928, in Hill (comp.)(1932), 225. Lodge added that Rutherford "feels about it much the same as I do" (*ibid.*, 224).

182 Hoch (1983), 237.

183 See, for example, the International Education Board's map of European centres of physics in 1926, reproduced in Kohler (1991), 263. The importance of competition for fellowships cannot be overestimated. In their deliberations, fellowship-awarding bodies made precisely the distinction I have outlined between experimental and mathematical physics. See, for example, Trowbridge's Diary, 8 June 1926, Series 12, General Education Board Archives, and compare Trowbridge's "Log of Visit to Cambridge, England," 17 April 1926, f. 410, Box 29, International Education Board Archives; Assmus (1990), 40-42. In this connection Kohler (1991), 165 ff. emphasises the Copenhagen-Göttingen axis. Cf. also Schweber (1986), 55-75; Jonas (1989); Aaserud (1990), esp. 25-27; and, for some cautionary remarks, Morrell (1993).

184 Rutherford to Meyer, 17 September, 22 September 1927; Meyer to Rutherford, 22 September, 27 September 1927, RP.

mathematical-theoretical physics a physical place of its own, a place dedicated both to individual contemplation and to the development and application of the mathematical technologies of theoretical work through communal discussion.[185] With the physical place, which served as a focus and as a passage-point for most of the theoretical physicists of the 1920s, had developed a sense of collective (community) identity and a socially constituted image of the enterprise and new social place of theoretical physics.[186] Most of the Cambridge mathematical physicists had spent some time in Copenhagen with Rutherford's blessing - Fowler in 1925, L.H. Thomas in 1926, Dirac in 1926-27, Birtwhistle in 1927, Hartree and Mott in 1928 - enabling them to keep abreast of, and disseminate, the latest developments.[187] Indeed, George Birtwhistle was in Copenhagen when he signed the preface to *The New Quantum Mechanics*, the first comprehensive English text on the new developments, and one which quickly found a place as a supplement to Dirac and Hartree's impromptu Cambridge lectures on wave mechanics.[188]

It was the emergence of wave mechanics, a theory "more terrifying" than the old quantum theory which it replaced,[189] a theory with profound consequences for the practice of physics, which worried Rutherford more than anything else in 1927 and 1928. The times were seeing, he said, "not only a rapid advance in experimental knowledge and technique but great activity in theoretical physics."[190] And as far as the experimentalist was concerned, "[t]he advent of the new or wave mechanics, with special reference to atomic problems, which promises to give an entirely new orientation to our ideas of the relation between radiation and matter, has much increased the difficulty, for the scientific man has

185 Mott (1987), 75, notes that at the Bohr Institute, theoretical physics was "a social phenomenon," in contrast to Cambridge, where it was a solitary activity. For an important analysis of styles of work in theoretical physics, see Warwick (1989).
186 Coben (1971), esp. 449, 454-455; Robertson (1979); Aaserud (1990). On the relationships between the social and physical place of contemplation, cf. Shapin (1991).
187 On the importance of the Bohr Institute to Cambridge mathematical physics, see, *inter alia*, Mehra (1972); Mott (1972, 1984, 1986, 1987); Weiner (1974); McCrea (1985); Aaserud (1990). Rutherford's extensive correspondence with Bohr (RP and BSC) also testifies to the importance of the link. For a comprehensive list of the visitors to the Institute, see Roberston (1979), 156-159.
188 Birtwhistle (1928), preface dated 1 October 1927; Darwin (1931); McCrea (1985, 1987); Jeffreys (1987).
189 Rutherford (1928c), 624.
190 Rutherford (1927c), 658.

to learn a new mathematical alphabet and language to keep in touch with this remarkable development."[191] The increased specialisation demanded by the new mechanics, as well as subverting the image of science and undermining the experimental ideology, also raised difficulties for administration, teaching and the general management of the discipline. It was becoming "more and more difficult for the scientific man to keep in close touch with the advances in even a relatively small branch of his main subject, much less read more than a fraction of the papers that are published in an ever-increasing stream," often provoking the "hopeless feeling that it is impossible to keep abreast with the flood of new scientific results and ideas or to distinguish the wheat from the chaff."[192] Opening the new H.H. Wills Physical Laboratory at Bristol, where Lennard-Jones had recently been appointed Professor of Theoretical Physics, Rutherford also drew attention to the new division of scientific labour engendered by the new mechanics: "With the ever-growing complexity of experimentation and technique, it is rare in these days that a scientific man can claim to be proficient in both of these branches. There has thus arisen the need that these complementary divisions should be adequately represented in a Department of Physics."[193]

Rutherford had tried throughout the 1920s to create an official post in mathematical physics at the Cavendish. Soon after his arrival in 1919, he had asked for the establishment of a Professorship of Theoretical Physics, but lack of funds had prevented the creation of such a position.[194] In 1923, he had again tried to attract Bohr to Cambridge as a Royal Society Yarrow Research Professor, an offer which Bohr ultimately felt obliged to turn down in view of his Danish commitments and obligations.[195] Three years later, Rutherford

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191 *ibid.* See also Badash (1987a).
192 Rutherford (1927c), 658.
193 *ibid.*, 659. On the new laboratory and physics at Bristol, see Keith (1984).
194 Rutherford, "History and Needs of the Cavendish Laboratory," PA 362, RP; "Memoranda on the Needs of Individual Faculties, Societies etc. at Cambridge," Royal Commission on Oxford and Cambridge Universities, 1919-1922, CUL.
195 For the suggestion and negotiations to bring Bohr to England, see Jeans to Bohr 17 July 1923, BSC; Rutherford to Bohr, 19 July 1923, RP and BSC; Bohr to Jeans, 3 August 1923, BSC; Bohr to Rutherford, 3 August 1923, RP; Rutherford to Bohr, 14 August 1923, RP and BSC; Bohr to Jeans, 22 August 1923, BSC; Bohr to Rutherford, 22 August 1923, RP; Jeans to Bohr, 29 August 1923; Rutherford to Bohr, 30 August 1923, RP. For Bohr's eventual refusal, see Bohr to Jeans, 9 September 1923, BSC.

told the International Education Board's Augustus Trowbridge in private discussion that what he needed most at the Cavendish was "an assistant to whom questions for mathematical solution could be put - not a colleague, but a mathematical expert under orders."[196] In 1928, in the wake of the new developments in Europe, he tried again, telling University authorities that "with a colleague on the mathematical side" he would be "much happier about the future of Physics in this University," because "[t]he development of Physics in the last decade has tended to bring the theoretical and experimental worker in closer contact to their mutual advantage, and I feel it is of vital importance to the future of Physics in general in Cambridge that everything possible should be done to promote this close association on which future progress so much depends."[197]

By this time, Rutherford was speaking from embarrassing personal experience. Having recovered from the illness which had prevented him from visiting Meyer in Vienna, he had presented his new satellite model at a meeting in Cambridge in October 1927. One of the new research students gave a first-hand account of the astonishing scene which ensued:[198]

> Rutherford gave his last paper in the Phil. Mag. to the [Cavendish] Physical Society and everybody in the lab. asked him as many skeptical questions as they could think of. It was rather amusing to see him on the floor being asked all manner of questions rather than having him tearing someone else to pieces. ... When he got through J.J. [Thomson] got up and commented on the quantized orbits in a field of force varying as the inverse fifth power. [He] pointed out that, by the ordinary laws of mechanics, under no circumstances could a closed orbit be stable in such a field. Rutherford's mouth opened about six inches. Obviously he had never thought of that. J.J. stood and waited for an answer. Finally after the cheering stopped Rutherford said that one could do anything with an orbit on the quantum theory. J.J. said it was the worst thing he had ever heard done by the quantum theory, then he walked out. I think Rutherford came nearer to losing his nerve than he ever did before. The crowd fairly howled and had no sympathy for him at all.

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196 Trowbridge, "Log of Visit to Cambridge, England," 17 April 1926, f. 410, Box 29, International Education Board Archives.

197 Rutherford to the General Board of Faculties, University of Cambridge, 24 January 1928, UA Min. III.6, CUL.

198 D.C. Rose to Gray, 28 October 1927, JAGP. The paper referred to is presumably Rutherford (1927d). On Rutherford's "traumatic" relationship with mathematical theory, see Badash (1987a). Compare also Berkeley spectroscopist R.T. Birge's confession to Aston, 28 December 1936, FWAP; Aston to Birge, 9 December 1936, 15 February 1937, RTBP.

Early in 1928, Rutherford succeeded in persuading the University authorities that "[i]n order to maintain the efficiency of the teaching for the Part II students, the services of an additional teacher are required whose duty shall be to give a special course of lectures of a theoretical character dealing with certain aspects of modern theory which have become of much importance in recent years" - by which he meant, of course, wave mechanics.[199]

Rutherford's cultural politics with respect to the new developments in theory emphasise the perceived danger posed to the integrity and autonomy of Cavendish-style experimental physics by the rise of wave mechanics as a theoretical discipline. While the technologies of wave mechanics were ones which must be domesticated and made safe, they bore no immediate relation to the experimental work of the Cavendish. In Vienna, on the other hand, Pettersson and his colleagues seem to have been unaffected by and oblivious to the powerful new theoretical developments, but remained implacable in their opposition to the latest experimental results from Cambridge. Cavendish physics was now under attack on two fronts.

6. Making the Experimenter Count: The Production of Knowledge and the Integrity of the Experimental Setting

At Meyer's Institut für Radiumforschung, the most recent results from Cambridge attracted great interest. The new mass-spectrograph, in particular, came in for critical analysis as Pettersson, true to form, sought to display the interpretative flexibility of Aston's new data. With his usual resilience, Pettersson embarked upon another of his enterprising re-interpretations of Cavendish data. Cleverly acknowledging that "[t]he possibility of drawing conclusions regarding the disintegrability of different elements from determinations of their atomic mass is highly important and appears most promising,"

199 "Report of the Faculty Board for Physics and Chemistry on additional University demonstrators required in the Departments of Physics, Chemistry and Mineralogy," Minutes of the Special Board of Physics and Chemistry, UA Min. V. 85, 27 January 1928, CUL. See also Seward (1928).

Pettersson proceeded to turn Aston's data to his own advantage by referring all measurements to hydrogen rather than oxygen, and recalculating atomic stability in terms of a quantity he called the *total packing* $\Delta m = A.m_H - m_A$, where A is the mass number and m_A the exact mass of an atom, and m_H the mass of hydrogen according to Aston.[200] He then proceeded to calculate the mass-energy balance for various hypothetical disintegration processes, arriving at the result that, from an energetic point of view, the disintegration of oxygen and carbon - still the main bones of contention - was perfectly "compatible with the present results from Aston's mass determinations."[201] Aston's results, in other words, could cut both ways. Suitably interpreted, they could be used to support both the Cambridge and the Vienna positions in the controversy.

In the hope of effecting a long-overdue resolution to the dispute, Chadwick finally arrived in Vienna on Wednesday 7 December 1927. He began at the Institut für Radiumforschung by interviewing Meyer, Schweidler, Przibram, Smekal and members of Pettersson's group. Though with Pettersson he had "the greatest difficulty in avoiding irrelevant matters," Chadwick was at least able to speak to Meyer about arrangements for the purchase of the remainder of the radium which had been loaned to Rutherford in 1908. Little else was settled during this initial session, however, and it was not until Friday 9th December that Chadwick was able to see the Vienna experimenters at work. He suggested that Schmidt's experiments on aluminium be extended to carbon, since that element "[contained] all the discrepancies in one result."[202] Pettersson, however, would brook none of it, and insisted on testing Chadwick's "power of counting small range H particles" and on demonstrating experiments with the Shimizu apparatus.

Pettersson demonstrated an experiment illustrating the disintegrability of aluminium, a display which Chadwick found inconclusive and "unimportant relative to the question of the disintegration of carbon." More heated discussions followed. The visit was fraught, and ended up with "a fierce and very loud discussion." Chadwick told Rutherford that it

200 Pettersson (1928a), 2.
201 *ibid.*, 13. But see Stuewer (1985), 292, 307 n.227.
202 Chadwick to Rutherford, 9 December 1927, RP; Stuewer (1985), 284-285.

would "not improve our relations," for although "Stefan Meyer, Schweidler and all with no direct interest in the question are exceedingly pleasant and friendly ... the younger ones stand around stifflegged and with bristling hair." Moreover, he added, "if you have seen Stetter's article in the last Phys. Zeit. you will understand how patient I have been, for I have not mentioned to Pettersson that I have seen it."[203] Stetter's review article of recent work in Vienna had appeared in the November issue of the *Physikalische Zeitschrift.*[204] In it, he had offered a robust defence of the Vienna results and had also tried to discredit the results of Bothe and Fränz. In exasperation, Chadwick told Rutherford that "[i]f this happens again I shall have to put things to Pettersson in their true perspective."[205]

From his own experience of managing scintillation counting experiments in Cambridge, Chadwick had a shrewd idea of where he might look for the source of the discrepant counts. As far as he could see, he reported to Rutherford, "the only way in which we can hope to reach a definite conclusion is to repeat Schmidt's experiments with Aluminium or rather extend them to carbon, and *it is essential that I should prepare the experiment.*"[206] Chadwick's insistence that he should conduct the experiment amounted to the demand that he be allowed to judge the Vienna experimenters by the standards - and the counting protocols - customarily imposed at the Cavendish Laboratory. The only means by which he could achieve this was to take charge of the experiment himself, something which Pettersson had so far resisted.

The following day, Saturday, Chadwick had further discussions with Pettersson and his colleagues, and examined Pettersson's apparatus for comparing α-particle and H-particle scintillations, which he found "quite nice but nothing to do with our argument." In the evening, he and the Vienna workers "made a short run with the Schmidt apparatus" in a test of the disintegration of carbon by polonium α-particles. While Chadwick himself saw "no H's beyond the range of the scattered α's," the Viennese counters, "two girls, managed to

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203 Chadwick to Rutherford, 9 December 1927, RP.
204 Stetter (1927b); Stuewer (1985), 283.
205 Chadwick to Rutherford, 9 December 1927, RP.
206 *ibid.* (my emphasis); Stuewer (1985), 284-285.

find a few." "Their methods of counting," he told Rutherford with surprise, "are quite different from ours."[207] This gave him the clue - and the chance - he had been looking for.

To draw attention to the human agency involved in the production of a piece of knowledge is simultaneously to point to the artifactuality of that knowledge.[208] When the group reconvened on Monday 12 December, Chadwick assumed control of the experiments, apparently taking advantage of Pettersson's absence to do so.[209] Without further ado he "arranged that the girls should count *and that I should determine the order of the counts*."[210] Making "no change whatever in the apparatus," Chadwick "ran [the Viennese counters] up and down the scale like a cat on a piano - but no more drastically than I would in our own experiments if I suspected any bias." This was the crucial point. The result was "that there was no evidence of H particles. ... The results do not prove that there is nothing from carbon but I think they make it doubtful that there is much"[211] (Table 3.1).[212]

The following day, encouraged by his findings, Chadwick carried out a second set of trials, in which it was again the experimenters, not nature, who were the object of the investigation. He made a very unfavourable impression on the Viennese workers. During the counting trials, according to one participant, there was no conversation; "the only noise was the rattling of Chadwick's keys." Chadwick came across as "cold, unfriendly, and completely lacking in a sense of humor [sic]," very much the judge and jury.[213] Confirming the previous evening's results, Chadwick confronted Pettersson with his findings. Pettersson became "very angry indeed," as did Meyer when he was told the

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207 Chadwick to Rutherford, 12 December 1927, RP; Stuewer (1985), 286. Chadwick added that "[n]ot one of the men does any counting. It is all done by 3 young women. Pettersson says the men get too bored with routine work and finally cannot see anything, while women can go on for ever." See Chadwick (1969), 61-62; Stuewer (1985), 286-287.

208 Ophir and Shapin (1991), 4.

209 Chadwick to Rutherford, 12 December 1927, RP. Pettersson's family had come to Vienna, which meant that Chadwick only saw him for 5 minutes during the day. See Brown (forthcoming), Chapter 5. I am grateful to Andrew Brown for sending me a draft copy of this chapter.

210 Chadwick to Rutherford, 12 December 1927, RP, my emphasis.

211 *ibid.*

212 Stuewer (1985), 286-287.

213 Rona (1978), 20; Stuewer (1985), 285.

Carbon exposed to α's		Range of α 3.5 cm	Angle 140°
	Absorption	No. of scintillations in 20 secs	Average
Scattered α's and H's	1.2 mm	13.7.11	10.3
H's alone	8.3 mm	1.2.1.1.2.2	1.5
	14.7 mm	3.3.0.1.2	1.8
	36.3 mm	1.5.1.0	1.75
	76.6 mm	0.0.0	0
		α particles cut off	
		1.0.3.3.0.6.2.3	2.25

Table 3.1: Results of scintillation counting trials during Chadwick's visit to Vienna, 12/12/27

(Chadwick to Rutherford, 12 December 1927, RP) following morning.[214] This was hardly surprising. From the Vienna perspective, after all, Chadwick had taken advantage of Pettersson's absence to run a series of 'control' experiments which amounted to an imposition of the protocols employed in Cambridge. When he had visited the Cavendish in May, Pettersson had effectively been no more than a tourist. He had been shown round the Cavendish and had seen the apparatus and techniques in use there, but he had remained a disengaged witness, an observer of the scene rather than a participant in it. Playing on the openness of his hosts - they had nothing to hide, and had tried to hide nothing - and on his status as a distinguished and authoritative visitor, however, Chadwick had managed to move from the 'public' world of order, demonstration and certitude - the "front" - to the 'private' space of confusion, trial-and-error and tentativeness - the dark "backstage."[215] He had become part of the experiment rather than a witness to the others' performance. It was the dissymmetry of this situation, crucially, which gave him the advantage.[216] While the Viennese had managed to produce perfectly consistent results with their own protocols, Chadwick demonstrated that they could not do so with the protocols he now imposed upon them.

Why, then, did the Viennese credit Chadwick's demonstration with any evidential force? Why did they offer no further defence for their claims? Why, in other words, did they yield to Chadwick's re-definition of the appropriate protocols for scintillation counting, thereby destroying the credibility of their own position ? One possibility lies in the memory of R.W. Wood's discrediting of Blondlot's N-rays, an episode which would have been familiar to Meyer as a paradigmatic example of 'psychological bias' in radiation science, a warning of what could happen to the credulous observer. The scintillation counting experiments, indeed, bore many similarities to Blondlot's N-ray experiments. The darkened room, the flashes of light, the delicate nature of the observations and the

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214 Chadwick (1969), 62; Stuewer (1985), 288. Meyer had written to Rutherford the previous day on Radium Commission business, and had added that he hoped "the personal intercourse between [Chadwick] and the Pettersson-Kirsch group may contribute to the bridging over of the remaining discrepancies." See Meyer to Rutherford, 12 December 1927, MP.

215 Goffmann (1971)[1959]. For some brief but illuminating remarks on the connections between 'backstage,' 'frontstage,' privacy and the integrity of knowledge, see Ophir and Shapin (1991), 12.

216 Compare Nye (1980); Latour (1987), 74-79; Ashmore (1993).

specificity of the protocols all figured large in the N-ray case as in the scintillation experiments. Indeed, the physical conditions under which the experiments were carried out made them susceptible to exactly the kind of intervention which Wood - and now Chadwick - had been able to make.[217] Add to that the Vienna workers' perception of Chadwick as an authoritative figure, as Rutherford's representative, as arbitrator, as *judge*, and one can begin to understand Meyer's position.

There may, however, be a second, and more interesting reason for the Vienna group's capitulation to Chadwick, for it seems that Meyer himself was no stranger to the art of sensational 'exposé.' Just a few years earlier, in 1924, Meyer had been involved in discrediting the famous 15 year-old medium Rudi Schneider in circumstances similar to those in which Chadwick had 'exposed' Pettersson. Occultism was rife in Vienna in the 1920s, and Meyer and his colleague Karl Przibram, had been engaged to investigate the true extent of Schneider's powers "in a strictly scientific manner."[218] Meyer described the "investigation" to a Vienna newspaper:[219]

> We went to the *séance* to which we were invited without any prejudice whatsoever and with the intention of obtaining an objective picture of what was taking place. *We had the opportunity of making a number of suggestions regarding control measures, and from the acceptance or rejection of these by the medium and his protector we could already form an idea as to how things would happen.* ... Everyone who has witnessed the manifestations which happen at the Rudi Schneider sittings must have noticed *how very different all dimensions appear in a darkened room, and how one can deceive oneself with regard to height and size. Dimensions in a darkened room are deceptive and the perspective appears completely altered.*

[217] Nye (1980). Compare Bok (1982), 64-66. Irrespective of the details of the case, my point is that the N-ray affair, which had taken place over twenty years previously, would have been *remembered* as an exposé. For an interesting deconstruction of Wood's intervention in the N-ray experiments, see Ashmore (1993).
[218] *Neue Freie Presse*, Vienna, 15 February 1924, quoted in Price (ed.)(1933), 22, my emphases. On Schneider, see Gregory (1985). The Meyer-Przibram episode is described on pp. 55-62. Gregory (*ibid.*) also describes Hans Thirring's close involvement with psychical research: see Thirring (1925, 1927).
[219] Price (ed.)(1933), 22-23.

The control measures suggested by Meyer and Przibram were only partly "successful," in that Schneider was still able to reproduce most of his usual manifestations. A new strategy had to be devised. So, continued Meyer:[220]

> In order to convince others of the correctness of our observations [doubting Schneider's ability to produce genuine manifestations of telekinetic phenomena], we decided to produce ourselves the Rudi Schneider phenomena, and Prof. Karl Przibram agreed to play the part of the medium. Last Sunday we gave a *séance* at my home ... The experiment succeeded in a surprising manner. Prof. Karl Przibram was able to show the guests all the manifestations produced by Rudi Schneider. It only required a few preparations to enable him to give the entire Rudi Schneider programme with all its levitations and telekinetic phenomena. After our show in the darkened room we had a repetition thereof in full light, when, to the amusement of our guests, we gave a complete explanation of all these very simple happenings.

The darkened room with its altered perspective, the controls,[221] the demonstration of hidden human agency, even the *ex post facto* explanation in the manner of a Poirot novel, Meyer had seen it all before.[222] He had done it himself. Meyer knew exactly what it was to deceive - and to be deceived - in a darkened room. He knew the importance of effacing - or highlighting - human agency.

It was, as Chadwick later put it, an "extremely awkward" situation.[223] Faced with Chadwick's claims, Meyer not only acceded, but offered to make an immediate public retraction of the Vienna work, a suggestion which Chadwick (perhaps rather embarrassed himself by the turn which events had taken) rejected. Instead, in keeping with Rutherford's

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220 *ibid.*, 23-24.

221 Chadwick, Constable and Pollard (1931), 464, note that "the strain of counting scintillations is such that the observers must be carefully controlled ..."

222 For a full account of Meyer's involvement in the Schneider case, see Gregory (1985), esp. 56-62. Owen (1989) gives an interesting perspective on the characteristics of the 'darkened room' as a site for the production of knowledge.

223 Chadwick (1969), 62; Stuewer (1985), 288. As I noted above, not all such visits need be destructive, as Doel (1992) convincingly shows.

insistence on maintaining privacy, it was agreed that the subject be allowed to fade into obscurity, and that no more be said about the matter.[224] This was to be a crucial decision.

Now, it is important to note that Chadwick's adjudication related only to the results of the scintillation counting work in Vienna. This work constituted much of the proof for Pettersson and Kirsch's claims, to be sure; but, as we have seen, the Vienna group were only too aware of the shortcomings of the scintillation method and had been making strenuous efforts to develop other techniques to corroborate their results. That work would continue. Indeed, with the elimination of the now-discredited scintillation method from their armoury, more effort than ever would be channelled into the development of other methods, including cloud chambers and electrical counters, which would allow them to continue their work in artificial disintegration.[225]

It is also important to stress that whatever happened in Stefan Meyer's office in the Institut für Radiumforschung on Wednesday 14 December 1927, the dispute was still *seen to be unresolved* by researchers who were not themselves directly involved but who had an interest in the controversy.[226] In Berlin, Bothe and Fränz had already made an intervention in the controversy in an effort to illuminate some of the moot points. Unaware (as far as we know) of Chadwick's visit to Vienna and of its *dénouement*, they continued to work on the problem of artificial disintegration, presenting their work as an intercession between the opposed camps. They would be joined by others, as I shall show in the next two chapters.

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224 Chadwick, "Comments on Paper" [Chadwick's comments on a draft of Feather (1962)], FEAT 23/6, NFP. For discussion of the deployment of privacy as a rhetorical category, cf. Shapin (1984); Shapin and Schaffer (1985), 69-79; Morus (1992), esp. 23.

225 For his part, Chadwick seems to have promised to carry out experiments in Cambridge using some of the methods developed in Vienna, with which he was clearly impressed. See Pettersson to Rutherford, 30 January 1928, SMP.

226 See, for example, A.S. Russell (1931a), 318; Stuewer (1985), 289-294, 301 n.122. Also see Meyer to Meitner, 13 January 1928; Meitner to Meyer, 23 January 1928, MTNR 5/12, LMP.

7. Conclusion: Radioactivity's Dark Secrets

Having negotiated a diplomatic silence about his findings in Vienna, Chadwick returned to Cambridge and informed Rutherford of the situation.[227] Action was clearly required. Chadwick's negotiated silence had been as much for the sake of the Cavendish as for the sufferance of the Viennese. The Cavendish Laboratory had lost much of its accustomed prestige and authority during the course of the Vienna dispute which, despite Rutherford's frequent pleas for discretion, was being conducted in the open literature and at scientific meetings. And while Chadwick's visit to Vienna had removed much of the basis for Pettersson and Kirsch's claims, the negotiated silence meant that the Cambridge workers too were bounden to secrecy. How, then, could they reassert the legitimate claims of the Cavendish Laboratory in the field of artificial disintegration and radioactivity without appearing to denigrate the work of the Viennese, who would doubtless continue their work in the field, having invested so much time and effort in it? It was a dark irony.

After discussions, Rutherford, Chadwick and Ellis, the Cambridge troika, decided to mobilise one of they key social technologies of the scientific community.[228] Conferences and congresses had been a common feature of science, as of many other forms of culture, since the nineteenth century.[229] In the shrinking modern world of the twentieth, they had become constitutive of the organization of scientific life.[230] But in an increasingly disparate and diverse scientific culture they also provided convenient fora for concentrated discussion of topics of particular importance or which were causing particular problems. The Solvay Conferences, in particular, provided a model in which all aspects of a single subject or problem area could be discussed at length and in a relatively informal fashion.[231]

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227 Stuewer (1985), 289-290.

228 On the notion of 'social technologies' as strategies for making and validating knowledge claims and particular kinds of knowledge-making practices see Shapin (1984), esp. 484; Shapin and Schaffer (1985), 69-79, esp. 77-78.

229 For an illuminating commentary, see Baldwin (1907), and cf. Hobsbawm (1987), 142 ff.

230 Schröder (1966); Salomon (1971); Schroeder-Gudehus (1973, 1978, 1982, 1990).

231 Mehra (1975), xiii-xxxii, 1-11, gives some of the background to the invention of the Solvay Congresses as an international forum for scientific debate. Cf. Abir-Am (1993); Crawford, Shinn and Sörlin (1993). For political parallels, especially in the 1920s, see Fair (1980); Dockrill and Goold (1981).

The Cavendish triumvirate therefore decided that they would host a small, informal conference over the coming summer, at which a "full and free dicussion of the outstanding problems" in radioactivity research might take place. Mindful of the need for absolute discretion concerning the Vienna episode, the 'official' theme of the meeting would be "β- and γ-Ray Problems," though it was clearly understood that discussion of other matters would not be precluded. The reputation of the Cavendish Laboratory had suffered considerably as a result of the Vienna and Berlin controversies; the proposed conference would provide the opportunity to bring the radioactivity community into direct contact with Cambridge physics and physicists, allowing them to see for themselves the work being done in the Cavendish Laboratory and to meet its research community. A list of potential participants was carefully drawn up and the invitations issued towards the end of February 1928.[232] The conference was set for July.

On his return from Vienna, Chadwick also told Rutherford of his discussions with Meyer about the Vienna radium. Rutherford wrote immediately to Meyer. The loan had, he told Meyer, "rendered possible the long series of investigations in Radioactivity by myself and my students and has been an invaluable aid in my researches."[233] Rutherford had bought 20 milligrams of the material for £540 in 1921. He now proposed to buy the remainder outright for £3,000, or about half the price per milligram he had paid in 1921.[234] Meyer, still desperate for funds for his Institut and doubtless thoroughly embarrassed by the outcome of Chadwick's visit, was agreeable to these terms.[235] It now fell to Rutherford to raise the money for the purchase - an goal which he quickly achieved after some hard lobbying in University circles in Cambridge. Aside from the obvious advantages to the Cavendish Laboratory, Rutherford stressed to the University that he was under "a strong moral obligation" to buy the material of which he had had the use "free of charge for nearly twenty years," especially if the money so exchanged would "be available for scientific

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232 Gray to Chadwick, 8 March 1928, JAGP.
233 Rutherford to Meyer, 21 December 1927, RP.
234 *ibid.*
235 Rutherford to Meyer, 7 February 1928; Meyer to Rutherford, 13 February 1928 [mis-dated 13 November 1928 in Badash (ed.)(1974)]; Stuewer (1985), 284.

work in Vienna and particularly to help the Radium Institute which is in extreme financial difficulties."[236] Arrangements were made for the £3,000 to be paid in six yearly instalments, beginning at the end of March, 1928.

The purchase of the Vienna radium did much to restore and consolidate good relations between the two laboratories after a seemingly interminable controversy. Rutherford wrote encouragingly to Pettersson in the new year: "There are so few workers in this difficult subject that we must try and pull together and settle our differences as far as possible by private correspondence rather than by controversies in the scientific journals, which in my experience do nothing but cause irritation."[237] Chadwick, too, wrote to Meyer hoping that he might soon visit Vienna "under more auspicious circumstances."[238] The Viennese, for their part, were also happy to return to the accustomed cordial relationship with Cambridge. In the new year, Pettersson sent Rutherford a spinthariscope of his own design, for which Rutherford was duly appreciative.[239] A short time later, Chadwick received a copy of a revised article on carbon from Pettersson, who had made several changes to his original draft at Meyer's suggestion so as to foster "a lasting *entente cordiale* between Cambridge and Vienna."[240] With Rutherford's election to Honorary Membership of the Vienna Academy of Sciences in June 1928, relations between Cambridge and Vienna were, formally at least, restored.[241]

At the practical level, however, action was required regarding the scintillation technique itself. As I have suggested, the decision to maintain a strategic public silence about the outcome of Chadwick's visit to Vienna was not simply based on Rutherford's proprieties about the correct conduct of scientific dispute. Notwithstanding the fact that fundamental and apparently inherent difficulties had been brought to light by Chadwick's visit, it was very much in the interests of the Cambridge group *that the scintillation method itself should*

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236 Rutherford to the Secretary of the General Board of Studies, University of Cambridge, 11 January 1928, UA Min. VII.21, CUL.
237 Rutherford to Pettersson, 9 January 1928, SMP.
238 Chadwick to Meyer, 21 December 1927, SMP.
239 Rutherford to Pettersson, 9 January 1928, SMP.
240 Pettersson to Meyer, 30 January 1928, Pettersson to Chadwick, 14 January 1928 (draft), SMP; Stuewer (1985), 290.
241 Vienna Academy of Sciences to Rutherford, 4 June 1928; Rutherford to Meyer, 15 June 1928, RP.

not be seen to be discredited. A humble graduate student articulated precisely what was at stake: "The series of experiments by Rutherford, Chadwick, Geiger and Marsden, which laid the foundation of the nuclear theory of the atom, depended almost entirely on the scintillation method for the detection of α-particles. This clearly illustrates the importance of the method of scintillation counting."[242] To re-evaluate the scintillation technique, then, would be to re-evaluate the principal source of experimental evidence for the nuclear model of the atom and the basis for fifteen years' work. It was a daunting prospect.

Chadwick assigned the problem of investigating the processes involved in scintillation counting to a pair of able graduate students who might at least hope to wrest doctoral dissertations out of it.[243] Julius Chariton had followed the trail blazed a few years earlier by Kapitza, having come to Cambridge in 1926 from Joffé's laboratory in Leningrad.[244] He and Clement Lea were set the task of investigating the process and mechanism of scintillation counting in detail. To their surprise, they found that although "practically all the fundamental data on which the modern conception of atomic structure is based were obtained by this method, very little systematic work has been done concerning the method itself and its limitations."[245] They need not have been surprised. As we have seen, there had been an implicit trust in the capacity of the scintillation method to yield secure results under proper conditions. The problem arose, as the Vienna debacle showed all too clearly, in establishing what those proper conditions ought to be.

Chariton and Lea investigated the mechanical processes of the production of scintillations by an assiduous comparison of different types of screens. More significantly, they also investigated the relationship between observer and apparatus by comparing the reponses of experienced against those of inexperienced observers. Three experienced, or

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242 Chariton Ph.D. (1928), 3-4 (see Appendix 2). Chariton's is the most informative contemporary assessment of Cavendish perceptions of the viability of the scintillation technique early in 1928. Comments on the method are conspicuously absent from the writings of Rutherford and Chadwick (but see Sargent (1985)).

243 For Chadwick's suggestion of the problem, see Chariton and Lea (1929c), 352; Stuewer (1985), 291. It seems not unlikely that the problem was set before Chadwick's visit to Vienna, though the outcome of that visit doubtless added point to the exercise.

244 Allibone (1987a), 31.

245 Chariton and Lea (1929a), 304; Stuewer (1985), 291.

"trained" counters were asked to count flashes. Their results were then compared with those from twelve inexperienced observers. Unsurprisingly - this was what 'training' *meant*, after all - the trained observers were found to perform more consistently while the untrained men produced less regular results which differed not only from those of the trained counters, but also from each other.[246] This study was completed by an investigation of the connection between the psychological and physical condition of the observer and the number of particles counted. Finally, Chariton and Lea investigated the relationship between the optical system used and the visibility of the scintillations, including a determination of the velocity of the slowest α-particle capable of producing a visible scintillation. Using an arrangement similar to that employed in Rutherford's electron-capture experiments of 1923-1924, their results showed values for doubly- and singly-charged α-particles of $0.31V_0$ and $0.23V_0$ respectively - values which agreed closely with Rutherford's own findings.[247] Both Chariton and Lea gained doctorates as a result of their work, Chariton in June 1928 and Lea two years later.[248] By the time Chariton and Lea had completed their investigations, the scintillation technique was beginning to drop out of use, largely because a new opportunity had presented itself.

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246 Chariton and Lea (1929a), 316-317.

247 Chariton and Lea (1929c), 335-343.

248 See Appendix 2. Chariton returned to Russia in July 1928, calling at the Vienna Institute *en route*. Arranging the visit, Chadwick told Meyer that Chariton's work had been "on the counting of scintillations and I think you will be interested in what he has to say": see Chadwick to Meyer, 23 June 1928, SMP; Stuewer (1985), 290. Between 1928 and 1930 Lea undertook an additional investigation of the alpha particles giving rise to the branch product radium C.

CHAPTER FOUR

MAKING TECHNOLOGY COUNT

Radio Culture and the Experimental Physicist

1. Introduction

The new year of 1928 saw Cavendish physics in crisis. The disintegration experiments and the scintillation technique upon whose evidence they were based were under a cloud of doubt and suspicion. Giving a series of Saturday afternoon lectures at the Royal Institution in March, Rutherford deplored this problem of certitude. He would, he told his audience, try to state "what was, and what was not, certainly known." Some minor details aside, the general genesis of the radioactive elements was "fairly clear." The outstanding problem, "the solution of which must perhaps be left to another generation," however, was radioactive decay and its relationship to nuclear structure. The prognosis was not good. As matters stood, he concluded, "there was an immense mass of material represented merely by observations and still lacking interpretation."[1] Much of that data relied upon the scintillation method, whose trustworthiness had been challenged by Chadwick's findings in Vienna.

The apparent unreliability of the scintillation method for quantitative work anywhere outside Rutherford's research room was only one of a number of problems facing the Cavendish Laboratory at the beginning of 1928. The resurgence of theoretical physics on the continent and the perceived threat it posed to the ideology of experimental physics (Albert Einstein, a cultural icon of science even in the 1920s,[2] was widely quoted on the drift of science away from the laboratory and towards pure mathematics[3]), the continuing challenge from Vienna and the ongoing controversy between Charles Ellis and Lise Meitner on the

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1 Rutherford (1928b), 315, 423.
2 Friedmann and Donley (1985); Lafollette (1990).
3 See, for example, "Einstein's Latest," *Nation* **128** (1929), 179-180, and, in response, Brunauer (1929). Compare also Lindenfeld (1990).

nature and interpretation of the β-ray spectrum all threatened to displace radioactivity from the commanding heights it had occupied a few years earlier by showing it to be a science riven with dissent. Closer to home, financial retrenchment within Cambridge University and the loss of £850 per annum of D.S.I.R. grant in 1927 threatened the position of physics within the University and of Cavendish physics within the British university system.[4] On all fronts, it seemed, Cambridge experimental physics was under attack. This chapter begins to set out the Cambridge response to that attack.

The response to the 'crisis of certitude' involved the appropriation and redeployment of material and conceptual technologies from elsewhere. It took three forms, all of which established new lines of research at the Cavendish. First, as I described in the previous chapter, attempts had been made in Vienna and Berlin to shed light on the Cambridge-Vienna controversy by the use of electrical counting methods, methods similar to that used by Geiger and Rutherford at Manchester before the war (which had been rejected, ironically it now seemed, in favour of the scintillation technique). Such methods had generally been found wanting in quantitative work, however, for they were more often than not as capricious as the scintillation technique - hence the contestability of Bothe's interventions in the controversy.[5] The rapid development of robust commercial valve technologies in connection with the expanding and diversifying radio industry in the late 1920s provided new resources for the experimenter. Just as they were used in industry and the home to convert electrical signals into wireless sound, valves could be used in the laboratory to amplify the small ionisation currents produced by the passage of ions to produce clicks in loudspeakers, deflections of a suitable galvanometer (responses which could then be totalled to yield quantitative results), or even to operate mechanical counters requiring no direct human intervention. From 1928, as I shall demonstrate in this chapter, much of the Cavendish research effort was devoted to improving the performance of valve amplifier systems by the careful selection of components and by the systematic elimination of the human observer from the measurement process.

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4 Cambridge University, Minutes of the Financial Committee of the General Board, UA Min. VII.21, 14 March 1927, CUL .

5 Bothe and Fränz (1927a, 1927b, 1928a); Pettersson (1928a, 1928b).

If the technical effort directed towards the domestication of the valve amplifier for use in the Cavendish validated new kinds of practice and established a new way of doing experimental physics, it also created new conditions for the further elaboration of those practices. In particular, it occasioned significant changes in the kinds of radioactive materials which might now be deployed in the laboratory. Radium lost much of its earlier significance, for its characteristic properties demanded especially elaborate modifications to the valve method. Polonium, a radium decay product, became instead the crucial source material, at least in the disintegration experiments. At the same time, however, the increasingly apparent inability of relatively small radioactive sources to yield incontestable evidence prompted efforts to develop alternative sources of atomic projectiles for use in the disintegration experiments. The trustworthiness and credibility of radioactive facts were widely seen to reside in the quantity and quality of the radioactive substances at the experimenter's disposal, as well as in the perceived competence of the experimenter himself. From the Cambridge perspective, much of the force of the challenge from Vienna had stemmed precisely from the fact that the radioactive resources of the Vienna laboratory matched, indeed exceeded, those of the Cavendish. It was against the background of the deadlock reached in the Vienna controversy, then, that workers at the Cavendish Laboratory sought, at Rutherford's behest in 1927-8, to develop particle accelerators to produce high-energy projectiles specifically for use in the disintegration experiments.

While the main thrust of the Cavendish response to crisis was to throw itself into programmes of technical development, it is in the context of the crisis of certitude, too, that Cavendish experimentalists' appropriation of the theoretical work of George Gamow must, I think, be seen. Even in the face of Rutherford's profound personal dislike of abstruse mathematical physics, the work of the Russian theoretician offered a new and potentially fruitful picture of the interaction between α-particles and nuclei. Appearing as it did in the summer and autumn of 1928, Gamow's model of quantum tunnelling suggested a convenient - and timely - way out of the impasse in which experimental physicists had, at length, found themselves. It was appropriated, first by the Cambridge physicists, then, as it became more widely known, by workers elsewhere. This opportunistic appropriation of Gamow's work, as well as providing rich new material for the embattled experimentalists, brought about a new set of relationships between experimental and mathematical physicists. While I defer substantive discussion of these issues to the next chapter, suffice to note here that Gamow's work established a *lingua franca* between the experimental and theoretical communities which served as the basis for an extended dialogue on the structure of the nucleus in the early 1930s.

This, then, was the three-fold Cambridge response to the Vienna controversy: the establishment of two new programmes of technical development and the appropriation of conceptual resources offering a radically new interpretation of nuclear structure and, in turn, new possibilities for experimental work. The Cambridge response shaped, and was in turn shaped by ongoing developments elsewhere. Crucially for my analysis, between 1928 and 1930 several laboratories with no strong tradition of radioactivity research entered the field of artificial disintegration in reponse to the ongoing controversy between Cambridge and Vienna. They were able to do so in virtue of the new instruments, techniques and practices which I have suggested were becoming (or, rather, were *being made*) relevant to the debate. In that sense, technical change and disciplinary development were two sides of the same coin. By 1930, as I shall show, at least half a dozen laboratories were engaged in experiments intended both to elucidate the structure of the nucleus and, by so doing, to shed light on the Cambridge-Vienna controversy. At the same time, however, the strategic decision taken in Stefan Meyer's office in December 1927 to remain silent about the outcome of Chadwick's visit meant that for almost two years, no-one outside Cambridge and Vienna knew that the two key laboratories involved in nuclear research no longer regarded the scintillation technique as trustworthy. As I shall demonstrate in the following chapters, that decision was to be a consequential one.

2. Response to Crisis: The 1928 Cambridge Conference

2.1 Electrical Counting Methods: The Geiger-Müller Counter

In February 1928, Rutherford received a welcome letter from his old pupil Hans Geiger. Geiger had accepted the Professorship of Physics at Kiel in 1925 where, following up his work with Bothe in Berlin, he had continued to elaborate and develop the electrical (point) counter, partly through systematic experiments to determine its mode of action, and partly, it seems, by sheer trial and error.[6] Working with research students Walter Müller and Otto Klemperer, Geiger had eventually found conditions under which the counter acted proportionally. He was able to report cheering news to his mentor:[7]

> Working with Dr. Klemperer I [have] found conditions under which the electric counter registers α-particles without being affected by β- or γ-rays. At least, we can place 4 mg Ra (my standard) 1.5 cm in front of the opening of the counter without ... noticing any effect on the string electrometer. At the same time any α-rays entering the counter give large deflections. This seems to work even with large openings (5 mm diameter and more). Besides, the natural effect is exceedingly small and of the order of 3 to 4 per <u>hour</u>. I have some hope that the counter will finally prove useful in experiments on artificial disintegration, but I am not quite sure yet.

Geiger's news opened up the possibility of using electrical counters with radium sources - good news indeed for, as we have seen, the Cavendish lacked polonium sources of sufficient strength to use in disintegration experiments with electrical counters. In March, there was more cheering news: another paper by Bothe and Fränz in *Naturwissenschaften*, once more suggesting the need for some kind of arbitration between Cambridge and Vienna. The authors announced that, using a point counter, an electrometer and the retrograde method of detection, they had again tested boron, carbon, aluminium and iron for disintegration protons, but with no success. During the spring, Geiger visited Bothe and was shown some

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[6] Geiger to Rutherford, 7 April 1926, RP; Trenn (1986); Rheingans (1988); Swinne (1988).
[7] Geiger to Rutherford, 12 February 1928, RP, emphasis in original; Geiger and Klemperer (1928); Klemperer (1928); Trenn (1972a); Trenn (1986), 126-127.

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A second, more comprehensive, paper from Bothe and Fränz in the June number of the *Zeitschrift für Physik* made it abundantly clear that in Berlin, as in Kiel, a great deal of labour was being expended to *make* electronic counters reliable and consistent.[12] Although scattered

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[8] Geiger to Rutherford, 8 March 1928, RP.
[9] *ibid.* This double counter arrangement and the confidence it inspired also figured in a later series of researches by Bothe and Kolhörster on the atmospheric penetrating radiation (höhenstrahlen). See Bothe and Kolhörster (1928, 1929a, 1929b). I shall return to this subect in Chapter Five.
[10] Bothe and Fränz (1927, 1928a, 1928b).
[11] Pettersson (1928b), 8-11.
[12] Bothe and Fränz (1928b). For another example of the work which had to be done to characterise the response of electrical counters to various operating conditions, and hence find by trial-and-error the conditions under which such devices might operate reliably and consistently, see the interesting case of the Belgian nuns, Desmet and and van Haeperen (1928). On the preferential development of particular lines of technology

α-particles and characteristic X-rays also produced deflections of the electrometer, these could be eliminated by careful choice of the gas used in the counter and by the use of absorbing foils. Systematic investigations of their conditions of operation and constant modification to achieve the degree of consistency desired made Bothe confident of his electrical counters and of his results. And those results continued to support Cambridge against Vienna.[13]

In Geiger's Kiel laboratory, meanwhile, Müller continued to investigate the properties and mode of operation of the improved point counter. In the spring of 1928, by systematic variation of the operating conditions, he found that when the point was positively charged, it gave nearly ten times as many counts as it did when negatively charged, with a corresponding increase in spontaneous discharges. Intrigued, Müller tested the effect of a similar reversal of polarity on a tube with a co-axial wire. The co-axial tube proved to be even more sensitive to radioactive sources and external disturbances than the improved point counter. Intense development work followed, and in May 1928 Müller could report that he had "put the finishing touches on an electronic current measuring instrument that is 1500 times more sensitive than that previously available. Needless to say the possible uses of such a device are endless, so we want to work with it here in Kiel for a year or so before we disclose its existence and thereby give away our competitive edge to all the other research institutions."[14]

Under this self-imposed secrecy, Müller continued to refine the co-axial counter and to investigate the optimum conditions for its operation. He constructed ten similar devices, whose sensitivity to external radiation he tested by shielding them beneath lead and iron plates. On 7 July, Geiger and Müller finally gave an account of their new 'Elektronenzählrohr' before a meeting of the German Physical Society in Kiel. They also dispatched an account of their work to *Naturwissenschaften*, where it appeared on 3 August.[15] With the new counter in the public domain, they set forth for Cambridge.

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through the commitment of resources to them, cf. MacKenzie (1990), esp. 10-12, 212-213.

13 Using data obtained from Blackett's photographs, Bothe (1928c) also made calculations of the relation between the range of disintegration protons and that of the bombarding α-particles.

14 Müller to his parents, quoted in Trenn (1986), 133-134. See also Rheingans (1988), 39-45.

15 Geiger and Müller (1928a); Trenn (1986), 113, 134; Rheingans (1988), 43-44.

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15 Geiger and Müller (1928a); Trenn (1986), 113, 134; Rheingans (1988), 43-44.

2.2 The Cambridge Conference

On Monday 23 July 1928, Geiger, along with Europe's other leading workers in radioactivity, arrived in Cambridge. They had been invited by the Cambridge troika, Rutherford, Chadwick and Ellis, for a "full and free discussion of the outstanding problems" in radioactivity.[16] It was the first time since the war that the experimentalists had met together. It was also the first such conference to be held in the Cavendish Laboratory. That it was convened in the wake of Chadwick's visit to Vienna was, perhaps, no coincidence.

In the warmth of a Cambridge summer's evening, the participants assembled in the Cavendish Laboratory, tucked away through an unassuming archway off Free School Lane, to hear Rutherford's opening address. The gathering was a large one. A glance at the guest-list is most instructive.[17] Given the theme of the conference, all the key protagonists in 1920s β- and γ-ray research were there, of course, including Meitner, Kolhörster and Bothe (fig. 4.1) of Berlin, Maurice de Broglie, Jean Thibaud, M. Frilley and Dragoliob Yovanovitch of Paris, Jacobsen of Copenhagen, Kohlrausch of Graz and Smekal of Vienna. Rutherford had most recently met Maurice de Broglie (whom he knew from the earliest Solvay Congresses, at which de Broglie had been a scientific secretary)[18] and Adolf Smekal at the 1927 Como congress. Smekal (fig. 4.2), one of the circle of theoreticians around Hans Thirring in Vienna (where he had been Honorardozent in the Abteilung für Technische Physik since 1923) had also met Chadwick during the latter's visit to Vienna the previous December.[19] Both de Broglie and Smekal had made significant contributions to β-ray research in the 1920s. They were to be key participants at the conference - and afterwards.

Also among the invitees were several of the younger European researchers who had not worked specifically on β- and γ-rays, but who had worked more generally in radioactivity. They had been invited as part of the troika's hastily cobbled-together 'foreign policy.'

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[16] "Conference on β and γ ray Problems" [programme], Box 8, JAGP, also in MEITN 5/3, LMP.
[17] See the conference seating plan in Chadwick's notebook for the May Term, 1928, CHAD III/4, JCP. For further details, see also Lise Meitner's notebook, July 1928, MTNR 3/17, LMP.
[18] M. de Broglie to Rutherford, 23 December 1911, RP. On de Broglie's role in the Solvay congresses, see Mehra (1975), 11; Leprince-Ringuet (1960), 298; Lépine (1962).
[19] Smekal (1924a, 1924b, 1926a, 1926b). On Smekal, see Forman (1975).

Fig. 4.1 Walther Bothe at the Cambridge conference, July 1928.

Source: Snapshot taken by Wynn-Williams, Cavendish Laboratory.

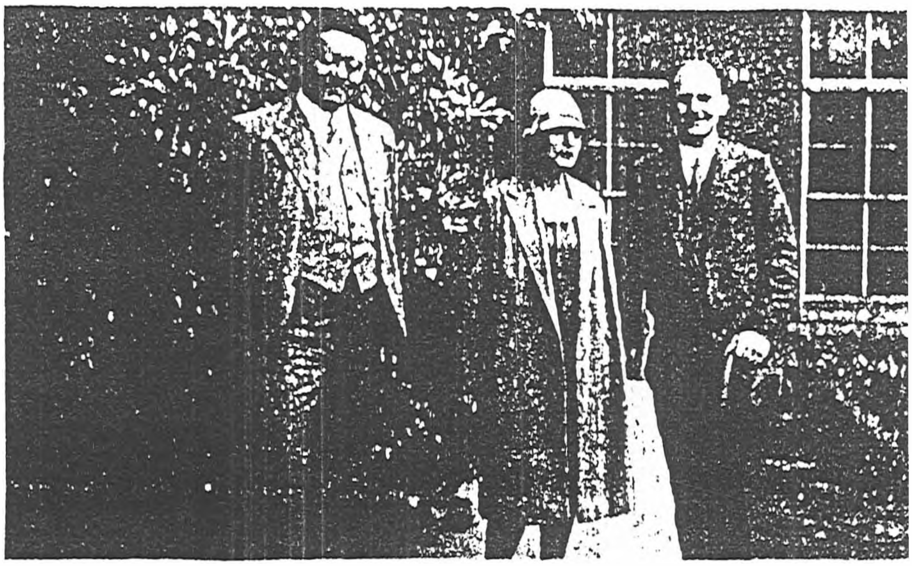

Fig. 4.2 Adolf Smekal [?], Lise Meitner and Hans Geiger at the Cambridge conference, July 1928.

Source: Snapshot taken by Wynn-Williams, Cavendish Laboratory.

Representing the Laboratoire Curie in Paris were the up-and-coming generation, Frédéric and Irène Joliot-Curie, while from Jean Perrin's neighbouring Institute for Physical Chemistry came Francis Perrin and Pierre Auger. From the Vienna Radium Institute, most pointedly, only one participant was invited: Ewald Schmidt, a 28 year-old Assistant at the Institute and one of Pettersson's circle. He had worked on neither β- nor γ-rays, but *did* have experience with electrical counting methods and in the use of polonium (it was Schmidt whose experiments on the disintegration of aluminium had impressed Chadwick's during his visit to Vienna the previous December).[20] Clearly, β- and γ-rays were only one item on the week's agenda.

Among the remaining participants were several of Rutherford's former students and colleagues who had helped lay the early foundations of β- and γ-ray research at Manchester. Among these were J.A. Gray, now at Queen's University, Kingston, Ontario,[21] J.M. Nuttall, still at Manchester, H.R. Robinson of Cardiff University,[22] and Andrade of U.C.L.[23] From the United States came Leon Francis Curtiss. He had originally undertaken graduate work with F.K. Richtmeyer at Cornell, where he obtained his Ph.D. in 1922. Winning a National Research Council Fellowship, he had studied at Cambridge for the next two years, where his research included a study of the natural β-ray spectrum of RaD and the development of an electromagnet for use with a β-ray spectrograph.[24] In 1926 he became a Senior Physicist at the National Bureau of Standards in Washington D.C., where he continued to work on radioactivity, including much work on the development of electric particle counters.[25]

A select few of the younger members of the British physics establishment were also asked to attend, including X-ray crystallography specialist W.L. Bragg, Rutherford's successor at

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20 Schmidt (1925, 1927, 1929); Rona and Schmidt (1927); Chadwick to Meyer, 23 June 1928, SMP. Chadwick explained that he would have "liked to invite also Dr. Stetter but our funds were not sufficient." It is significant that Stetter's most recent work had also been on the development of electronic counting methods.

21 Lewis (1967).

22 Andrade (1957).

23 Cottrell (1972).

24 Curtiss (1926a, 1926b).

25 Curtiss (1928a, 1928b).

Manchester, and G.P. Thomson of Aberdeen. C.G. Darwin of Edinburgh, another colleague of Rutherford from the pre-war Manchester days, added mathematical weight to the British contingent. An invitation was also extended to George McKerrow. An exact contemporary of Chadwick, McKerrow had graduated from Gonville and Caius College, Cambridge, in 1913. During the war he had spent some time at Farnborough, where he had come to know Aston, G.P. Thomson and the Chudleigh Mess crowd. In 1923, he joined the Research and Education Department of the Metropolitan-Vickers Electrical Company as Scientific Liaison Officer to the Director, A.P.M. Fleming, in which capacity he acted as a link between Fleming, the Metropolitan-Vickers Research Department and the universities (it was through McKerrow and John Cockcroft, for example, that much of the contact between M-V and the Cavendish Laboratory was mediated).[26]

Both in the circumstances which gave rise to it and in its organisation, the Cambridge meeting was an unusual one. It was an opportunity for discussion and reflection, a chance to take stock. Each speaker was asked to indicate the general scope of their paper at its outset, and to single out "three or four problems of interest." A short round of questions would follow immediately "to ensure that everyone appreciated the trend of the discussion." The speaker would then treat each of the highlighted problems in turn, pausing after each for further discussion. This organisation of business was specifically designed to be "more flexible and promote better discussions" than the reading of a formal paper lasting an hour followed by a protracted discussion. The troika's deliberate policy to invite only the younger workers in radioactivity so as to promote informality and to keep the numbers manageable also helped to ensure that problems were given a full airing and that all points of view were represented in the discussions.[27]

Those discussions ranged widely. While the nominal theme of the conference was "β- and γ-Rays," there was plenty of scope for the airing of more general issues in radioactivity.

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26 Niblett (1980), 85-153; McKerrow-Cockcroft correspondence, CKFT 20/59, JDCP; Bohr to W.L. Bragg, 17 January 1924, Reel E4/9, BSC. On McKerrow, see Allibone (1984a, 1987c). Surprisingly, McKerrow is not mentioned in Hartcup and Allibone (1984).
27 "Conference on β and γ Ray Problems" [programme], Box 8, JAGP. For some illuminating remarks on the participation of graduate students in the discussions, see Pollard (1969), 160.

Smekal, for example, scheduled to give the closing paper on "Disintegration Theories," was invited, "should it prove desirable," to discuss "α-particle disintegration … in a general way."[28] For Chadwick, at least, this and the Cambridge-Vienna controversy clearly constituted part of the week's hidden agenda. The details of the disintegration experiments were certainly discussed *sotto voce* among the interested parties, for Schmidt reported back to Meyer in a comprehensive letter that "[e]verything which arose in private discussion in connection with atomic disintegration has been for me most remarkable."[29] While Schmidt's brief conversations with Rutherford had not progressed beyond pleasantries about the English summer weather, he was, he reported, able to discuss matters with Chadwick, now "significantly more accessible than in Vienna," in "a very pleasant and peaceful way" and at much greater length. Chadwick told Schmidt that his own recent experiments had shed no further light on matters, though a renewed attack would begin again in the autumn.

Schmidt discussed the Cambridge-Vienna controversy openly with several of the other participants, reporting to Meyer that "the other men here ask occasionally about the state of the controversy but are thoroughly neutral." He told Bothe how pleased the Viennese were about his attempts to settle the controversy and "repeatedly tried to indicate [our] desire for an unpolemical treatment of the differences." Bothe, for his part, seemed to have made a concession, having dropped his suggestion that the Vienna observers were counting β-particle scintillations, though he remained convinced of the correctness and finality of his own experiments, becoming "irritated by any doubt as to the definiteness of his own conclusions." Pettersson's scepticism about Bothe's counter was apparently shared by Lise Meitner, who seemed to favour the Viennese position, certainly insofar as the interpretation of Bothe's experiments was concerned. She agreed with Schmidt that Bothe's method of calibrating his counter was questionable, "and also explained that she had, with respect to [a] discussion of Bothe's talk in Berlin, made the objection that the Viennese would rightly

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28 "Conference on β and γ Ray Problems" [programme], Box 8, JAGP.

29 Schmidt to Meyer, 26 July 1928, SMP. I am grateful to Roger Stuewer for providing me with a copy of this letter, from which all the following quotations are taken. It is unclear whether scintillation counting or Chadwick's visit to Vienna were mentioned during these informal discussions.

consider his results with C[arbon] as proof of atomic disintegration."[30] Chadwick, on the other hand, seemed to be well satisfied with Bothe's results, though he told Schmidt privately that he shared neither Bothe's interpretation of his Berlin experiments nor his explanation of the differences between Cambridge and Vienna.

This debate about the reliability of Bothe's electrical counting methods and their relevance to the Cambridge-Vienna controversy brings us to the crux of the Cambridge conference, for electrical counting methods were the focus of much of the week's discussion. During the course of the week, Schmidt and the other participants learned of the existence of Geiger's new 'Elektronenzählrohr.' The news caused quite a stir, though further details of the device were hard to come by. As Schmidt told Meyer in exasperation: "Prof. Geiger, who is also here, is supposed to have built a new strongly proportional functioning meter, about which I could unfortunately find out nothing further. It seems that Prof. Geiger himself wants to investigate the questions with this instrument."[31] Geiger's preliminary description of the new counter was in press by this time, of course, but for the time being Geiger was keeping the particulars of the new device largely to himself.

As the week's proceedings came to a close, Schmidt summed up his impressions for Meyer. All in all, he reported, the Cambridge workers seemed "to be coming slowly to the conviction that a final decision can only be reached with a method of proof deemed by all sides to be without objection," though what that method might be remained unclear. From the Vienna perspective, Bothe's electrical counters seemed every bit as unreliable as the scintillation method. Matters seemed as confused as ever.

As Schmidt returned to Vienna and the other visitors returned to their own laboratories, Chadwick and Rutherford paused for reflection and consolidation before making preparations to admit a new group of research students for the autumn term. Rutherford thought the conference had definitely served "a valuable function."[32] Radioactivity researchers from laboratories in Europe and the United States had been brought together for a week, they had

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[30] On the important notion of calibration and its contestability, see Collins (1985), 104-106.
[31] Schmidt to Meyer, 26 July 1928, SMP.
[32] Rutherford to G.H. Briggs, 22 August 1928, MS Add. 8832/5, CUL.

talked, exchanged ideas and established personal contact. In the process, much of the lost prestige of the Cavendish Laboratory (particularly in the eyes of the younger European workers) had been recovered. More to the point, however, several of the participants had been sensitised to the ongoing controversy between Cambridge and Vienna. Both the dispute itself and Bothe's equivocal attempts at mediation had been discussed quite openly during the week, and the need for further clarification had been apparent to all - so apparent, in fact, that a number of the participants at the conference came away with a determination to join the disintegration work themselves in an attempt to arbitrate between the two sides and settle the controversy.[33] Crucially, Frédéric and Irène Joliot-Curie, Maurice de Broglie and Adolf Smekal left Cambridge with the firm conviction that it was in the field of artificial disintegration that the most exciting and innovative work in radioactivity remained to be done. As I shall show in the next chapter, their interventions would change the social and intellectual geography of the discipline fundamentally and irreversibly.

2.3 Seeing the Light: Artificial Disintegration in Vienna, 1928-1930

At the Institut für Radiumforschung, still reeling after Chadwick's visit,[34] Schmidt's return with news of the conference, of Geiger's new counter and of recent work in Cambridge precipitated a burst of work on electrical counting techniques. Building on earlier work done in the laboratory in an attempt to confirm the results of Pettersson and Kirsch,[35] Schmidt and Stetter, in a new collaboration, concentrated their efforts on the valve amplifier technique.[36] Using the apparatus in conjunction with an electrometer, they developed an instrument - the 'Röhrenelektrometer' - with a proportional response, hoping to be able to discriminate between α-rays, protons and other ionizing particles. If such a discrimination could be effected, the device would find immediate use in the disintegration experiments where it

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[33] See, for example, Meitner to Ramstedt, 30 October 1928, MTNR 5/15, LMP.
[34] Pettersson to Meyer, 3, 7, 14, 19, 30 January 1928, SMP.
[35] Ortner and Stetter (1927).
[36] Schmidt and Stetter (1929, 1930a, 1930b); Schmidt (1929).

might yet vindicate Pettersson and Kirsch.[37]

Pettersson, meanwhile, was suffering the repercussions of his brush with the Cavendish. In 1928 he applied for the professorship of physics at the University of Stockholm vacated by the death of Svante Arrhenius the previous autumn. Of the four "sakkunniga" (members of the appontment committee), Carl Benedicks and John Koch recommended Pettersson for the post, while Martin Knudsen and Manne Siegbahn both opted for another candidate, Erik Hulthén.[38] Siegbahn in particular was vociferous in his opposition to Pettersson, since the Vienna disintegration results conflicted not only with those of Rutherford and Chadwick but now also with those of Bothe and Fränz.[39] Hoping to undercut Siegbahn's opposition, Stefan Meyer solicited favourable testimonials on Pettersson's behalf from Marie Curie, Georg von Hevesy and Kasimir Fajans.[40] Representing Pettersson in Stockholm, Benedicks even called in an old debt (apparently without Pettersson's knowledge[41]) by soliciting a testimonial from Jean Perrin, the Parisian physical chemist, whom he had supported for the Nobel Prize in 1926.[42] Despite these efforts on Petterson's behalf, however, it was Hulthén who was ultimately appointed to the post. Dejected, Pettersson remained in Göteborg, where he devoted increasing amounts of his time and energy to oceanography.[43]

He maintained both his interest in radioactivity and artificial disintegration and his connections with the Vienna group, however. In 1929 he published an article entitled "H-Particles made Visible," in which he described a modification of the Shimizu apparatus developed in Vienna to render visible precisely those disintegration protons at issue in the controversy with Cambridge. He also described a version in which proton tracks could be "simulated" by paraffin, enabling a direct visual or photographic comparison to be made

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37 Schmidt and Stetter (1929).
38 "Sakkunnigeutlåtande Över de Sökande Till Den [Mediga] Professuren i Fysik vid Stockholms Högskola, 1928," Swedish National Archive, Ecklesiastikdepartementets konseljakt nr 33, 21 December 1928. I thank Thomas Kaiserfeld for supplying me with copies of these documents. See also Stuewer (1985), 291.
39 Pettersson to Meyer, 10 October, 28 October, 3 November and 18 November 1928, SMP.
40 Hevesy to Meyer, 3 November 1928; Curie to Meyer, 9 November 1928; Fajans to Meyer, 25 November 1928, SMP.
41 Pettersson to Meyer, 18 November 1928, SMP.
42 Perrin to Benedicks, 4 November 1928, Swedish National Archive, Ecklesiastikdepartementets konseljakt nr 33. On Perrin, see Nye (1972); Pestre (1984); Crawford, Heilbron and Ullrich (1987), 102-105.
43 Deacon (1966), 407-412; Stuewer (1985), 291.

between 'genuine' H-particles and any putative disintegration products. Much of the controversy with Cambridge had centred on the issue of the proper and unambiguous identification of protons in the presence of other kinds of particles. Pettersson now offered, as a parting shot, a direct method of calibration by which genuine protons could be distinguished from ersatz.[44]

Pettersson's colleagues at the Institut für Radiumforschung, too, continued their work in artificial disintegration. In mid-1930, for example, they averred that many of their results had "been confirmed by other workers, whereas, with regard to other results, discrepancies still exist."[45] The Vienna workers, aided and abetted by Pettersson, applied to the Rockefeller Foundation for a sum of $5,000 to enable them to continue their investigations through the development of "two new and very promising departures" - the valve electrometer and a spectroscopic technique which they hoped would be "able to do for artificial disintegration what Ramsay's work on the spectra of helium derived from a radium solution did for the science of radioactivity."[46]

While the Viennese researchers sought funds to continue their work on artificial disintegration, Cambridge's Cavendish, too, was beginning an ambitious new programme of technical development. Like Schmidt and Stetter's work on the valve electrometer, that programme had its origins in the July conference.

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44 Pettersson (1929b), 131.

45 Illustrating clearly that they continued to defend their position, even after Pettersson's departure. See [Pettersson?] to Rockefeller Foundation, 9 July 1930, SMP.

46 [Pettersson?] to Rockefeller Foundation, 9 July 1930; L.W. Jones to Pettersson, 28 November 1930, SMP.

3. Electrical Culture at the Cavendish Laboratory: A Portrait of the Physicist as a Young Ham

Aside from the personal contacts established with the younger workers on the continent, researchers at the Cavendish Laboratory reaped two immediate material benefits from the July conference.[47] Chadwick had discussed the laboratory's polonium famine with Lise Meitner. Returning to Berlin, Meitner forwarded a small amount - 1.96 milligrams - of the increasingly precious material, with detailed instructions as to its experimental deployment and manipulation.[48] Chadwick confessed that he felt "most rapacious" in taking the polonium, but quickly put it to work in the Cavendish.[49] A week later, there was more good fortune when Geiger dispatched one of his new 'Elektronenzählrohr' tubes to Cambridge. The tube, number 22, was "rather a small one" though there should not be "the slightest difficulty to get it going."[50] He offered to supply more tubes as required. The radioactivists had rallied round to assist the Cavendish in its hour of need. Their supplements to the material resources of the laboratory were put to good use.

Chadwick set a graduate student the task of repeating Bothe's work in an attempt to shed light on the electrical method. Hugh C. Webster was one of the new arrivals in October 1928, one of four 1851 Exhibitioners to be admitted that year. Born in Tasmania in 1905, he had studied at the Universities of Tasmania and Melbourne, where his 1928 M.Sc. thesis on "Energy Levels in Atoms" had been supervised by T.H. Laby. Appraised for his "experience, remarkable power of understanding and [ability to expound] a difficult branch of theoretical physics," Webster had been Laby's first choice (of three candidates nominated by Melbourne) for the 1851 Exhibition Scholarship.[51] As was customary, Webster spent his first few weeks in the 'Nursery,' where the induction course included the calibration of a γ-ray electroscope, qualitative experiments on the heat conductivity of gases at low pressures

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[47] But cf. Chadwick (1969), 63.
[48] Meitner to Chadwick, 4 August 1928; Chadwick to Meitner, 13 August 1928; Meitner to Chadwick, 17 October 1928; Chadwick to Meitner 22 October 1928, MEITN 5/3, LMP.
[49] Chadwick to Meitner, 13 August 1928, MEITN 5/3, LMP.
[50] Geiger to Rutherford, 17 August 1928, RP; Trenn (1986), 134.
[51] H.C. Webster Nomination Papers, File ii/47, 1851 Archives, ICL. Brief biographical details on Webster can be found in the *Clare [College] Association Annual 1979-1980*, 79-80.

and exercises in vacuum technique, the range-determination of polonium α-particles and - of course - the determination of individual efficiency at scintillation counting by the Geiger-Werner method.[52]

Webster coped easily with the Nursery. He had received a thorough grounding in experimental research at Melbourne, having carried out investigations in X-ray spectroscopy,[53] and was soon set to work on his first research problem. Chadwick assigned Webster the task of repeating Bothe's work with the point counter, work which had been much discussed during the July conference, and about which Chadwick remained sceptical.[54] Bothe had also discussed with Meitner and Chadwick some new results in which γ-radiation seemed to be excited in light elements by bombardment with polonium α-particles, and it was these which Webster set out to replicate, using a Geiger-Müller counter and Meitner's polonium, suitably prepared by Chadwick. Geiger's original tube had ceased to work within weeks of its arrival in Cambridge, so Webster constructed several new ones.[55] After some initial difficulties which he ascribed to "the use of unsuitable iron wire," he was able to make a number of "satisfactory" instruments. The tubes varied a great deal in their physical dimensions and characteristics, and were therefore referred to individually by number.

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52 "Report on the Research Work at the Cavendish Laboratory, Cambridge, under the supervision of Sir Ernest Rutherford, carried out by H.C. Webster, M.Sc." (hereafter *Report 1*); Webster to the Secretary to the Commisioners of the Exhibition of 1851, 3 October 1928, File ii/47, 1851 Archives, ICL. B.W. Sargent, another new 1851 Exhibitioner (and Webster's partner in the Attic course) recalled (1985), 209) that:

> Chadwick called Webster and me into his office and gave us a 1-hour's talk on the physics of scintillation-counting, including the then unpublished work of Chariton and Lea. ... Although Webster and I had been in the Cavendish Laboratory only a week or ten days, we had gained the impression that the scintillation method was going out of fashion. Nevertheless, we feared that one or both of us might be assigned a research investigation involving the counting of scintillations for the Ph.D. degree. Knowing how slow and fatiguing the method was, we were determined to avoid it. Accordingly, we discussed whether we should cheat to keep our efficiencies below a level that Chadwick might regard as useful. When our efficiencies worked out at only 85% each, we decided that cheating was unnecessary.

53 H.C. Webster Nomination Papers, File ii/47, 1851 Archives, ICL.

54 Bothe (1928b); Bothe and Fränz (1928a, 1928b, 1928c); Schmidt to Meyer, 26 July 1928, SMP.

55 Geiger to Rutherford, 21 September 1928, RP: "We have found lately that the wires of our counting tubes only last for about a week or two. So I am afraid that the tube which I have sent you will also have stopped to work. We prepare the wires now in a different way and apparently with better success"; Webster, *Report 1*, 5; Webster, "Report to the Royal Commissioners for the Exhibition of 1851" (submitted 24 September 1930: hereafter *Report 2*), 1851 Archives, ICL, 63.

Counter No. 57 with its internal diameter of 1 cm and internal length 6.5 cm behaved differently from Counter No. 2, whose internal dimensions were 2.5 cm diameter and length 10 cm. Erratic and temperamental, the counters were no less problematic than the scintillation screen, their actual mode of operation remaining something of a mystery, even to their inventors.[56] Owing to the "uncertain action of the counter," Webster spent most of his time characterising the tubes' response to background radiation and to variations in the operating conditions, rather than in using them to repeat Bothe's experiments.[57] After eight months or so, he had made little progress.

Notwithstanding their capriciousness and variability, the new devices raised many of the recording problems associated with the scintillation technique. The central iron wire of the tube was connected through a condenser system to a string electrometer, whose kicks due to the passage of ionising radiation "were counted by eye, first directly through a microscope and later by means of a projection method."[58] Direct visual counting of kicks was liable to produce severe eye-strain, however, especially given the large numbers involved (several hundreds of kicks in a single run).[59] For this reason, Webster decided in March 1929 to construct an "automatically recording system," in an attempt to eliminate the human observer - himself - from the measurement process. Ruling out the possibility of photographic recording on financial grounds, Webster constructed a system using a commercial 3-stage valve amplifier "in order to make the current surges in the counter produce clicks in [tele]phones."[60] By artificially distorting the signal, valves made it possible to amplify the

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[56] Webster, *Report 2*, 59; Trenn (1986); Rheingans (1988). Sporadic efforts had been made through the 1920s at the Cavendish Laboratory and elsewhere to elucidate the action of electrical counters. See, *inter alia*, Kovarik (1919a); Appleton, Emeléus and Barnett (1924); Geiger (1924); Kutzner (1924); Zeleny (1924); Wulf (1925); Emeléus (1926); Kreidl (1927); Schmutzer (1927); Taylor (1928); Curtiss (1928a, 1928b, 1930a, 1930b, 1930d); Webster, *Report 2*, 58-62. Blackett recalled that "the Geiger counter was a very delicate instrument. As he put it: "In order to make it work you had to spit on the wire on some Friday evening in Lent." One had to be initiated into all the mysteries in order to get any results at all" (J.L. Heilbron's summary of an unrecorded interview with Blackett, 17 December 1962, AHQP, also quoted in De Maria and Russo (1985), 254).

[57] Webster, *Report 1*, *passim*. The Cavendish Laboratory was not, perhaps, the best place in which to be constructing and testing Geiger-Müller tubes; it had "long been used for radioactive work, involving the handling of radium emanation, etc. The air contain[ed] an appreciable amount of emanation, and consequently all apparatus is covered with some active deposit," giving very high background radiation. Webster solved this problem by moving to another laboratory which had not previously been used for radioactive work.

[58] Webster, *Report 1*, 6.

[59] *ibid.*, 7-10.

[60] Webster, *Report 1*, 12; *Report 2*, 66-67. The Loewe 3NF tube (3 stages of resistance-capacity coupled

small ionization currents produced in the Geiger electrical counter to such an extent that the passage of ionizing particles through the counter could be registered as a click in a loudspeaker or as the sudden deflection of a galvanometer, "just as the wireless engineer [could] turn bass voice to tenor."[61] This was basically the technology which had been displayed by Lindemann and his colleagues at the British Empire Exhibition in 1924. It had also been deployed in Vienna by Ortner and Stetter, who had found it suitable for demonstration purposes, but too unreliable for quantitative work. In the late 1920s, however, valves represented one of the fastest-growing areas in the emerging field of electronics and radio. The burgeoning wireless and broadcasting industry and a ready market for home entertainment led to vast improvements in valve design, performance and stability and to the availability of off-the-shelf components (see figs. 4.3-4.4).[62] Such components could often be used unmodified in the laboratory, as in Webster's experiments, but the 'wireless culture' of the 1920s also produced individuals able to adapt, modify and combine such components in ways that could be *made* useful in a research setting.

In the 1900s and 1910s glass-blowing had been the art acquired by many physicists for their work on gas discharges. Francis Aston, for example, had come to the Cavendish in 1910 as a self-taught but accomplished glass-blower. The 1920s saw the entry into the universities of students with new repertoires of technique drawn from their cultural milieu. The rise of organized broadcasting and the phenomenal development of wireless during the decade (the turnover of the British radio industry increased from £7.8 million in 1926 to almost £30 million in 1931[63]) was producing a culture of amateur radio enthusiasts, avid readers of wireless magazines and constructors of their own sets. According to an Oxford science undergraduate of the 1920s, there had "never been anything comparable in any other

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amplification) used by Webster was designed and marketed by Loewe Radio A.G. of Berlin-Steiglitz. It was one of a series of multiple-valve tubes developed by the company from 1927 to capitalise on a German tax on the use of radio receivers, which was partly determined by the number of tubes in the receiver. See Tyne (1977), 446-447.

61 Blackett (1933), 80.

62 On valve research and development in the 1920s, see for example Maclaurin and Harman (1949); Dalton (1975), **2**, 115-119; Stokes (1982); Clayton and Algar (1989), 115-122; Geddes and Bussey (1991). Tyne (1977) is a comprehensive and indispensable guide to valve development up to 1930. For a contemporary account of such developments, see Fleming (1924).

63 Plummer (1937), 45; Maclaurin and Harman (1949).

Wireless World JULY 10, 1920.

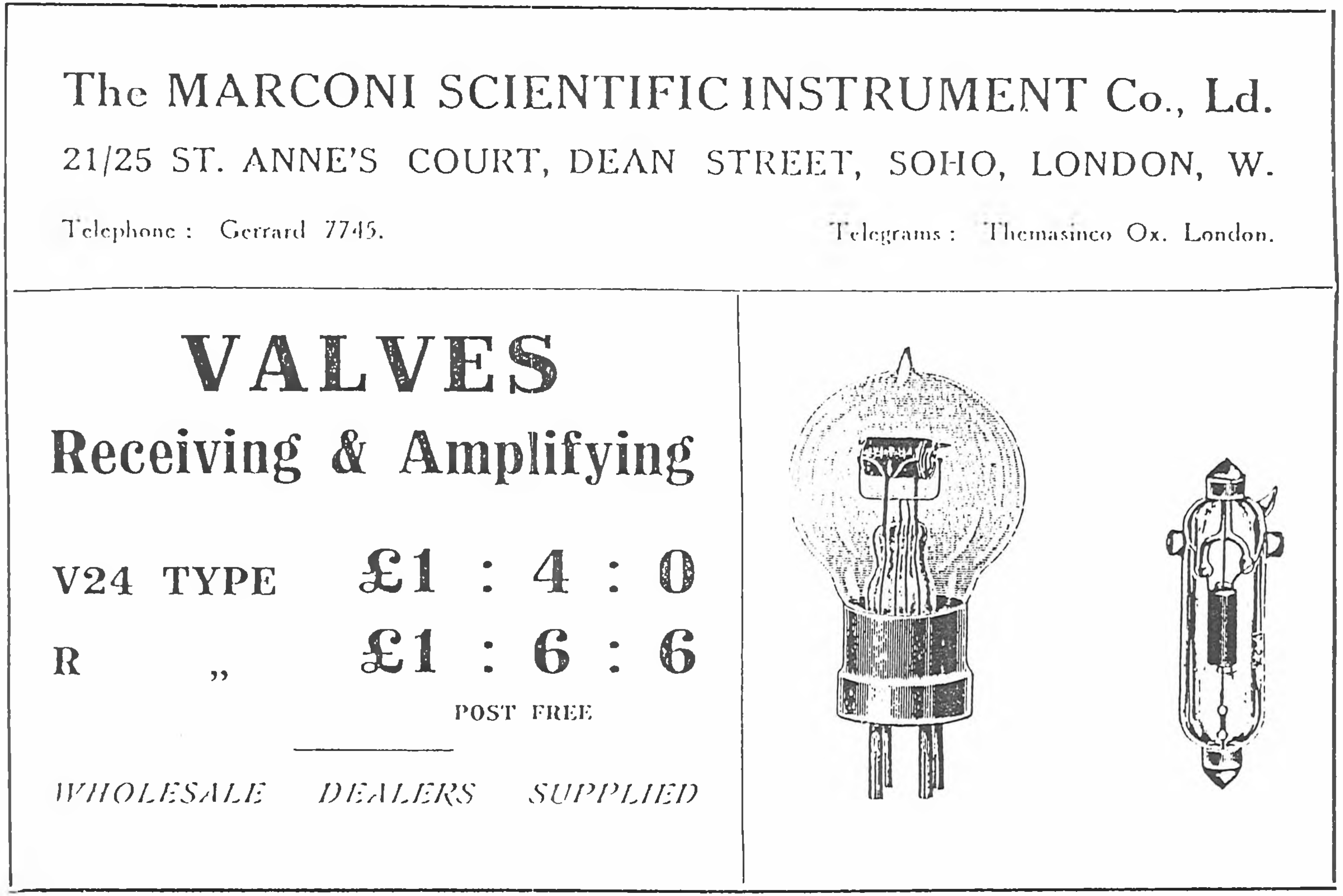

Fig. 4.3 Proprietary valves manufactured by the Marconi Scientific Instrument Company in the 1920s.

Source: *Wireless World*, 10 July 1920.

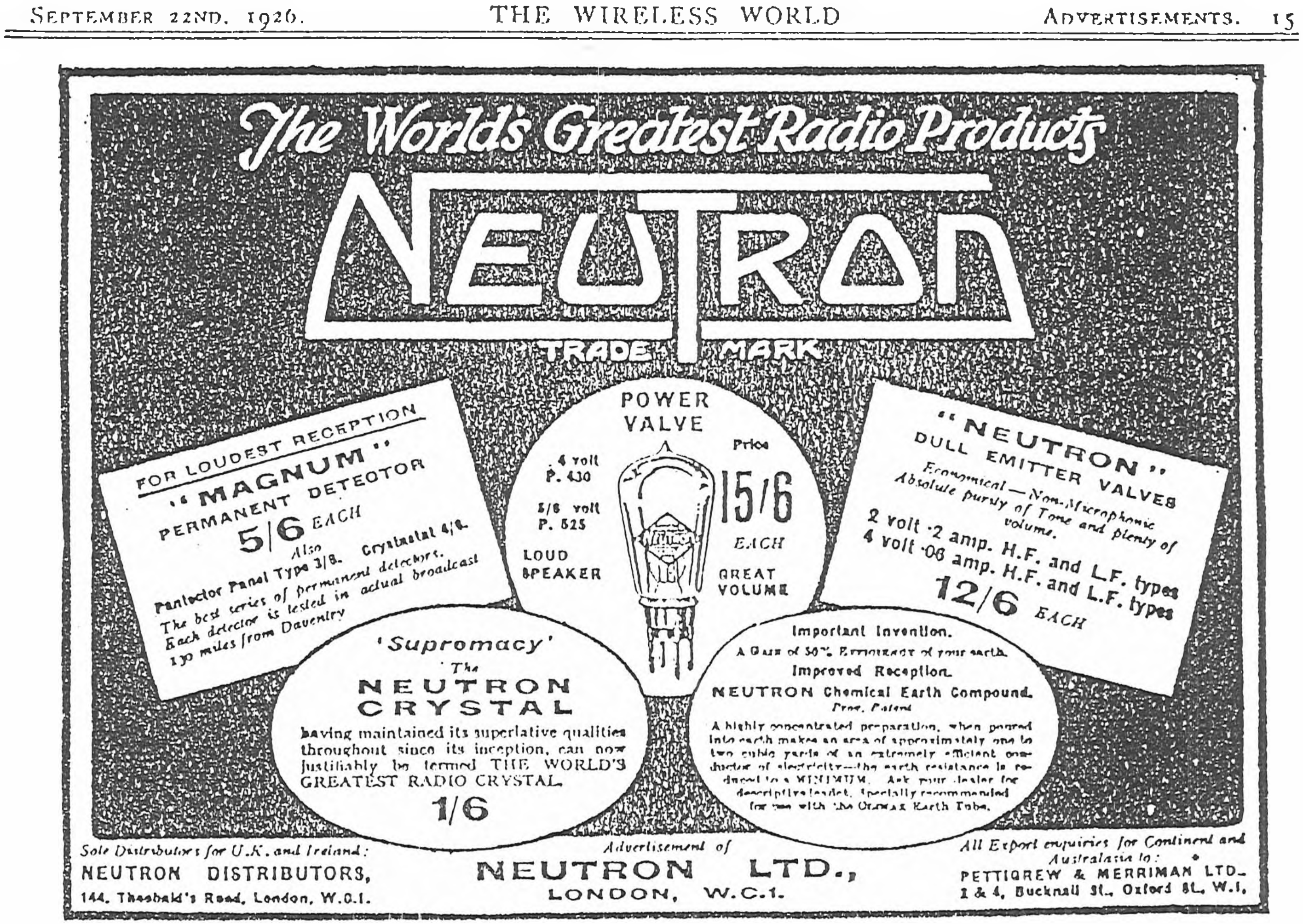

Fig. 4.4 Advertisement for 'Neutron' valves, 1926.

Source: *Wireless World*, 22 September 1926.

period of history to the impact of radio on the ordinary individual in the 1920s ... it was as near magic as anyone could conceive, in that with a few mainly home-made components simply connected together one could conjure speech and music out of thin air."[64]

The more ambitious enthusiast - of which the Cavendish had its fair share - "could always make modifications that might improve his aerial or his receiver and give him something to boast about to his friends."[65] Catering to those with such aspirations, a tranche of popular magazines for the mechanically minded enthusiast burst into print in the 1920s, including *Modern Wireless* (1923), *Wireless Constructor* (1924) and *Wireless Magazine* (1925). The oldest such magazine, *Wireless World* (1913), 'The Paper for Every Wireless Amateur,' ran a column called 'The Experimenter's Notebook.' 'Amateur' is perhaps not the right word to characterise Cavendish devotees of the wireless, however, for W.B. Lewis, a research student (and later collaborator) of Rutherford in the early 1930s, made a careful distinction between the quotidien realms of 'popular' radio and the kind of enterprise increasingly occupying the attention of the Cavendish 'boys.' He warned Cambridge University radio hams that "If you derive your knowledge of radio developments from the Wireless World, World Radio and the other weeklies, or more weaklys, you must not suppose that your knowledge is up to date."[66] Being an amateur in Cambridge was a serious business.

The field of wireless research was rapidly assimilated into academe in the 1920s. At the Cavendish, E.V. Appleton and his student J.A. Ratcliffe developed and institutionalised a school of ionospheric and radio physics with Rutherford's active support,[67] while the radio

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[64] Jones (1978), 30-31. It is difficult for the modern reader to appreciate how widely distributed wireless skills were in the 1920s, but it is perhaps worth remembering that 'black-boxed' radio sets only became available in the early 1930s. Even then, they were expensive, so that most listeners relied upon kits from which to construct their own radios. Contemporary assessments include Allen (1939)[1931], 221; Graves and Hodge (1991)[1940], 89. For splendid accounts of wireless and 'valve culture' in the 1920s, see Dalton (1975), **2**, 37-68, 95-132; Bussey (1990); Geddes and Bussey (1991), 9-215. Also relevant is Landes (1969), 423-430. The cultural impact of radio and mass communication in the same period is discussed by LeMahieu (1988), while Price-Hughes (comp.)(1946), 49-53, gives the industrial perspective.

[65] Jones (1978), 30-31. For the importance of amateur radio and wireless technology to laboratory practice in the 1920s and 1930s, see Tuve to J.A. Fleming, 16 January 1930, Box 9, File 'MS Notes on Orders 1930,' MATP; Tuve (1970), 166; Cornell (1986), vi, 13-26 and *passim*; Heilbron and Seidel (1989), 127. Unlike Heilbron and Seidel, who see radio and valves as minor stepping stones *en route* to Lawrence's cyclotron, I want to see such technologies as resources upon which experimentalists could draw - or not - in the *emergent* context of laboratory practice.

[66] W.B. Lewis, "Wireless Soc. Presidental Address 1933," Box 35, folder 6, WBLP, emphasis in original.

[67] On Appleton, see Ratcliffe (1966); Clark (1971); Crowther (1974), 260-263. For Ratcliffe and ionospheric physics at the Cavendish and elsewhere, see Ratcliffe (1929); Tuve (1970); "Fifty Years of the

and recording industries became a major source of employment for physics graduates, several Cavendish students finding their vocation in this field.[68] What I want to emphasise here, however, is that from the mid-1920s on, close links developed between the Appleton-Ratcliffe group, those engaged in work on electrical counters and the dedicated radio amateurs.[69] Lewis, himself a frequent contributor to the technical letters columns of several wireless magazines,[70] was only one of several such devotees in the Cavendish in the late 1920s and early 1930s. His colleague Eryl Wynn-Williams, an ebullient 26 year-old Welshman, was another.

A "true enthusiast" and lover of the "jargon" of radio,[71] Wynn-Williams had come to the Cavendish from Bangor in 1925 to work on problems connected with short electric waves.[72] Early in 1927, apparently at his own initiative, he exploited a recent development by Greinacher in Switzerland to construct a valve amplifier for ionisation currents, using off-the-shelf "Marconi 215" and "Osram 215" valves, which he found by trial-and-error to give good results.[73] With the various components "mounted on a baseboard," the apparatus could be "conveniently carried from place to place and quickly connected up as required"[74] - just like a radio set. Such skills were not unique to Cambridge, however. As we have seen, Greinacher's work was also quickly taken up by Ortner and Stetter in Vienna, who, facing

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Ionosphere," special issue of *Journal of Atmospheric and Terrestrial Physics* **36** (1974); Crowther (1974), 264-265; Ratcliffe (1978); Budden (1988), esp. 671-685. Rutherford's strong support for such developments is indicated in Rutherford (1927a), and by an exchange with Laby, who had asked about the prospects of another of another of his students being admitted to the Cavendish as an 1851 Exhibition scholar. Rutherford replied: "I would be prepared to admit him to the laboratory, and Ratcliffe would be glad to look after him on the wireless side, on which I have just spent a good deal of money." See Rutherford to T.H. Laby, 20 July 1931, RP.

68 Compare Frisch (1979a), 13.

69 Appleton, Emeléus and Barnett (1924); Emeléus (1924, 1926); Taylor (1928). Radio research also retained a strong military connection from its development during the war: "In these days when the word wireless suggests broadcasting or hams it is possible to forget that the pioneer personnel was mainly provided from naval and army signals, but one does not have to go far out of the broadcasting [realm] to realise that these still provide a solid background" (Lewis, "Wireless Soc. Presidental Address 1933," Box 35, folder 6, WBLP). On military wireless research during the war and afterwards, see, in the first instance, Hartcup (1988).

70 See, for example, Lewis (1929a, 1929b, 1931, 1932a, 1932b), and the series of notes and letters on wireless and related topics in Box 35, folder 6, WBLP; Lewis (1979, 1984); Lovell and Hurst (1988), 456-457.

71 Pollard (1991), 32. See also Ward (1987), 81-82.

72 C.E. Wynn-Williams Nomination Papers, File iii/36, 1851 Archives, ICL.

73 Greinacher (1924, 1926, 1927); Wynn-Williams (1927); Wynn-Williams (1957), 53. See also Ramelet (1928).

74 Wynn-Williams (1927), 821.

the same kinds of problems, developed it in a similar way.[75] Both laboratories recognised the possibilities inherent in the new technique, and both took advantage of it. Light (in all senses), portable and adaptable, the valve amplifier could not have presented a greater contrast with the scintillation method. And it was in the context of the uncertainty already beginning to surround scintillation counting that Wynn-Williams' innovations were taken up enthusiastically in the Cavendish.[76]

At Rutherford's instigation, the new device was immediately put to use in a 'straightforward' investigation: a redetermination of Z, the rate of emission of alpha particles by radium - one of the fundamental radioactive constants.[77] This was no random choice of experiment. Z had recently been redetermined at the Cavendish by H.M.Cave and H.J.J. Braddick,[78] providing a ready means of calibration for the new technique, since "[a]part from the different type of counter the general method of the experiment was the same as that of other workers who have used the direct counting method."[79] The radioactive source used in the experiment was similar to that used by Braddick and Cave, its strength being measured by comparing its γ-ray activity with that of the laboratory standard radium source, itself calibrated against the national radium standard at the National Physical Laboratory.[80] From the γ-ray measurements and the α-particle counts obtained, it should then be possible to calculate the rate of emission of α-particles from the radium C in the standard source, and hence Z, the rate of emission from radium itself. Such, at least, was the principle.

In collaboration with Cave, who joined the new investigation, and F.A.B. Ward, a first-year research student,[81] Wynn-Williams assembled a device consisting of a simple ionization

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75 During the July conference, Vienna's Ewald Schmidt was shown Wynn-Williams' amplifier, and reported back to Meyer that Ortner and Stetter's arrangement was "already now significantly superior" (Schmidt to Meyer, 26 July 1928, SMP).
76 Under advice from John Cockcroft, A.P.M. Fleming of Metropolitan-Vickers subsequently expressed an interest in employing Wynn-Williams at the company's research laboratories. See McKerrow to Cockcroft, 2, 12, 28 June 1932, CKFT 20/59, JDCP.
77 Ward, Wynn-Williams and Cave (1929), communicated to the Royal Society by Chadwick. The authors thank Rutherford for suggesting the problem (*ibid.*, 730). On the important radioactive constants, see M. Curie *et al.*, (1931).
78 Braddick and Cave (1928).
79 Ward, Wynn-Williams and Cave (1929), 714.
80 *ibid.*, 723, 728. The comparison of γ-ray activites was carried out using a conventional γ-ray electroscope surrounded by 2 cm of lead.
81 Ward (1987), 81-82.

chamber, a valve amplifer and an Einthoven galvanometer, whose final deflection could be reckoned to be proportional to the initial ionization caused by the passage of, say, an α-particle through the chamber. A 5-valve amplifier was employed, using a Marconi V24 as the first (input) valve, a DEH 610 and two DE 5b (all Marconi) in the intermediate stages, and an LS5 as output valve. With such an arrangement, the team claimed an amplification factor of about 10^9. But this was achieved at a price. The experimental environment required considerable manipulation to provide conditions under which the apparatus would behave reliably and consistently. In a place such as the Cavendish, noise and mechanical vibration were a constitutive part of experimental work and of laboratory life in general. The valve amplifier was found to be very sensitive to this everyday commotion, however. In order to "protect the apparatus as completely as possible from electrical and mechanical disturbances, to which it was very sensitive," therefore, the complete amplifier arrangement and the filament heating batteries were "enclosed in a metal box some 50 cm cube, which was suspended from the roof of the room by four long metal springs,"[82] an engagingly novel arrangement and an ingenious use of laboratory space.

While the spring arrangement insulated the valve amplifier from routine disturbances, other elements of the data production and reduction processes also required special attention. Instead of observing and counting the kicks of the Einthoven galvanometer by eye, Wynn-Williams, evidently something of an amateur photographer as well as a radio enthusiast, constructed a special camera "similar to the cameras used in electro-cardiographs" to produce a photographic record of the galvanometer's movements. A small shutter, worked by electric clock, was arranged to cast a shadow on one edge of a moving strip of photographic film at 1-second intervals, providing a time-scale. By this means, up to 500 particles a minute could be resolved on the record, though there were "a very few cases where such a large number of particles entered in rapid succession that it was difficult to tell to within one, how many the complicated response of the galvanometer represented." Even so, it seemed that the uncertainties due to such causes amounted to only 1 part in 500, a reasonable enough figure

[82] Ward, Wynn-Williams and Cave (1929), 718.

when the total number of particles counted was of the order of 10^5.[83]

The counting process itself was not entirely straightforward. The 400 feet long paper film was developed in the laboratory dark-room, where it was "wound, sensitive side out, on to specially made frames of wood impregnated with paraffin wax, each frame taking about 25 feet of paper." After development and fixing in large shallow dishes, "the sections of the record were joined together again in their proper sequence by short lengths of gummed paper and the complete record was then counted." The counting of the individual deflections from the photographic record was "done visually, the observer passing the record slowly in front of him and recording each kick observed on a mechanical counter actuated electromagnetically by means of a battery and tapping key." Again, however, optical problems arose in connection with the type of paper from which large numbers of deflections were to be counted. Any kind of gloss paper was found to be "extremely irritating to the eyes,"[84] leading to the eventual selection of Kodak Rapid Platino-matt bromide paper which did not induce such problems.

Though it raised different sorts of operational problems - the need for insulation from disturbance, special film, mechanical counters and the rest - the valve method was clearly no less elaborate than the scintillation technique. And, like the scintillation technique, the deployment of the amplifier methods in experimental practice summoned up a complex social organisation with its own characteristic division of labour.[85] Nevertheless, by mid-1929, the technique had been domesticated (and the laboratory environment correspondingly altered) to such an extent that Wynn-Williams and company could report a definite result, having produced a value for Z of 3.66×10^{10}, in "good agreement" with the value of 3.69×10^{10} found by Braddick and Cave. The trial had been a success. But the agreement in the Z values was not really the point. In developing and operating the proportional counter, Wynn-Williams and his colleagues had gained much experience with valve methods and with the technicalities of producing manageable data from them. In so doing, they had also learned

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83 *ibid.*, 727-728, 730.
84 *ibid.*, 720.
85 For contemporary comments on the emergence of collaborative, multi-skilled research efforts, see Blackett (1933), esp. 71.

something of the limitations and potentialities of such methods. Above all, however, they had shown that with concerted effort, radio valve technologies could be made reliable, consistent and useful in the laboratory.

So, the success of the first valve amplifier owed much to the labour invested in it by Wynn-Williams, Ward and Cave. Months of patient, methodical work had yielded a technique whose capacity, workings and potential for further development were relatively well understood, and which could be applied to the problems of pressing interest in the Cavendish Laboratory. Several such applications now suggested themselves. As Greinacher had shown in his pioneering work on the valve amplifier, for example, one of the great advantages of the proportional counter was that it could be used "to distinguish between different types of ray, for example between an H-particle and an α-particle, for the ionization produced per centimetre path by an H-particle is only about one quarter of that produced by an α-particle." The valve counter should therefore "prove a powerful instrument in the investigation of the artificial disintegration of the elements and it is for this purpose that it has been developed."[86] With the experience gained by Wynn-Williams and the others, the possibility of deploying the amplifier method in the contested disintegration experiments was rapidly becoming a practical prospect.

Heartened by his boys' success, Rutherford extolled the virtues of the new electrical counting methods to Stefan Meyer in June 1929. At the Cavendish, he reported, "we have been occupied the last year or two in developing electrical methods for counting α particles and hydrogen particles in the presence of a strong β and γ radiation," adding: "I think if much more progress is to be made on artificial disintegration, it is essential to tackle it by electrical methods and count a large number of particles. The scintillation method is quicker for a preliminary survey but is not ideal for quantitative investigations which are now necessary"[87] - the first implicit acknowledgement of the troubles surrounding the scintillation technique. A new programme of technical development was emerging, and with it a new regime of laboratory practice. Although it would "inevitably be a long and heavy business to

86 Ward, Wynn-Williams and Cave (1929), 717.
87 Rutherford to Meyer, 10 June 1929, SMP.

get useful quantitative data," the valve counter had proved itself. The scintillation technique could be put quietly to one side. Things were beginning to look promising.

As Rutherford began to trust the amplifier method and to credit its advocates, however, a difficulty emerged which not only made public the carefully-concealed doubt surrounding the scintillation method, but which also threw the development of electrical techniques into uncertainty. The challenge was the more puzzling - and the more urgent - because it came from an unexpected quarter. Bergen Davis, Professor of Physics at Columbia University, and his graduate student Arthur H. Barnes, had undertaken a series of experiments to determine the way in which the chance of an electron being captured by an α-particle varied with their relative velocity. Their results seemed to show not only that electron capture by α-particles was much more frequent than Rutherford's earlier work had suggested, but also, more controversially, that such capture occurred at a series of discrete energies, corresponding to the energy levels of the electron in Bohr-type orbits around the helium nucleus. Crucially, their claims were based on the results of scintillation counting experiments.

4. Electron Capture Re-Visited: The Columbia Heresy

4.1 Electron Capture and the Davis-Barnes Experiment

Born in 1869, the son of a New Jersey farmer, Bergen Davis had become interested in physical science at an early age. In 1891, savings and an inheritance had enabled him to enter Rutgers College, where he graduated in 1896. Following graduation he taught at the School for the Deaf in New York City. In New York he became acquainted with Ogden N. Rood, Head of the Department of Physics at Columbia University, who gave him permission to work in the University's physics laboratories. A year later, in 1899, Davis was awarded a University Fellowship and began graduate work at Columbia. After receiving his doctorate in 1901, Davis, like many other graduate students, went overseas to continue his education and receive a broader-based training in contemporary scientific research. A John Tyndall Fellowship from Columbia enabled him to spend a year in Göttingen and a year at the Cavendish Laboratory, Cambridge.[88]

The year at Cambridge in particular had "a profound effect on Davis and his future career in research."[89] J.J. Thomson's intuitive approach to problems in the 'New Physics' appealed to Davis, whose training led him to attack problems experimentally rather than from a more abstract or theoretical perspective. On his return to Columbia in 1903, when he became Tutor in Physics, Davis brought with him a strong interest in gas discharges, and soon had a group of students working in the field. This established a line of research at Columbia which continued well into the 1920s; not for nothing did students often refer to Davis' laboratory as "The Little Cavendish"![90] In the first few years of the century, before radioactive substances became concentrated in a few laboratories, Davis was also one of those who dabbled in radioactivity.[91] After a succession of promotions - Instructor from 1907-1909, Adjunct Professor 1909-1913, Associate Professor 1913-1919 - Davis became Professor of Physics at Columbia in 1919, a post which he held until his retirement in 1939 at the age of seventy.

Davis' interest in the capture of electrons by α-particles had started, as we have seen, in the early 1920s. Linking G.H. Henderson's work at the Cavendish Laboratory with the testimony of the cloud chamber, Davis had proposed a mechanism for electron capture which implied that all α-particles should capture the first and second electrons at the same velocities, irrespective of their initial speeds. This was, he had suggested, "a matter of sufficient importance to determine experimentally."[92] But as far as workers at the Cavendish Laboratory were concerned, Davis' was a voice crying in the wilderness. Henderson's return to Canada and the fact that Davis' own major interests lay elsewhere meant that the

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88 This brief biography of Davis is based on Webb (1960). For a more general discussion, see Geiger (1985); Kevles (1987).
89 Webb (1960), 69.
90 *ibid.*, 70. See also Rigden (1987), 40.
91 Rutherford to Davis, 15 April 1905, 4 January 1908, 3 March 1909, BDP; Davis and Edwards (1905); Badash (1979a), 48; Sinclair (1988).
92 Davis (1923).

suggestion fell on barren ground, and for the next five years, Davis continued his research on X-ray diffraction, also making some contributions to the elucidation of the Compton effect.[93]

Davis' opportunity to follow up his 1923 idea came in 1928 when a Columbia graduate student, Arthur Hart Barnes, was casting around for a research problem for his dissertation. Barnes, then aged 24, had taken his A.B. at Columbia in 1924 and an A.M. in 1926, and had decided to stay on to work towards a Ph.D. under Davis' supervision. Davis suggested the electron capture problem which had exercised him five years earlier, and Barnes took up the work enthusiastically. Working in close collaboration, Barnes and Davis constructed the apparatus shown schematically in fig. 4.5. A stream of electrons from the oxide-coated filament F is superimposed on a beam of α-particles from polonium deposited on the end of a pointed rod at S. Electrons and α-particles therefore travel together for some distance in the evacuated tube, where they have opportunity to collide and combine. In order for them to have a reasonable chance of doing so, according to Davis' supposition, their speeds had to be roughly matched. This was done by accelerating the electrons through a voltage V_n acting between filament and grid G. Crucially, the α-particles and other charged bodies in the tube can be deflected by a magnetic field at M and their presence detected by observing scintillations "in the usual manner" on a zinc sulphide screen. If an α-particle captured an electron between F and G, reasoned Barnes and Davis, its charge would be reduced and it would not experience the full magnetic deflection. The number of scintillations counted per minute would therefore be observed to decrease, indicating the processes occurring in the tube. Most significantly, a reduction in the observed number of scintillations would signify that capture was occurring, since fewer particles would be deviated sufficiently to reach the screen.[94]

So much for theory. In practice, a number of modifications were necessary in order for the experiment to produce coherent results. The original velocity of the polonium α-particles was reduced from 1.59×10^9 cm/sec to 1.45×10^9 cm/sec, for example, so that the number of

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[93] Davis to Bohr, 24 March 1924, BSC; Davis (1922, 1925a, 1925b, 1927); Webb (1960), 72-74. For Davis' interest in the Compton effect, see Stuewer (1975), 255, 257, 261, 281 n.127.

[94] Davis and Barnes (1929).

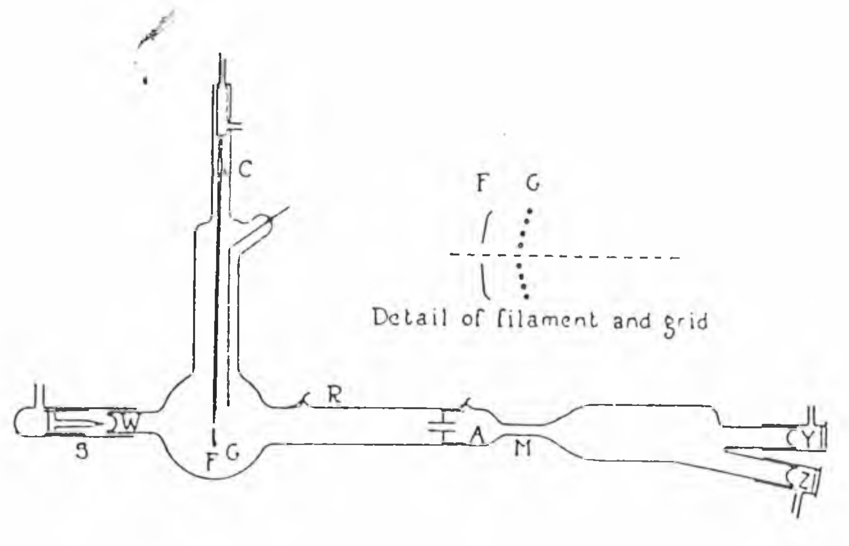

Fig. 4.5 Schematic diagram of the apparatus used by Davis and Barnes in electron capture experiments, Columbia University, 1929-30. α-particles from the source S enter the tube at window W and travel through an electron beam to scintillation screens at Y or Z.

Source: Barnes (1930), 218.

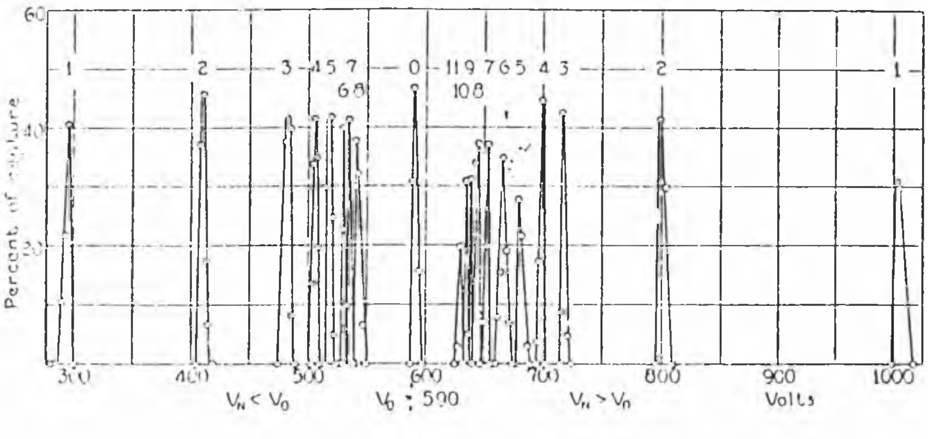

Fig. 4.6 Discrete electron capture peaks obtained by Davis and Barnes plotted as a function of electron accelerating voltage. At the central peak, labelled 0, electrons and α-particles have the same speed.

Source: Davis and Barnes (1929), 153.

scintillations recorded at the zinc sulphide screen was reduced to manageable proportions - about 60 per minute. Davis and Barnes were much worried about the possibility of errors in the counting process, taking special precautions to ensure that the counting of scintillations was not "influenced by suggestion" or by foreknowledge of the expected results. They used two tests to circumvent such errors. Changes in voltage were made without letting Barnes, the observer, know in advance what the changes were. In addition to that elementary precaution, a small electromagnet could be used to change the direction of the α-particles' path, again without Barnes' prior knowledge. Barnes proved his reliability to Davis' satisfaction on both counts by noticing such changes immediately, thereby also demonstrating the adequacy of the tests themselves.[95] With these precautionary measures in place to guarantee the integrity of the scintillation counting by guarding against bias, the Columbia team had every confidence in their results. And those results turned out to be quite surprising.

Davis and Barnes' initial findings are shown in fig. 4.6, in which percentage capture is plotted against the applied voltage. They found a sharp peak, representing significant electron capture, at V_0=590 volts. This was not unexpected, for at this voltage the speeds of the electrons and α-particles were the same. To their astonishment, however, they also found two series of sharp peaks on either side of V_0, apparently signifying that electrons were *only* being captured when their velocity relative to that of the α-particles had certain definite and characteristic values. Moreover these characteristic velocities could be shown to correspond to the orbital energies of electrons in the ionised helium atom. In fig. 4.7, which represents the α-particle and chasing electron at the moment of capture, the velocities of the particles are u and w respectively. The circle around α represents a Bohr orbit, v being the velocity an electron would have in this orbit. The condition for capture of a free electron by an alpha-particle is that $v=(u-w)$ or $v=(w'-u)$, i.e. the relative velocity of electron and α-particle must equal the velocity which an electron would have in an orbit if captured. The situation can also be expressed in a different way, by transferring the system of coordinates

[95] Davis and Barnes (1931).

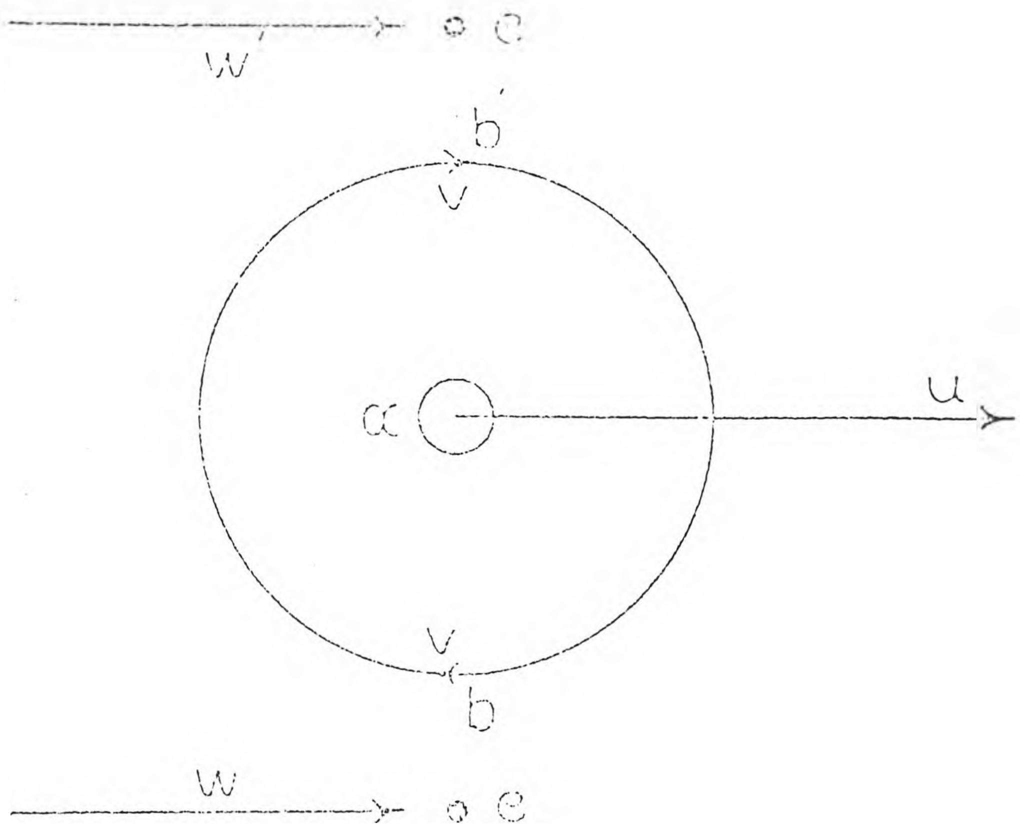

Fig. 4.7 Diagram illustrating the kinematic process of electron capture, as visualised by Davis and Barnes. u is the velocity of the α-particle, w the velocity of the chasing electron. The circle around α represents a Bohr orbit, v the velocity an electron would have in this orbit.

Source: Davis and Barnes (1929), 154.

to the alpha-particle. In this case the α-particle is at rest and the electron appears to approach from the right with velocity v if $V_n<V_0$, or from the left with velocity v if $V_n>V_0$ (v is in each case the relative velocity of alpha-particle and electron). Velocities w and w' are those acquired from the applied field V_n, and the velocity that the approaching electron may acquire from the doubly-charged nucleus is ignored. The energy equations are then:

$$\tfrac{1}{2}mv^2 = \tfrac{1}{2}m(u - w)^2$$

$$\tfrac{1}{2}mv^2 = \tfrac{1}{2}m(w' - u)^2$$

Since $E_n e=\tfrac{1}{2}mv^2$; $V_0 e=\tfrac{1}{2}mu^2$; $V_n e=1\tfrac{1}{2}mu^2$; where E_n = ionisation potential of singly ionized helium from the energy-level of quantum number n, we obtain:

$$E_n = (V_0^{1/2} - V_n^{1/2})^2 \quad \text{for } V_n<V_0$$

$$E_n = (V_n^{1/2} - V_0^{1/2})^2 \quad \text{for } V_n>V_0.$$

The calculated series terms and the observed values of E_n at a series of voltages V_n are shown in the table (Table 4.1). The penultimate column is calculated using $E_n=4(13.54)/n^2$ (volts), on the basis that the ionized helium atom is a hydrogen-like structure with a double charge. While Davis and Barnes admitted that "[t]he agreement between the last two columns of the table is not good," they provided a range of reasons to explain why this was so. The arrangement of filament and grid, for example, might be a source of trouble, since a change in the potential V_n might alter the distribution of the electrons. They also invoked the space-charge effect which "must have been strongly present" at such large electron currents as 60mA. "This and other matters that may affect the results," they concluded on a promissory note, "will be investigated."[96]

So significant did Davis consider Barnes' early results to be that they were put before the scientific community in a preliminary announcement in the *Physical Review* of 1 July 1929,

[96] Davis and Barnes (1929), 155.

n	$V_n < V_0$	E_n	$V_n > V_0$	E_n	Mean of E_n	$E_n = 54.16/n^2$
1	295	50.6	1005	54.9	52.0	54.16
2	410	16.7	800	15.6	16.2	13.54
3	483	5.33	720	6.45	5.67	6.01
4	505	3.29	700	4.71	4.00	3.38
5	519	2.28	681	3.27	2.77	2.16
6	531	1.56	667	2.41	1.98	1.50
7	535	1.34	653	1.59	1.46	1.10
8	538	1.19	645	1.23	1.21	.84
9			638	.96		.67
10			635	.88		.54

Table 4.1 Table showing calculated and experimental values for electron capture at discrete velocities.

Source: Davis and Barnes (1929), 155.

which "purposely omitted" full details and descriptions of apparatus. Barnes signalled his intention to continue the investigation and promised a fuller account of the work in due course.[97] Excitement ran high in the Columbia camp over the new results. Innovative, exciting and significant research was good for the department and good for the University, which was about to celebrate its 175th anniversary.[98] It was also a perfect expression of Davis' personal philosophy of science in which new phenomena and new regularities in nature were the proper and only goal of scientific investigation. As he put it, in a phrase redolent of his teacher J.J. Thomson: "Men dream of the excitements ... and the adventures of exploration of unknown lands, of the ascent of a mountain or the conquests of the air. These may be thrilling adventures [but] both in value and thrill they are not to be compared to the discovery of a new phenomenon or a new law of nature."[99] The discovery of the Davis-Barnes effect, in the wake of the disclosures of other significant "effects" by Compton, Raman and others, seemed to vindicate that philosophy and to provide a new and fruitful line of research. Unfortunately, it also brought Columbia to the centre of controversy.

4.2 The Davis-Barnes Effect: Initial Reactions

Within a month of its publication, the Davis-Barnes 'preliminary' paper attracted commentary. First off the mark was Elliott Q. Adams of the General Electric Company, who complained that Davis and Barnes had given no explicit mechanism for the effect they had observed.[100] Offering an explanation based on the earlier work of Henderson, who had shown that a beam of alpha-particles when passed through matter contains He^+ ions,[101] Adams considered a collision of a He^+ ion in the n^{th} quantum state with an electron

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97 Davis and Barnes (1929).

98 On the importance of research at Columbia see, for example Hawkes (1930); "Columbia University. Faculties making Special Studies in Various Fields," *New York Times*, 5 April 1931, 24; *A Quarter Century of Learning, 1904-1929* (New York, 1931). An account of the 175th anniversary celebrations can be found in the *Columbia Review* 1 (1930). A more general account of attempts to improve the status of research in American universities in this period can be found in Geiger (1985).

99 Davis (1932), 613.

100 Adams (1929).

101 Henderson (1925).

approximately satisfying the Davis-Barnes equation $E_n=(V_0^{1/2} - V_n^{1/2})^2$. Ionization of the He^+ ion gives an alpha-particle (He^{++}) and two electrons "practically at rest with respect to the stream of α-particles." Capture of both electrons would give rise to two He^+ ions of high quantum state. On reaching the n^{th} quantum state by radiation, the He^+ ions would again be ionised, "giving four electrons, which upon capture give four He^+ ions." The process, according to Adams, would therefore be cumulative.

Adams' proposed mechanism was rebutted by Barnes, who pointed out that Adams' suggestion necessitated the simultaneous presence in the tube of a large number of α-particles in the electron stream. Under the experimental conditions, however, the number of α-particles passing through the electron stream had deliberately been cut down to aproximately 60 per minute. Since the electrons traversed the entire length of the tube in 4×10^{-8} seconds, it was "extremely improbable ... that there will ever be more than one alpha-particle among the electrons at any instant." Adams' cumulative mechanism, he concluded, "would therefore appear to be impossible."[102]

Barnes' response is informative, for it indicates the way in which both he and Davis pictured the processes taking place inside the tube in terms of simple images of the dynamics of the interacting particles. Both paid careful attention to the practical management of the experiment on the basis of this imagery. That same imagery also served to convey the purpose and results of the experiment to a wider audience. A talk by Davis in November 1929 following his election to the National Academy of Sciences (an indication of the American physics community's high esteem for Davis' work) provided irresistible fodder for a *Science News* reporter, who described the "story of chase and capture in the submicroscopic world of physics" being played out at Columbia. This "new light on the behaviour of the nucleus of the helium atom in making electrons its own" promised to yield much information on the constitution of helium and of matter in general, a point reiterated by Davis when he won a $2,500 Research Corporation prize in recognition of his X-ray work.[103] To heap praise upon praise, the Columbia experimenters' confidence in their work

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102 Barnes (1929).

103 "Helium Heart's Affinity," *Science News-Letter*, 30 November 1929; undated clipping in 'Biographical'

was boosted yet further that same month when Marie Curie visited Davis' laboratory as part of the University's 175th anniversary celebrations. She was impressed with what she saw. Reporting back to her daughter Irène at the Laboratoire Curie in Paris, she told of:

> Davies [sic] who is studying the capture of electrons by α rays. A young assistant who counts the scintillations is really the person on whom rests the certainty [sûreté] of the observations. The work seems important [sérieux] and interested me very much ... The laboratory is large, well set out and has many resources; it made a very good impression on me.[104]

With Davis' election to the National Academy of Sciences, recognition for the electron capture experiments and the glowing endorsement of Marie Curie, it seemed at the end of 1929 as if Columbia's star was firmly in the ascendant.

4.3 'A Great Puzzle ...": Theoreticians' Responses to the Davis-Barnes Experiment

By the end of 1929, Barnes had completed the promised second paper, a full technical account of his earlier experiments with Davis and a summary of his more recent work. The paper appeared in the February 1, 1930 number of the *Physical Review*.[105] In it, Barnes described the construction and operation of a second experimental tube. The new arrangement had been constructed with four goals in mind: (1) to reduce the time of passage of an α-particle through the electron stream to a known small value; (2) to make possible the detection of scintillations due to singly charged and neutral particles; (3) to allow the effect of varying the velocity of the α-particles to be investigated; and (4) to facilitate the investigation of the captureless intervals located on each side of the central peak.[106] The experiments had clearly become much more sophisticated since the publication of the preliminary paper. Their interpretation, however, was another matter entirely.

As Barnes continued to improve the experiment and to follow up new lines of investigation

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folder, Box 2, BDP; Webb (1960), 75.

104 M. Curie to I. Curie, 5 November 1929, in Ziegler (ed.)(1974), 314-315, on 314.

105 Barnes (1930).

106 *ibid.*, 221.

suggested by his results, a small group of mathematical physicists were struggling to bring the Davis-Barnes observations within the compass of atomic theory. Coming as they did in the wake of the elucidation of wave mechanics, the Columbia results offered the theoreticians the perfect opportunity to spread their mathematical wings and attempt to 'explain' the new phenomena. They seized upon Davis and Barnes' results with alacrity, much as they seized upon the contemporaneous disclosure of the Raman effect. The new, almost predatory, relationship developing between theoreticians and experimentalists was perfectly captured by an ever-perceptive Rutherford in November 1929:[107]

> In watching the advance of science, and particularly of the physical sciences today, one cannot fail to be struck by the very close connection between theory and experiment - a relation which is probably more intimate than at any other period of scientific history. Every new experimental observation is at once seized upon to test whether it can be explained by existing theories, and if not, to find the modifications necessary to include it in the general theoretical scheme of natural processes. The mathematical analysis often suggests the possibility of unexpected relations which can be made the subject of fruitful experimentation. These two, in a sense, complementary branches of physics profoundly react and interact with each other, and their united efforts lead to a greatly accelerated rate of advance in knowledge and understanding of the essential principles involved. The rapidity of advance in physics, which has been so marked a feature in the last decade, is mainly due to this close combination of theory with experiment.

Bohr, the maestro of theoretical physics, agreed with this *aperçu*, telling Rutherford in 1930 that "[i]n view of the latest theoretical development *almost every problem has acquired renewed interest, and we are all longing for new experimental facts.*"[108] With the Davis-Barnes effect, the theoreticians had plenty to keep them occupied. There was, for example, a "fundamental disagreement with the quantum theory" involving radiation of double frequency. In the reported experiments, the electron did not fall from rest into a given energy level (except at V_0, when the velocities of electron and α-particle were equal). Rather, it had an initial energy equal to that of the energy level in which it was captured. So, twice the

[107] Rutherford (1929f), 878. Compare with the emphasis on experiment in Rutherford (1923d).
[108] Bohr to Rutherford, 3 February 1930, RP, my emphasis.

normal energy should be radiated upon capture, raising the question of whether the radiation was of two quanta of normal frequency, or of one quantum of double frequency.[109] In fact, lines in the spectrum corresponding to radiation of double frequency were never observed, casting some doubt on the whole phenomenon. The capture probabilities also gave rise to some difficulties. In their experiments Davis and Barnes found capture of a different order of magnitude than anyone else had previously done in similar work. Theoreticians were also puzzled by the "extraordinary constancy" of the percentage capture across large variations in experimental conditions. Furthermore, to add to the growing list of objections and discrepancies, the narrowness of the capture peaks seemed to be extraordinarily consistent, given the fact that "electron velocities ... would not have been at all sharply defined."[110]

The autumn of 1929 saw much debate in parts of the theoretical community, both in America and in Europe.[111] Barnes' earlier objection to Adams' proposed mechanism (see above) was itself discounted by E.C.G Stueckelberg and P.M. Morse of Princeton, who showed "by quantum mechanical means" that the Davis-Barnes peaks were caused by "some mechanism involving more than one electron and one alpha-particle."[112] They admitted that their comments were inconclusive, however, since the values of the constants in their formulae were not known. This tone of slight bewilderment was echoed across the Atlantic, where the experiments were seen as "a great puzzle to anyone interested in atomic physics."[113] According to mathematical physicist Joseph Mayer, researchers at Göttingen's Second Physical Institute were "much interested in, and sceptical over, the work of Bergen Davis on the recombination of alpha particles and electrons." Max Born had his students working on various calculations connected with the Davis-Barnes phenomenon, though their results, too, were inconclusive. At one point, it had seemed as if there might be a sound mathematical basis for the effect:[114]

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109 Barnes (1930), 228.
110 Langmuir to B. Davis, 8 May 1930, BSC.
111 For the transmission of wave mechanics from Europe to the United States, see Coben (1971); Schweber (1986). Compare also Assmus (1992a, 1992b).
112 Stueckelberg and Morse (1930a, 1930b).
113 Bohr to Langmuir, 3 August 1930, BSC.
114 Mayer to Lewis, 5 December 1929, GNLP.

> One of Born's students has calculated out the matrix elements for the transition into the continuum and finds that there really are these maxima at energies mirror-wise above the ionization to the lines below ionization. The calculation of the matrix elements finally convinced everyone here that the experiment was true. It turned out later that a Japanese had calculated and found these maxima in the 'uebergange Warschernlichkeit' before, but evidently had not realized what they meant.

Within weeks, however, these claims were rescinded, doubt settling in once more. Mayer hastened to correct his earlier remarks to Lewis:[115]

> In my last letter I said that the Bergen Davis effect had been calculated. That is evidently false. ... Born made a mistake when he interpreted the Japanese calculations as being those of the Bergen Davis effect. Now the student of Born who had made the calculation independently says that when the 'Normalizierung' factor is introduced the curve loses the maxima. To me it sounds as tho' if he once got probability maxima at the place where Bergen Davis finds increased combination, that he probably calculated his 'normalizierung factor' on a wrong assumption to get rid of them. Anyway at the present moment official opinion is that one cannot find these probability maxima on the new mechanics.

In a sense, the work of the Göttingen theoreticians was defining the character and adequacy of wave mechanics just as much as it was defining the character of the Davis-Barnes effect. Gilbert Lewis, the Berkeley physical chemist, made exactly this point when he told Mayer that the Davis-Barnes work had been "reviewed at our [Berkeley] colloquium and Lawrence and Oppenheimer felt that the whole thing must be an illusion, but I stood out for the probable correctness of the experiment and felt that if quantum mechanics could not account for these higher states or maxima above the ionizing potential, it probably would have to be modified until it could."[116] This was the point: should one rule the Davis-Barnes experiments out because they conflicted with wave mechanics, or should wave mechanics be altered to accommodate them?

Bohr articulated one solution to this 'theoretician's regress' when he commented pessimistically that it had been "impossible to bring the results of these experiments in line

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115 Mayer to Lewis, 29 December 1929, GNLP.
116 Lewis to Mayer, 14 January 1930, GNLP.

with *what is considered at present as a reliable foundation of atomic theory.*"[117] When he visited Manchester in May of 1930, Bohr discussed the Columbia work with E.J. Williams, a gifted young physicist whose principal work was on the subject of atomic collisions.[118] During the same trip, Bohr gave the Scott lectures at Cambridge, where he also discussed the Davis-Barnes effect with interested parties. Confusion abounded, however. On his return to Copenhagen, Bohr wrote to Douglas Hartree: "With Jacobsen I have again discussed the problem of the capture of free electrons by α-particles, and although we quite agree with Williams, that the remark I made about the definition of velocity in the experiment of Bergen Davis and Barnes was based on a mistake, we still think that their results in various respects involve such paradoxes that it is difficult to agree in their conclusions. But of course one must be prepared for surprises ..."[119]

The contingency of the Copenhagen school's decision to regard wave mechanics as adequate and the Davis-Barnes experiments as suspect was displayed by the fact that several theoreticians elsewhere found resources to develop and sustain wave mechanical interpretations of the Columbia work. In May 1930, for example, it was claimed that "[a]pplication of the methods of wave mechanics is capable of furnishing an interpretation of the recent experimental results of Davis and Barnes on the seizure of electrons by α-particles."[120] And as late as February 1931, Jean Louis Destouches, theoretician, pupil of Louis de Broglie and evidently closely connected with Maurice de Broglie's group of experimentalists, constructed a theory of the Davis-Barnes effect based on the formula developed by Stueckelberg and Morse which gave good agreement between calculation and the observations.[121] Within the elevated circles of the Copenhagen school, however, scepticism, confusion and uncertainty continued to reign.

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117 Bohr to Langmuir, 17 June 1930, BSC, my emphasis. Compare Collins (1985).
118 Blackett (1948).
119 Bohr to Hartree, 5 June 1930, BSC; Jacobsen (1930a).
120 Wataghin (1930), abstracted in *Nature* **126** (1930), 1014.
121 Destouches (1930, 1931). Significantly, at least one of these papers was communicated to the Académie des Sciences by Maurice de Broglie. On Destouches and French theoretical physics, see Pestre (1984), Ch.4, esp. 119-134

4.4 'A Sport Played by Graduate Students': Cambridge Attempts to Replicate the Davis-Barnes Experiment

The theoreticians' confusion was mirrored at the Cavendish Laboratory, where the Columbia work attracted Chadwick's attention. Davis and Barnes' results, startling in their own right, also implicitly contradicted the earlier work of Henderson and Rutherford, and were therefore deserving of serious attention. That the Columbia experimenters were using the scintillation method made them a particular cause for concern. Chadwick knew, more than anyone, the fragility of the scintillation technique - that had been the main lesson of his visit to Vienna, after all. Now, two years later, at the moment when the Cavendish had abandoned the 'unreliable' scintillation technique in favour of electrical methods, a pair of outsiders with no previous experience of scintillation counting claimed to have produced these astonishing results. Beyond Davis' early sally in 1905, they had no previous record of publication in the field of radioactivity which, as those within it well knew, required more than a passing familiarity with the relevant laboratory techniques. As Rutherford had put it to Bohr during the controversy with the Vienna workers, "[a]ll the experiments look easy, when they are really very difficult and full of pitfalls for the inexperienced."[122]

In November 1929, Chadwick therefore set a graduate student the task of repeating the Davis-Barnes experiment. He chose Webster, whose attempted replication of the Bothe-Fränz experiments had ended inconclusively over the summer, but who had demonstrated his ability to cope effectively with the new electrical counting techniques.[123] Webster was to be joined by Norman de Bruyne, who had just completed some work on the effect of high electric fields on thermionic and field emission, and was therefore in search of a new project. In the hallway outside Chadwick's office, the two research students constructed an apparatus as similar as possible to the Columbia arrangement, "save that an electrical counter was used instead of a scintillation screen."[124] The apparatus is shown schematically in fig.4.8. A is

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122 Rutherford to Bohr, 18 July 1924, RP.
123 Webster, *Report 1*. As de Bruyne recalled the episode, "Rutherford said [the Davis-Barnes results] seemed inconceivable, but someone would have to repeat the experiment." N.A. de Bruyne, Reminiscences, unpublished typescript, Trinity College Cambridge, partially reprinted in Hendry (ed.)(1984), 81-89, on 88.
124 Webster (1931a), 118.

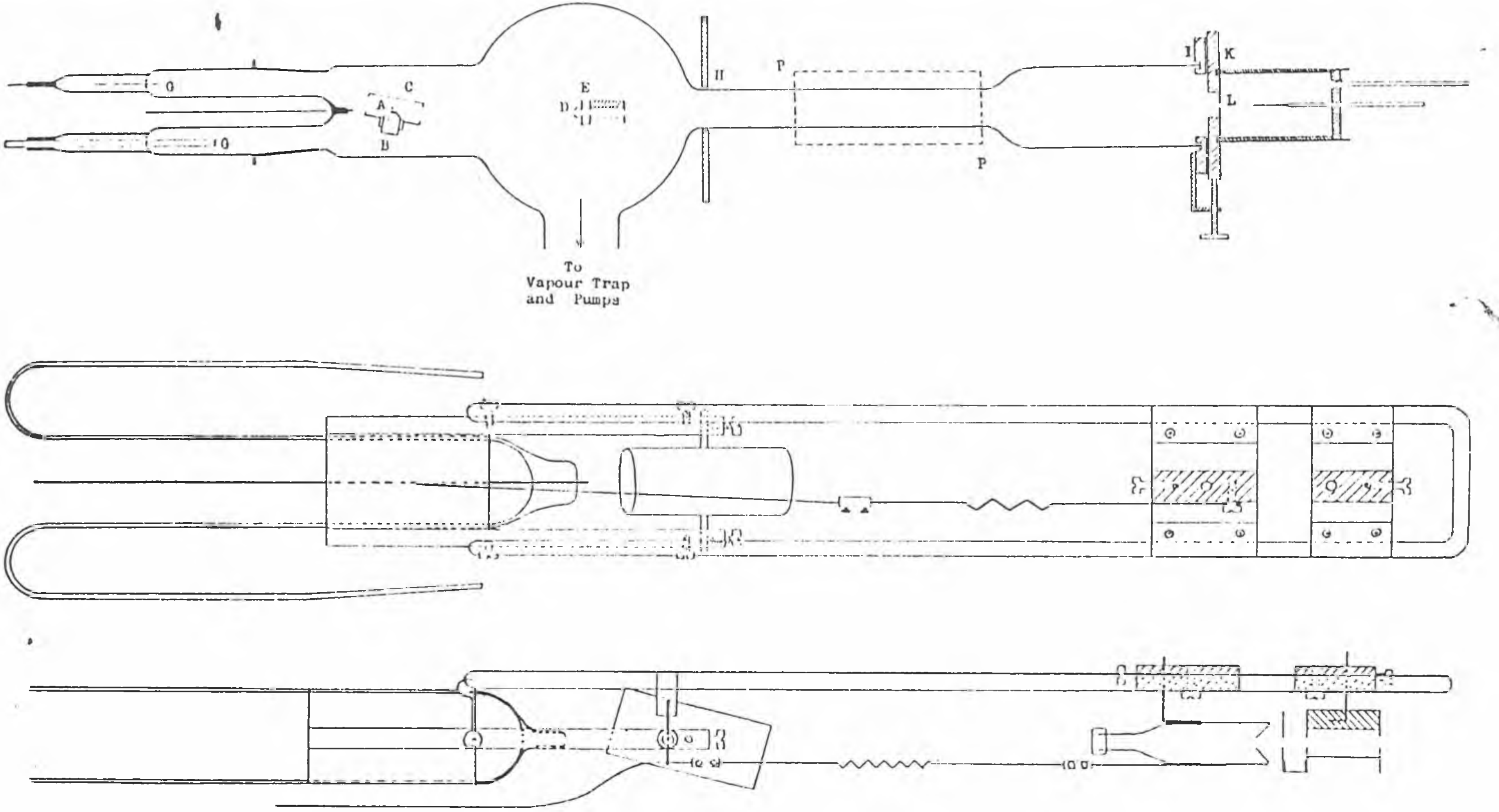

Fig. 4.8 Apparatus used by Webster and de Bruyne in an attempt to replicate the work of Davis and Barnes. The polonium source is placed on a platinum button at A. D and E are the electrode system. PP denotes the ends of the pole-pieces of a large electromagnet of about 2,000 gauss. The electrical counter is at the extreme right of the apparatus, reached via a slit L.

Source: Webster (1931a), 119.

the polonium source, encased in a brass box with mica windows "since it was found that otherwise the spread of polonium through the apparatus, due to aggregate recoil, etc., might give rise to difficulties."[125] The electrode system consisted of cathode D and anode E, the α-particle beam being defined by a third slit. PP denotes the region of magnetic field. A movable slit system I, K, L, defined the approach to the electrical counting equipment. The geometrical alignments of source and electrodes were adjusted so that the beam of undeflected α-particles was central in the horizontal plane but *below* the axis of the apparatus in the vertical plane. The magnetic deflection with this arrangement was upward, so that "beams of doubly-charged, singly-charged and neutral (if any) particles could then all be obtained, well-separated, with a suitable magnetic field."[126] The apparatus was completed by a high vacuum system giving pressures of 10^{-5} mm when the electrodes were cold.

As to the counting apparatus itself, the latest valve technology was incorporated, thanks to de Bruyne's connections with the electrical industry. In the summer of 1929, one of the (many) occasional visitors to the Cavendish, A.W. Hull of the General Electric Company's Schenectady research laboratories, brought news of a very recent development: the thyratron. Invented by Hull's Schenectady colleague Irving Langmuir, the thyratron was a triode valve containing a small quantity of some inert gas, such as mercury vapour. This unusual feature gave the device characteristics which enabled it to be used as a relay, leading Hull to suggest to interested parties in Cambridge that the device might be used to trigger a mechanical counter.[127] Within days of Hull's proposal, the thyratron had been taken up with enthusiasm at the Cavendish, where it was quickly adapted and brought into use with Wynn-Williams' electrical counting apparatus. Again, the young researchers relied on their home-grown expertise and ability to improvise:[128]

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125 *ibid.*
126 *ibid.*, 122.
127 Hull (1929a, 1929b). For electronics research at General Electric, see Hawkins (1950); Brittain (1980).
128 Wynn-Williams (1957), 54. Wynn-Williams adds the twist to the story: "Unfortunately, before de Bruyne's thyratron could be used on a live experiment, it met with a sad accident a day or two later, when lent to another Cavendish enthusiast."

> While we were still pessimistically wondering how long it would be before we could lay our hands on one of these new thyratrons, a Cavendish enthusiast, N.A. de Bruyne, had opened up an old T.15 transmitting valve, introduced a globule of mercury, evacuated, baked out and sealed off the valve. He proudly presented this to us with the casual remark, "Here's a thyratron for you." So, within a few days of the talk with Hull, we were able to verify that a thyratron really could be used for automatically counting α-particles.

After graduating in Physics in 1927, de Bruyne had spent the long vacation of that year not, like his contemporaries, under Chadwick's tutelage in the Nursery, but at the research laboratories of the British General Electric Company at Wembley, where he learned high-vacuum technique from Norman Campbell (himself an old Cavendish man) and B.S. Gosling.[129] These personal links with G.E.C. proved useful to the Cavendish, much like John Cockcroft's association with the Metropolitan-Vickers Company. Just as M-V supplied electrical engineering and other industrial equipment, General Electric, the British Thomson-Houston Company and other electrical concerns supplied materials to sustain Cavendish investigations into the properties and uses of valves and electrical components.[130]

de Bruyne lost no time in obtaining components for his and Webster's experiment. Using thyratrons presented *gratis* by the General Electric Company and the British Thomson-Houston Company,[131] Webster and de Bruyne developed an arrangement whereby "the surges of current through the counter produced by the entrance of the α-particles were registered by means of an amplifier, thyratron, and counting machine." Webster developed a high-tension supply for the Geiger counter and, following experience gained in his earlier

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129 de Bruyne (1984), 85; Clayton and Algar (1989).

130 Hartcup and Allibone (1984), esp. 26-57; Niblett (1980), 85-153; Allibone (1984a). For background to the Metropolitan-Vickers Co., consult Dummelow (1949). Fleming and Pearce (1922) nicely capture the ethos of research at M-V. The link between the Cavendish and General Electric is less well-known than that with Metropolitan-Vickers. In 1926 the International Education Board's Augustus Trowbridge hinted at a rather close connection between Rutherford (as Director of the Cavendish Laboratory) and G.E.C., noting that Rutherford hoped to raise funds for research from the 'Industry and Research Board' (D.S.I.R.?), otherwise "he might have to pass the hat in America - thought the General Electric people would chip in, as they evidently have done in the past in some of R.'s plans." See Trowbridge, "Visit to Cambridge, England, 17 April 1926. Re: Cavendish Laboratory," International Education Board Archives, Box 29 File 410, Rockefeller Archives Centre, New York.; "Gifts to the Cavendish Laboratory," *Cambridge University Reporter* , 23 October 1923, 142. In 1928 the Vickers Company sold its shares in Metropolitan-Vickers to the General Electric Company, uniting the two enterprises 'financially though never spiritually' (Allibone (1984a), 162) under the aegis of Associated Electrical Industries Ltd.: see Jones and Marriott (1970). On the British Thomson-Houston Co. and its largesse, see Price-Hughes (comp.)(1946); de Bruyne (1984).

131 de Bruyne and Webster (1931), 115; de Bruyne (1984), 85-88.

investigations, "[t]he whole electrical apparatus was carefully screened to avoid spurious registrations due to external electrical disturbances."[132] Finally, the element of *bricolage* surfaced again: a modified telephone call meter served as the counting machine, the time occupied in registering an impulse being brought down to less than a tenth of a second.[133] With this sophisticated apparatus in place, Webster and de Bruyne set out to repeat Davis and Barnes' work.

It was less easy than they had supposed. Counting, even with the new mechanical arrangement which had been designed specifically to eliminate the human observer from the counting process, was fraught with difficulties. The sheer numbers of particles, so often the cause of troubles in scintillation counting, also caused particular problems for the automatic electrical aparatus. When the particles arrived at a rate of over 200 per minute, for example, "the recording apparatus apparently missed an appreciable fraction of them."[134] Acutely aware of such shortcomings with the electrical method, Webster dedicated a considerable portion of his report on the work to "the various tests applied to see whether [the apparatus] was functioning properly."[135] While the experiments seemed to show no evidence of periodic capture of the type observed by Barnes, an element of doubt persisted as to whether this was due to the non-existence of the effect or the inability of the electrical apparatus to reveal it.[136] Webster and de Bruyne had reached an impasse. Not knowing whether the electrical apparatus was at fault, or whether it was the fault of himself and Webster, whether their polonium source was insufficient for the task or whether Davis and Barnes had genuinely made some mistake, de Bruyne wrote directly to Barnes seeking clarification. He received "a very nice answer but not an effective one in clearing up the doubtful points."[137] It was a difficult situation. de Bruyne soon became so disillusioned with the work of replication and with the general ethos at the Cavendish, in fact, that he "ceased to take an

132 Webster (1931a), 122.
133 Webster (1931a, 1931b); de Bruyne and Webster (1931).
134 Webster (1931a), 122.
135 Webster, *Report 2*, 53.
136 For comparable studies of the 'regress' inherent in such failed attempts to replicate experiments, see Collins (1975); Collins (1985), 29-111; Pinch (1986); Schaffer (1989).
137 Chadwick to Feather, 22 April 1930, NFP.

active interest in the experiment" in March 1930 and left the laboratory altogether soon after that.[138]

Chadwick stepped in. Hoping to break the deadlock, he wrote to Norman Feather, spending the academic year 1929-30 at Johns Hopkins University in Baltimore at the invitation of R.W. Wood, who wanted to introduce radioactivity research to the Department of Physics.[139] Apart from Cavendish gossip and arrangements for Feather's impending return to Cambridge, Chadwick's letter contained what he described as a "sting in the tail." First, though, he quickly sketched the background:[140]

> You have, I suppose, read the papers of Davis and Barnes, and of Barnes in the Phys. Rev. on the capture of electrons by α particles. The whole affair is most mysterious and to me incomprehensible. I cannot understand even the experimental arrangement from Barnes' description. de Bruyne and Webster have been trying the experiment but without result. They are going to have one big last try and then close down. While I have the utmost difficulty in believing Barnes' results I still cannot believe that so much detailed experiment can be founded on error.

Then came a "sting in the tail," a plea:[141]

> Could you in the course of your travels, or as you return, or in any possible way, have a look at [Barnes'] arrangement and see if he knows what he is doing. One or two statements in his paper make me very suspicious but even so I can't see how he can be entirely wrong. Has he got a properly defined beam of α particles? Why does an effect take place when he merely accelerates the electrons without deflecting his beam? etc. etc. You will have thought of the crucial points already.

Chadwick ended, characteristically, with an apology. Just over two years previously, he himself had been involved in a similar episode of investigation. Doubtless with that at the back of his mind, he confessed to Feather: "This is a nasty job to give you but it is really most important to know whether the experiments are right or wrong."[142] Chadwick's

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138 de Bruyne (1984), 88.
139 See Feather, "Reminiscences of the Cavendish Laboratory, 1926-1937," unpublished typescript in FEAT 45/7, NFP, 4-5; Feather (1962), 141; Feather (1974); Wheeler (1979), 224; Cochran and Devons (1981), 257, 269.
140 Chadwick to Feather, 22 April 1930, NFP.
141 *ibid.*
142 *ibid.*

comments betoken the state of alarm at the Cavendish over the inability of both scintillation *and* electrical counting methods to yield secure, consistent and reliable results. Even with mechanical counting equipment which ostensibly eliminated the human observer completely, the decision as to whether Barnes and Davis' experiments were correct had to rely, in the end, on a human judgement as to the Columbia researchers' experimental competence.

Feather did indeed visit Columbia, though we do not know when. What we may be sure about, however, is that he was pre-empted, for events were unfolding at a rapid pace.

4.5 Industrial Values: Irving Langmuir and Electrical Counting Methods

Just a few days before Chadwick's plea to Feather, Davis himself had given a one and a half hour colloquium at the Schenectady research laboratory of the General Electric Company in which he presented the most recent experimental results from Columbia. In the discussion afterwards, Davis was "very enthusiastic" about his results, and generated a certain amount of interest among G.E. staff, particularly Irving Langmuir and Willis R. Whitney.[143] Langmuir, at least, was already *au fait* with the Columbia electron capture work. He had recently co-authored with K.T. Compton a long survey of fundamental processes in gas discharges, in which pointed reference was made to the "most interesting and surprising results" of Davis and Barnes. Summarising the Columbia experiments briefly, the two authors were quick to point, however, that there was "as yet no satisfactory explanation of these experiments."[144] But the theoretical confusion was the least of their concerns. That the experiment "should have yielded any result at all" was surprising, since electron capture required that the electron approach within a distance of the order of 10^{-5} cm of the α-particle - "a distance much larger than the kinetic theory atomic radius."[145] The recombination cross-section implied by the results was therefore about a million times larger than that suggested

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[143] Langmuir laboratory notebook NB 2038, 21 April 1930, ILP. For industrial research at G.E., see Hawkins (1950); Wise (1980, 1985); Reich (1983); D.E. Nye (1985).
[144] Compton and Langmuir (1930), 203. This review appeared in the April number of *Reviews of Modern Physics* and was thus already in print when Davis gave his Schenectady talk.
[145] Compton and Langmuir (1930), 204.

by the atomic theory, a point which had not escaped the notice of theoretical physicists and which was the source of much of their puzzlement.

The colloquium gave Davis' audience a chance to clarify some of the ambiguous elements of the Columbia work. Davis was questioned about how the whole spectrum could be examined experimentally, since making counts at 0.1V steps from 330 to 900V would be a major effort. It transpired that Davis and Barnes had found by some preliminary work that the peak voltages matched the Bohr orbit velocities, and then had a fairly precise idea of where to look for the other peaks. They "explored around" in the region of the expected peak, with the result that "they got [the peaks] with extraordinary precision - so high, in fact, that they were sure they'd be able to check the Rydberg constant more accurately than it can be done by studying the hydrogen spectrum, which is something like one in 10^8."[146] Among the other unpublished results announced in Davis' talk was the fact that the percentage of capture in the experiments was always about 80%, which puzzled several of his listeners. When questioned about the dependence of capture on current density, Davis claimed that there was no dependence. His questioners persisted:[147]

> We asked: "How much could you change the temperature of the cathode?"
> "Well," he said, "that's the queer thing about it. You can change it all the way down to room temperature."
> "Well," [Langmuir] said, "then you wouldn't have any electrons."
> "Oh yes," he said. "If you check the Richardson equation and calculate, you'll find that you get electrons even at room temperature and those are the ones that are captured."

These results were in many ways "still more remarkable than any that he had published."[148] As Langmuir reported it to Bohr:[149]

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146 Langmuir (1989)[1953], talk at G.E.C. Knolls Atomic Power Laboratory, 18 December 1953, trancribed and edited by R.N. Hall as "Pathological Science," *Physics Today* **42** (1989), 36-47. During his career Langmuir developed an interest in 'pathological science.' In this talk he outlined some of the examples he had come across, including N-rays, mitogenetic rays, e.s.p. and flying saucers (it is perhaps no coincidence that *Physics Today* published this article at the height of the debate over cold fusion). Where I have been unable to cite strictly contemporaneous records I have used this source with caution.
147 Langmuir (1989)[1953], 39.
148 *ibid.*
149 Langmuir to Bohr, 2 June 1930, BSC, emphasis in original.

> it was found that 60 to 80 per cent of the alpha particles captured electrons even when the hot cathode which furnished the electrons was cold; the current by direct observation being then less than 10^{-14} amperes. This would mean that only during $^{1}/_{1000}$ of the time was there even one electron in the tube. Yet the voltage which determined the velocities of those electrons had to have a fixed value within 0.02 volts in order that capture might occur.

Davis had a ready reply to this point, however. He first agreed that Langmuir's objection had "seemed like a great difficulty." But, he went on, "it isn't so bad because now we know that the electrons are waves. So the electron doesn't have to be there at all in order to combine with something. Only the waves have to be there and they can be of low intensity and the quantum theory causes all the electrons to pile in just at the right place where they are needed."[150] Even in the face of such a creative use of the idea of wave-particle duality, Davis' Schenectady audience, perhaps understandably, remained sceptical.

At length, Willis Whitney, director of the research laboratory, "suggested that our lab offer to cooperate with Davis to utilize Geiger counter to count α-particles"[151] Most of the work of the Schenectady research laboratories concerned gas discharges, vacuum tubes and, increasingly, electronics. A.W. Hull, who had introduced thyratrons to the Cavendish in 1929, was a member of the laboratory's staff. Langmuir himself was one of the foremost authorities on gas discharges, lamps and valve technology. With this collected expertise at its command, Whitney's department maintained much the same kind of *quid pro quo* relationship with academic physicists as did A.P.M. Fleming's Metropolitan-Vickers research department at Manchester. Materials and information would flow out in return for news of the latest developments in academic research. As we have seen, this patronage even extended to the Cavendish Laboratory, where G.E.'s largesse (along with that of Metropolitan-Vickers and the British Thomson-Houston Company) had done much to improve working conditions in the 1920s.

As in England, this mutually beneficial relationship between G.E. and the academy was maintained by personal visits, of which Davis' to Schenectady had been an example.[152]

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150 Langmuir (1989)[1953], 39.

151 Langmuir laboratory notebook NB 2038, 21 April 1930, ILP.

152 On the links between G.E. and academic researchers, see, for example, Pegram to A.W. Hull, 24

Davis' work at Columbia seemed an ideal candidate for G.E.'s benificent interest, for it touched on many of the laboratory's key concerns. Clarence W. Hewlett was then working at G.E. on the development of Geiger counters in the construction and operation of which he had already accumulated much experience (fig. 4.9).[153] The suggestion that Davis be given an electrical counter, "maybe at a cost of several thousand dollars or so for the whole equipment, so that he could get better data"[154] was, nevertheless, a remarkable proposition, given the difficulties which still plagued the instrument and the continuing problems of calibration. Mindful of this, Langmuir "urged checking up on Davis and Barnes extraordinary results" before G.E. committed themselves to help, a suggestion with which Whitney agreed.[155] The cost of G.E.'s patronage was to be an on-site inspection by Langmuir himself.

4.6 The Anatomy of A Visit

So it was that on Wednesday 23 April 1930, Langmuir and Hewlett travelled to New York to visit Davis' laboratory at Columbia University. They received a warm welcome; Davis was "very proud" to show them the laboratory, apparatus and results. The visitors were ushered into a room with a long table at which Barnes sat. The room itself was dark "except for the dial of a clock."[156] There was also another table "where an assistant named Hull sat looking at a big scale voltmeter ...[which] ... had a scale that went from one to a thousand volts." A year or two younger than Barnes, Harvard L. Hull was also a Ph.D. student of Davis, though Langmuir took him for an assistant. Hull claimed to be able to read hundredths of a volt from the scale, and thought "he might be able to do a little better than that." Here, then, was the origin of the experiments' extraordinary accuracy. But Langmuir directed his attention elsewhere. Having examined the apparatus, finding it "very well designed and

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December 1931, 2 March 1932, Box 23, GBPP.

153 "Engineers of G.E. Probing Mysteries of Cosmic Rays," *Schenectady Gazette*, 16 June 1932, cutting in Box 101, ILP.

154 Langmuir (1989)[1953], 39.

155 Langmuir laboratory notebook NB 2038, 21 April 1930, ILP.

156 Langmuir (1989)[1953], 39.

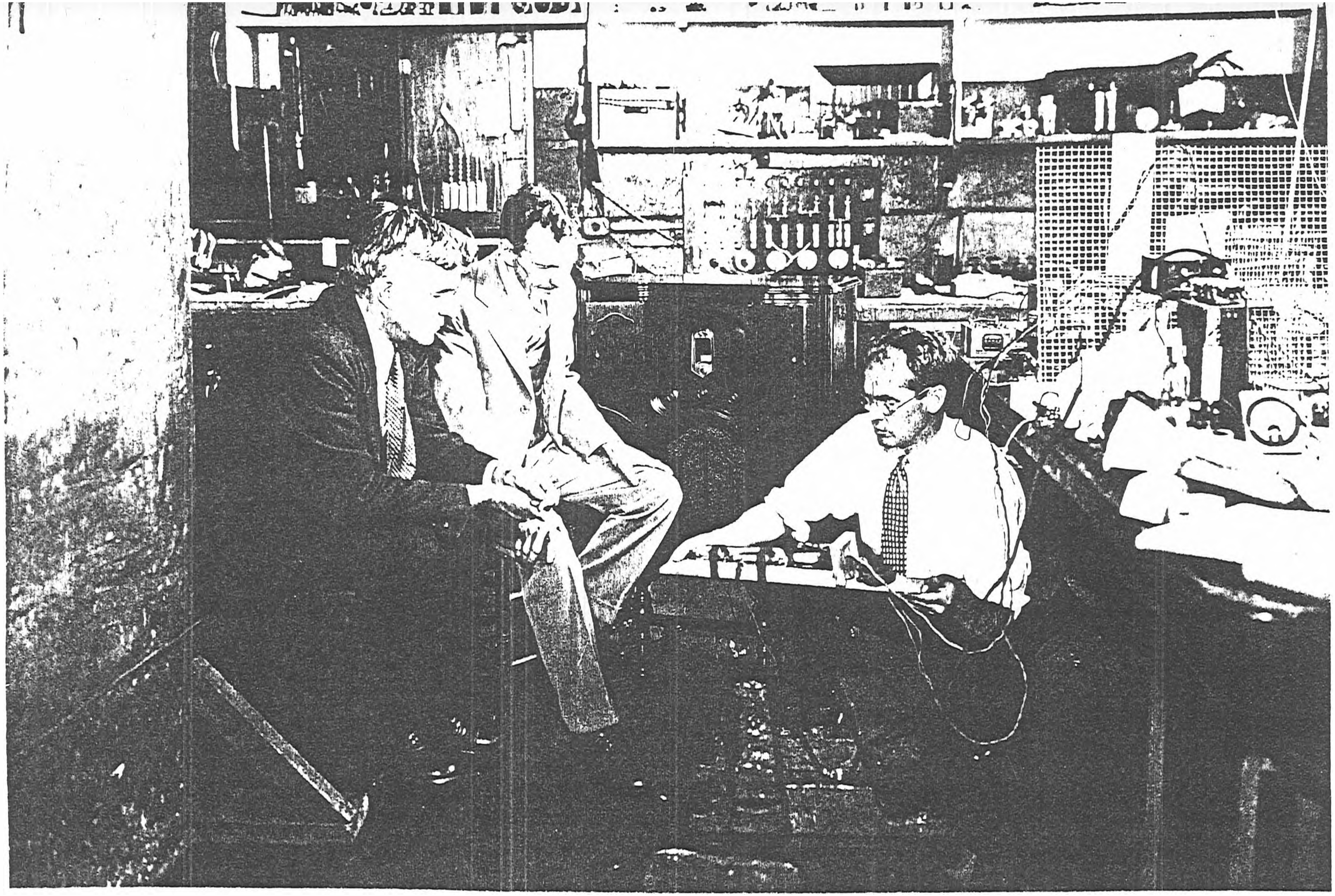

Fig. 4.9 Irving Langmuir (left) and two others with electrical counting tube at G.E. Schenectady research laboratories, 17 June 1932.

Source: Box 101, ILP.

constructed, but ... in no sense a piece of precision apparatus," he "asked to observe the scintillations."[157]

In accordance with the usual practice, the group accustomed their eyes to darkness by sitting in the darkened room for half an hour before any counting took place. Then the testing began:[158]

> The optical system seemed very good, and most of the scintillations appeared very bright. There were, however, a few fainter ones among them. With the microscope set in a position to count neutral particles and without any magnetic field, I counted, in successive two minute intervals, 71, 70 and 87 particles, and then, with the magnetic field applied, counted 17 at a similar interval. I felt that the count of 17 was really reliable, but that I probably missed some when as many as 30 to 40 particles appeared per minute.

Clearly, the operation was not as simple or straightforward as Langmuir might have thought. In successive two-minute intervals, Hewlett then counted 90 and 128 with no magnetic field, and 30 with the field. Whereas Langmuir had counted only particles arriving within the field of the microscope, however, Hewlett counted as particles "all flashes resulting from α-particles striking beyond the edge of the field of view which produced visible effects."[159] Barnes completed the round by counting 256 with no field, and an average of 25 with the field on.

For five hours, Langmuir put Barnes through his paces at scintillation counting. Fifty separate runs were carried out (see table). The plus and minus signs under 'K' signify whether (+) or not (-) Barnes knew the applied voltage during a given run in advance of his counting. Barnes was asked to find a typical peak. The voltages were set by Hull in consultation with Barnes, so that "Barnes knew what the voltages were to be before they were applied, and, in many cases, himself chose the voltage which was to be used." The voltages for runs 1-4 were chosen by Barnes. After two low counts at 325.01 and 325.02 V,

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157 Langmuir to Davis, 8 May 1930, copy in BSC. This 22-page letter, based on Langmuir's own contemporaneous notes, describes his visit and findings in some detail. For reasons which will become apparent, Langmuir later sent a copy of the letter to Bohr in Copenhagen. All the following quotations are derived from this source.
158 Langmuir to Davis, 8 May 1930, copy in BSC.
159 *ibid.*

Run No.	Applied voltage	K	Scintillation Count	Stop Watch Time
1	325.01	+	52	
2	325.02	+	48	
3	325.02	+	76	
4	325.015	+	107	
5	325.10	-	96	
6	325.015	+	54	
7	0	+	25	
8	325.05	+	60	
9	325.10	+	55	
10	325.20	-	78	
11	326.00	+	37	1:50
12	325.015	+	64	1:35
13	325.025	+	76	1:45
14	325.03	+	81	1:45
15	325.03	+	101	1:47
16	325.03	+	101	1:44
17	0		lunch	1/2 hr
18	0	+	48	
19	0	+	57	
20	0	+	58	
21	325.03	+	110	
22	325.03	+	97	
23	325.03	+	25	
24	0	+	16	
25	325.03	+	25	
26	0	+	22	
27	325.03	-	55	
28	325.03	-	94	
29	320.00	-	45	
30	325.05	-	102	
31	325.03	-	47	1:55
32	325.03	-	10	"out of focus"
33	325.03	-	58	1:47
34	320.00	-	--	"tired"
35	320.00	-	75	
36	320.00	-	91	
37	325.03	-	60	1:52
38	325.03	-	61	1:52
39	894.500		81	
40	894.475		69	
41	894.500		88	
42	894.500			
43	894.400 drift		85	
44	955.75		75	
45	955.70		109	
46	955.70		104	
47	955.70		109	
48	955.00		95	
49	0		93	
50	0		69	

Table 4.2 Results of scintillation counting trials during Langmuir's visit to Columbia University, 23 April 1930. '+' or '-' under K signifies whether (+) or not (-) Barnes knew the value of the applied voltage (Source: Langmuir to Davis, 8 May 1930, copy in BSC).

"it was agreed that the [known] peak was probably between the two points."[160] With the intermediate voltage of 325.015, 107 neutral particles were obtained, and "Barnes and Hull seemed satisfied that this count ... corresponded to a peak."

During this process, Hull had been holding the voltage constant by "continuous adjustment of the potentiometer." Langmuir decided to intervene. He asked Hull, "in a whisper, to change the voltage to 325.10." Hull was surprised. Langmuir "could see immediately that to him this was a surprisingly large alteration to make ... His reaction indicated to me that it was not a common procedure to make such extreme variations as 0.08 volts." Langmuir "repeated that I wished purposely to make such a variation and asked him to set the voltage at the figure given." Barnes, unaware of the new voltage, counted 96. Hull "seemed much surprised at this value and there was some discussion of it. It was concluded that the peak had probably shifted somewhat from the last time that observations were taken a day or two previously, such a shift being explainable on the basis of possible change in contact potential." Langmuir's suspicions were aroused.

The voltages for the next four runs were chosen by Barnes and Hull "without interference" from Langmuir. Inconsistencies with the earlier counts became apparent, though Langmuir did not lay too much emphasis on these. At run 10, Langmuir whispered to Hull for a second time, "asking him to set the voltage at 325.20, so that Barnes did not know what the voltage was to be." The count was 78. Barnes was told of this result, after which Langmuir "suggested that we make a still more violent change in voltage and go to 326. Barnes, knowing of this violent change, obtained a count of 37." Just as Chadwick had done in Vienna, Langmuir now took charge completely. He "asked Barnes to satisfy himself as to the exact position of the peak before we made any further experiments." So the voltages for runs 12-16 were chosen by Barnes and Hull. Langmuir found their procedure "extremely significant":

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160 *ibid*

> In their discussion of the high values obtained at 325.10, they had worked themselves into a conviction that the peak which they originally though was at 325.015 must actually lie somewhat higher than this ... They went back to [325.015] in observation no. 12 and found only 64, and were more firmly convinced than ever that the peak had shifted to higher voltages. They did not notice, however, that this shift must have taken place since observation No. 4 was made and not during the preceding few days. They then took the slightly higher voltage of No. 13 and obtained a higher count, and then took 0.005 volts higher, coming to the values given in observation 14 and got a still higher count of 81. Two further checks on this gave two values of 101. By this time Barnes and Hull, judging by their conversation, were thoroughly satisfied that they had located the peak at 325.03.

Barnes had reached this conviction, however, "without ever having tried any higher voltage except the ones I [Langmuir] had suggested ... which gave results hardly consistent with this conviction."

The group broke for lunch, "which was eaten in the darkened room." Afterwards, Langmuir resumed control. He "asked that some readings be taken showing the difference in count with voltage on and with voltage off" (runs 18-22). These data were "of interest in showing very consistent results, namely, values of about 54 with zero volts, and 103 on the "peak"." Langmuir demanded an explanation of why the zero count had risen from 23 to 55. Barnes suggested that cooling of the filament explained the change. Pressed by Langmuir for a fuller explanation, Barnes speculated that "turning off the filament current would make a still further change in the zero count, but that the zero count was always obtained every time the filament temperature was changed so that it would never affect the final results." To Langmuir, an authority on such matters, "this attitude seemed extraordinary":

> He [Barnes] clearly had never made any investigation of the effect of the filament temperature on the zero count. he did not even <u>know</u> that the nickel parts did warp with change of filament temperature. This was merely one of the many illustrations he gave me that he possessed an almost unlimited supply of "explanations" of any peculiar results that might be obtained. In other words, he has established the habit of disregarding any changes in his count due to any causes other than those which he wishes to consider important.

As the tension mounted, Hewlett took some counts with the voltage on and the voltage off (runs 23-26). He found "no appreciable difference between the voltage on and the voltage off." Moreover, "his count agreed approximately with his original count and was very different from the values 50 to 110 just obtained by Barnes," a fact rejected by Barnes as irrelevant. Scenting blood, Langmuir played what he later called "a dirty trick": "I ... made out on a piece of paper a schedule of voltages which I originally made out in the following form: V,V,0,V,V,V,V,0,0,0,V,V, and asked Hull to use the voltage 325.03 wherever I had indicated a V and use zero volts where I had indicated a zero. The object of this test was to make a run exactly like those which had been used in observations 18 to 22, except that now Barnes was not to know the order in which the voltages were applied." Before the series started, however, Langmuir realized "that a test made in this way would have no significance, for when Hull applied a definite voltage he was very busy regulating it by means of a potentiometer and was leaning forward over the potentiometer, whereas whenever the voltage was thrown off, he merely opened a switch and leaned back in his chair having nothing to do within the two minute interval." The geography of the darkened room and the dispositions, even the precise *gestures* of the actors became crucially important for the execution of Langmuir's test - or, rather, Langmuir was able to foreground these elements to *make* them relevant to the outcome of the experiment: "Barnes was sitting only about 4 feet away and was facing Hull, and there was plenty of light in the room to see whether Hull was adjusting the voltage or not" (see plan, fig. 4.10). Langmuir therefore asked Hull to use 320 volts (at which there should have been no capture) instead of zero. During run 29, however, he "noticed that Hull was leaning back in his chair doing nothing, and he whispered to me that of course this voltage [320] was so far from the peak that there was no use in regulating the voltage." Langmuir "whispered to him to "act just as though he were regulating the voltage." From that time on," according to Langmuir, Hull "played his part well."[161]

It was a charade. Barnes, completely disoriented by Hull's play-acting, produced a series of wildly inconsistent counts (runs 27-38). Knowing that he was performing badly, he

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[161] This highlights the point made in the previous chapter, where I suggested that during his visit to Vienna, Chadwick was being shown a performance which, while it might have been enacted in good faith by the Viennese, was *to him* a sign of the inauthenticity and inappropriateness of the Vienna counting protocols. For comparable accounts of the constitutive role of gesture in 'authentic' performances, see Goffmann (1971)[1959]; Sudnow (1978); Connerton (1989); Bremmer and Roodenburg (eds.)(1991); Schaffer (1992); Sibum (1992).

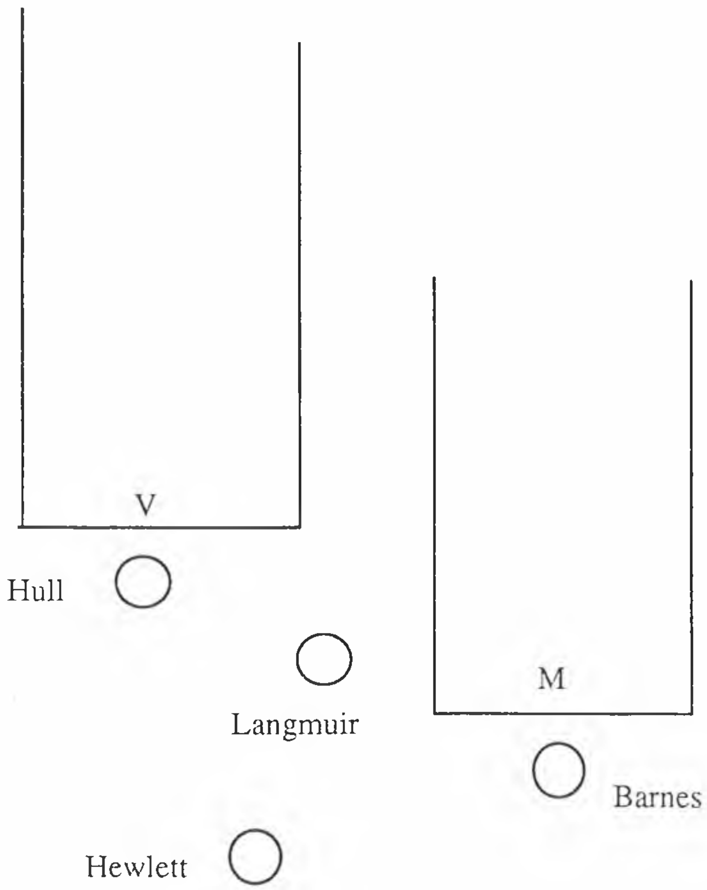

Fig. 4.10 Langmuir-Hewlett visit to Columbia, 23 April 1930.

Disposition of personnel in dark room during counting trials

(Langmuir, "Lecture Notes, 1948-56," Box 91, ILP) explained his own poor performance by claiming that the microscope was out of focus (run 32) and that his eyes were tired (run 34). By the end of the series, however, Langmuir had seen enough, and thought it "useless to continue the experiments any further." He "told Barnes that it was obvious that he was not on a peak because his counts were fluctuating over wide values regardless of whether the voltage was at one position or another." Over Barnes' protestations and excuses, Langmuir, now transformed from sceptical inquirer to judge-and-jury, left Hewlett to continue experiments with Barnes and Hull (runs 39-50), and went to see Davis to pronounce sentence. What had started as a visit motivated by curiosity and scepticism had, in the space of a few hours, apparently turned into the complete destruction of the basis for all of Davis and Barnes' claims.

Presented with Langmuir's account of the day's proceedings, Davis was "simply dumbfounded." As Langmuir put it, Davis was "so sure from the whole history of the thing that it was utterly impossible that there never had been any measurements at all that he just wouldn't believe it." ""It absolutely can't be," he said. "Look at the way we found those peaks before we knew anything about the Bohr theory. We took those values and calculated them and they checked exactly."[162] With his confidence in Barnes unbroken, Davis read a paper on electron capture a meeting of the American Physical Society on 24 April, making public the results he had presented at Schenectady. The new work had the same effect on his A.P.S. audience as it had had at G.E., however. Joseph Mayer conveyed the general feeling of scepticism to Gilbert Lewis when he reported facetiously that Davis had created "the greatest stir ... He gets combination of electrons and alpha particles now without having any electrons there ... Maria [Goeppert Mayer] wrote to Born about it and pointed out that α particles are still necessary for the experiments."[163] Despite Davis' robust defence of them,[164] the Columbia experiments were becoming a standing joke.

Anxious to get to the root of the matter, Langmuir pressed home his own criticisms. He wrote Davis a comprehensive, 22-page letter explaining in detail the control experiments he

162 Langmuir (1989)[1953], 40.
163 Mayer to Lewis, 19 May 1930, GNLP.
164 Davis to Langmuir, 5 May 1930, Box 3, ILP; Langmuir to Bohr, 2 June 1930, Reel E4/23, BSC.

had performed and why the results were so damaging to Barnes' position. He drew three major conclusions from his day at Columbia:[165]

> (1) There is unmistakeable evidence that Barnes' counts are determined wholly by his mental attitude and not by experimental conditions. The changes in the count brought about merely by making him think that he is at a peak are fully as great as those that can be produced by actually changing the experimental conditions.
>
> (2) When Barnes thinks he is at a peak his counts are from 4 to 5 times greater than those obtained by Hewlett or myself, whereas when Barnes thinks he is not at a peak, the values range from 1 to 2 $^1/_2$ times those obtained by us.
>
> (3) Barnes seems to have developed a remarkable ability in explaining away the results that are not in accord with his perconceived ideas as to what the results should be. He makes no attempt to check up the reliability of these explanations.

Langmuir had done some homework. On returning to Schenectady, he had consulted an article by Geiger in the *Handbuch der Physik* which outlined the 'proper' protocols for mid-1920s scintillation counting practice. It recommended that the number of particles to be counted should not exceed 40 per minute "and that the results are not at all accurate with more than 50 per minute," that for accurate work two observers should count simultaneously, and so on - all the precautions that had been made explicit during the Cambridge-Vienna controversy.[166] More significantly, perhaps, Langmuir also cited the discussion of scintillation counting in Pettersson and Kirsch's *Atomzertrümmerung*, drawing Davis' attention to the particularly elaborate protocols imposed by the Viennese - multiple counters, limited counting times, frequent rests and so on. In the light of the complex organisation of scintillation counting experiments in European laboratories, then, Barnes' solo efforts, in which he counted up to 150 particles a minute, looked decidedly suspect.

Barnes undoubtedly took great pride in his abilities as a counter. This pride was, in a sense, his downfall. He apparently found it "no effort at all to count," and even claimed to be able to "carry on a conversation with someone in the room while counting" without affecting

[165] Langmuir to Davis, 8 May 1930, BSC. On the attribution of self-deception, see Bok (1982a), 64-66.
[166] Geiger (1927), 143-145.

the accuracy of his results. He had boasted to Langmuir about Marie Curie's visit the previous November, during which she had said that her experience was that no one could count in a reliable way for more than two hours a day. Barnes knew that "this did not apply to him, for he could count 6 hours a day without difficulty."[167] Langmuir's 'exposé' had done no more than show what was already well-known in Cambridge and Vienna: that the results produced by the scintillation technique depended on the protocols used in deploying it. Langmuir had demonstrated not so much that Barnes was wrong, as that Barnes was counting scintillations *in an inappropriate way*, just as Chadwick had shown in Vienna that Pettersson's counters could not produce the results he, Chadwick, thought they should be producing.

Norman Feather found much the same as Langmuir when he visited Columbia on Chadwick's behalf. Years later, he recalled (still evidently with some amusement) that when he went to Columbia at Chadwick's request, he found that Barnes had been counting scintillations diligently for some eight hours a day - a far cry from the strict conditions which were imposed at the Cavendish, where counting was never carried on for more than about an hour a day. Unlike those at the Cavendish who produced quantitative data through carefully controlled and disciplined counting procedures, however, Barnes relied on what one might call 'simple ennumeration.' He considered it a major virtue of his work, for example, that his results were "based upon the counting of over 700,000 alpha-particle scintillations."[168] Unfortunately, simple ennumeration was not enough. In view of all this, Langmuir rubbished Barnes' claims as being due to "psychological errors." In fact, the "rather weird nature" of Barnes' results was "about what one would normally expect from errors of this kind."[169] Langmuir's criticisms cut deep. When the abstracts of the papers given at the Washington A.P.S. meeting - 114 of them - were published in the 1 June number of the *Physical Review*, that of Davis, Barnes and Hull was "witheld for revision by the authors" - the only one of 114 abstracts not published.[170] While Barnes undertook a new series of

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167 Langmuir to Davis, 8 May 1930, BSC.
168 Barnes (1930), 228.
169 Langmuir to Davis, 8 May 1930, BSC.
170 See *Physical Review* 35 (1930), 1415-1446, 1433.

confirmatory experiments, Davis took six months' leave of absence to travel around Europe and the Far East.[171]

4.7 Denouement: A Matter of 'Proof'?

Five weeks after his visit to Columbia, Langmuir reported his findings to Bohr, sending him a copy of the 22-page letter he had written to Davis. He had concluded, he said, "that all of the "results of Davis and Barnes are due to psychological errors made by Barnes." Davis however, was "not yet convinced of this fact and will probably not publish a correction for several months." In view of this fact, Langmuir thought it "only fair to the men who are attempting to explain the experiments of Davis and Barnes that they should know how strong the probability is that there is no sound experimental basis for the effect Davis and Barnes claim to have found."[172] Bohr was the natural person to inform; not only had he told Langmuir's colleague A.W. Hull of his "firm conviction that there must be some error in the experiments of Davis and Barnes," but, as Langmuir pointed out, he also knew "most of the men in Europe who have been seriously puzzled in attempting to reconcile the wave mechanics with these experiments."[173] These efforts could now be abandoned. Asking for Bohr's discretion in the use of the Davis letter, Langmuir suggested that he "merely use it when necessary in order to prevent either experimental repetition of the experiments or theoretical work in attempting to account for it,"[174] a view which echoed Rutherford's policy of circumspection and quiet diplomacy.

In contrast to his insistence on public propriety to save Davis embarrassment, Langmuir's attitude to the fate of the hapless Barnes was less sympathetic. Given the probability that Barnes' results were based wholly on error, he railed at Davis privately, it was "obvious that he should not receive his Doctor's degree which is based so largely on this work." More to the point, Langmuir was, "as a Columbia alumnus ... against any man receiving a degree

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171 Webb (1960), 77.
172 Langmuir to Bohr, 2 June 1930, BSC.
173 *ibid.*
174 *ibid.*

with such evidence as now exists of the unreliability of his work."[175] Moreover, in fairness to "the physicists of the world" Davis should refrain from publishing a third paper on electron capture until he could *prove* that the peaks existed - though what would constitute such a proof was now far from clear.

Bohr had more sympathy for Barnes. "[F]rom personal experience," he noted mysteriously, "everyone will understand that all observations have their psychologic aspect, and that especially the counting of scintillations, which involves so great a strain of the eye, is very apt to give rise to illusions."[176] What was needed now was for the Columbia workers to publish "a proper account of the real state of affairs," thereby setting the record straight, and removing any possibility of further confusion. At the same time, he concurred with Langmuir that "it is not even worth while at present to repeat the experiments." He felt sure that Langmuir's views would "be felt as a great relie[f] to many physicists, who have found it impossible to bring the results of these experiments into line with what is considered at present as a reliable foundation of atomic theory."[177] Bohr had just returned from a visit to England, where he knew that workers at the Cavendish and in Manchester had become extremely concerned about the Columbia work. He immediately forwarded Langmuir's letter to Chadwick in Cambridge, asking him to pass it on to Hartree at Manchester. Discretion would be assured, said Bohr, and he would not use the letter again "unless I feel that information about your views may be essential in saving time and labour of some other physicists."[178]

Despite Bohr's assurances about discretion, however, the news circulated quickly among interested parties. At the summer gathering of theoretical physicists at Ann Arbor in July 1930, at least two different sources recounted the story of Langmuir's visit to Columbia.[179] Atomic theorists heaved a collective sigh of relief. The Davis-Barnes experiment had been a cause for concern for precisely the same reason that it had worried Chadwick and Webster at

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175 Langmuir to Davis, 8 May 1930, BSC.
176 *ibid.*
177 Bohr to Langmuir, 3 August 1930, BSC.
178 Bohr to Langmuir, 17 June 1930, BSC.
179 Mayer to Lewis, 15 July 1930, GNLP.

the Cavendish Laboratory: the theoreticians needed to know whether the experiments were trustworthy or not in order to know whether or not their mathematical models were correct or needed further adjustment, in just the same way that Webster needed the same information in order to know whether his electrical counting equipment was at fault. With the discrediting of Davis and Barnes' work, wave mechanics would live to fight another day.

In Cambridge, meanwhile, Feather's return with news of the breakdown of Davis and Barnes' claims, and Chadwick's receipt of a copy of Langmuir's letter via Bohr eased the situation somewhat for Webster. Over the summer, Webster laboured to complete his final report to the Commissioners of the 1851 Exhibition. As we have seen, he had been unable to publish the results of his first researches on the penetrating radiations excited in the light elements, preferring instead to "defer publication until more consistent results were obtained" - a decision, he noted ruefully, "which has proved rather unfortunate since an account of similar work has recently been published by Bothe and Becker."[180] Now, a year later, his second piece of research was also sacrificed on the altar of propriety. He had to settle for telling the Commissioners that "[n]o complete account of this experiment has been, or will probably be published, on account of certain considerations of a somewhat confidential nature concerning the experiments of Davis and Barnes."[181] Nevertheless, in an attempt to salvage some credit for his labours, Webster soon afterwards submitted a short note to *Nature* "with the approval of Sir Ernest Rutherford."[182] The letter, brief but damning, appeared on 6 September 1930, by which time the Davis-Barnes experiments were already a busted flush, at least to those 'in the know' outside Columbia.[183]

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180 Webster, *Report 2*, Preface, dated 16 August 1930.

181 *ibid.* Adjudicating on Webster's report, Owen Richardson of King's College, London, noted that Webster had been "very unfortunate," his experimental investigations having yielded "little in the way of definite results": "The reality of the radiation which was the objective of his first research was disputed and he set about to make experiments to settle it. Unfortunately, the radioactive material at his command seems to have been inadequate to lead to a definite conclusion. ... The object of his second research was to verify by an independent method some very remarkable results by two American experimenters. Webster's results here were entirely negative. I cannot see where he can have made a mistake, and I think the American results must be untrustworthy" (O.W. Richardson, "Report on the Work of H.C. Webster," file ii/47, 1851 Exhibition Archives, ICL).

182 Webster, *Report 2*, Preface; Webster (1930).

183 When they put the finishing touches to their book *Radiations from Radioactive Substances* in October 1930, for example, Rutherford, Chadwick and Ellis made no mention whatever of the work of Davis and Barnes. Their account of the capture and loss of electrons (written by Rutherford) relied entirely on Rutherford's work earlier in the 1920s. See Rutherford, Chadwick and Ellis (1930), 119-133.

As far as the Columbia workers themselves were concerned, the rumour mill continued to turn. By September, the hearsay was circulating that Barnes was repeating his experiments with electrical counters, and that captures were being registered, though in smaller quantity than the scintillation technique had seemed to indicate. Davis and Barnes, reported one commentator, were "not [being] very communicative, especially after the Langmuir episode."[184] In October, Barnes was (reportedly) still having difficulty repeating his own experiments, fostering the impression that "bad mistakes were made."[185] Fortified by his leave of absence, Davis, on the other hand, retained a residual optimism about the electron capture experiments. He wrote to Pegram from Darjeeling in October:[186]

> I have been waiting for favourable news about the experiment; but the more I think the matter over the more I think Barnes may have deceived himself. That is one aspect. The other is: how could he have gotten such constant and unexpected results? Also I found that [Sagane] in Japan had made the experiment also [using the] scintillation method and had found the same as Barnes about. ... I understand the workers at [the] Cavendish, using Geiger counter failed to find the effect. Possibly it does not exist and possibly there are critical conditions.

Despite his optimism about the possibility of critical conditions for the successful execution of the experiment, however, Davis returned to Columbia in February 1931 to find that Barnes was now having great difficulty reproducing his earlier results.[187] By March, Davis was "about ready to give up the α particle experiment."[188] The end now seemed inevitable.

A formal retraction of all the Columbia work on electron capture appeared in the *Physical Review* on 15 May 1931.[189] Their earlier results, pointed out Davis and Barnes, had "depended on observations made by counting scintillations visually." From the outset, they had fully realised the possibility that "the number of counts might be greatly influenced by suggestion," and had actually taken precautions to guard against bias - precautions which

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184 Breit to Tuve, 15 September 1930, MATP.
185 Zinn to Gray, 8 October 1930, JAGP.
186 Davis to Pegram, 30 October 1930, Box 3, GBPP.
187 Zinn to Gray, 8 October 1930, 8 February 1931, Box 1, JAGP.
188 Zinn to Gray, 23 March 1931, Box 1, JAGP.
189 Davis and Barnes (1931), dated 25 April 1931.

they had "thought at the time to be entirely adequate." Langmuir, on the other hand, had concluded "that the checks applied had not been sufficient, and convinced us that the experiments should be repeated by wholly objective methods." Taking advantage of J.R. Dunning's expertise with electrical counting methods and having constructed four new experimental tubes, they did observe capture of the kind they had originally reported, but "following prolonged observation the effect seemed to disappear."[190] They found themselves unable to confirm their earlier work. Davis and Barnes' involvement with electron capture ended with that admission, signed on 25 April 1931.

5. Conclusion

Davis and Barnes' public retraction of their results made public much of the carefully-concealed doubt surrounding the scintillation technique. Praised and publicised by Rutherford in 1923, the method had, in the space of only five or six years, completely lost its credibility through two damaging disputes. In a sense, the details of the first, the Cambridge-Vienna controversy, had remained private, as a deliberate strategy of the protagonists. But the second, the Columbia episode, had now brought the difficulties of the technique into the public domain. And, in retrospect, it was easy to suppose that what had happened at Columbia might well have happened in Vienna - or in Cambridge. So a response was required. Chadwick officially recorded Cambridge's loss of faith in the technique in November 1930: "The scintillation method, though simple and powerful, has certain disadvantages which cannot be avoided. The strain of counting the scintillations is such that the observers must be carefully controlled, and they can be allowed to count only for very limited periods, amounting on the average to about 6 hours per week. The accumulation of results by the scintillation method," he concluded, "is thus a long and tedious process ..."[191] Cambridge had now officially and publicly repudiated the scintillation technique.

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[190] Davis and Barnes (1931).
[191] Chadwick, Constable and Pollard (1931), 464, dated 21 November 1930.

Yet the consequences of the Columbia episode were not all negative. Davis turned to work in atomic disintegration and transmutation, seeing this as the field where the future of physics lay, and towards which resources should now be directed.[192] One of the productive consequences of the 'Columbia Heresy' was that it introduced electronics and electrical counting methods to Columbia in the person of John Ray Dunning. Dunning had come to Columbia from Nebraska Wesleyan University at the invitation of George Pegram. Like Wynn-Williams and others at the Cavendish Laboratory, he applied his interest in electronics to the development and domestication of electrical circuitry for counting particles. As early as April 1931 he had, according to Davis, "improved the Geiger counter to such an extent that it is almost an instrument of precision."[193] Davis himself acquired considerable enthusiasm for electrical registration methods, telling a colleague excitedly in October 1931 that the Columbia laboratory had "given up electrometers entirely," having "put them on the shelf" in favour of a General Electric FP-54 amplifier tube "so sensitive that each α particle that flies across [the] chamber gives a big throw on [the] galvanometer."[194]

So Columbia joined Cambridge, Berlin and Vienna in the development of electrical counting methods - and in nuclear research. A new network of laboratories was beginning to take shape, defined by shared technique and practice. Yet, as the Columbia episode illustrates, the contours of that network were also determined by contingencies such as the decision to keep the details of Chadwick's visit to Vienna private, or the decision to make Langmuir's visit to Columbia public. The emergent network of laboratories was constituted in the shadow of controversy. In a certain sense, it was also constituted by the contingencies of developments in the radio and valve industries. If Wynn-Williams' and his colleagues' enterprise in constructing the first viable valve-amplifier at the Cavendish speaks eloquently for the abilities of the new generation of researchers entering the laboratory in the later 1920s,

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192 Davis' enthusiasm for atomic disintegration can be judged from Davis (1932).

193 Davis and Barnes (1931). For Dunning's early work at Columbia, see, for example, Dunning (1933, 1934a, 1934b); Dunning and Pegram (1933); Embrey (1970). Lapp (1947),1, notes that "[a]bout 1930 F.M. Eck (of the present Eck & Krebs Company) began working with Columbia University on the construction of G[eiger]-M[üller] tubes which were later to become essentially standard as the Eck & Krebs thin-walled glass counter tube." For Eck & Krebs as commercial glass-blowers and for later proprietary counters, see Korff (1946), 198-199.

194 Davis to Gray, 29 October 1931, Box 8, JAGP.

it also indicates the way in which researchers in the Cavendish and elsewhere were developing the capacity to respond quickly to increasingly rapid developments both in the electronics industry and in other laboratories. Patrick Blackett captured the moment well:[195]

> A rapid change is taking place in the technique of experimental physics. New methods are constantly being invented, and each new advance of technique increases our knowledge of the physical world by making possible experiments which were before technically impossible. In part these changes come from within the laboratories themselves, from the technical innovations both of those engaged in fundamental research and of others who may have specialised in the study of a single method, with little care for the results to be obtained by its use. But to an important extent the technique of the experimental physicist is influenced by the technical achievements of industry.

Blackett highlights precisely what I have tried to emphasise in this chapter: the constitutive role of new technical and industrial developments in the work of the laboratory, a role well illustrated by General Electric's part in the Columbia heresy.

In the wake of the Vienna and Columbia controversies, the forging of the electrical counting technique and the domestication of the valve amplifier into laboratory practice ratified new kinds of manipulative abilities as 'skills' relevant to experimental physics, as well as giving the experimenter "a beautiful instrument of the utmost flexibility and power."[196] The new importance accorded to electrical methods also had profound consequences for the organization of the workplace and for the types of experiments which might now be performed. Indeed, the two went together, for the new forms of multi-skilled cooperative social organisation emerging in the laboratory of the late 1920s opened up new experimental horizons in which "[t]he collaboration of an electrical engineer, a radioactive chemist, and an expert in valve circuits, may make possible an experiment which would be impossible for one alone."[197] If social and technical change and scientific knowledge were intimately and inextricably linked, the change from scintillation to electrical counting was emblematic of that link.

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195 Blackett (1933), 68.
196 *ibid.*, 80.
197 *ibid.*, 71.

CHAPTER FIVE

UNCLEAR PHYSICS

Artificial Disintegration, Cosmic Confusion and a 'New Ray'

1. Introduction

> The divergences between the two series of investigations on the artificial disintegration of the light elements carried out at Cambridge and at Vienna have still to be satisfactorily explained. As these relate to the detection of scintillations, it is obvious that some of the doubtful points would be cleared up by obtaining photographs of the disintegration in a Wilson cloud chamber or by using an electrical method of detecting the particles of disintegration. First steps in both these directions have already been taken and, it is hoped, will lead ultimately to a solution of the difficulty.[1]

Oxford radiochemist Alexander Russell's 1931 summary of the discrepancies and difficulties still facing the performance and interpretation of the experiments on artificial disintegration carried out in Cambridge and Vienna over the previous decade was an important and a judicial one. In the preceding chapters of this dissertation, I have argued that the discrediting of the scintillation method in controversy and through a series of destructive and increasingly publicised laboratory visits had profound consequences both for the production and maintenance of certitude within the laboratory and for the way in which the contours of the experimental community subsequently developed. During the summer and autumn of 1930, with the public denouement of the Columbia episode, it became increasingly plausible to suppose that the now self-evident unreliability of the scintillation technique might also have been responsible in some way for the discrepancies between Cambridge and Vienna in the artificial disintegration experiments. Hindsight, as they say, is an exact science.

[1] A.S. Russell (1931a), 318.

Russell's commentary also demonstrates by way of corollary, however, that outside the small circle of those who knew of Chadwick's visit to Vienna in 1927 and its outcome, the dispute between the Cavendish Laboratory and the Institut für Radiumforschung was widely seen as ongoing well into the 1930s.[2] This chapter is devoted to an exploration of the consequences of that fact. Those consequences must be central to any understanding of the development of nuclear physics as a discipline, for the widespread perception of an ongoing dispute between Cambridge and Vienna had important effects on the intellectual contours and the geographical distribution of the community of researchers engaged in experiments on artificial disintegration. In particular, I suggested in the previous chapter that as a result of the special conference on radioactivity held in Cambridge in 1928, several researchers turned their laboratories to the study of the atomic nucleus, recognising this as a field in which they could hope to make significant contributions. In this chapter, I substantiate that claim by showing how three key groups - Hoffmann and Pose at Halle and teams in the laboratories of Maurice de Broglie and Madame Curie in Paris - entered the field of artificial disintegration in a more or less explicit attempt to resolve the contested issues in the dispute between Cambridge and Vienna. They were followed by groups in Berlin, Rome, Washington and, as I suggested in the previous chapter, New York, so that by the early 1930s, at least half a dozen laboratories were concentrating their attention and their resources on the atomic nucleus. In the wake of controversy, a transnational disciplinary community was taking shape.

If this emergent network of experimentalists recognised itself as a cohesive community (and I shall suggest that it did), it defined itself more or less explicitly in terms of shared problems and common repertoires of technical and conceptual practice. The sudden and rapid expansion of the disciplinary field, which I chart in detail in the following pages through the rapid spread of electrical counting methods - technologies which cultural critic Lewis Mumford would have called *instruments of multiplication*[3] - and the other material resources necessary for participation in the disintegration experiments, produced a modern

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2 See, for example, Livingston and Bethe (1937), 295; Frisch (1979a), 64; Stuewer (1985), 292-294.
3 Mumford (1934), 241.

and *reactive* experimental culture in which the two well-established laboratories, Cambridge and Vienna, found themselves in competition with the newcomers for scientific credit and cognitive authority. As the new electrical technologies yielded new types of data requiring the negotiation and stabilisation of novel forms of evidence and the construction of a new account of the structure of matter, the Cambridge-Vienna dispute lost much of its original relevance and petered out, as it were, rather than ever reaching a definitive closure. New forms of laboratory organisation emerged in response to the constraints imposed by the operation of the new instrumentation. Stability in the nucleus hinged crucially on stability within and between laboratories.

An examination of the strategies used by experimenters who sought to domesticate the new electrical counting technologies shows that theoretical developments, too, played a foundational role in the emergence of the experimental nuclear physics community.[4] Again, however, I want to suggest that the shadow of the Cambridge-Vienna dispute is the background against which the emergent experimental community's reception and appropriation of new conceptual and theoretical developments must be seen. In particular, George Gamow's wave-mechanical model of radioactive decay and artificial disintegration provided experimenters with resources which enabled them to reconceptualise their approach to nuclear disintegration. Coming as it did at the precise moment when electrical counting methods were beginning to replace the scintillation technique, the application of wave mechanics to the nucleus gave experimenters a new set of interpretative devices which they could use to make sense of the flood of new experimental data yielded by the new laboratory technologies. Conversely, those data provided fodder for further theoretical elaboration. In that sense, the emergent experimental and theoretical communities provided an audience each for the other.

I begin this chapter, then, with an account of the Cambridge response to George Gamow's wave mechanical treatment of nuclear problems, for an analysis of the ways in which Cambridge (and other) experimentalists used Gamow's work in the late 1920s is

[4] And therefore suggesting the need for a radical reappraisal of the relationship between 'experimenters' and 'theoreticians' in this period. I begin to lay the foundations of such a reappraisal below.

crucial to any understanding of the changing experimental problematic for nuclear researchers in the late 1920s, and therefore to an appreciation of the social and technical development of nuclear physics itself. Such an analysis also sheds much light on the symbiotic relationship emerging between theoreticians and experimentalists in radioactivity, and on the increasingly circumscribed nature of those categories themselves. If, as Rutherford is once reputed to have said, "They [the theoreticians] play games with their symbols but we in the Cavendish turn out the real facts of nature,"[5] the "real facts of nature" owed much to the theoreticians' sport, as I shall now demonstrate.

2. "Our Theoretical Friends": Cambridge Experimentalists' Responses to Wave Mechanics

In May 1931, the young Russian theoretical physicist George Gamow signed the preface to his monograph, *Constitution of Atomic Nuclei and Radioactivity*. He dedicated the book to the Cavendish Laboratory, Cambridge. Why? Why devote a book on theoretical physics to an institution devoted to experiment and renowned in the 1920s for its uncompromising attitude to developments in high theory? The analysis developed in the previous chapters begins, I believe, to supply an answer, for I have shown how, after the initial formulation of wave mechanics and its successful application to a number of atomic problems, theoretical physicists - its inventors, interpreters and proprietors - began to extend their analyses to problems outside the original remit of quantum mechanics in an attempt to assess the wider applicability of the new mathematics.[6] In particular, I showed that one of the ways in which theoreticians attempted to assess the viability of wave mechanics was by using the new theory to account for novel physical phenomena like the Davis-Barnes effect. Another

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[5] Blackett (1972), 58.

[6] According to Paul Dirac, for example, "The general theory of quantum mechanics is now almost complete, the imperfections that still remain being in connection with the exact fitting of the theory with relativity ideas ... The underlying physical laws necessary for the mathematical theory of a large part of physics and the whole of chemistry are thus completely known, and the difficulty is only [that] the exact application of these laws leads to equations much too complicated to be soluble." See Dirac (1929), 714; Kragh (1990), 87-164.

was to attempt to apply the new tools to a different set of problems entirely - those connected with chemical bonding. After 1927, mathematically-trained chemists (an unusual breed, admittedly[7]) took up the new quantum mechanics to develop theories of bonding, leading to the "genesis of the science of sub-atomic theoretical chemistry."[8] A third, and perhaps more obvious, way was to attempt to apply quantum mechanics to nuclear problems.

Such attempts had effectively begun in 1928. Almost from the outset, however, they had been problematic.[9] As with the wave-mechanical interpretation of the Davis-Barnes effect outlined in the previous chapter, in which decisions about the legitimacy and the existence of the experimental phenomenon were bound up with judgements about the completeness and adequacy of the theory ostensibly being used to 'explain' it, the character of wave mechanics *was itself at issue* when difficulties arose in applying the theory to nuclear phenomena.[10] As early as the autumn of 1928, for example, the American-based theoretician Gregory Breit, visiting Europe, noted that "[i]t is suspected that the structure of nucleii [sic] cannot be understood without some revision of the theory ... practically every letter from Heisenberg to Pauli contains some considerations about the nucleii. The forces binding nucleii together are almost not understood as yet."[11] When he returned to the United States in January 1929, Breit summarised his impressions of the situation in Europe:[12]

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[7] One thinks of Fritz London and Walter Heitler in this connection. For some remarks on the reception and extension of wave mechanics after 1927, see Mulliken (1989), 60-63; Heims (1991); Laidler (1993), 331-353.

[8] Pauling (1928), 174. According to the contemporary judgement of Neville Sidgwick, Oxford chemist and promoter of the application of atomic physics in chemistry, "[i]f the line separating mathematics from physics is blurred, that between physics and chemistry has vanished." See Sidgwick (1931), 270.

[9] Among recent treatments of this question see, for example, Cassidy (1981); Stuewer (1983), 38-42; Hiebert (1988), 58-60; Aaserud (1990), 42-45, 47-48; Cassidy (1992), 267-290.

[10] Compare Pickering (1984a, 1984b).

[11] J.A. Fleming, "Extracts from letter dated November 17, 1928, by Dr. Breit reporting informally on his work at Zurich," enclosed in Fleming to J.C. Merriam, 1 December 1928, quoted in Cornell (1986), 206.

[12] Breit, "Report for September 1928 to January 1929," 16 January 1929, Box 15, MATP; Cornell (1986), 206. See also Breit to Tuve, 7 October 1928, Box 4, MATP; Rabi to Pegram, 15 March 1929, GBPP; Kevles (1987), 222.

> The rate of progress in theoretical physics proved not to be as rapid as could be expected from the developments of the last two years. Advances are being made along secondary rather than fundamental lines ... The reason for this is the scarcity of experimental information about the atomic nucleus. Very helpful data for the solution of the remaining problems in theoretical physics can be obtained by experiments on nuclear disintegrations (this is brought out very strikingly by Gamow's new work on the theory of nuclear disintegration).

Breit's observation was a perceptive one, for the work of Gamow had begun to shed new light on the vexed problem of artificial disintegration, not least because it offered opportune new resources to the embattled experimentalists.[13]

Following the July 1928 conference, as I demonstrated in the previous chapter, both the Cambridge and Vienna teams had continued to defend and elaborate their work in artificial disintegration. Experimentally, they did so by developing electrical counting methods (with all the difficulties thereby entailed) to supersede the discredited and obsolescent scintillation technique.[14] Conceptually, too, there were new developments. Indefatigable as ever, Pettersson turned Aston's packing-fraction data to his own advantage in an attempt to justify the Vienna work, a lead followed after Pettersson's departure by Stefan Meyer. Meyer began a conceptual re-evaluation of the problem of nuclear constitution, highlighting the possibility that neutral particles might play a more fundamental role in nuclear processes than had hitherto been suspected.[15] These initiatives from Vienna may have stimulated Chadwick to publish some calculations he had made a few years earlier on the mass-energy balance in disintegration reactions (unfortunately, he noted, the atomic masses of the elements had "not been determined with sufficient accuracy to afford any test of the mass relations").[16] Like Meyer, Chadwick began to reconsider the possibility that neutral particles - 'neutrons'[17] - might be involved in the disintegration experiments, and introduced experimental modifications to test for such a contingency.

The possibility that neutral particles might be involved in nuclear disintegrations flowed

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[13] In so doing, of course, it was also itself legitimated. For a helpful comparative study of experimental physicists' reception of the principle of relativity, see Warwick (1989), 208-261.
[14] Schmidt and Stetter (1930a, 1930b).
[15] Meyer (1928a, 1929a, 1929c).
[16] Rutherford and Chadwick (1929): Chadwick (1969), 36.
[17] Rutherford and Chadwick (1929)[CPR 3, 221-222].

from Rutherford's latest modification of the satellite model, which allowed for the possibility that the nuclear satellites might include neutral particles, thus helping to explain the building-up of heavy nuclei.[18] Originally, he had envisaged these satellites as neutral α-particles, though, harking back to his Bakerian Lecture of 1920, he had left open the possibility that neutral particles "of mass 2 or 3" might also be present.[19] Developing the satellite model into a quantitative form, Rutherford extended the analogy further: "From a study of the relative masses of the isotopes by Aston, it seems not unlikely that neutral satellites of mass 2 or mass 3 and possibly even of mass 1 - neutrons - may exist in the strong electric fields of the central nucleus."[20] The 'neutron,' the close combination of a proton and an electron first articulated in the 1920 Bakerian Lecture, was therefore a potential disintegration product, and it might even be that the outstanding discrepancies between Cambridge and Vienna could be explained by the existence of such a particle. As Rutherford put it, "in the artificial disintegration of elements ... only protons of mass 1 are ejected, but it is difficult to be certain whether the proton which is ejected exists in the nucleus as a charged particle or as part of an electrically neutral combination held in equilibrium at a distance by polarizing forces."[21]

Chadwick's experiments to test for the presence of neutral particles (by the application of a magnetic field) were, in the event, negative, and he found "no reason to suppose that the particles observed ... were not all protons."[22] The possibility of neutral particles rejected, the impasse remained. Towards the end of 1928, however, a new voice was heard from distant Copenhagen. The voice offered a completely new interpretation of nuclear phenomena, based on the methods and mathematics of wave-mechanics. It was the voice of George Gamow.

Educated at the University of Leningrad, the 24 year-old George Gamow had spent the summer of 1928 at Max Born's institute in Göttingen, where he had developed a theory of

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18 See Stuewer (1986a), 338-349.
19 Rutherford (1927b)[CPR 3, 179].
20 Rutherford (1927d)[CPR 3, 200]; Stuewer (1986a), 344.
21 Rutherford (1927d)[CPR 3, 200-201].
22 Rutherford and Chadwick (1929)[CPR 3, 201].

radioactive α-decay based on the new techniques of wave mechanics.[23] According to Gamow, the potential function describing the nucleus was Coulombian up to a certain point, rose to a maximum, then fell again. Crucially, in this new analysis, an α-particle nearer the nucleus than the potential maximum and with less potential energy than that maximum, would still have a certain probability of escape from the potential well. Gamow made an approximate calculation of this probability, and derived a logarithmic relationship between the disintegration constant and the energy of the expelled α-particle which corresponded in a satisfying way with the long-established Geiger-Nuttall law, one of the fundamental relationships in radioactivity.[24] This was recognised by all as an impressive accomplishment.

Coupled with the increasingly familiar concept of nuclear energy levels, Gamow's work, implicitly supported by the almost simultaneous appearance of a nearly identical theory authored by Gurney and Condon,[25] offered a new means of understanding both radioactive disintegration and its converse, the phenomenon of artificial disintegration.[26] As early as February 1929, shortly after Gamow's return from his first visit to Cambridge, Bohr wrote to 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 the 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."[27] Weight was added to such a picture by Salomon Rosenblum's disclosure of the 'fine structure' (magnetic spectrum) of α-particle spectra in the spring of 1929 using Aimé Cotton's *grand électro-aimant* at the

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[23] Gamow (1928a, 1928b, 1928c); Gamow and Houtermans (1928). For a comprehensive account of the emergence and development of Gamow's theory, see Stuewer (1986b). See also Gamow (1970), 68 ff.; Stuewer (1972).
[24] Gamow (1928a); Stuewer (1986b), 160-161.
[25] Gurney and Condon (1928, 1929); Stuewer (1986b), 164-170. Gurney had worked at the Cavendish Laboratory between 1925 and 1927, when he became familiar with the experimental and conceptual work of Rutherford and Chadwick.
[26] Gamow (1928b), esp. 510.
[27] Bohr to Fowler, 14 February 1929, BSC.

Bellevue Laboratory in Paris.[28] On the basis of this, and the Cambridge work, Gamow theorised that since α-particles could be emitted with several different energies, α-particles emitted with less than the maximum energy might well be accompanied by γ-rays to restore the energy balance.[29]

While such a possibility had emerged out of Rutherford's 1927 satellite model and had been discussed at the 1928 conference,[30] Gamow's work provided the interpretation with a rationale and an independent justification. It also suggested new ways of going on. The notion of discrete nuclear energy levels raised the possibility of resonance - the idea that the chance of an α-particle penetrating the nuclear potential barrier might depend in some way on its energy being the same as one of the characteristic energy levels. In a short letter to *Nature* in April 1929, R.W. Gurney had drawn attention to "the possibility of resonance phenomena if we take into account the solutions of the Schrödinger equation which for certain ranges of energy give ψ-functions the amplitude of which inside the nucleus is large compared with that outside. For this seems to indicate that variation of the velocity of the incident α-particle may be accompanied by an enormous fluctuation in the probability of penetration when the energy approaches and enters the range of energy corresponding to one of the possible quasi-discrete levels."[31]

Now, it is important for my argument here to stress that theoreticians' contributions to the analysis of artificial disintegration between 1928 and 1931 were unambiguously articulated in the context of the Cambridge-Vienna controversy, *even if they offered no immediate prospect of a resolution of the controversy.* When he drew attention to the possibility of resonance processes, for example, Gurney carefully noted that "[t]he application of quantum mechanics may modify the interpretation [of artificial dis-

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28 Rosenblum (1929a, 1929b); M. Curie and Rosenblum (1931, 1932). Rosenblum was a protégé of Marie Curie, who prepared the radioactinium sources for use in his experiments. See also M. to I. Curie, 4 August 1929, in Ziegler (ed.)(1974), 301-302; Reid (1974), 275-276; Pestre (1984), 77; Shinn (1986); Pflaum (1989), 274; Mladjenovic (1992), 203-204.

29 Gamow (1930a); Chadwick and Gamow (1930); Feather (1962), 138.

30 By Chadwick, Bothe and Meitner. See Chadwick's notes on Oliphant (1972a), CHAD II/1, JCP.

31 Gurney (1929). Such a possibility was also discussed by Gamow and Fowler in 1929, after which Fowler worked up the theory with A.H. Wilson, concluding however that the results of a mathematical analysis "do not correspond to the conditions of any conceivable experiment." See Fowler and Wilson (1929), 501.

integration], but seems to throw no light on the origin of the discrepancies between the results obtained at Cambridge and Vienna."[32] On the other hand, it seems clear that one of the reasons for the enthusiastic Cambridge response to Gamow's theory was precisely that it *could* be interpreted as supporting Cambridge against Vienna in the controversy. Gamow calculated the probability with which RaC' α-particles would penetrate an aluminium nucleus, finding that the results gave a "fairly good" agreement with the values determined by Rutherford and Chadwick. Extending this analysis to a comparison with the effect of polonium α-particles, Gamow found that these results, too, were in agreement with the Cambridge experiments (and with those of Bothe and Fränz), but stood in "flagrant contradiction" to those of the Viennese, as he pointedly emphasized.[33]

At Bohr's prompting, Gamow visited Cambridge in January 1929. He received a warm welcome, both socially and intellectually.[34] On 7 February, Rutherford chaired a discussion on nuclear structure at the Royal Society. By way of introduction, he summed up the accomplishments in the fifteen years which had elapsed since the last such discussion. He identified three important developments: "the proof of the isotopic constitution of the ordinary elements," "the proof of the artificial disintegration of the elements by bombardment with alpha particles" and "the study of the wave-lengths of the penetrating gamma-rays which arise during disintegration of the radioactive nucleus."[35] The latter, in particular, had proved to be "of great interest" for the information it was beginning to shed on "the modes of vibration, to use a general term, of the particles constituting the nucleus."[36] Aston's work, too, was portrayed as central to any understanding of nuclear structure. But the main business of the meeting was an account of Gamow's new theory, with a distinct emphasis on the novel interpretative possibilities it raised. On Gamow's model, for example, "particles can accomplish feats that are quite

[32] Gurney (1929). See also Atkinson and Houtermans (1929); Fowler and Wilson (1929); Atkinson (1930).
[33] Gamow (1928b), 515; Gamow (1928c); Stuewer (1986b), 171, 177. In what follows, I shall only deal with experimentalists' responses to Gamow's work. For an account of theoreticians' reactions, see Stuewer (1986b), 171-176.
[34] See Gamow to Bohr, 6 January 1929; Bohr to Fowler, 14 February 1929, BSC.
[35] Rutherford (1929c), 373-374. Rutherford's continuing emphasis on proof and certainty is worthy of note.
[36] Rutherford (1929c), 374.

impossible on classical mechanics. Instead of the α-particle being required to jump the [potential] barrier in order to escape [the nucleus], the particle, or rather the wave system with which it is identified, leaks out through the barrier and finally emerges with a kinetic energy equal to the total energy it possessed inside the barrier." "It sounds incredible," boomed Rutherford from the President's chair, "but it may not be impossible."[37]

Rutherford was followed by Chadwick and Ellis, who gave brief and carefully-worded descriptions of recent experimental work in Cambridge.[38] The floor was then opened to Gamow and Fowler, who offered an account of "such help as the new quantum theory can give in the discussion of the structure and properties of the nucleus."[39] Gamow developed a model of the nucleus in which "an assembly of α-particles with attractive forces between them, which vary rapidly with the distance, may be treated somewhat as a small drop of water in which the particles are held together by surface tension,"[40] while Fowler drew an amusing analogy for the new interpretation of radioactive decay: "You may say that any one of us present has a finite chance of leaving this room without opening the door, or, of course, without being thrown out of the window." All in all, Fowler concluded that Gamow's was "a very beautiful theory, and on its broad lines we may be absolutely confident that it is right."[41]

Fowler's broad optimism was shared by the others present. According to Rutherford:[42]

> We are now in a position to form a picture of the gradual building up of atomic nuclei. Probably in the lighter elements the nucleus is composed of a combination of α-particles, protons, and electrons, and that the parts of the nucleus attract one another strongly, partly it may be owing to the dis-tortional forces and partly also to the magnetic forces. We can only speculate as to the nature of these forces. ... We may thus suppose that the nucleus consists of a tightly packed structure near its centre gradually becoming less dense

[37] Rutherford (1929c), 379. Pollard (1969), 158, and Stuewer (1986b), 177, stress Rutherford's reservations about Gamow's theory, and point out that in the 1930 book *Radiations from Radioactive Substances* (Rutherford, Chadwick and Ellis (1930)), both the satellite and the wave-mechanical models of the nucleus were presented.
[38] Rutherford (1929c), 383-386. Also see Chadwick's notes for this discussion, CHAD II/1, JCP.
[39] Fowler, in discussion of Rutherford (1929c), 387.
[40] Gamow, in discussion of Rutherford (1929c), 386.
[41] Fowler, in discussion of Rutherford (1929c), 387-388.
[42] Rutherford (1929c), 381-382.

> towards the outside. This system is surrounded by a potential barrier which prevents the α-particles from escaping. This static view of the atom may not commend itself to my theoretical friends who may wish the α-particles to have complete freedom of motion within the nucleus. Such a point of view is quite legitimate and can be reconciled with the essential ideas I have put forward.

In one sense, this was merely the latest expression of Rutherford's evolving conception of the atom and its intimate structure. More significantly, however, Rutherford concluded his remarks with an expression of his "strong belief in the ingenuity of our theoretical friends," an indication of the new relationship being forged between Cavendish experimentalists and theoretical physicists, and an implicit acknowledgement of the new value to be attributed to mathematical theory in the experimental workplace.[43] In the wake of the crisis of certitude, a friend in need was a friend indeed.

While Gamow's work opened the possibility of a reinterpretation of radioactive decay and nuclear constitution, it also made an important contribution to another emergent line of development at the Cavendish. Against the background of the deadlock between Cambridge and Vienna in the artificial disintegration controversy, E.T.S. Walton, an 1851 Exhibitioner from Dublin, had begun work at the Cavendish in 1927 on the production of fast electrons, joining the general programme for the production of fast particles being developed by T.E. Allibone.[44] By May 1928, however, it had become "obvious that the indirect method of producing fast electrons, suggested by Sir Ernest Rutherford, was not likely to lead to any positive results and it was not considered advisable to spend any more time on it."[45] Walton instead suggested a method of accelerating positively charged particles. Working with John Cockcroft, he therefore commenced a programme of

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43 Pollard (1991 and personal communication) draws a distinction between Rutherford's personal views of wave mechanics and its reception by other Cavendish figures, especially Chadwick.

44 See Walton's "Report of Work Done During First Year of Overseas Science Research Scholarship (1927-28)," Walton file ii/43, 1851 Exhibition Archives, ICL. For the development of fast particle research at the Cavendish, see Allibone (1984a); Allibone (1987a); Cockcroft (1984); Hartcup and Allibone (1984), 37 ff.; Hendry (ed.)(1984), 10-21; Walton (1982, 1984). On Walton, see Hartcup and Allibone (1984), 39-57.

45 Walton, "Report on Work Done During Second Year of Science Research Scholarship (Overseas)," Walton file ii/43, 1851 Exhibition Archives, ICL (hereafter Walton, *Report 2*).

technical development directed towards the construction of a linear accelerator for the artificial production of fast particles.[46]

When Gamow arrived in Cambridge in January 1929, he brought with him the series of as-yet unpublished plots and calculations relating to the penetration of the nuclear potential barrier by positively charged particles. Discussions between Gamow and Cockcroft (fig. 5.1) produced a series of calculations indicating a 6 in 1000 probability that high-energy (300 kV) *protons* might be able to penetrate the nuclear barrier and effect a disintegration (fig. 5.2).[47] This obviated the need to accelerate particles to the huge voltages which had previously been thought necessary to effect nuclear disruption. Development work therefore began to concentrate on the acceleration of protons *à la* Gamow. Much of the apparatus was developed, constructed or supplied by the Metropolitan-Vickers company, with which both Cockcroft and Allibone were associated.[48] Apart from their important contributions to the electrical engineering aspects of the work, Metro-Vick were also responsible for a key innovation in the mundane material technology of the laboratory. The invention of 'Apiezon oil' by C.R. Burch at Metro-Vick's Trafford Park Physics Section revolutionised the use of diffusion pumps, in which it replaced mercury as the operating fluid. The oil, and a corresponding range of Apiezon greases of unusually low vapour pressure, were supplied to Cavendish workers before they were available commercially, giving them a distinct edge in the competitive scientific climate of the late 1920s.[49] Gamow's work added another dimension to that advantage.

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[46] Walton, *Report 2*; Rutherford to Shaw [Secretary to the Commissioners of the Exhibition of 1851], 22 May 1929, Walton file ii/43, 1851 Exhibition Archives, ICL; Cockcroft (1984).

[47] J.D. C[ockcroft], "The Probability of Artificial Disintegration by Protons," TS, undated but probably December 1928-January 1929, CKFT 20/80, JDCP. See also Walton, *Report 2*; Gamow (1970), 68-69. Hartcup and Allibone (1984), 40, note that Gamow had sent a manuscript describing his work to Rutherford in December 1928, and that Cockcroft saw this manuscript. Compare Hendry (ed.)(1984), 15; Cockcroft (1984); Stuewer (1986b), 176-179.

[48] Allibone (1984a, 1987a) stresses the central importance of the collaboration with Metropolitan-Vickers to the development of high-voltage work at the Cavendish. Niblett (1980), 85-153, is the most comprehensive study of the relationship between M-V and the Cavendish in this period. See also Oliphant and Penney (1968), 148-151; Hartcup and Allibone (1984), 41-57; Crane, Glow and Johnson (1990). For more general background on the relationship between the universities and industry, see Sanderson (1972a, 1972b).

[49] Allibone (1984a), 162; Hartcup and Allibone (1984), 44. On Burch and the wider importance of Apiezon oils and greases, see Allibone (1984b), 12-15. For the development of Cockcroft and Walton's work in the period 1929-30, see Walton, "Report on Work Done During Tenure of Science Research Scholarship," TS dated 30 August 1930, Walton file ii/43, 1851 Exhibition Archives, ICL.

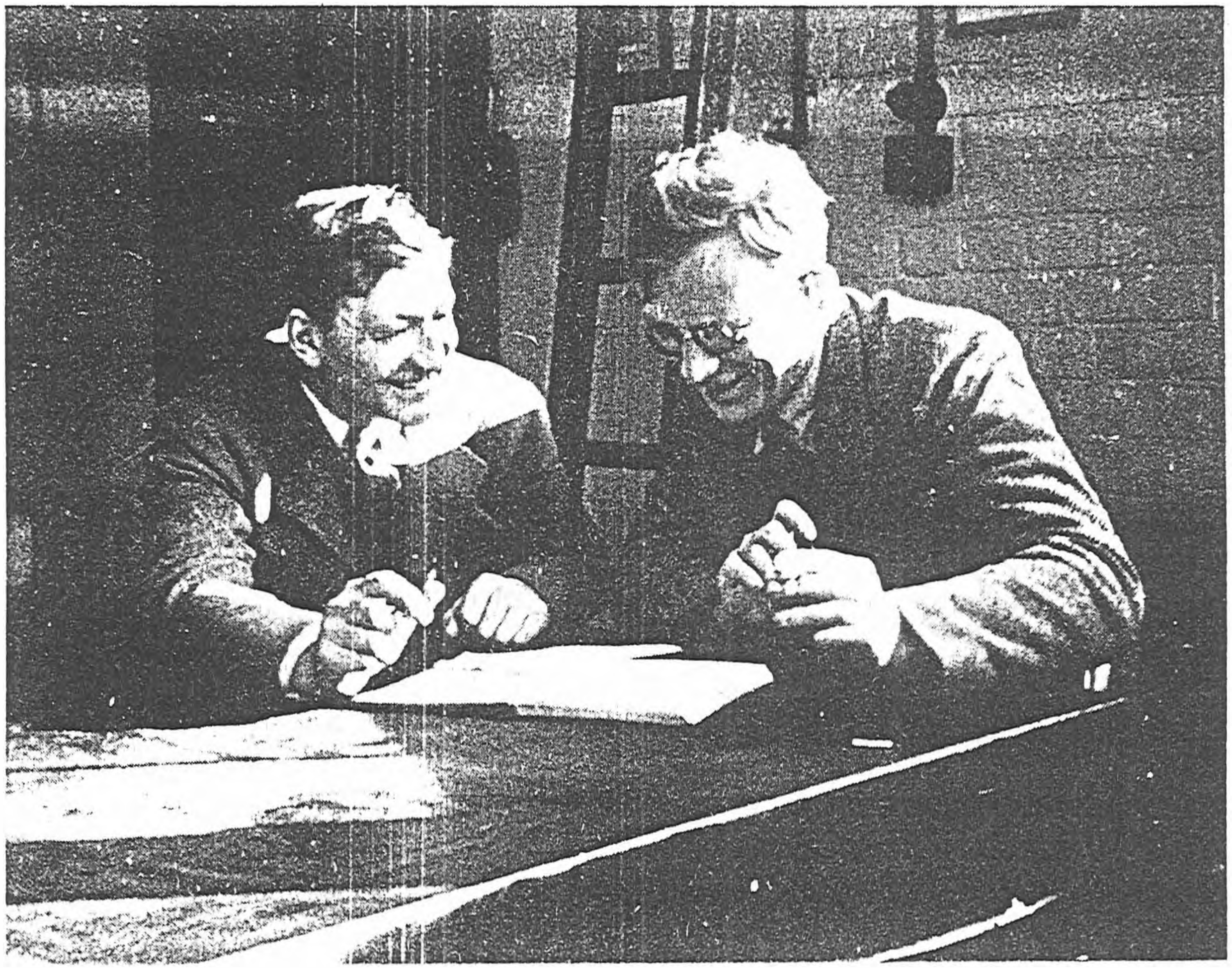

Fig. 5.1 The new relationship between experiment and mathematical theory. John Cockcroft and George Gamow, Cavendish Laboratory, 1930.

Source: Cavendish Laboratory.

The Probability of artificial disintegration by protons.

On Gamow's theory, the probability of an α particle entering the nucleus after coming within the effective collision radius r_m is

$$w_1 = e^{-\frac{16\pi e^2 Z}{hv} J_K}$$

Z being the atomic no. of the bombarded material, v the velocity of the α particle and J_K being a function of

$$k = \frac{m r_m v^2}{4\sqrt{2} e^2 Z}$$

given on the appended curve. r_m is taken to be the radius at the peak of the potential energy curve of the α particle in the field of the nucleus, and is taken as $1.21\ 10^{13}(\text{Atomic Wt.})^{\frac{1}{3}}$.

For a proton disintegration taking account of the half charge the probability becomes

$$w_1 = e^{-\frac{8\pi e^2 Z}{hv} J_K'}$$

J_K being modified by an increase of $\sqrt{2}$ K to take account of the half charge.

The calculated probabilities are given below.

Volts.	α particle Al.	Proton Al.	Proton Boron.
3.10^6	0.20	1.00	1.00 ·
1.10^6	10^{-6}	0.062	0.55
5.10^5	10^{-15}	10^{-5}	0.055.
3.10^5	10^{-26}	10^{-8}	0.0059
2.10^5			$2.27.10^{-4}$

Thus a 300 k.v. proton should be 1/34th as efficient as a Polonium α particle. (in Boron / in Al.) The range of a 300k.v. proton is 5mm in air. Taking the no. of disintegrations in Al. by Polonium as 10^{-6} of the incident α's we should expect $1.8\ 10^6$ disintegrations per microampere of protons, per mm of air equivalent.

JDC.

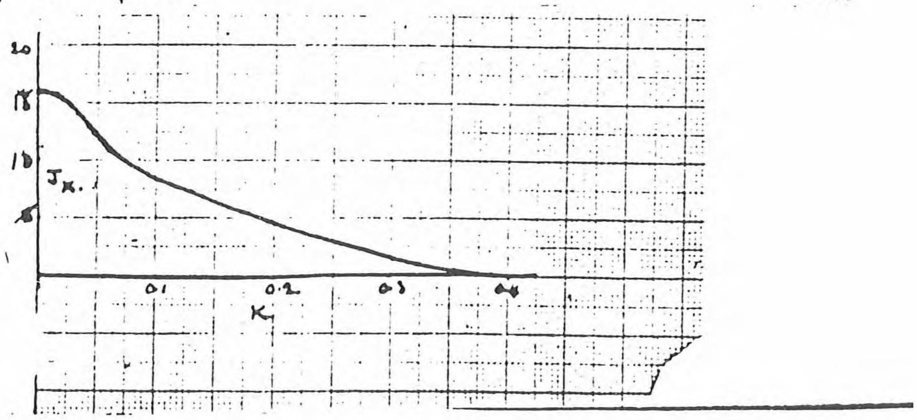

Fig. 5.2 Cockcroft's memorandum to Rutherford on the probability of nuclear disintegration by artificially accelerated protons.

Source: AHQP.

So useful was Gamow to the Cavendish experimentalists, in fact, and so rich the new resources he provided, that he returned to England for an extended stay. Spending the academic year 1929-30 in Cambridge on a Rockefeller Fellowship specially arranged for him by Rutherford,[50] Gamow directed his intellectual labours towards the elaboration of nuclear theory, building on his contribution at the 1929 Royal Society discussion to develop a comprehensive theory of the relationship between the mass-defect curve obtained from Aston's mass-spectrographic data and the particulars of nuclear constitution. Still picturing the nucleus as "built from α-particles in a way very similar to a water-drop held together by surface tension,"[51] Gamow made a series of preliminary calculations to explore the relationship between nuclear constitution and the nature and type of disintegration experienced by particular nuclei. During his year in Cambridge, Gamow also collaborated with Chadwick to work up a more comprehensive theory of artificial disintegration on the basis of Gamow's wave mechanical analysis of the disintegration process, a subject to which I shall shortly return.

While the Cambridge reception of Gamow's work created a new social and intellectual space in which Cavendish experimenters began to regard mathematical theory as relevant and valuable in a prospective way to the work of the laboratory, it also opened Cambridge experimentalists' eyes to the possibility of a wider dialogue between experiment and 'high' mathematical theory. In December 1929, for example, Chadwick carried out a series of experiments to "test" a prediction based on the new mechanics. Nevill Mott, a graduate of the Cambridge Mathematical Tripos, had become interested in wave mechanics in 1926. Like Gamow, Hartree and others, he had spent the autumn of 1928 at Bohr's Institute in Copenhagen. Following up some work by J.R. Oppenheimer, Mott showed that, according to the principles of wave mechanics, α-particles scattered by helium gas should interfere with the projected helium nuclei, resulting in a variation of the scattering intensity between

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[50] Gamow (1970), 76 ff.; Stuewer (1986b), 179. According to Wilson (1983), 559, the fellowship was arranged by Bohr.
[51] Gamow (1930b), 632.

classical and wave mechanics.[52] Mott's analysis predicted that wave-mechanical scattering would give double the classical intensity at a scattering angle of 45°; Chadwick set out to make "a test of this application of the new mechanics."[53] Using apparatus similar to that deployed in earlier scattering experiments[54] and, significantly, the scintillation method of observation, Chadwick demonstrated to his satisfaction that, for polonium α-particles, the observed scattering approached that predicted by Mott's wave-mechanical theory.[55] The apparently successful outcome of this "very simple but pretty expt."[56] vindicated Mott's calculations and did much to strengthen the developing links between experimentalists and theoreticians.[57]

All things considered, then, it is fairly clear that by the time Gamow ventured back to Bohr's Institute in 1930, he had acquired a considerable reputation and an appreciative Cambridge audience - Rutherford told Bohr, for example, that he hoped some new work on long-range α-particles would be "in accordance with Gamow's general ideas which we find exceedingly useful."[58] It is plausible to suppose, therefore, that Gamow's dedication of his

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52 Mott (1930b). See also Mott (1928, 1929, 1984, 1986, 1987). Mott went to W.L. Bragg's department in Manchester in 1929, but returned to a teaching fellowship at Gonville and Caius College, Cambridge, in 1930. He subsequently published a book - *An Outline of Wave Mechanics* (1930a) - based on his Cambridge lectures.

53 Chadwick (1930), 115.

54 Rutherford and Chadwick (1925, 1927).

55 Chadwick laboratory notebook CHAD III 1/5, JCP; Chadwick (1930), 119-122. Given the analysis I developed earlier relating to the obsolescence of the scintillation technique, Chadwick's use of this method calls for comment. Chadwick was clearly acutely aware of the difficulties implicit in his use of the technique, noting that "the only source of large error in the final estimation of the scattering by helium lies in the actual observation of the scintillations produced on the zinc sulphide screen." At the same time, however, he was (as we have seen) thoroughly confident in *his own* abilities as a scintillation counter. He was also confident in the reliability of Crowe, who was acknowledged for "his help in arranging the experiment and in counting scintillations." Crowe had been one of the first to be trained in scintillation counting in 1919, and had assisted at all the subsequent experiments using the method. As I suggested in Chapters 3 and 4, the difficulty with the scintillation technique was in disciplining *others* to count 'correctly' and consistently. Coming as it did only a few months before the denouement of the Columbia affair, this "solo" effort by Chadwick was, I believe, the penultimate experiment carried out in the Cavendish using the quantitative, disciplined scintillation method.

56 Chadwick to Feather, 22 April 1930, NFP.

57 Chadwick told Feather that he had seen Mott's paper "at an early stage," and had " done the expt. long before it was published." See Chadwick to Feather, 22 April 1930, NFP. Mott recalled that after the successful outcome to the scattering experiment, Chadwick took him to see Rutherford, who said "If you think of anything else like this again, come and tell me." See Mott (1972); Mott (1984), 127; Mott (1986), 30. See also Massey and Feather (1976), 21-22, 58-59.

58 Rutherford to Bohr, 24 January 1931, RP. See also Rutherford, Chadwick and Ellis (1930), 328-333, 572-575. The appreciation did not always extend to Gamow's personality or working methods, however: see Chadwick to Feather, 22 April 1930, NFP.

first book to the Cavendish Laboratory indicated his appreciation of the extremely sympathetic hearing which his work had received there, as well as being an implicit acknowledgement of the role the Cavendish was playing in furnishing theoreticians with data for their recondite speculations. In that sense, it tells us much about the changing attitudes of experimentalists in the Cavendish Laboratory between 1929 and 1931, for the openness to new ideas stands in contrast to their earlier hostility towards new theoretical developments. In broader terms, however, it also stands as testimony to deep structural changes taking place in the division of labour between experimentalists and theoreticians in the period 1928-1932 - a change nicely captured by Blackett from the perspective of the jobbing experimentalist:[59]

> To-day an experimenter cannot always be expected to understand fully the theoretical implications of his work. It is not often that an experimenter is gifted enough as a mathematician to be able to read with profit the theory of the polarisation of an electron beams, after an eight-hour day in the laboratory looking for a leak in the apparatus with which he is trying to discover if the theory is true.

In the preceding chapter, I began to show how the relationship between experimentalists and theoreticians developed after the emergence of wave mechanics, and how the categories of 'experimentalist' and 'theoretician' themselves became increasingly circumscribed by the development of an interpretative community of theoretical physicists, centred particularly in Copenhagen and Göttingen.[60] I cited evidence to suggest contemporary recognition of the emergence of a new relationship, in which the emergent community of theoreticians used experimental data to test and extend their mathematical theories. The appropriation of Gamow's work by Cavendish experimentalists adds a pleasing element of symmetry to that account. But that appropriation must, I think, be seen in terms of the crisis of certitude overshadowing the Cavendish in 1928 and 1929. In the

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[59] Blackett (1933), 88. Blackett tempered his remarks with an appreciation of the true qualities to be cultivated by the experimentalist. The experimental physicist must be "a Jack-of-All-Trades, a versatile but amateur craftsman, ... enough of a theorist to know what experiments are worth doing and enough of a craftsman to be able to do them" (*ibid.*, 67).

[60] For the use of the term 'interpretative (interpretive) community,' Fish (1980), esp. 338-371, is a useful resource and Warwick (1992) a good exemplar.

end, for Rutherford and Chadwick, the chief virtue of wave mechanics was the new insight it gave into the processes of radioactive decay and artificial disintegration, their prime experimental concerns.[61]

3. Making Stability in the Laboratory and in the Nucleus: Artificial Disintegration, 1929-1931

While they formed an important and powerful part of his audience, researchers in Cambridge were not the only constituency for the new analysis of artificial disintegration developed by Gamow. The field was an expanding one. In the preceding chapter, I described the tenor of the 1928 Cambridge conference on radio-activity, and suggested that as a result of the discussions which took place during the meeting, several researchers subsequently turned their attention towards the question of nuclear disintegration. In June 1929, for example, Stefan Meyer was able to write optimistically to Rutherford: "The experiments on artificial disintegration are now begun also in some other laboratories in Paris, Berlin, Frankfurt, Halle, etc. *and we will hope that the still remaining differences will be cleared up by and by.* Of course every new beginner will at first have to surmount all the difficulties, which are already overcome in the laboratories in Cambridge, where this sort of work originated, and in Vienna."[62] It is to the entry of these newcomers (and the crux of my thesis) that I now, at last, turn.

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[61] Rutherford to Bohr, 24 January 1931, RP.
[62] Meyer to Rutherford, 5 June 1929, RP, my emphasis.

3.1 Resonance Disintegration and Proton Groups: Artificial Disintegration at Halle

The issues discussed at the Cambridge conference, the growing (and increasingly public) divergences between Bothe and the Cambridge workers on the one hand and the Viennese on the other, and the apparent openness of the artificial disintegration problem together constituted the setting in which several groups of researchers entered the field themselves in an explicit attempt to shed light on the contested issues. One such contribution came from two researchers at the University of Halle. Their involvement in artificial disintegration stemmed, directly or indirectly, from the 1928 Cambridge conference.

Shortly after his return from Cambridge, in the autumn of 1928, the Vienna theoretician Adolf Smekal was appointed Professor of Theoretical Physics at the University of Halle. He was impressed with the experimental facilities of the Physical Institute there,[63] and although his own interests turned increasingly towards solid-state physics, Smekal seems to have had a hand in encouraging a twenty-three year old Halle graduate to take up work on the problem of artificial disintegration. Heinz Pose had studied at the universities of Königsberg, Göttingen, and finally at Halle, where he took his doctorate with Gerhard Hoffmann in 1928 on the subject of electron diffusion in the noble gases.[64] Hoffmann and Pose then began a collaboration on artificial disintegration, using an extremely sensitive duant electrometer designed by Hoffmann and polonium obtained from the Radiological Institute in Prague and from Meyer's institute in Vienna.[65]

The Hoffmann two-leaf electrometer (fig. 5.3) consisted of a platinum needle 0.025 mm thick enclosed in a cylindrical box split into two parts, and suspended on a Wollaston wire of thickness 0.003 mm, with a 2 × 2 mm mirror above the needle. The duants were thermally and electrically insulated and enclosed in a thick walled copper casing, so as to minimise the effect of air currents from temperature differences inside the instrument. Evacuation of the instrument eliminated all remaining air currents and surface leakage. The

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63 Forman (1975), 464; Jungnickel and MacCormmach (1986), **2**, 293 n.133.
64 Pose (1928).
65 Pose (1929a, 1929b). Hoffmann and Pose (1929), 415, thank Smekal for "die freundliche Vermittlung."

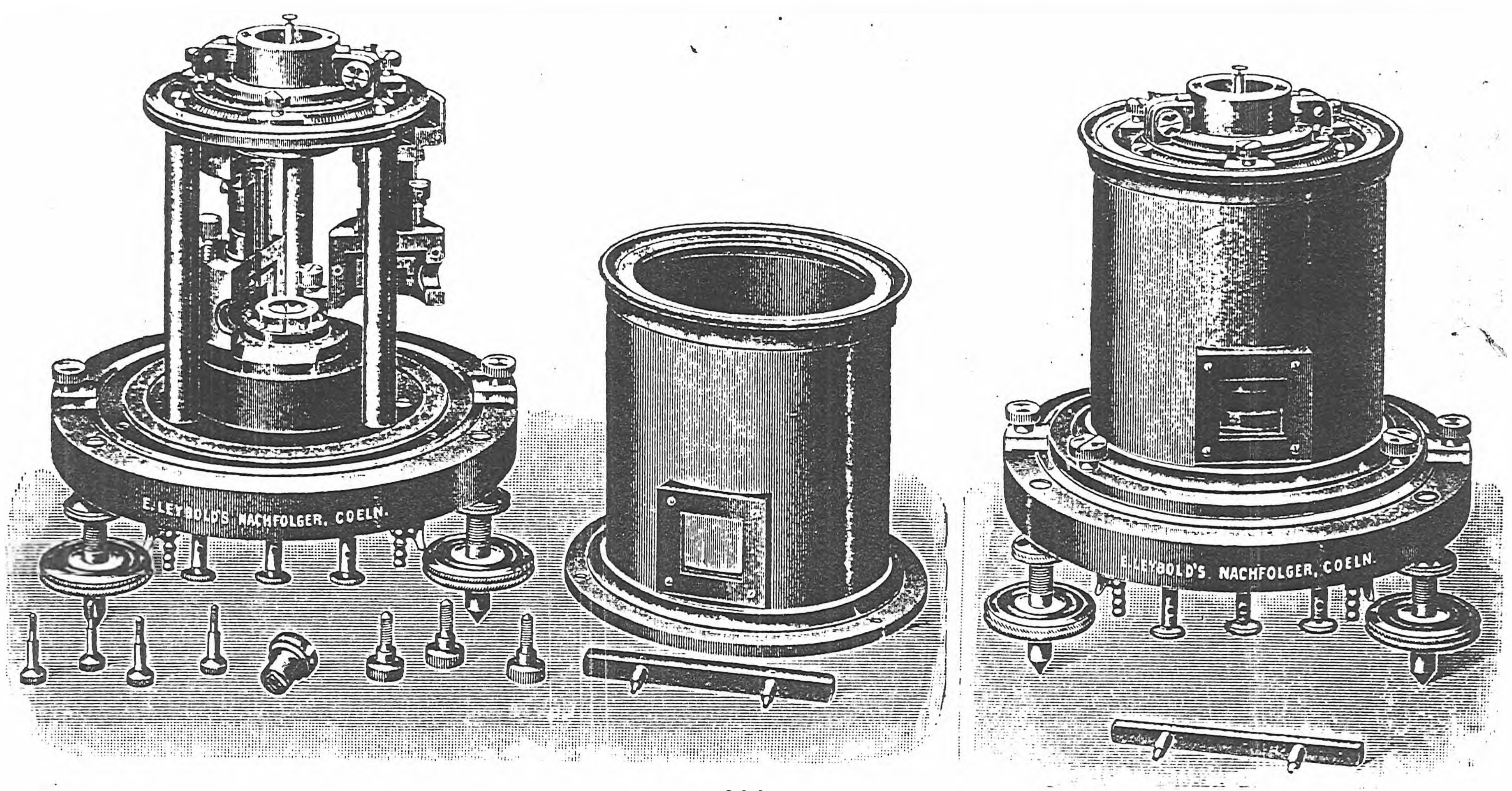

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Fig. 5.3 The Hoffmann duant electrometer.

Source: E. Leybold's Nachfolger A.G. Catalogue of Electrical Measuring Instruments, Box 344, MATP.

constancy of the zero reading thus attained was combined with great sensitivity - the device could be made to register 0.05 millivolts by a deflection of 1 mm at 1 metre from the scale. This high sensitivity was achieved at the cost of recovery time, however. The indicating system required an adjustment time of up to a minute, which meant that the device was only useful for counting particles arriving at low rates.[66]

Using this sensitive instrument in connection with an ionisation chamber, Hoffmann and Pose investigated the disintegration of aluminium, Pose later extending the experiments to include beryllium, iron and carbon. The apparatus and schematic set-up are shown in fig. 5.4. The button under P houses the polonium source, while the aluminium foil (or other target) is mounted at L or F. If the foil is at F, one or more sets of gold foil are placed at L to slow down the α-particles from the source. Sheets of mica at G allow the disintegration protons to be retarded and the effect of this absorption measured for different velocities. A permanent photographic record of the kicks of the electrometer was made, eventually yielding 'distribution-in-range' curves like fig. 5.5.

Pose's results lent immediate credence to the Cambridge-Berlin position. He found that aluminium, beryllium and iron all gave disintegration protons of the order of 5×10^{-8} Hs per α-particle at an angle of 135° between the primary and secondary rays, while carbon, significantly, showed no comparable effect.[67] Pose's experiments also revealed an interesting new phenomenon. Unlike conventional distribution curves, which merely sought to evaluate the ranges of the ejected protons as a diagnostic index, Pose considered the entire shape of the curve, "the conditions of the experiment being so fixed as to make this shape significant."[68] In the case of aluminium, Pose found that unimpeded polonium α-particles of range 3.72 cm produced disintegration protons in three distinct groups - the first with ranges up to about 30 cm, a second with ranges up to 47 cm, and a third, smaller group with ranges above 60 cm. Pose gave an account of his preliminary results at a

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66 On the Hoffmann electrometer, see the 1928 catalogue of E. Leybold's Nachfolger A.-G., copy in 'Oscillographs' file, Box 344, MATP.
67 Hoffmann and Pose (1929), 414-415; Pose (1929a, 1930b).
68 Darrow (1931), 642.

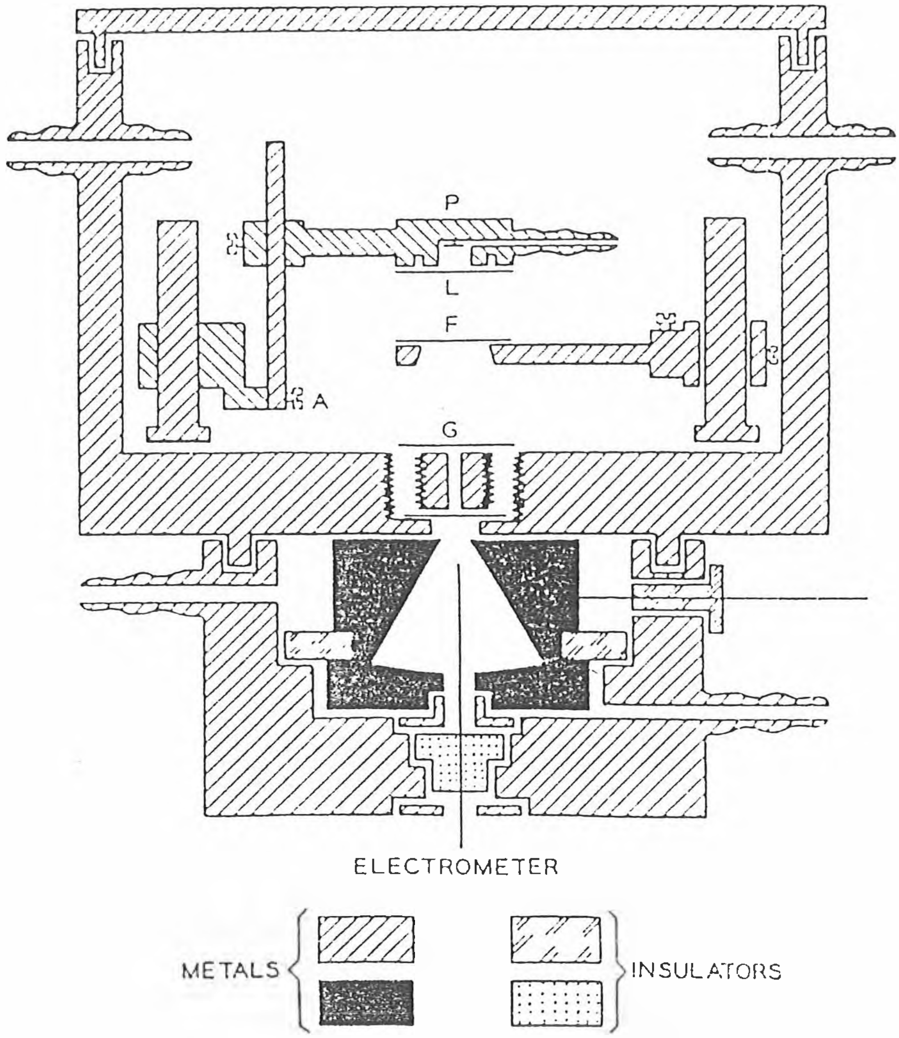

Fig. 5.4 Schematic diagram of Pose's apparatus for artificial disintegration using the Hoffmann electrometer.

Source: Darrow (1931), 642.

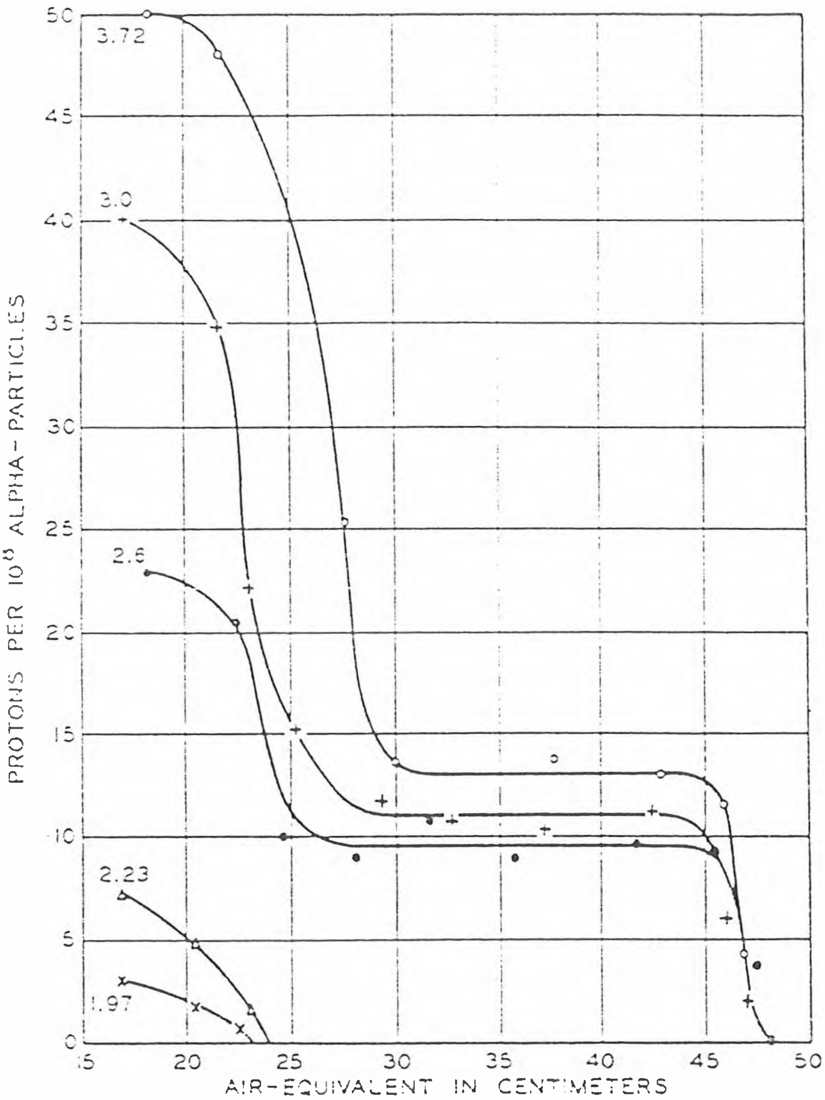

Fig. 5.5 Pose's distribution-in-range curves for disintegration protons ejected from aluminium by polonium α-particles.

Source: Darrow (1931), 644.

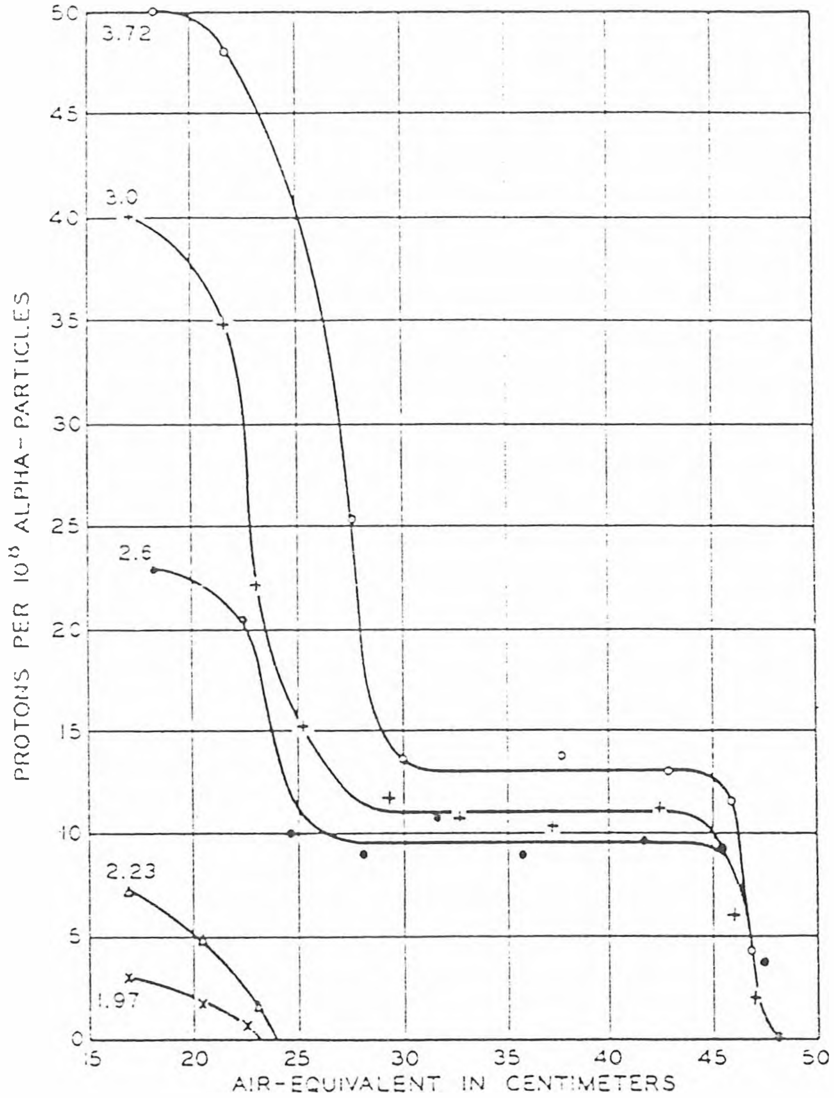

Fig. 5.5 Pose's distribution-in-range curves for disintegration protons ejected from aluminium by polonium α-particles.

Source: Darrow (1931), 644.

conference of German physicists in Prague in September 1929, and was immediately confronted by Stetter, who reasserted the claims of the Viennese workers.[69]

On his return to Halle, Pose next attempted to determine the relationship between the energy of the impinging α-particle and the production of disintegration protons. Early in 1930, he found that protons were ejected by α-particles with narrowly-defined ranges of velocity, an observation which could be interpreted in terms of Gamow's new theory as a resonance effect between the α-particle and the characteristic energy levels of the nucleus. Pose also found that slow α-particles were just as capable of producing disintegration as fast ones, which again seemed in accord with Gamow's tunnelling hypothesis.[70] This phenomenon was widely discussed in 1930 and 1931.[71] According to K.K. Darrow, for example, Pose's investigations had "destroyed what formerly seemed to be the natural assumption that the slower the alpha-corpuscles, the less must necessarily be their ability to transmute," and therefore represented the "newest and [most] sensational work" in artificial disintegration.[72]

Pose's work was particularly well-received in Cambridge, being significant enough to be included in an appendix, added at the last minute, to the comprehensive 1930 treatise *Radiations from Radioactive Substances* by Rutherford, Chadwick and Ellis. Although they stressed (as usual) that much further work was necessary, the Cambridge trio concluded optimistically that "the phenomenon of artificial disintegration now promises to reveal the intimate structure of nuclei of the lighter elements."[73] So, while Pose continued with an investigation of the spatial distribution of the disintegration protons from

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69 Pose (1929b). For Stetter's remarks, see Pose (1929b), 782.

70 Pose (1930c). See also Pose (1930a, 1930b); Darrow (1931), 641 ff.; Stuewer (1986b), 178.

71 According to a visitor to a Berlin seminar in 1930, however, Pose presented a paper "which led to a very spirited discussion, Fraulein Meitner in particular taking exception to some of his statements." Nevertheless, there were "over 100 people present, the front-row seats being taken up by Einstein, Planck, Laue, Wehnelt, Schroedinger, Nernst, and others of the same caliber [sic]." See Hafstad to Fleming, 1 October 1930, quoted in Cornell (1986), 238.

72 Darrow (1931), 641, 642. Also see "Physicists Now Sure Vibrations Occur in Heart of Atom," *Science News Letter*, 28 March 1931, 199.

73 Rutherford, Chadwick and Ellis (1930), 575; Chadwick laboratory notebook CHAD III 1/9, JCP. For further remarks on Cambridge views of Pose's work, however, see Gamow (1970), 78-80.

aluminium,[74] his earlier experiments were taken up with alacrity in the Cavendish Laboratory.

3.2 Discipline in the Workplace: Artificial Disintegration in Cambridge, 1929-1931

While Wynn-Williams and Ward continued to develop valve methods of recording, applying them to the detection of α-particles in the presence of β- and γ-radiations,[75] Chadwick began to develop a fully-fledged disintegration programme using electrical counting methods.[76] In 1929 he set a pair of graduate students, J.E.R. Constable and E.C. Pollard, the task of constructing a proportional amplifier similar to that constructed by Wynn-Williams the previous year, but modified so as to be able to count protons rather than α-particles, a rather more difficult objective. The job took six months, but the experience gained - much of which involved "merely standard amplifier practice"[77] - was useful when the construction of a second apparatus later fell to the lot of Horace Nutt, a young apprentice on the laboratory technical staff.[78] The particles to be counted enter an ionisation chamber. The rise in potential produced by ionising particles (disintegration protons) is amplified by the sequence of valves and operates the recording instrument, an Einthoven string galvanometer. Following Wynn-Williams' original design, a moving strip of paper recorded the kicks of the galvanometer, producing a permanent photographic record. Given the expensive and time-consuming nature of the photographic technique,

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[74] Pose (1930d, 1931a).

[75] Rutherford, Ward and Wynn-Williams (1930); Wynn-Williams and Ward (1931); Rutherford, Wynn-Williams and Lewis (1931); Lewis and Wynn-Williams (1932). See also Wynn-Williams (1931); "The Origin of the Gamma Rays," lecture delivered by Rutherford at the University of Göttingen, 14 December 1931 (sound recording), Record.A.1300, CUL, referring to recent unpublished experiments.

[76] Chadwick, Constable and Pollard (1931), 464. Chadwick also rejected the Hoffmann electrometer (as used by Pose) because "the period of the electrometer is so long that the rate at which particles can be counted is limited to about not more than 1 per minute," rendering the method "not very suitable for general use."

[77] Chadwick, Constable and Pollard (1931), 465.

[78] Pollard and Constable had first spent some months attempting to construct a Geiger-Klemperer counter. See Pollard (1969), 154-155; Pollard (1991), 33. According to R.G. Stansfield, Nutt subsequently specialised in the manufacture of such amplifiers. In his spare time he ran a dance band, and was "reputed materially to supplement his pay from the Laboratory by winning dance competitions in partnership with his wife, as well as by supplying labs in Spain and elsewhere with equipment such as standard pattern Cavendish linear amplifiers." See "The Cavendish Society and its Post-Prandial Proceedings," copy in IN 23, Cavendish Laboratory Archives catalogue, CUL; Goldhaber (1979), 88.

however, "[in] preliminary investigations and in cases where no permanent record of the protons was required, the string galvanometer was replaced by telephones [headsets],"[79] whose clicks could be counted by attentive listeners.

For the instrument to operate at its maximum sensitivity, it was found to be important that the initial rise of potential should be as great as possible. Trial and error revealed that this requirement was best fulfilled by the Marconi D.E.V. valve, which had the advantage that it was fairly easily obtainable commercially.[80] The intermediate stages of the amplifier used Marconi D.E.H. 610 valves, while the output stage consisted either of a D.E.P. 610 or a P.T. 25 valve. Each stage of amplification was "carefully screened from the others by enclosing each valve and its accessories in separate compartments of a metal box."[81] There was good reason for this close attention to the arrangement of the individual components, for the first stage of the amplifier possessed microphonic qualities. In order to eliminate "feed back," additional circuitry was introduced, and separate grid bias batteries were used on each valve. And, again, it was necessary that the apparatus be protected from mechanical disturbances by supporting each valve in rubber sponges and by placing the whole amplifier on "a firm stone pillar."[82] All this work was necessary "to make the amplifier stable."[83] If the physical integrity of the amplifier could be guaranteed, the integrity of the results would, it was supposed, follow.

The careful organisation of the laboratory environment did not exhaust the precautions needed to make the amplifier and its data reliable. As with the scintillation technique, certitude and the truswortliiness of the experimental data hinged upon the stability of social organisation within the laboratory.[84] Managing that organisation was the key task facing Cavendish experimenters in 1929 and 1930. Two key problems arose. Though the clicks

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[79] Chadwick, Constable and Pollard (1931), 467.
[80] *ibid.*, 465. On the characteristics of the D.E.V. valve, see Tyne (1977), 382-383. The D.E.V. valve had first been used by Schmidt and Stetter in Vienna. For some remarks on the contingency attending the use of such materials in the laboratory, see Blackett (1933); Bowden (1984), 139.
[81] Chadwick, Constable and Pollard (1931), 465.
[82] Such features had been part of the laboratory's internal architecture from its foundation in the 1870s. See Schaffer (1992b), esp. 36.
[83] Chadwick, Constable and Pollard (1931), 465.
[84] Compare Galison (1985); Schaffer (1988).

produced in the headphones by protons were "as a rule easily audible" against a very noisy background, great difficulty was experienced in counting "protons of extremely high speed,"[85] raising the possibility of infidelity in the counts. In an attempt to circumvent such difficulties, one of the key certitude-making practices in the scintillation experiments was imported into the electrical counting work. The amplification was made large enough "to work several sets of telephones in parallel, so that two or more observers could count simultaneously."[86] In other words, one of the key warranting devices of the scintillation technique, the simultaneous counting of particles by multiple observers, *was also central to the production of experimental certainty in the use of the electrical method* (fig. 5.6).

Certitude was also threatened by the valve amplifier's microphonic qualities, leading to the imposition of a new form of discipline on experimenters. Where the scintillation technique had demanded a darkened room, complete concentration, counting for strictly defined periods, and so on, the electrical method demanded silence. Moreover, where the scintillation method had required a few well-disciplined individuals (those actually involved in the experiments), the electrical counting method demanded a more widely distributed and all-encompassing discipline. Experimenters (and everyone else in the laboratory) now had to "[move] about on tiptoe and [avoid] knocking the benches or speaking loudly,"[87] for fear of confounding the counting apparatus. But in a physical laboratory, such conditions were alien and difficult to achieve. One irate researcher, upset by the regular disruptions to the counting experiments, went so far as to construct a large sign bearing the legend "Talk Softly Please," which could be illuminated when counting was in progress (fig. 5.7).[88] As eyes were replaced by ears, artificial darkness was

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[85] Chadwick, Constable and Pollard (1931), 467.
[86] *ibid*; Hughes (1992).
[87] Oliphant (1972a), 37. In Berlin, Bothe sent a technician to ask Otto Frisch not to whistle in the corridor of the Physikalische-Technische Reichsanstalt as it "confused him in counting particles." See Frisch (1979b), 65.
[88] Lewis (1979), in an illuminating commentary on the operation of the electrical counting apparatus, describes the practical management of the problem of spurious counts, noting that in the period under consideration, "the output from the amplifier was taken to an electro-magnetic oscillograph *and* recorded on photographic paper tape. This allowed each individual count to be assessed for interfering spurious counts, including microphonics, and also for superposition of pulses where the amplitude was significant. ... For certain purposes for recognizing and eliminating microphonics a magnetic tape recorder of the early

Fig. 5.6 Jack Constable and Ernest Pollard counting proton 'clicks' in tandem using valve amplifier and headsets, Cavendish Laboratory, ca. 1930.

Source: Cavendish Laboratory.

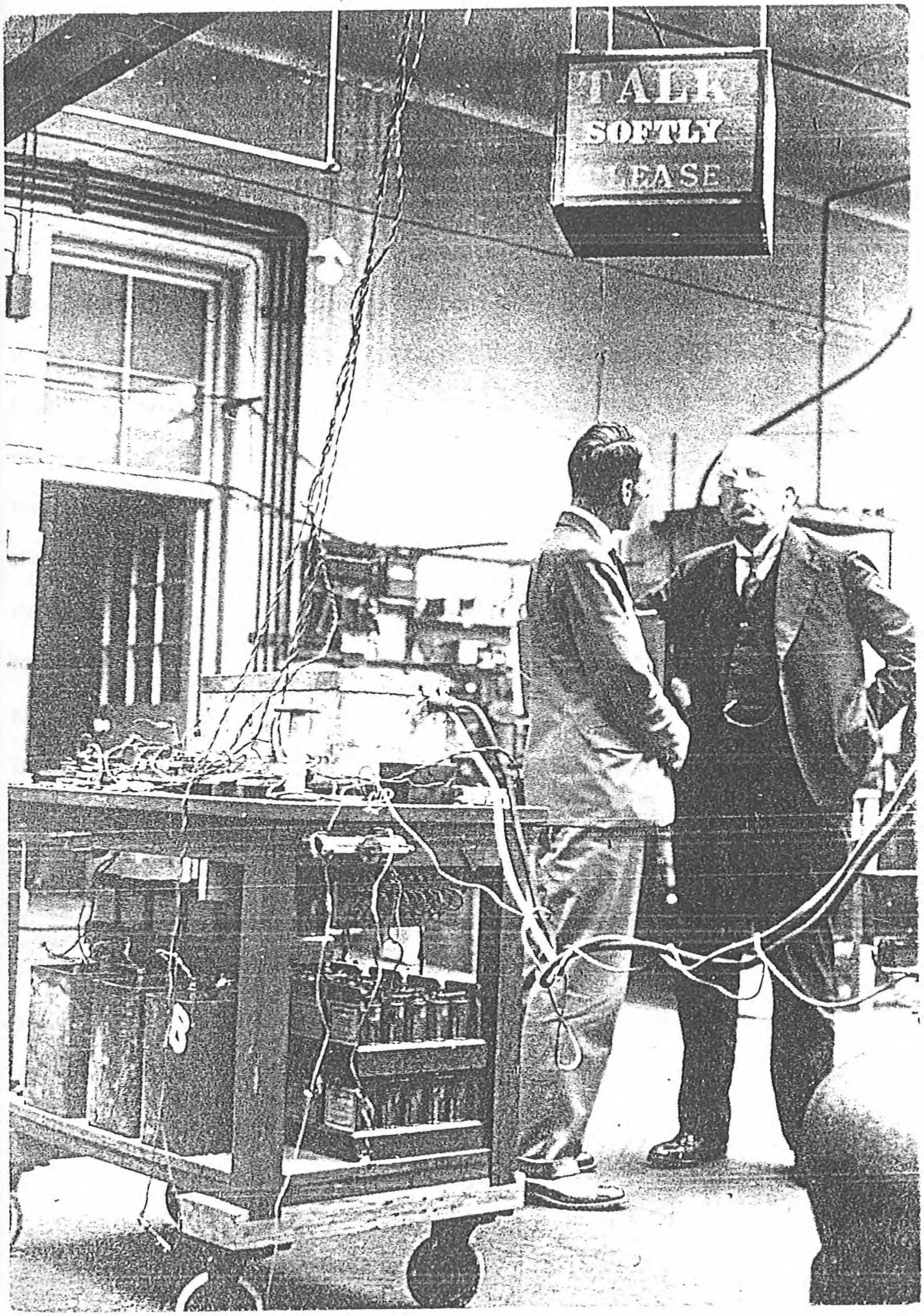

Fig. 5.7 Discipline in the workplace: Wynn-Williams valve amplifier set (on trolley) with power supply beneath, ca. 1932. Rutherford and Ratcliffe photographed by Wynn-Williams with "Talk Softly Please" illuminated. The sign was normally illuminated when counting was in progress.

Source: Cavendish Laboratory.

replaced by reverential and unnatural silence. The introduction of electrical counting methods did not eliminate the strict discipline required by the scintillation method: it merely redistributed it more widely.

So: stability in the nucleus depended both on the disciplinary order of the laboratory and on the stability of the delicate instrumentation used to disclose it. If the use of electrical counting methods created new problems of technique (and therefore of laboratory discipline), however, it also promised to manifest new phenomena and features of interest. Using the newly-constructed valve amplifier, Chadwick, Constable and Pollard set out to investigate in detail the conclusion that the energy change in disintegration was not always the same for nuclei of a given element. Observing the disintegration protons directly and at right-angles to the direction of the incident α-particles, they plotted absorption curves for the disintegration products from lithium, boron, carbon, nitrogen, oxygen, fluorine, sodium, magnesium, aluminium, silicon, phosphorus and sulphur. Except for the cases of fluorine and sodium, all the elements which yielded disintegration protons did so in distinct groups. In the case of boron, for example, two distinct proton groups were found, with ranges of 32 and 76 cm in the forward direction.[89] Chadwick interpreted these results "by the assumption of definite levels for the protons and the α-particles in a nucleus." He also suggested the existence of two modes of disintegration, "one in which the α-particle is captured by the nucleus, and the other in which a proton is ejected without capture of the α-particle."[90] In some cases of collisions involving capture (such as boron, for example), it was possible that the product nucleus might be in an excited state, and that the emission of protons of less than the maximum possible energy might be accompanied by γ-rays, accounting for the balance of the energy. There spoke the voice of Gamow.[91]

Blattnerphone type installed at the end of the laboratory was sometimes used." See Lewis (1979), 11. Duncanson (1984), 91, gives a slightly different version of the sign story.

89 There was the possibility of a third group of range about 16 cm but, noted the authors, "on account of the large number of natural protons emitted by our polonium source, it was difficult to decide whether any part of this group could be ascribed with certainty to boron" (Chadwick, Constable and Pollard (1931), 469-470). Clearly, the management of hydrogen contamination had not improved since 1920.

90 Chadwick, Constable and Pollard (1931), 486; "Mysterious Nucleus of Atom Yields Secrets to Bombardment," *Science News Letter*, 25 April 1931, 266.

91 Chadwick, Constable and Pollard (1931), 486; Chadwick and Gamow (1930). See also Massey and Feather (1976), 60.

In April 1930, writing to ask Feather to investigate the Davis-Barnes electron capture experiments at Columbia, Chadwick confided that the disintegration experiments had gone "quite well and, if I am allowed a few guesses, very well - almost excitingly well." Because of the contaminated polonium source he remained uncertain of the results, however, and would "have to repeat, possibly even try radium C sources," a "terrifying prospect on account of the βs and γs, which we have even yet not really eliminated."[92] The continuing troubles with the electrical counters demanded cleaner and stronger sources of polonium so as to increase the certainty of the experiments. But the Cavendish could raise no such source. For the 1929 scattering experiments, Chadwick had raided the laboratory radium supply to work up a new polonium source of nearly 8 millicuries - the strongest which had yet been deployed in Cambridge. After the success of the Mott trials, Chadwick and his collaborators were able to use the same source in the new series of disintegration experiments.[93] Even so, it had been insufficient to produce unambiguous effects of the kind they were looking for. The conditions of the experiment, they complained, were "far from ideal, and we cannot expect the results to show singularities, such as the presence of groups of protons, as clearly as under conditions more precisely defined."[94] More polonium was needed. And by a piece of sheer good fortune, Feather was able to supply it.

During his year at Johns Hopkins, Feather had carried out some work on the absorption of β-particles and on the time-distribution of α-particle scintillations,[95] but had managed to do little to encourage others to take up the study of such problems, mainly due to the lack of radioactive sources in Wood's laboratory. He had eventually obtained some radium from the nearby Kelly Hospital, where he struck up a friendship with Fred West, the clinician in charge of the hospital's radium and, coincidentally, a fellow Yorkshireman.[96] Each day's production of radium emanation, about 700 millicuries, was pumped off and sealed in a single glass 'seed.' When after some time such seeds had lost their clinical value, they still

[92] Chadwick to Feather, 22 April 1930, FEAT 23/6, NFP.
[93] *ibid.*
[94] Chadwick, Constable and Pollard (1931), 469.
[95] Feather (1930a, 1930b).
[96] Feather, "Reminiscences of the Cavendish Laboratory, 1926-1937," unpublished typescript in FEAT 45/7, NFP, 4-5; Feather (1962), 141-142; Feather (1974); Cochran and Devons (1981), 257, 269.

contained an appreciable quantity of radium decay products, including polonium. In 1930 the hospital had several hundred of these 'hot' tubes in storage pending disposal. With the cooperation of West and his colleague Dr. Burnam, Feather was able to tap this source of material for his own work, and lost no time in informing Chadwick of its existence.

The possibility of acquiring some polonium from Baltimore filled Chadwick with unwonted enthusiasm. It would be, he told Feather breathlessly, "a marvellous stroke of business if you could collect a large quantity of Radium D etc. - the larger the better of course." He described his experiments so that Feather would "know why I am so anxious to get polonium ... [and] ... with what joy I read your letter." His closing request for Feather to try "some spell-binding of your own in the Baltimore Hospital"[97] bore fruit, for West and the Kelly Hospital were happy to oblige. In the late summer of 1930, Feather returned to Cambridge with more than a year's production of dead radon seeds, "the equivalent of some 130 millicuries of polonium,"[98] enough to match the supply available even in the Laboratoire Curie. With the future needs of the Cavendish Laboratory in mind, West even began to save old tubes for despatch to Cambridge. The Cavendish, it seemed, now had an assured supply of polonium.[99] It would be put to good use.

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97 Chadwick to Feather, 9 June 1930, NFP.
98 Feather, "Reminiscences of the Cavendish Laboratory, 1926-1937," unpublished typescript in FEAT 45/7, NFP, 5.
99 West to Feather, 5 December 1930, NFP.

4. The French Connection: Radioactivity in Paris, 1928-1931

Heinz Pose was by no means the only researcher to begin research on artificial disintegration as a result of the 1928 Cambridge conference. As I showed in the previous chapter, a large contingent from Paris had attended the meeting, among them Frédéric and Irène Joliot-Curie,[100] Marcel Frilley and Dragolioub Yovanovitch, all of the Laboratoire Curie; Pierre Auger and Francis Perrin of Jean Perrin's Institute for Physical Chemistry; and Maurice, duc de Broglie, with his co-worker Jean Thibaud. At Cambridge, the French scientists clearly learned a great deal more about the Vienna controversy than had appeared in print. As a result of their visits to Cambridge, de Broglie and the Joliots returned to Paris with a new goal: to attempt to shed fresh light on the controversy by taking up the artificial disintegration experiments themselves. I shall deal with them in turn, for a discussion of the different strategies adopted by the two groups in switching to work on the nucleus provides an illuminating comparison, and does much to display the kinds of material resources necessary for participation in nuclear research.

4.1 The Laboratoire de Broglie: From X-Rays to Radioactivity

Born in 1875 (making him a near-contemporary of Rutherford) and educated in Paris, Maurice, duc de Broglie, had spent some years in the French Navy, during which he had become interested in science. Leaving the Navy, he established a small private laboratory in his Paris residence on rue Chateaubriand, in which he undertook experimental studies of gas discharges and ionic motion. He had been elected Secretary of the first Solvay Congress in 1911, after which he and his younger brother Louis became interested in problems relating to the structure of matter.[101] During the war, de Broglie undertook

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[100] Massey and Feather (1976), 21, imply that only Irène Curie attended the conference, though Chadwick's seating plan lists both (see CHAD III/4, JCP). It is certainly the case that Irène had already acquired a considerable reputation in radioactivity research by 1927, whereas Joliot was just beginning his career.
[101] Weill-Brunschvicg and Heilbron (1970); Mehra (1975); Pestre (1984), esp. 172-174; Bensuade-Vincent (1987), 42, 144.

research in the fast-developing field of radio and, like Rutherford in England, worked on the detection of submarines.[102] As de Broglie's scientific interests developed, the rue Chateaubriand laboratory was gradually extended to the higher floors of the building and, later, to another site in rue Lord Byron, a few streets away in the 8th arrondissement. By the 1920s the rue Chateaubriand laboratory occupied a rather extensive suite of connected rooms, accommodating de Broglie and several co-workers - Jacques Trillat, Jean Thibaud and Alexandre Dauvillier among others - who worked with de Broglie in his experimental investigations of X- and γ-rays.[103]

Provided only with laboratory space by de Broglie, these 'independent' researchers relied for financial support on the Caisse des Sciences or on what sponsorship they could muster from industrial firms like Thomson or Philips.[104] Nevertheless, by 1928 de Broglie and his co-workers were recognised authorities on X- and γ-rays. The duc himself had published a book on the subject in 1922,[105] and had co-authored another with his brother Louis[106] - contributions which were recognized by Rutherford's presentation of the Royal Society Hughes Medal to de Broglie in November 1928.[107] Given its theme, then, it was natural that de Broglie and Thibaud should be invited to the Cambridge conference. The only extant record of de Broglie's visit is a snapshot, taken by Wynn-Williams, of de Broglie and Hans Geiger (himself Hughes Medallist the following year[108]) standing awkwardly together in the Cavendish courtyard (fig. 5.8). It is clear, however, that de Broglie was deeply impressed by the outstanding difficulties in the disintegration experiments (difficulties which, as we have seen, were fairly openly discussed at the conference).

102 Leprince-Ringuet (1960), 298.
103 For a good description of the rue Chateaubriand laboratory in the later 1920s, see Leprince-Ringuet (1960), 297-300; Lépine (1962). See also Wilson (1961), 31; Pestre (1984), 70, 84-85, 89. On de Broglie's work, see Lépine (1962); Weill-Brunschvicg and Heilbron (1970); Wheaton (1983), 263-270, 274-278.
104 Leprince-Ringuet (1991), 55. I am grateful to Dominique Pestre for sending me a copy of this book. For comments on the funding of French science in the 1920s, see Weart (1979), 1-36; Pestre (1984), 272-284; Paul (1985).
105 M. de Broglie (1922).
106 M. de Broglie and L. de Broglie (1928).
107 Rutherford (1929b), 22-23. The Hughes Medal was established in 1902 for original discovery in physical sciences, particularly electricity and magnetism. It was awarded annually "to such person as the President and Council may consider the most worthy recipient."
108 Rutherford (1930a), 202-203.

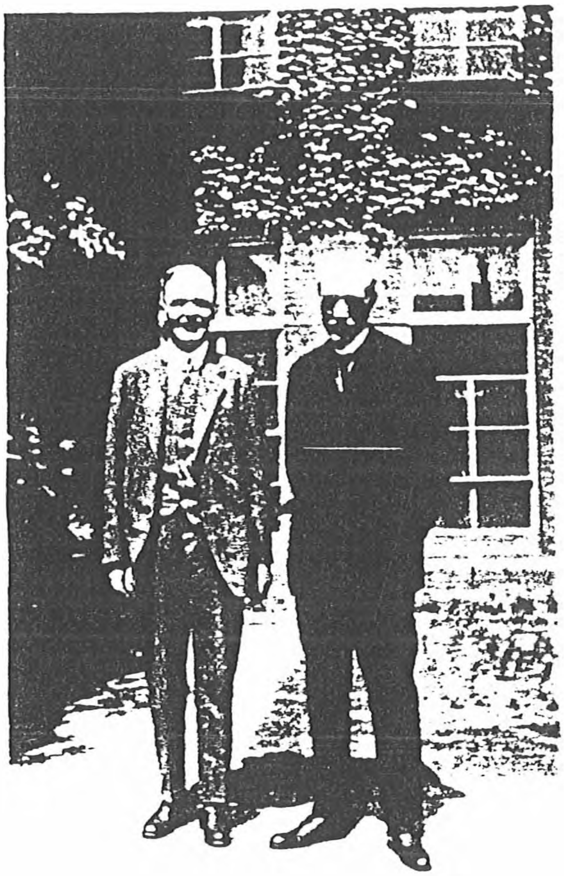

Fig. 5.8 Hans Geiger (l) and Maurice, duc de Broglie (r) at the Cambridge conference, July 1928.

Source: Snapshot taken by Wynn-Williams, Cavendish Laboratory.

Judging that artificial disintegration was a field which would provide fertile ground for new discoveries, as well as for the development of technique, de Broglie decided that his laboratory ought to make a strategic change of direction towards the study of atomic nuclei.[109]

How, then, did one go about turning a reasonably well-equipped laboratory over to such work? Money was no object: de Broglie was happy to buy cloud chambers and all the other proprietary materials necessary for full participation in the disintegration experiments. When it came to the in-house construction of apparatus which could not be obtained commercially, however, de Broglie and his immediate circle apparently lacked the requisite electrotechnical expertise. Needing a skilled assistant to help with the construction of the otherwise unobtainable electrical counters and to carry out experiments under his direction, de Broglie therefore engaged Louis Leprince-Ringuet, a distant cousin of Trillat who had paid a casual visit to the rue Lord Byron laboratory during a stay in Paris.[110] Leprince-Ringuet was perfectly suited to the job. He had trained as a submarine cable engineer, and had worked for the P.T.T. - a background which served him extremely well when it came to designing apparatus for the detection of weak electric currents.[111] Fascinated by de Broglie and captivated by the idea of scientific research, Leprince-Ringuet was persuaded to join the new project.

He joined Trillat in the rue Lord Byron laboratory where he undertook a short apprenticeship (some preliminary work on X-rays) to familiarise himself with the experimental apparatus and practical technique of radioactivity - the equivalent, perhaps, of the Cavendish Nursery course.[112] He then began work with de Broglie, supported by a small grant from the Caisse National des Sciences. de Broglie had already acquired two Shimizu expansion chambers,[113] though in the absence of the other instruments required for experimental investigations he had immersed himself in the conceptual and theoretical

109 Leprince-Ringuet (1960), 299-300; Pestre (1984), 78; Leprince-Ringuet (1991), 35.
110 Leprince-Ringuet (1991), 33-36.
111 *ibid.*, 37.
112 *ibid.*, 36.
113 Presumably from the Cambridge Scientific Instrument Company. See Leprince-Ringuet (1960), 300.

aspects of radioactivity, developing a model in which radioactive disintegration was to be regarded as the culmination of a series of regular 'swells' produced by interaction between the nucleus and an external radiation field.[114] Leprince-Ringuet's first assignment was to develop new methods of electrical amplification and registration, *à la* Greinacher and Wynn-Williams. As we have seen, few such instruments then existed (and none were yet in use in France), so the method had to be developed from scratch - not a particularly difficult task for someone with Leprince-Ringuet's background and training. So as to expedite matters, however, de Broglie engaged additional technical assistance, and a small experimental group was formed, consisting of Leprince-Ringuet and his colleagues Roger Louvigny, René Fradin and Eugène Boulanger.[115]

With development work well in hand, de Broglie gave a lecture at the Conservatoire National des Arts et Métiers on "Recent Progress in the Artificial Disintegration of the Elements" at the end of April 1931.[116] It was an excellent summary of work undertaken in the field up to that moment, covering both the experimental and conceptual aspects of disintegration research, including the most recent work by Pose, Bothe and Chadwick. Surveying the various methods of detecting atomic fragments, de Broglie dismissed the scintillation method, which "remains delicate on account of the fatigue induced by its use and the rather subjective character of the fleeting and feeble flashes which the observer must count."[117] One must therefore rely upon electrical counting methods, he concluded. A few days later, he and his young protégé presented the fruits of their labours to the Société française de Physique: an operational valve amplifier.[118] The laboratoire de Broglie was in business.

During the summer of 1931, Leprince-Ringuet applied the new apparatus to the detection of the disintegration products of aluminium, attempting to verify his conclusions by comparing the results with those obtained for scattered protons from hydrogen-containing

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114 de Broglie (1930).
115 Leprince-Ringuet (1991), 37-38.
116 de Broglie (1931).
117 *ibid.*, 21.
118 de Broglie and Leprince-Ringuet (1931a); Leprince-Ringuet (1931a, 1931b).

substances.[119] The apparatus gave rise to many of the same technical and operational difficulties found in Cambridge, however, so that for the next six months, Leprince-Ringuet devoted much effort to the elimination of the microphonic effects and to the elaboration of a detection system.[120] Crucially, however, de Broglie's laboratory also lacked a chemist with the radio-chemical skills necessary to prepare and manipulate the essential polonium sources,[121] so that Leprince-Ringuet's interests soon moved away from the disintegration experiments and towards the study of cosmic rays, which could be investigated using the same kinds of equipment, but without the need for the hard-to-come-by (and even harder to prepare) radioactive material.[122] In that sense, de Broglie's laboratory, with its characteristic research style and ethos, its emphasis on technique and on goal-directed research, and its distinct lack of radioactive materials stood in sharp contrast to the other Parisian laboratory involved in radioactivity research: the Laboratoire Curie.

4.2 The Laboratoire Curie: From Radioactivity to Transmutation

Maurice de Broglie's laboratory was a recent convert to the study of radioactivity and the nucleus. Marie Curie had virtually founded the discipline. Throughout the 1920s, as we have seen, Curie's laboratory had continued to work on what one might call 'traditional' aspects of radioactivity - radioactive constants, the characterisation of radiations, and so on. Towards the end of the decade, however, the laboratory began to work on the more physically-orientated problems exercising scientists at the Cavendish and elsewhere. The driving forces behind the change were Irène and Frédéric Joliot-Curie.

Irène Curie had entered the Institut du Radium in 1918 at the age of 21 as her mother's assistant. She had already acquired considerable technical experience, having helped her mother train operators for X-ray installations during the war,[123] and began independent

[119] Leprince-Ringuet (1931a); de Broglie and Leprince-Ringuet (1931b).
[120] Leprince-Ringuet (1933).
[121] Leprince-Ringuet (1960), 300-301; Leprince-Ringuet (1991), 61-2.
[122] Leprince-Ringuet (1934). See also Leprince-Ringuet (1982, 1983).
[123] For the Curies' role in the application and development of medical X-ray technology during the war, see M. Curie (1921); E. Curie (1938), 289-307; Reid (1974), 194-206; Pflaum (1989), 199-212.

research work in 1921 with a series of investigations on measurement techniques in radioactivity and a study of the atomic weight of chlorine from various sources (designed to elucidate the then-vexed question of isotopes).[124] A series of solo and collaborative investigations followed,[125] until in 1926 she married Frédéric Joliot. Joliot, a student of Paul Langevin at the School of Industrial Physics and Chemistry, came to the Laboratoire Curie late in 1924.[126] Funded by a Rothschild scholarship, Joliot's initial research concerned the development of methods for studying electrolytic deposits of the radioelements, a subject which also formed the basis for his doctoral thesis, submitted in March 1930.[127] Following his marriage to Irène in 1926, however, the couple effectively began to work together as a research team, their first joint papers appearing in 1928.[128]

As part of the up-and-coming generation in French radioactivity research, the Joliots were among the carefully-selected invitees to the special Cambridge conference. Perhaps it is a coincidence that by 1928, taking full advantage of the unequalled stockpile of radioactive materials at the Laboratoire Curie, they were beginning to work on the properties of polonium, at the precise moment when polonium was becoming critically important to artificial disintegration and to the deployment of electrical counting methods.[129] After their visit to Cambridge, where they heard first-hand of the difficulties facing the experimentalists and of the new developments in laboratory technique, the Joliots, like de Broglie, seem to have acquired a definite interest in artificial disintegration and the Cambridge-Vienna controversy. In that respect, as they must surely have realised,

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124 I. Curie (1921, 1922, 1923a, 1923b, 1923c); Pflaum (1989), 231-241.
125 I. Curie (1925a, 1925b, 1925c, 1927); I. Curie and Fournier (1923); I. Curie and Chamié (1924a, 1924b); I. Curie and Yamada (1924, 1925a, 1925b); I. Curie and d'Espine (1925); I. Curie and Béhounek (1926); I. Curie and Mercier (1926). See also the correspondence between Irène and Marie Curie in Ziegler (ed.)(1974), 228 ff.
126 On Joliot, see Blackett (1960a); Biquard (1965), esp. 19-31; Goldsmith (1976), 19-63; Pflaum (1989), 243-266. See also Bensaude-Vincent (1987).
127 Joliot (1927, 1929, 1930a); Goldsmith (1976), 36-37.
128 I. Curie and Joliot (1928a, 1928b, 1929). See also Goldsmith (1976), 37; Six (1987), 41-42; Pflaum (1989), 272-279. Weart (1979), 3-59, esp. 39-40, gives an interesting insight into the Laboratoire Curie and its research organisation in the late 1920s and early 1930s. For the mid-1930s, see Goldschmidt (1990), 8-19.
129 I. Curie (1929b); I. Curie and Joliot (1929, 1931a, 1931b); I. Curie and Lecoin (1931); Joliot (1929). See also I. Curie to M. Curie, 23 March 1927, in Ziegler (ed.)(1974), 273-274; Trenn (1980); Pflaum (1989), 274-275. It is useful to recall (a) that much of Irène's earlier work had been on polonium, and (b) that Elisabeth Rona of the Vienna Institut für Radiumforschung spent some months in the Laboratoire Curie in 1928 learning how to prepare and manipulate polonium sources. See Rona (1928); Rona and Schmidt (1928); Rona (1978), 22.

the unparallelled resources of the Laboratoire Curie placed them in a most advantageous position. Like de Broglie, they started to make preparations.

Deliberately and painstakingly, they began to accumulate polonium, until by the end of 1931, they had "almost quadrupled" the amount available for research.[130] Moreover, they adopted the deliberate policy of keeping the material concentrated in one mass, rather than distributing it between the various researchers in the laboratory, in an attempt to "force" significant findings.[131] The Joliots also began to acquire the other resources necessary for research in artificial disintegration. Joliot constructed a variable-pressure cloud chamber, while in November 1931 they purchased a Hoffmann electrometer (fig. 5.9), a big event for the laboratory on account of the continuing shortage of funds.[132] Like de Broglie, they were acquiring the tools of a new trade. Unlike de Broglie, however, the Joliots made no initial effort to acquire electrical amplification apparatus. With the strong radioactive sources at their disposal and the sensitive Hoffmann electrometer (as used by Pose), they had little need of valve amplifiers.[133] In the precarious economic climate of the early 1930s, the radioactive resources of the Institut Curie were a powerful asset.

While the Joliots' return to Paris with news of the Cambridge meeting shaped their own future research plans, it also had consequences for other researchers in the Laboratoire Curie. By the end of 1928, unaware (we must assume) of Chadwick's visit to Vienna and its outcome, Madame Curie had set someone to work on the contested issue of scintillation counting (writing a testimonial for Pettersson's application to the chair at Stockholm in November 1928, almost a year after Chadwick's visit, Curie ironically remarked on the continuing controversy between Cambridge and Vienna: "It seems to me ... that some of the methods used by M. Pettersson have been favourably received and that, among the results he has obtained, some are acknowledged. To get a clearer picture, my laboratory

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130 I. Curie (1929b); Goldsmith (1976), 37-38; Weart (1979), 40-41; Pflaum (1989), 275, 287.
131 Weart (1979), 41, 298 n.7, quoting G.C. Wick. I shall return to this issue below.
132 Joliot (1931a); I. Curie (1932); Blackett (1960a), 88-89; Goldsmith (1976), 39-40; Pflaum (1989), 272, 288. In the late 1920s the Radium Institute did relatively well financially compared to other Parisian laboratories. In 1928, for example, the Caisse gave 40,000 F each to Curie and Perrin, and 20,000 F apiece to Fabry and Urbain. See Pestre (1984), *passim*; Paul (1985), 301-302. For the Joliots'attempts to solicit testimonials in support of their research, see I. Joliot-Curie to Rutherford, 26 May 1931, 5 June 1931, RP.
133 Compare Feather (1962), 138-139.

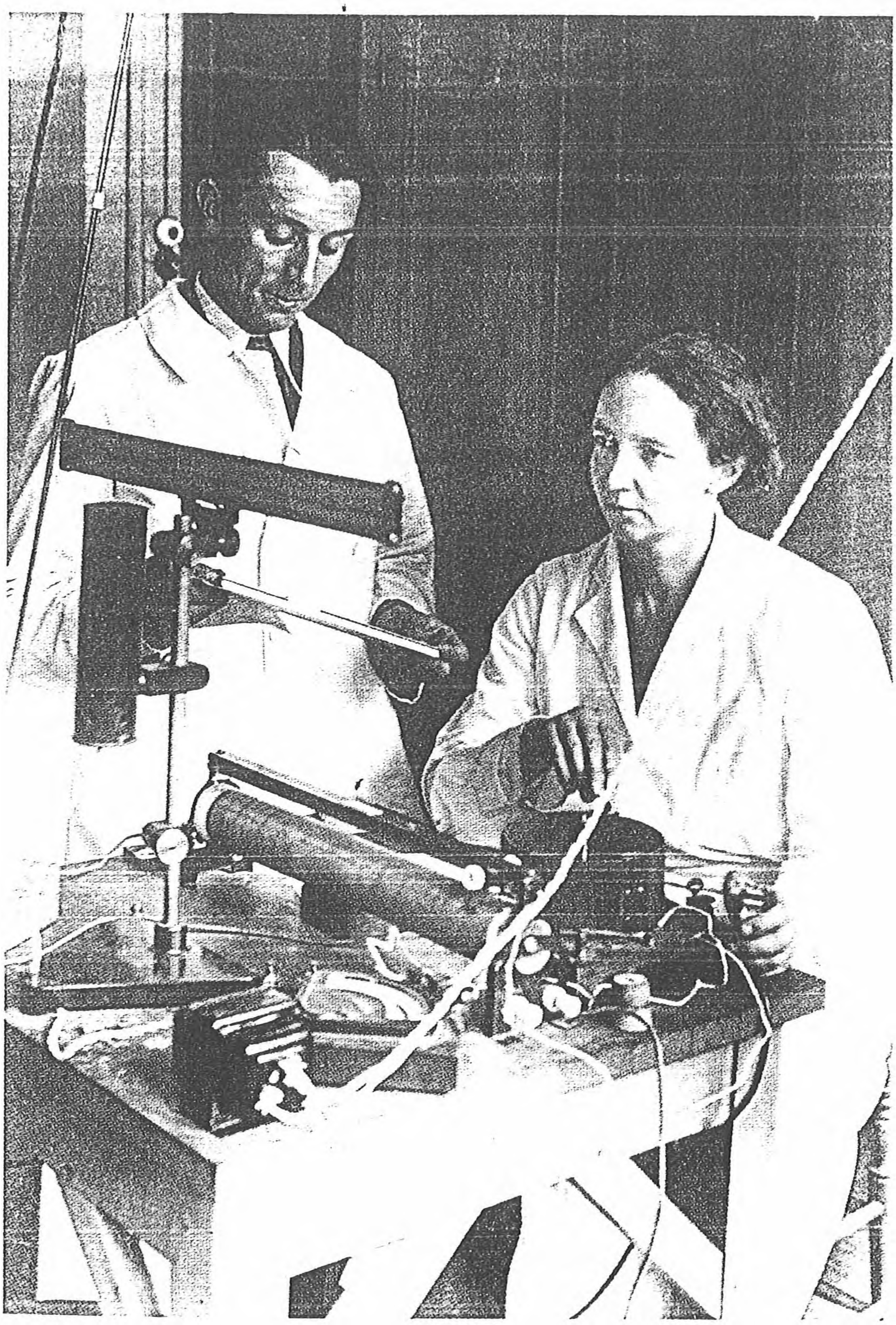

Fig. 5.9 Fréderic and Irène Joliot-Curie with Hoffmann electrometer, 1934.

Source: Institut Curie, Paris.

has started research on the contested issues …").[134] C. Pawlowski, a visiting *boursier*, took on the task of assessing the reliability of the extant work on disintegration using the scintillation method.[135] In a series of papers published over the next few years, Pawlowski examined the disintegration of aluminium by polonium α-particles. The results supported Cambridge in some respects (such as the number and range of disintegration protons), and Vienna in others (the minimum energy of the α-particles necessary to produce disintegration).[136] Through 1930 and 1931, Pawlowski made a systematic comparison of the electrical (Geiger), scintillation and cloud chamber methods using protons from hydrogen, cellophane, paraffin and other materials in order to calibrate the instrumental responses to a known effect, much as Pettersson had done. Towards the end of 1931, Pawlowski returned to the artificial disintegration of the light elements, again using the scintillation technique.[137]

Pawlowski's experiments served only to strengthen Curie's faith in the scintillation method. This fact emerged at the meeting of the Société française de Physique in May 1931 at which de Broglie and Leprince-Ringuet showed off their first amplifier set. Since Rutherford's discovery of artificial disintegration in 1919, reported de Broglie:[138]

> subsequent experiments at Vienna by Pettersson and Kirsch, also by the scintillation method, have shown different results than those of Cambridge. [Pettersson and Kirsch] find in general that the elements are much more easily disintegrated than the English claim, and this important question has made little progress. … *This stagnation was due in large part to the uncertainties of observing scintillations, which is tiring and depends too much on the observer.*

Madame Curie, present at the meeting, congratulated Leprince-Ringuet on his accomplishment, but disassociated herself from de Broglie's criticism of the scintillation method, and firmly opposed the increasingly prevalent view that the method would have to be abandoned. The technique had, she said, "rendered great service," and the

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[134] Curie to Meyer, 9 November 1928, SMP.
[135] For Pawlowski, see I. to M. Curie, 9 March 1929, in Ziegler (ed.)(1974), 296; M. Curie (1930).
[136] Pawlowski (1929a, 1929b).
[137] Pawlowski (1930, 1931, 1932).
[138] de Broglie and Leprince-Ringuet (1931a), my emphasis.

disagreements between the various workers "cannot be attributed solely to this method, but to other observational and interpretative difficulties."[139]

But Curie's optimism was not widely shared. Within a couple of months, it would be completely shattered by the revelations from Columbia. It was with an air of finality that K.K. Darrow summarised matters in a 1931 review essay on "Transmutation." The Cambridge-Vienna controversy, he wrote:[140]

> was made peculiarly difficult to judge by the fact that for several years no-one outside of these two schools essayed to enter the field. Eventually, however, several did; the researches of Bothe and Fränz, of Pose, and of Pawlowski, spoke for the lower efficiencies of transmutation believed in by Rutherford, rather than the higher ones accepted at Vienna. Many studies of scintillations, many comparisons of the scintillation method with the other methods, have resulted from this controversy, and will probably be regarded in the course of time as its enduring good.

As Darrow's remarks suggest, the Cambridge-Vienna dispute, "one of the most famous controversies of modern physics,"[141] gradually petered out after 1931. With the revelations about the scintillation technique, on the one hand, and the introduction of new experimental and theoretical methods (with the consequent emergence of new phenomena like resonance disintegration) on the other, the issues at the core of the controversy lost their relevance. In that sense, the controversy *never* reached a definitive 'closure': the debate simply moved on, as it were, bringing new issues to the fore and rendering others irrelevant.[142] And with the multiplication of sites at which electrical counters, valve amplifiers, cloud chambers and so on were being deployed, the network of laboratories with the capacity to participate in the disintegration experiments increased from two to half a dozen. The community was being re-shaped. Within that community, de Broglie and the Joliots would be key players.

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[139] Curie, in discussion of Leprince-Ringuet (1931b).
[140] Darrow (1931), 641.
[141] *ibid.*
[142] Compare Stuewer (1985), 293-294.

5. "Synthetic Cosmic Rays": A New Nuclear Radiation

5.1 A Competitive Culture: Laboratory Secrets and the Sociology of the Visit

With the gradual reorientation of a number of researchers towards issues raised by the Cambridge-Vienna and Cambridge-Columbia controversies, and with the consequent emergence of a more extensive international network of laboratories addressing similar concerns with much the same kinds of material and conceptual resources, the style and character of experimental disintegration research changed in significant ways. A *competitive* and *reactive* inter-laboratory culture developed, in which each laboratory stood ready to seize upon, repeat and exploit observations made elsewhere to its own maximum advantage. In this agonistic context, small differences in instrumentation, technique or the strength of a polonium source could have important consequences for the credibility of an experiment and the certainty of its interpretation (we have seen, for example, how Chadwick remained uncertain of his experimental results on account of the weakness of his polonium source). While experimenters sought to exploit their strengths (and to mitigate their weaknesses) to their own benefit, however, an obvious tension arose between the powerful ideological norm of full disclosure in the literature (essential for the replication of results and the maintenance of credibility) on the one hand, and the desire to maintain secrecy so as to stay ahead of the field for as long as possible on the other. In such a situation, knowledge of the resources at competing researchers' command became essential to playing the 'disintegration game' successfully.[143]

These tensions and the emergent culture of secrecy were most forcefully brought out in the laboratory visit. While some of the skills and practices necessary for participation in the disintegration experiments were widely distributed in culture (the radio skills necessary for

143 Wilson (1983), 561, notes that in 1932 Rutherford swore Cockcroft and Walton "to strict secrecy" about the results of their proton acceleration experiments. For a characterisation of this competitive culture as a 'game,' see, for example, Abelson (1974). Some of the motives underlying a culture of secrecy are analysed by Bok (1982a, 1982b). For an interesting parallel in an earlier period and an introduction to the literature on secrecy in science, see Eamon (1990).

the construction of valve amplifiers, for example), others were much more local and tacit. One of the key strategies in the transmission of experimental techniques from site to site was therefore the visit.[144] In the summer of 1930, for example, Bruno Rossi, a young Italian physicist, arrived in Berlin to spend a few months in Bothe's laboratory. Using much the same kind of instrumentation as that deployed in the disintegration experiments, Bothe was continuing his collaboration with W. Kolhörster on the characterisation of the penetrating radiations from the atmosphere. Rossi had read Bothe and Kolhörster's work and became interested in the nature of the these radiations. He and a small group of co-workers at the Physics Institute of the University of Florence had improvised their own Geiger-Müller tubes and the associated electronics and had undertaken some preliminary research, but now wanted to know more.[145]

Rossi, who had also developed an electronic coincidence-counting circuit with a time resolution of 10^{-3} seconds (a distinct improvement on the device used by Bothe and Kolhörster[146]) found to his surprise that Bothe's Geiger counters were more stable than those he had constructed himself in Florence, the potential applied to the tube being less critical for its successful operation. Having extorted an oath of secrecy from Rossi, Bothe confessed that "my counters do not have a steel wire as advertised; they have an aluminium wire."[147] Clearly, there was more to the practical art of the Geiger-Müller counter (and to Bothe's management strategy) than met the eye.[148]

It was from Bothe's laboratory, too, that a new series of observations emerged in August 1930 which illustrate perfectly the operation of the fiercely competitive and reactive community I have just characterised. Bothe, also continuing his work on the disintegration

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144 For the importance of visits in the transmission of tacit and embodied skills, see Collins (1985), esp. 51-78, 129-157.

145 Rossi (1985); Rossi (1990), 10-11. See also Kolhörster to Schonland, 31 August 1929, BFSP.

146 On the early development of the coincidence counting technique, see Pfotzer (1985), and for Rossi's early experiments using the new technique, see Rossi (1985), 59-62; Rossi (1990), 13-14.

147 Rossi (1990), 16. See also Rossi (1930b).

148 De Maria and Russo (1985), 254. Rutherford to Schonland, 15 May 1929, BFSP, suggests that Bothe and Kolhörster had "jumped in" on Geiger's work. While at Bothe's laboratory, Rossi met P.M.S. Blackett and arranged for one of his young co-workers, Guiseppe Occhialini, to visit the Cavendish Laboratory to learn something about cloud chamber technique. Rossi's own programme of technical and conceptual learning continued into the 1930s. In 1932, for example, he visited de Broglie's laboratory to learn the technique of the proportional amplifier. See Leprince-Ringuet (1960), 301.

experiments with H. Becker, published an account of the hard nuclear γ-rays emitted during the disintegration of several light elements. In itself this was not surprising for, as we have seen, Gamow's theory led one to expect precisely such emission. With beryllium and boron, however, Bothe reported an unusual observation: the penetrating power of the excited radiation was of the order of magnitude of the hardest radioactive γ-rays.[149] Concentrating on boron, Bothe and Becker made a detailed investigation of the proton groups produced during disintegration by polonium α-particles. Bothe presented this work at a conference in Zürich in May 1931, the first of a series of gatherings that year at which the same small group of experimental and theoretical physicists came into frequent contact with each other and at which nuclear issues were discussed. Among those present at Zürich, for example, were Blackett, Gamow, and, significantly, Maurice de Broglie, Leprince-Ringuet and the Joliots.[150]

Reviewing recent work on the excitation of nuclear radiations, Bothe surveyed the excited γ-radiation yields from the light elements (fig. 5.10). Basing his interpretation explicitly on the work of Gamow, he undertook a detailed analysis of the mass-energy balance of the various possible modes of disintegration, rationalising the three proton groups from boron as follows:[151]

$$^{4}He + {}^{11}B = {}^{14}C + {}^{1}H \qquad \text{Group I}$$

$$^{4}He + {}^{10}B = {}^{13}C^{*} + {}^{1}H = {}^{13}C + \gamma + {}^{1}H \qquad \text{Group II}$$

$$^{4}He + {}^{10}B = {}^{13}C + {}^{1}H \qquad \text{Group III}$$

In Group II, α-particle bombardment produces an excited nucleus of a carbon isotope which reverts to a stable state by emission of nuclear γ-radiation. In the case of beryllium,

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149 Bothe and Becker (1930a, 1930b); Feather (1962), 138; Six (1987), 38-41. See also Bothe (1930) and Bothe and Becker (1930c, 1930d) for a related set of investigations. For comments on the electrical counters and the "Zählkrise" in Bothe's laboratory at this time, see Bothe to Schonland, 30 October 1930, BFSP.

150 For the proceedings of the meeting, see *Physikalische Zeitschrift* **32** (1931), 649-692; Six (1987), 43-44.

151 Bothe (1931); Six (1987), 43-44.

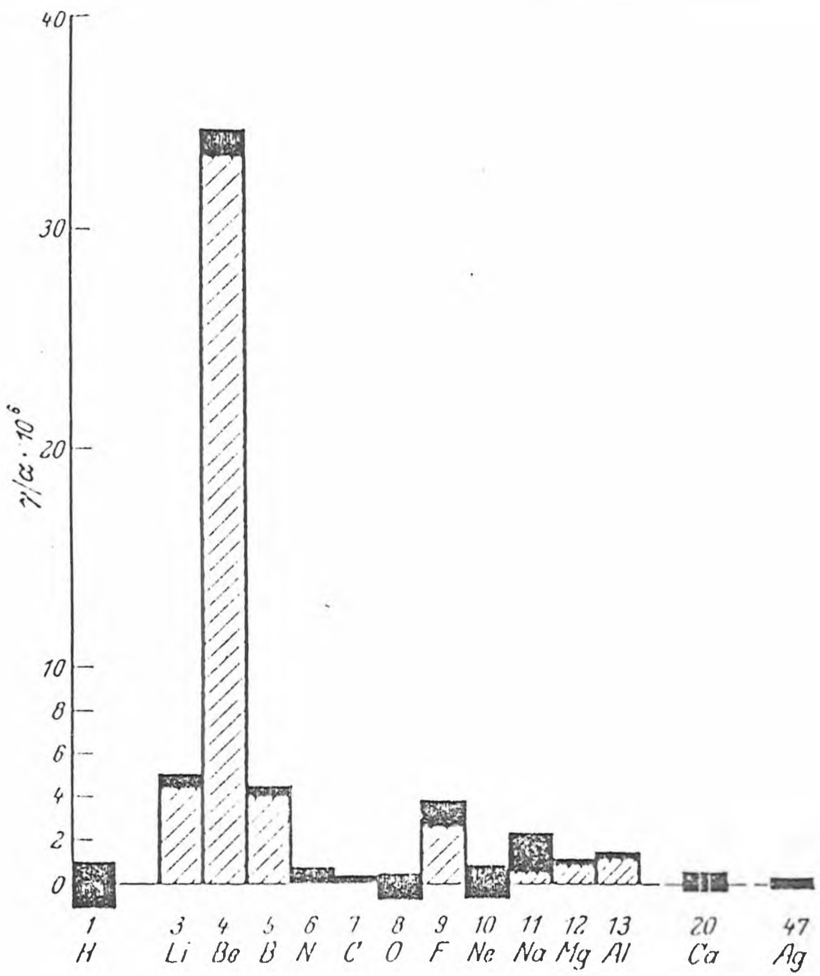

Fig. 5.10 Bothe's chart illustrating the excitation of nuclear γ-rays from light elements under bombardment by polonium α-particles.

Source: Bothe (1931), 662.

however, no disintegration protons were observed, but there was excitation of a similar γ-radiation which he therefore interpreted by the schema:

$$^{4}He + ^{9}Be = ^{13}C + \gamma$$

In other words, the α-particle must be captured whole by the beryllium nucleus in a synthetic process. Estimating the energy of the boron γ radiation as about 10^6 eV, Bothe emphasized the difficulty of characterising the weak nuclear γ-rays above background noise and, therefore, the provisional nature of his conclusions. Among his audience, however, there were those with the resources to take up and perhaps extend the experiments. When the Joliots returned to Paris, they immediately began to make plans to work on the new γ-radiation.[152] They prepared their strongest polonium source yet, a source of about 200 millicuries. Used with a simple ionization chamber, the source was sufficient to enhance the effect to the point where more reliable, more certain, measurements could be made.

In Cambridge, too, Bothe's results had attracted attention. Webster, now free after his disastrous involvement with the Davis-Barnes affair, undertook the task of repeating the German work, using the new polonium source recently prepared by Chadwick - the largest yet employed in the Cavendish, as I explained above.[153] Already familiar with the relevant technique from his first year's research, Webster repeated Bothe's work and made an estimate of the energy of the secondary γ-radiation, which turned out to be not more than 7 MeV. Webster also noted that the secondary radiation emitted in the direction of motion of the bombarding α-particles was more penetrating than that emitted in the opposite direction. This anisotropy was clearly strange for an electromagnetic radiation, and Webster began to wonder whether, in fact, the secondary radiation might consist of particles. Thinking that such particles would produce ionization tracks in a cloud chamber, Webster enlisted the help of F.C. Champion, another graduate student, to undertake a series

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[152] The Zurich meeting was held from 20-24 May. On 26 May, Irène Curie wrote to Rutherford seeking a testimonial for "an application for new means of scientific research that I will make in a few days." See I. Joliot-Curie to Rutherford, 26 May 1931, 5 June 1931, RP.

[153] Chadwick (1969), 72.

of expansion chamber experiments. No such tracks were found.[154] Webster presented his work at a meeting of the Kapitza Club on 7 July 1931, where he spoke on "The nuclear absorption with the gamma-rays produced artificially in Beryllium."[155] Soon afterwards, however, he took up a post at Bristol. In keeping with contemporary Cavendish practice, his results were not published.[156]

In September and October 1931, the great and the good of the physics world converged on London and Cambridge for a series of multiple scientific celebrations in honour of the British Association, Faraday's discovery of electromagnetic induction and James Clerk Maxwell.[157] As part of the Clerk Maxwell celebrations, Cambridge was besieged by physicists at the beginning of October, among them Bohr, Planck and Millikan.[158] And within weeks, yet another international gathering brought many of the same people together once again. This time, the setting was Rome. This meeting had a significance over and above that of the usual scientific conference or celebratory gathering, however: it signalled the re-orientation of another research group towards the study of the nucleus.

5.2 October 1931: The Volta Conference at Rome

From the time of his arrival in Rome in 1926 until the end of the decade, Enrico Fermi and his small group of researchers - Eduardo Amaldi, Franco Rasetti, Bruno Pontecorvo, Ettore Majorana, Emilio Segrè and Oscar D'Agostino - had worked on spectroscopy and atomic physics.[159] In September 1929, Orso Mario Corbino, senator, Professor of experimental physics at Rome and Fermi's scientific patron, told the Società Italiana per il Progresso delle Scienze that the Rome group would begin research in a new field: the artificial transmutation of the atomic nucleus. In a speech entitled "The New Goals of Experimental

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154 Webster (1932), 440. Chadwick was away on holiday when these experiments were carried out. He returned to find that Webster and Champion had discarded the trackless photographs. See Chadwick (1969), 71. On Champion, see Roqué (1992).
155 Kapitza Club Minute Book, 7 July 1931, CKFT 7/12, JDCP.
156 *Clare [College] Association Annual 1979-80*, 79; Feather (1962), 139-140.
157 Rutherford (1931b); Howarth (ed.)(1931); Eve (1939), 346-349.
158 See "Clerk Maxwell Celebrations, 1931," CAV 11/3, CUL.
159 Fermi (1928); Segrè (1970), 65 and *passim*; Holton (1978), 163-164.

Physics," Corbino pointed out that "while great progress in experimental physics in its ordinary domain is unlikely, many possibilities are open in attacking the atomic nucleus." "This," he affirmed, "is the most attractive field for future physicists."[160] It was also an opportunity for Italy to "regain with honour its lost eminence" in physics. The attack would take place under Fermi's leadership.

With this new objective in their sights, the Rome group set out carefully and deliberately to acquire the instruments and the knowledge, practical and conceptual, which would enable them to gain a foothold in the field of atomic transmutation. In order to acquire the requisite practical skills, the social technology of the visit again proved critically important. Members of the group made "expeditions" to the laboratories of established workers, continuing a trend started a year or two earlier by Rasetti's sojourn at Millikan's Caltech laboratory and Segrè's visit to Zeeman in Amsterdam. Early in 1931, Franco Rasetti travelled to Lise Meitner's laboratory at the Kaiser Wilhelm Gesellschaft in Berlin, where he learned how to build a cloud chamber, to make Geiger counters and, above all, to prepare the all-important polonium sources necessary for the disintegration experiments.[161] Emilio Segrè's trip to Stern's laboratory in Hamburg, and Eduardo Amaldi's to work with Debye at Leipzig, were less directly concerned with radioactivity, though they served as an excellent introduction to other important forms of laboratory practice, especially vacuum technique. As Segrè put it, the plan was "that we would all go to a place where you learn a new experimental technique, and bring them all back ... with an eye to enlarging our field ... And so then we would have more variety, more freedom."[162] The acquisition of experimental technique was complemented by extensive study of the literature on the nucleus. In the autumn of 1931, Amaldi gave a series of seminars in which Rutherford, Chadwick and Ellis' *Radiations from Radioactive Substances* was read and discussed.[163] All in all, then, the Rome group had, by late 1931, taken significant steps towards

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160 Quoted in Holton (1978), 165.
161 Rasetti subsequently became a Research Associate at Columbia University, and produced an influential book, *Elements of Nuclear Physics* (Rasetti (1937)). See also Rasetti (1932a, 1932b).
162 Quoted in Holton (1978), 167.
163 Holton (1978), 166.

establishing itself in the rapidly expanding field of transmutation, bringing to six the number of laboratories actively working in the field.[164]

The group inaugurated its new research programme by hosting a major international conference to which all those already working in the nuclear and related fields were invited.[165] Motivated by many of the same nationalist aspirations which had inspired the Como Congress of 1927 (the opening reception was presided over by the Duce Mussolini himself) and sponsored by the Reale Accademia d'Italia, the meeting took place in Rome between 11 and 17 October. Nine countries were represented. Among the participants (fig. 5.11) were Bothe, Geiger, Meitner, Millikan and Marie Curie, while from the ranks of the theoreticians came Bohr, Heisenberg, Pauli and Sommerfeld, as well as the large Italian contingent. The largest single group represented Cambridge, whence came Aston, Blackett, Ellis and, of the theoreticians, Fowler and Mott.[166]

While the conference provided another forum for the theoreticians to express their continuing confusion over the wave mechanics of the nucleus, little new emerged. As *Nature* reported it, "[a] considerable part of the conference was devoted to the discussion of the general applicability of our present theoretical ideas to nuclear problems, and it appeared, largely through the important contributions of Prof. N. Bohr, that we cannot expect the present quantum mechanics to apply to the nucleus without undergoing such a fundamental change that it might almost be said to involve a new mechanics, including the present quantum mechanics as a limiting case." The difficulty was not simply one of the practical application of wave mechanics to the nucleus, however: that, after all, had been a problem for the previous three years. The broader issue of the relationship between experiment and mathematical theory was also involved, for "it appears difficult to calculate

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164 i.e. Cambridge, Vienna, Halle, Paris (de Broglie), Paris (Joliot-Curie) and Rome. One might also include New York, where Dunning was beginning to introduce electrical counting methods at Columbia, and Washington D.C., where a group under Merle Tuve were beginning to establish themselves in nuclear research (see following chapter).
165 For informative accounts of the conference and the background to it, see Weiner (1974); Holton (1978). The proceedings appeared in 1932 as *Atti dei Convegno di Fisica Nucleare* (Rome: Reale Accademia d'Italia, 1932).
166 It is surprising that neither Rutherford nor Chadwick attended. Rutherford had earlier expressed his interest: see Lodge to Rutherford, 21 May 1931; Rutherford to Lodge, 23 May 1931, OLP.

Fig. 5.11 Participants at the Rome Congress, October 1931. In the front row, left to right: Richardson, Millikan, Curie, Marconi, Bohr, Aston, Bothe, Rossi.

Source: Hendry (ed.)(1984), 23.

the probabilities of occurrence of the different nuclear processes to within even an order of magnitude, so that *it is quite impossible to decide whether there is or is not a discrepancy between theory and experiment* in comparing such experimental results as the number of long-range α-particles and the number of quanta of the corresponding radiation emitted by the excited nucleus."[167]

Bohr began to develop a strategy which might explain away some of the difficulties facing nuclear researchers. He had been sceptical of the conservation of energy for some time. In May 1930 he had told the Chemical Society of London that "as soon as we inquire into the constitution of even the simplest nuclei, the present formulation of quantum mechanics fails essentially," being unable to explain even "why four protons and two electrons hold together to form a stable helium nucleus."[168] Recognising that "a departure from the law of conservation of energy would involve very strange consequences," Bohr nevertheless insisted that "the essential stability of atoms [is] an implicit assumption in the whole classical description of natural phenomena, and we cannot therefore be surprised if classical concepts fail in accounting for their own foundation." Bohr concluded on a pessimistic note: "Just as we have been forced to renounce the ideal of causality in the atomistic interpretation of the ordinary physical and chemical properties of matter, we may be led to further renunciations in order to account for the stability of the atomic constituents themselves."[169]

Cambridge's Charles Ellis likened the situation to that which had originally given rise to wave mechanics. At first, he commented, "it appeared that the wave mechanics was precisely the theory required to deal with nuclear problems." In the fullness of time, however, this had come to seem "an unduly optimistic view, since there are at least two phenomena presented by the nucleus which resist the attacks of the wave mechanics to just the same degree as intensity problems did the old quantum dynamics or the emission of light spectra the classical theory." "It may be," he concluded, "that only a slight extension

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167 In other words, the "theoreticians' regress" surfaced again. See "The Volta Conference at Rome," *Nature* **128** (1931), 861, my emphasis. See also M. Curie to I. Curie, 13 October 1931, in Ziegler (ed.)(1974), 336.
168 Bohr (1932b), 379.
169 Bohr (1932a), 130.

of the theory will be required, but we must not lose sight of the possibility that the explanation of these two phenomena may require a new theory which will absorb the wave mechanics just as the wave mechanics absorbed the quantum theory."[170] Again, then, the character, content and adequacy of wave mechanics were bound up with the availability and reliability of experimental data.

As far as experimental data were concerned, most of the "nuclear phenomena which are open to experiment" were also discussed at length, including nuclear moments, the artificial and natural disintegration of the elements, the absorption of radiation by the nucleus, the stationary states of α-particles in the nucleus and their relationship to the γ-rays, "and also the transference of energy from the excited nucleus to the electronic structure." Nuclear electrons appeared "to introduce problems which are not found elsewhere in physics," a point emphasised in the discussion on the continuous spectrum.[171] "The general impression seemed to be," reported *Nature*, "that a definite stage in attacking the problem of the nucleus had already been reached in that recognized experimental methods had been adopted, and the general scope of the information they could provide was understood," even if the information itself remained vague, confusing and sometimes contradictory.[172] Ellis expressed the situation succinctly: "Experimentally it is not easy," he lamented, "but in our present absence of knowledge almost any measurement is of value."[173]

5.3 Waves, Particles and "Artificial Cosmic Rays"

Ellis' pessimism was offset by a lively debate about the nature of the atmospheric penetrating radiations. During 1930 and 1931, fuelled by Millikan's ever more vocal pronouncements on the subject, cosmic rays (a term coined by Millikan in 1925), höhenstrahlen or penetrating rays, as they were variously known, had come increasingly to

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170 Ellis (1931), 607-608.
171 "The Volta Conference at Rome," *Nature* **128** (1931), 861.
172 *ibid.*
173 Ellis (1932a), 117.

occupy the attention of experimental physicists.[174] Cosmic rays were known to have a penetrating power far in excess of any other known radiation (the next most powerful radiations were γ-rays from radioactive sources). Since the penetrating power of γ-rays was expected to increase steadily with their energy, Millikan interpreted cosmic rays as high energy γ-rays. His claim that cosmic rays were the sign of the synthesis of elements in space, or, as he more dramatically put it, the "birth cries of the elements," was greeted with scepticism by many scientists, but was widely reported in the press.[175] For Millikan in the late 1920s, the atom building hypothesis was a confirmation of his faith in the evolution of the elements, a way of avoiding the "heat death" of the universe and "a little bit of experimental finger-pointing" towards a creator who was "continually on his job."[176]

Millikan's interpretation was contested by several researchers, among them Bothe and his erstwhile student Bruno Rossi, who both argued for a corpuscular interpretation of the radiations on the grounds that the measured energy of secondary particles produced by the cosmic radiation far exceeded that required by Millikan's interpretation.[177] This was the first time the various proponents of the corpuscular theory had presented their views in such a united way, and it marked the beginning of what was to be a protracted debate over the

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[174] See, for example, the important "Discussion on Ultra-Penetrating Rays," *Proceedings of the Royal Society* A **132** (1931), 331-352.

[175] Millikan (1924, 1925, 1930); Kargon (1982), 122-150, esp. 139-141; Kargon (1981, 1983). See also Brown and Hoddeson (eds.)(1983); Sekido and Elliott (eds.)(1985). The recent studies of De Maria and Russo (1985, 1987) and De Maria, Ianello and Russo (1991) promise to shed new light on our understanding of early cosmic ray research.

[176] Millikan (1931), 170; Kargon (1982), 144.

[177] Rossi (1931, 1932); Bothe (1932b). Rutherford had visited Bothe in the spring of 1929, and told B.F. Schonland:

> I saw Bothe and Kolhörster and their experiments which are excellent. Geiger has done very much the same thing but between ourselves they have rather jumped in on his method. Personally I am very doubtful whether Bothe can definitely settle the question that the rays are corpuscular. We have made calculations on this point on the idea of waves à la Millikan and it looks to me difficult to decide the question at once. However Bothe seems very confident but you can quite understand there is a good deal of psychology in their attitude ...

See Rutherford to Schonland, 15 May 1929, BFSP. See also Rossi (1981, 1985); Kargon (1982), 142-144; Rossi (1990), 17-21. The extent to which the different interpretations reflected the different kinds of instruments used by Millikan (electroscopes) and Bothe-Rossi (coincidence counters) remains to be examined. Compare De Maria and Russo (1985, 1987); Six (1988); De Maria, Ianello and Russo (1991).

nature of the penetrating radiation.[178] More to the point for my argument, however, that debate also provided a context in which Bothe's anomalous excited nuclear γ-radiations could be interpreted and given meaning.

The day after Rossi's attack on Millikan, Bothe gave a full account of his own work on the artificial nuclear γ-radiation, surveying the processes and energetics involved in the artificial disintegration of different isotopes.[179] It was suggested (apparently by Millikan) that Bothe's penetrating nuclear γ-rays might be compared experimentally with the cosmic rays "in order to discover whether their properties would help to explain each other's nature."[180] In the autumn and winter of 1931, many commentators linked Bothe's rays with the cosmic rays.[181] *Science News*, for example, reported in December that Bothe and Becker had discovered "[a] successful though inefficient method of tapping the energy of the atom nucleus to obtain synthetic cosmic rays."[182] The produced a remarkable diagram illustrating the production of these "artificial cosmic rays" (fig. 5.12), though they also published an interview with Bothe in which he repudiated such an interpretation of the nuclear γ-radiation. His experiments, he observed carefully, had shown that while their penetrating power approached that of the cosmic radiation, the γ-rays from beryllium "still behave completely like a normal gamma radiation and quite differently from the cosmic rays, ... further strong support for the idea that the cosmic rays have a particle-like nature in the lower layers of the atmosphere."[183]

Following the Rome conference, Millikan himself continued to promulgate his views during a European tour. Speaking at the Institut Henri Poincaré in Paris on 20 November, and at the Kapitza Club in Cambridge three days later, Millikan displayed a series of recently obtained cloud-chamber photographs which had just been sent to him by C.D.

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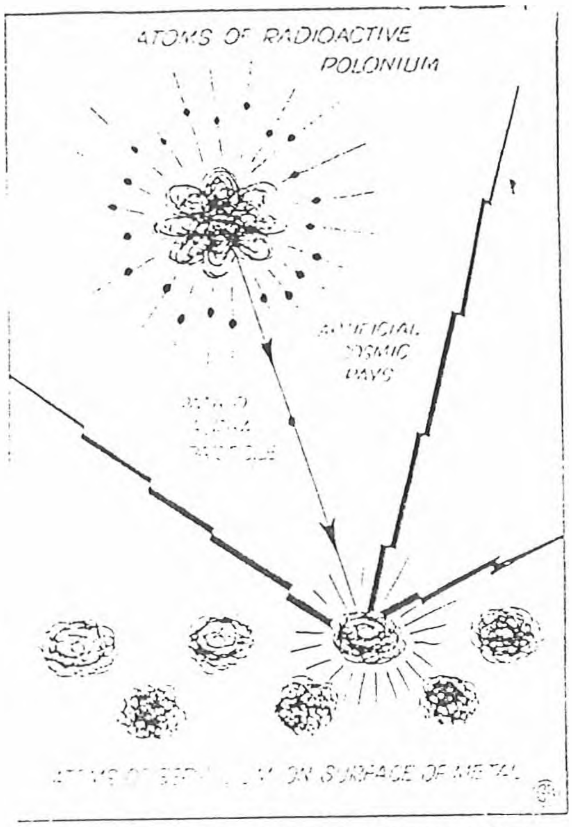
178 Cassidy (1981); Rossi (1981, 1985); Kargon (1982, 1983); De Maria and Russo (1987); Brown and Rechenberg (1991); De Maria, Ianello and Russo (1991).
179 Bothe (1932a).
180 Crowther (1932a). Compare Millikan (1931); Millikan and Bowen (1931).
181 See, for example, "Artificial Gamma Radiation Approximates Cosmic Rays," *Science News Letter*, 21 November 1931, 323; "Cosmic Rays Again Shown Like Radium Gamma Rays," *ibid.*, 19 December 1931, 392.
182 "Some Advances in the Sciences During 1931," *Science Supplement* 10 (25 December 1931), 5.
183 "German Physicist Interprets Experiments With Cosmic Rays," *Science News Letter*, 12 March 1932, 159-160, on 160; Bothe (1932b).



POWERFUL BOMBARDMENT

This diagram shows how radioactive polonium sends out showers of speeding alpha particles, of which only one in a great number scores a hit on a beryllium atom and thus sets up a cosmic radiation.

Fig. 5.12 'Artificial Cosmic Rays.' Science News' interpretation of Bothe's experiments on the production of nuclear γ-radiation from beryllium by α-particle bombardment.

Source: "Tapping of the Atom's Energy Achieved in New Experiment," *Science News Letter*, 12 March 1932, 159.

Anderson, his student and cosmic-ray collaborator at CalTech.[184] According to Anderson, some new facts stood out "quite definitely" in these photographs, chief among them the apparent ejection of high-energy protons and electrons from nuclei by cosmic rays - providing the "first scientific evidence that electromagnetic radiation can disrupt the innermost structure of matter."[185] These photographs made a deep impression upon Millikan's audiences, as I shall now show.

6. "A new kind of ray"?: The Neutron

6.1 Paraffin, Protons and the J-Effect

Within days of the Rome conference, the Joliots had taken up work in earnest on the Bothe radiation.[186] They had been accumulating polonium for some time. Early in November 1931, they and their new assistant Pierre Savel prepared a fresh polonium source of some 100 millicuries strength.[187] As we have seen, the new source could have been divided among the researchers in the laboratory, but the Joliots decided, presumably with the blessing of *la patronne*, to keep it concentrated in one powerful mass in an attempt to elicit new and potentially interesting results.[188] It gave them a decisive advantage over other laboratories, and they knew it. They installed a Hoffmann electrometer similar to that used by Pose, one of the first such instruments in France.[189] Before beginning attempts to repeat

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[184] Anderson to Millikan, 3 November 1932, RAMP; Millikan (1932); Minutes of the Kapitza Club, CKFT 7/2, JDCP; Hanson (1963), 140-141; Anderson and Anderson (1983); Skobeltzyn (1983), 114. Six (1987), 75-76, and Hendry (ed.)(1984), 22, correctly note the significance of Millikan's visit to Cambridge.
[185] Anderson to Millikan, 3 November 1932, RAMP; "Disintegration of the Atomic Nucleus by Cosmic Rays,"*Science Supplement*, 11 December 1931, 10; Feather (1962), 140; De Maria and Russo (1985), 243-244. See also J. Boyce to Cockcroft, 8 January 1932, JDCP; Auger and Skobeltzyn (1929).
[186] See Skobeltzyn to Joliot, 17 October 1931, IFJCP.
[187] Joliot-Curie notebook, 4 November 1931, IFJCP; Pflaum (1989), 275-276, 289-290. On Savel, see also Goldsmith (1976), 39-41.
[188] Joliot-Curie notebooks, November 1931, IFJCP; Pflaum (1989), 275, 288. Weart (1979), 41 and 298 n.7, quotes G.C. Wick, who stressed what Joliot had told him about keeping the polonium concentrated.
[189] Pflaum (1989), 288-289. The instrument took more than a month to instal, the difficulties being worsened by the breakage of the thread holding the mobile needle.

the experiments of Bothe and Becker, however, they carried out a series of experiments which aimed to characterise the radiations from the polonium source. On 25 November, they began to measure the total and individual ionisation currents produced by the γ rays of polonium and the excited γ-rays of beryllium in order to estimate the correction needed to take account of the ionisation due to the source.[190]

In December, the Joliots began to work once more on the penetrating radiation from beryllium. At first, they made measurements of the ionisation current with various sources and screen arrangements at a series of pressures, arriving at overall figures for the intensity of the beryllium radiation. They attempted to characterise the radiation by a series of tests of its behaviour under changes in the experimental conditions - demonstrating, for example, that a sheet of paper placed between the polonium source and the beryllium hindered the action of the γ-radiation.[191] Keeping Dmitri Skobeltzyn, who had spent some time in Paris (and who had also attended the 1928 conference) informed, Joliot remarked briskly that "Mme. Joliot and I have been rather busy with experiments concerning the phenomenon discovered by Bothe and Becker of the production of penetrating gamma rays issuing from light nuclei when they are bombarded with alpha particles. We have discovered new and interesting results ...."[192]

In the following weeks, other light elements were subjected to bombardment by polonium α-particles and examined for signs of the penetrating Bothe γ-radiation.[193] It became apparent from these investigations that polonium α-particles acting on beryllium produced a γ-radiation about five times as intense as that from polonium itself - a figure much greater than that originally claimed by Bothe and Becker, but one more in line with the results recently announced by Bothe at the Rome congress.[194] By extrapolation, a value

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190 Joliot-Curie notebook, 25-28 November 1931, file 13/3, IFJCP. From these experiments it became clear that the radiations from the polonium source were far more complex than they had expected. The next fortnight was therefore spent recalibrating the polonium source.

191 Joliot-Curie notebook, 2 December 1931, file 13/3, IFJCP.

192 Joliot to Skobeltzyn, n.d., but ca. December 1931-January 1932, quoted in Biquard (1965), 39.

193 In general, these experiments attempted to determine the energy of the emergent radiation by finding out how much of a suitable absorbing material (usually lead) was required to stop it. The results would then be compared with the stopping curve for γ-rays of known energy such as those from ThC". See I. Curie (1931a, 1931b).

194 I. Curie (1931b).

of 15-20 MeV was obtained for the energy of the beryllium radiation, indicating that it belonged to "a region of energy intermediate between the γ rays of the radioelements and the least penetrating cosmic rays."[195] Under similar conditions, boron was found to produce a penetrating radiation of energy about 11 MeV, while lithium yielded radiation of energy 0.6 MeV. These results were quickly written up and presented to the Académie des Sciences on 28 December 1931, a temporary "marker" of their achievements on the new radiation, lest other laboratories should be working along the same lines.[196]

In their accounts the Joliots saw no reason to differ from Bothe's tentative explanation for the origin of the penetrating γ-radiation: the beryllium nucleus captured an α-particle forming a new isotopic nucleus, ^{13}C, with a lower overall energy than the original nucleus. The difference in the binding energies of the nuclear constituents was then released in the form of the penetrating radiation. Well aware of the many problems attending such an explanation, however, Joliot added an important caveat to his paper, stressing the tentativeness of the interpretation. This caution was echoed in January 1932 in a letter from the theoretician Guido Beck, then working in Leipzig with Werner Heisenberg. Beck had taken a great interest in the Joliots' paper in the December *Comptes Rendus*, but pointed out that "grave difficulties" surrounded the interpretation of their experiments:[197]

> Your suggestion on the origin of the γ radiation of Li-6 and B-10 is fascinating. I have had occasion to discuss the beryllium radiation with M. Chadwick and recently in Rome with M. Bothe. There is no possibility of explaining this effect on existing theory. Theory would always give an intensity too small by a factor of about a million, if we neglect the unknown influence of the nuclear electrons. It is so much the more important to have information on the appearance of this entirely new effect in other elements.

At the same time, Beck pointed out that experiments by G.T.P. Tarrant and L.H. Gray (of the Cavendish Laboratory), Meitner and others had shown the need to modify the Klein-Nishina formula (used to model the interaction between quanta and matter) to take account

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[195] I. Curie (1931b). The same conclusion was reached by Joliot in his study of the penetrating radiation produced in boron: Joliot (1931b).
[196] I. Curie (1931b); Joliot (1931b).
[197] Beck to Joliot, 20 January 1932, IFJCP.

of the role of nuclear absorption.[198] At a time when many theoretical ideas were beginning to be doubted, the Joliots can have hoped to offer nothing more than a very tentative rationale for their findings.

Returning to the artificial γ-ray work after Christmas, the Joliots began to make further modifications to the experimental arrangement in the hope of obtaining a better characterisation of the penetrating rays. Following Millikan's interpretation of Anderson's cloud chamber photographs, in which cosmic rays seemed to disintegrate nuclei to produce protons and electrons, the Joliots fitted their ionization chamber with a window, near which they placed a variety of materials in order to see whether the beryllium radiation produced secondary protons. They found that only hydrogenous substances - cellophane, paraffin and so on - yielded protons, whose maximum energy turned out to be about 4.5 MeV. Joliot dashed off a letter to Beck, reporting the new phenomenon:[199]

> In following up our experiments ... we have brought to light the following phenomenon: Bodies containing hydrogen emit H-rays under the action of the γ of Be and B (for γ_{Be} range in air 26 cm approx., for γ_B range in air 8 cm approx., experiments done with paraffin) ... I have been able to photograph the trajectories of these protons in the Wilson apparatus. We have supposed that the protons are emitted by a Compton process [?] You will find the first details of these experiments in a note in C[omptes] R[endus] which should appear next Monday.

Beck, meanwhile, continued to reflect on the Joliots' earlier experiments, becoming "more and more convinced" that the work was "of the greatest importance for the theoretical treatment of these phenomena":[200]

> Since the γ radiation from beryllium cannot be explained on a model composed only of heavy particles [protons and α-particles] it is necessary that we consider the nuclear electrons. The picture I would propose for this effect would be: The α-particle is retained in the nucleus of Be forming C^{13} - exactly as you have described - exciting a nuclear electron in a higher stationary state, which is in turn strongly

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[198] For a discussion of this issue, see Brown and Moyer (1984); Roqué (1992).
[199] Joliot to Beck, 23 January 1932, IFJCP. See also Six (1987), 62-65.
[200] Beck to Joliot, 4 February 1932, IFJCP. For the problems of the nuclear electron hypothesis in the early 1930s, see Stuewer (1983).

> coupled with the electromagnetic field of the radiation to emit a γ photon before it has time to re-emit the captured α particle ... I have made some sketchy calculations from which it appears that the numerical values in the explanation of the experimental facts are quite reasonable. In particular ... we now have good reason to speak of excited stationary states of nuclear electrons too. Following this idea one must suppose that the effect is only produced when the energy liberated by the absorption of an α-particle is about equal to the energy of excitation of the nuclear electron, which would give something of the impression of a resonance character for a certain speed of the incident rays. Bothe and Chadwick have results of this kind and it would be of great importance to check and confirm this result, showing directly the discrete character of the electronic states in the nucleus.

Beck's rough calculations yielded results for the energy of the beryllium γ rays of the order of 40 million volts which, considering the provisional character of his model, he found "sufficiently in agreement with your results. Previously I had had great difficulties since M. Chadwick gave me a value of about 5 million volts, which would have made the difference too big."[201]

Beck's suggestions arrived too late, however. On 18 January 1932 the Joliots had presented a joint paper to the Académie, in which they made public the most recent work. Although they again acknowledged the great difficulty involved in interpreting the phenomena, they took advantage of their prerogative as leaders in the field to suggest a tentative explanation for their results:[202]

> If we suppose that the photons can transmit part of their energy to protons by a process analogous to the emission of electrons projected by the Compton effect, we find that the energies of the radiations of Be and B would be of the order of 50.10^6 and 35.10^6 eV respectively. The great difference between these numbers and the ones we have given (15 to 20.10^6 and 11.10^6 eV) is insufficient reason to reject this hypothesis, given the considerable errors in the evaluation of the quantum energies of the penetrating rays.

This explanation was not the arbitrary (or, worse, incorrect) one that later commentators have assumed it to be. As I have shown, the Joliots had good reason for supposing the

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201 Beck to Joliot, 4 February 1932, IFJCP.
202 I. Curie and Joliot (1932a), 274 [CWJC 360].

existence of a completely new kind of effect, a novel form of interaction between radiation and matter. The Compton effect was less than a decade old, and it was not unreasonable to suppose that another form of it, applicable to heavier particles than electrons, might exist.[203] Within a month, the Joliots had generalised the phenomenon and designated it the J effect.[204]

The recent speculations of Millikan seemed to provide a reasonably sound basis for such an interpretation.[205] And it was with Millikan's seminar in mind that the Joliots planned their next round of experiments. *If* the beryllium radiation were akin to cosmic rays, then the cosmic rays, too, should be able to scatter protons from hydrogenous substances like paraffin ("whatever the interpretation one may give this phenomenon," they noted, "it is probable that it takes place for all radiations of high quantum energy, in particular for cosmic rays, if they are electro-magnetic in nature").[206] In order to pursue this consequence of their interpretation, the Joliots set in train a request to make further investigations at a recently-opened high-altitude research station on the Jungfraujoch in Switzerland, on the grounds that:[207]

> We have found that the very penetrating γ rays excited by the nuclei of some light elements (beryllium, boron) by polonium α-particles can project high-speed protons when they pass through hydrogen-containing bodies. *We believe that cosmic rays must [likewise] project hydrogen nuclei and probably also the nuclei of heavier atoms. If this is so, this new phenomenon would play an important role in the study of cosmic rays*; in order to produce evidence of it, it is advantageous to work at high altitude, where the intensity of the cosmic radiation is much greater.

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[203] On the Compton effect, see Stuewer (1975). For the Joliots' appropriation and extension of it, see Six (1987), 76.
[204] Curie and Joliot (1932b); Feather (1962), 143.
[205] Curie and Joliot (1932a), footnote 2. Compare Feather (1962), 140. Six (1987), 75, suggests that Marie Curie had suggested the Compton effect interpretation. In that regard, see M. Curie (1926).
[206] Curie and Joliot (1932a), 275 [CWJC 360].
[207] I. and F. Joliot to Lacroix, 5 February 1932, IFJCP, my emphasis. See also Maurain to I. and F. Joliot, 3 February 1932; Hess to F. and I. Joliot, 26 January, 26 February 1932; Weyss to I. Joliot, 7 April 1932; Solomon to Joliot, 2 May 1932, IFJCP. For a good description of the facilities of the station, see "The Jungfraujoch Scientific Station," *Nature* **128** (1931), 817-820. See also "Hochalpine Forschungsstation Jungfraujoch," copy in IFJCP; Korff (1985).

At the end of February, only a few weeks before they were due to leave for the Jungfrau, however, the Joliots heard that their tentative interpretation had been contested. The rebuttal came from the Cavendish Laboratory. From Bohr's Institute in Copenhagen, Joliot's friend Jacques Solomon wrote ironically:[208]

> I have read with interest what you say about "neutrons" in your recent papers, and a recent trip to Cambridge has made me, for the moment, a believer in the new idea. I made some calculations to see whether one can interpret the experimental results with a simple neutron model, and I found serious difficulties. Certainly these neutrons, if they exist, have curious properties vis-à-vis electrons. One can easily interpret their action on nuclei, but with the electrons there are great difficulties.

6.2 The "Possible Existence of a Neutron"

At the Cavendish Laboratory, Chadwick and his colleagues had been following developments in Europe closely, though not slavishly. In the autumn of 1931, Chadwick had commenced a second round of disintegration experiments in order to investigate sceptical reports on Pose's experiments from Meitner[209] and de Broglie and Leprince-Ringuet,[210] while Webster continued his work on the Bothe-Becker γ-ray experiments in the contamination-free Solar Physics Observatory.[211] During 1931, Chadwick also enlisted a visiting German researcher, Wolfgang Riezler, to look for evidence of α-particle resonance levels. Riezler had spent much of his year in Cambridge investigating α-particle scattering by the scintillation method,[212] and had then designed and partially constructed an apparatus

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208 Solomon to Joliot, 29 February 1932, IFJCP, ironic quotes nicely signifying Solomon's rapid reinterpretation of the phenomena described in the Joliots' papers.

209 Made public at the Zurich meeting in May. See *Physikalische Zeitschrift* **32** (1931), 661; Chadwick and Constable (1932), 48. For Chadwick's own brief description of his research goals at this time, see Chadwick (1931).

210 de Broglie and Leprince-Ringuet (1931b).

211 Webster (1932), 453.

212 Riezler (1931); Duncanson (1984), 92. It should be remembered that there existed in the Cavendish at this stage only two linear amplifiers - those of Wynn-Williams and Chadwick. Supervised by Chadwick, Riezler's was, I believe, the last use of the quantitative scintillation technique in the Cavendish, for in the next year or two, several more amplifier sets were constructed (see Lewis (1979)).

to detect α-particle resonance levels in aluminium by large-angle scattering.[213] When he returned to Germany, the experiment was taken over by Chadwick and W.E. Duncanson, a second-year research student. Despite Chadwick's preparation of a strong new polonium source, however, the experiment was not successful due, it seems, to the geometrical arrangement of the apparatus. Disappointed, Chadwick resumed his disintegration experiments with Constable.[214]

For this new series of investigations, Chadwick and Constable (Pollard had meanwhile taken up an Assistant Lectureship at Leeds[215]) modified their apparatus in two ways. First, by using "new materials for the ionisation chamber" and by "assembling it outside the laboratory,"[216] the Cambridge investigators were able to reduce the natural effect of the ionisation chamber by a factor of about 5 and therefore to improve the sensitivity of the instrument. Secondly, and more significantly, by deploying "much stronger polonium sources than in the earlier experiments," they were able to "improve the geometrical conditions" of the experiment "to a sufficient degree to permit the resolution of the proton groups emitted by fluorine and aluminium in their disintegration."[217]

Derived from the materials Feather had secured from the Kelly Hospital, Baltimore, and originally prepared for the Riezler experiment, the strong new polonium source was clearly fundamental to the success of the enterprise, for the earlier experiments had yielded "only a weak indication of resonance" which was difficult of explanation, while the latest round of trials gave greatly increased resolution of the disintegration proton groups.[218] With the new

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213 Duncanson (1984), 92.
214 *ibid.*, 93.
215 Personal communication. While at Leeds, Pollard contributed a number of papers to the literature on artificial disintegration, Chadwick having given him eight ampoules from the Baltimore material. See Pollard (1931, 1932, 1933a, 1933b, 1933c). See also Pollard and Davidson (1942); Pollard (1982).
216 Chadwick and Constable (1932), 49-50.
217 Chadwick and Constable (1932), 50, acknowledging the generosity of Drs. Burnam and West of the Kelly Hospital, Baltimore.
218 The key function served by the new polonium was to give much stronger effects i.e. effects well above the "noise" of the electrical counters. The more one could raise an effect above background, the more certain one could be that one was seeing a genuine phenomenon. Certitude was clearly linked to the strength of the polonium source one could muster. In a context of radical uncertainty, obtaining a strong polonium source was therefore crucial to the success of the experimental enterprise (I am grateful to Prof. E.C. Pollard for information on this point). On the co-definition and simultaneous emergence of 'background noise' and 'phenomenon,' compare Trenn (1986); Galison (1987), 255-257 and *passim*.

polonium source and an oscillograph (or string galvanometer with photographic record), Chadwick and Constable now found that under the impact of polonium α-particles, aluminium yielded eight distinct groups of protons, which they interpreted as being due to "penetration of the α-particles through four resonance levels, each level giving rise to a pair of groups."[219] Fluorine, on the other hand, gave six proton groups, with less concrete evidence for the existence of two (or possibly three) resonance levels. They estimated the width of the resonance levels by determining the maximum and minimum velocities which the bombarding α-particle must have to liberate the corresponding proton groups, finding that in the case of aluminium, for example, the levels had a width of some 250,000 eV.[220] They also verified their earlier assumption that α-particle capture might occur in two distinct ways, the first corresponding to the "emission of a proton of the shorter group and the formation of an excited nucleus, which must later change to the stable nucleus corresponding to the second type of capture, in which a proton of long range is emitted." The energy released in this transformation of the excited nucleus would, of course, be emitted as γ-radiation, tying in the disintegration experiments with the recent work of Bothe, Becker, Riezler and the as-yet unpublished experiments of Webster.

When the Joliots' December paper arrived in Cambridge in mid-January, it evidently caused some alarm, for a full account of Webster's work was hastily dispatched to the *Proceedings of the Royal Society*.[221] Entitled "The Artificial Production of Nuclear γ-Radiation," Webster's paper offered a robust defence of the notion that the beryllium radiation was "electromagnetic in nature," especially in the light of suggestions "that cosmic radiation may consist of protons, or neutrons, and the usefulness of the conception of neutrons in accounting for astrophysical and nuclear phenomena." His cloud chamber experiments with Champion, after all, had failed to reveal any significant evidence of a proton-neutron combination: in fact, "only one track was observable, and the position of this showed that it could not possibly have been due to a corpuscle originating in the

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219 Chadwick and Constable (1932), 49 (quote), 55-60, 68 (Nutt). Nutt became Chadwick's personal assistant in 1931. See Massey and Feather (1976), 18.
220 Chadwick and Constable (1932), 60-65.
221 Webster (1932); Webster to Gray, 7 March 1932, JAGP; Feather (1962), 139-140.

beryllium."[222] So, against a range of alternative interpretations such as the possibility of the existence of neutral particles, Webster, like Bothe and like the Joliots, *chose* to regard the beryllium radiation as a γ-radiation.[223]

Within a week or two of the despatch of Webster's paper to the Royal Society, the Joliots' second paper arrived in Cambridge. It provoked a strong reaction from both Rutherford and Chadwick.[224] In an atmosphere of deep secrecy, which even Cavendish researchers found strange, Chadwick began his own experiments on the Joliots' observations.[225] Using essentially the same apparatus that he and Constable had deployed in the disintegration experiments, and with the new, strong polonium source, Chadwick repeated the hydrogen observations. He then extended them by investigating the effect of the beryllium radiation on helium, lithium, beryllium, carbon, air and argon. A week's "strenuous work"[226] was needed to establish that the beryllium radiation produced characteristic recoil atoms in all the target elements. These results, he noted, were "very difficult to explain on the assumption that the radiation from beryllium is a quantum radiation, *if energy and momentum are to be conserved in the collisions*."[227] Out of his observations, Chadwick carved a new interpretation: the difficulties would "disappear ... if it be assumed that the radiation consists of particles of mass 1 and charge 0." The capture of the impinging α-particle by the Be^9 nucleus would then result in the formation of a nucleus of C^{12} with the emission of the postulated uncharged particle at speeds of up to 3×10^9 cm/sec. Such an interpretation would also explain Webster's observations of the anisotropic scattering of

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222 Webster (1932), 440-441, footnoting Langer and Rosen (1931) on 'neutrons.' See also Feather (1960a), 264-265; Brown (1978), esp. 25-26.
223 And went on doing so for some months. Compare Webster to Gray, 7 March 1932, JAGP; Webster to I. Curie, 11 July 1932, IFJCP; Chadwick (1969), 71.
224 Chadwick (1962), 161.
225 Chadwick (1969), 70 ff.; Cockcroft (1984), 75. So private did Chadwick keep his work, in fact, that no contemporaneous records appear to survive of it. On Chadwick's work in January and February 1932, see Feather (1962), 141-142; Six (1987), 65-67. Note, however, that Six (1987, 1988) is more concerned to understand why Bothe and the Joliots *failed* to discover the neutron than with Chadwick's arguments in favour of such a particle.
226 Chadwick (1969), 70 ff. See also Chadwick (1962); Feather (1960a, 1962, 1974).
227 Chadwick (1932a), my emphasis.

protons very nicely. The Joliots' alternative, Chadwick cautioned, could "only be upheld if the conservation of energy and momentum be relinquished at some point."[228]

Around 9th February, he asked Feather to look for any possible effects of the beryllium radiation in the Shimizu cloud chamber. To their satisfaction, they saw short, heavy tracks a few millimetres long in the enclosed air, which experience led them to believe to be nitrogen recoil tracks, produced by impact of a heavy neutral particle. Following the apparent success of Shimizu chamber experiment, Feather set up a larger cloud chamber so as to photograph the tracks, the first photographs being obtained on 16th February.[229] The photographic evidence was crucial to Chadwick's judgement about the status of his interpretation of the beryllium radiation. A day later, with proof in hand, Chadwick wrote a short letter to *Nature* describing the previous two weeks' experiments. Its title: "Possible Existence of a Neutron." In one sense, Chadwick's work had ended. In another, it had barely begun.

7. Conclusion

"N is for Nutt who discovered the neutron"

Line from the 1932 Cavendish Alphabet, written and performed by Norman Feather at the dinner of the Cavendish Physical Society, December 1932[230]

That the idea of a neutral particle should emerge from the Cavendish need not surprise us, for, as we have seen, it had been a part of the interpretative framework of Cambridge experimentalists throughout the 1920s. As I have shown, for example, such an entity had consistently figured in the later versions of Rutherford's satellite model of the nucleus. Indeed, as recently as 1927 Chadwick himself had undertaken an investigation of whether 'neutrons' were emitted in artificial disintegration processes, hoping that such emission

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228 Chadwick (1932a).
229 Feather laboratory notebook, 15 February 1932 ff., FEAT 6/1, NFP.
230 Goldhaber (1979), 88.

might help explain the remaining discrepancies between Cambridge and Vienna. Neutrons were familiar objects in the Cavendish of the 1920s, even if they weren't 'known' at all.[231]

Chadwick's commendably cautious announcement of the "*Possible* Existence of a Neutron" in February 1932 owed nothing to false modesty or to scientific ideal. Within the Cavendish, as we have seen in this chapter, electrical counting methods had only slowly become sufficiently stabilised and domesticated to be relied upon to yield clear and unambiguous evidence. The all-embracing discipline of silence, the temperamental electronic equipment (if Nutt did not discover the neutron he was certainly the midwife) and the vagaries of the mass-determinations of the light nuclei all conspired to leave room for significant doubt in Chadwick's mind about the measurements which had led him to postulate the new particle. Yet not to publish would open the possibility that any of the other laboratories actively involved in nuclear research might step in and take credit for such an interpretation. In this wider disciplinary context, the rapid increase in the number of laboratories interested in nuclear questions after 1928 gave rise to a culture in which every new observation which emerged had the potential to be the crucial piece of evidence necessary to make sense of the nucleus. At the same time, that culture was also characterised by debates which sought to initiate a fundamental reappraisal of the assumptions underlying atomic physics - speaker after speaker at the Rome conference in October 1931, for example, had emphasised the difficulties facing nuclear research - so that Chadwick announced his tentative discovery in a context which was defined by conceptual contradictions, experimental uncertainty and a high degree of inter-laboratory competition.

In that sense, then, Chadwick's postulation of the existence *and discovery* of a neutral particle was a high-risk strategy. What the Germans and the French had suggested, after all, actually made sense. The work of Gamow provided a plausible rationale for the emission of γ-rays in artificial disintegration processes. The line of thinking initiated by Millikan during his European tour in November 1931 provided a rationale for the disintegration of nuclei (and therefore, plausibly, for the ejection of protons from

[231] See, for example, Rutherford, Chadwick and Ellis (1930), 523.

hydrogenous materials) by cosmic (γ-) rays. And if the Joliots' Compton hypothesis (proposed no less tentatively than Chadwick's neutron, remember) violated the principle of conservation of energy, Bohr's recent speculations on non-conservation provided an authoritative justification for such a possibility. So, in February 1932, the neutron was, in a certain sense, the *least* plausible interpretation of the phenomena disclosed by Bothe's experiments.

Why, then, was the neutron accepted so quickly by experimentalists and theoreticians alike? I have argued in this chapter that a number of laboratories turned to the experimental study of the nucleus as a result, direct or indirect, of the 1928 Cambridge conference. In particular, Heinz Pose at Halle and Maurice de Broglie and the Joliot-Curies in Paris began experimental nuclear research, joining the laboratories of Rutherford, Meyer, Bothe and Meitner. They were joined by the physics department of Columbia University, New York, which entered the field as a direct result of its clash with the G.E.C. and the Cavendish over the issue of scintillation counting. And they were followed by laboratories in Rome, Washington and elsewhere. While, in a way, this expansion reflected the post-war recovery and the rapprochement in international scientific relations, as well as the increasing numbers of researchers engendered by the scientific career structures set in place for researchers after the war, the decision to enter this particular field also reflected the widespread perception that the Cambridge-Vienna controversy was ongoing and represented a significant challenge. By 1932, half a dozen or more laboratories had acquired, or were in the process of acquiring, the materials and equipment necessary for nuclear research - polonium, valve amplifiers, and so on. It is no surprise, then, that these were the very same laboratories at which 'neutrons' started appearing in the spring and summer of 1932, as new of Chadwick's announcement travelled around the community. The neutron landed, so to speak, on prepared ground.

CHAPTER SIX

CONCLUSION

From Radioactivity to 'Nuclear Physics': A Tale of Two Heresies

"Anything good has to be done in three labs before you can believe it"[1]

1. Introduction

The year 1932 is customarily seen as a turning point in the history of nuclear physics. The discoveries of the neutron and the positron, the artificial disintegration of the atom and the disclosure of a heavy isotope of hydrogen are collectively taken to constitute the *annus mirabilis* of the discipline. But that term, evocative as it is, has an interesting and little-known history of its own. In 1934, the Cavendish Laboratory initiated an appeal to raise funds for the construction of an accelerator comparable to those being built in the United States. Wealthy "friends of science and of Cambridge" would be invited to give generously in support of the work of the Cavendish to enable it to keep up with developments elsewhere. As part of this enterprise, Rutherford asked Arthur Eddington, author of the recent best-seller *The Expanding Universe*, to write a brief account of the Cavendish and the work being done there. Eddington made a special tour of the laboratory in October 1934, and produced a lyrical 17-page pamphlet for circulation to potential benefactors. Describing the most recent work of the laboratory, Eddington crafted what subsequently became the central ornament of the historiography of nuclear physics:[2]

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[1] Chadwick ca. 1930, quoted by E.C. Pollard (personal communication).

[2] Eddington (1935a), 11. Eddington himself may well have appropriated the term from Russell (1931b). See also Sullivan (1931); Graham (1981), 69-87.

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

Eddington's effortless invention of the history of nuclear physics was a masterpiece, not least baecause it produced the Austin bequest of 1936 which allowed the Cavendish to follow the pace increasingly being set in America.[3] But his little vignette has also provided fodder for historians. Captivated by so eloquent a locution, subsequent commentators have used the term *annus mirabilis* unproblematically (and unhistorically) to describe what they consider in retrospect to be a key moment in the evolution of nuclear physics.[4] With it, however, they have also implicitly accepted Eddington's construction of an antecedent fallow period, "ten years of frustrating failure"[5] during which experimentalists "plodded patiently on" whilst theory flourished. Look again at Eddington's text, however, and we find some remarks which have hitherto eluded the attention of the historian. Eddington pointedly stressed the scientific community of which the Cavendish was a part. "It would be alien to the prevailing spirit of co-operation and interchange," he wrote, "to represent the Cavendish as self-sufficient and aloof." "Discoveries made in Cambridge may be the final step in an advance begun elsewhere; discoveries made elsewhere may be the final step in an advance begun in Cambridge," so that it "must be understood that brilliant as the Cavendish contributions have been they are elements in a wider picture."[6]

In this final chapter, I want to indicate some of the ways in which the analysis developed

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[3] Wilson (1983), 588-589; Heilbron and Seidel (1989); Aaserud (1990), 51.

[4] The term was first appropriated by Eve, Rutherford's biographer, who in fact used it *twice*: once to describe 1908, the year Rutherford obtained the loan of an amount of radium from the Vienna Academy of Sciences (the *annus mirabilis* of radioactivity), and again to describe 1932 (the *annus mirabilis* of nuclear physics). See Eve (1939), 176, 433 (where the phrase is actually attributed to R.H. Fowler). Subsequent appropriations can be traced from there.

[5] Wilson (1983), 446.

[6] Eddington (1935a), 11.

in this dissertation might help us to understand the emergence and characteristics of the enterprise which became known as 'nuclear physics' by looking at the "wider picture" which Eddington considered so important. I begin by considering responses to Chadwick's claimed discovery of the neutron, for in a certain sense it was the neutron which both defined and ratified the emergent community, acting as a point of convergence upon whose existence experimenters and theoreticians alike could agree, even if they subsequently disagreed (as they did) about the particle's nature and properties.

2. Replicating Neutron Research

2.1 Early Neutron Work in Cambridge

As we know, Chadwick published his tentative claim to have discovered a 'neutron' on 17 February 1932 in a letter to *Nature*, where it appeared ten days later.[7] On the 23rd, exhausted but "mellowed" after having been wined and dined by Kapitza, Chadwick regaled a meeting of the Kapitza Club with details of his discovery. Within days of Chadwick's talk, several researchers at the Cavendish were working on various aspects of the new particles, Fowler telling Bohr that "Chadwick's neutrons have rather overwhelmed everything for the moment."[8] And there it might have ended, with researchers at the Cavendish quietly pursuing their investigations, and no-one outside the small community of interested parties any the wiser. Within the week, however, the story had found its way into the newspapers. By comparison with Chadwick's muted announcement in *Nature*, however, the press coverage went to the other extreme. "New Type of Ultimate Particle Found by Cambridge Scientist," screamed the headlines. "Profound Effect on Modern Knowledge."[9] Chadwick, even more reticent than usual until he had confirmed the new

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[7] Chadwick (1932a).
[8] Fowler to Bohr, 1 March 1932, BSC; Dee (1932); Feather (1932, 1933a, 1933b); Massey (1932a, 1932b); Aaserud (1990), 53.
[9] Crowther (1932a).

hypothesis to his own satisfaction, refused to divulge full details of the experiments to journalists.[10] Yet the press published rather full accounts of his research, its consequences and its prospects. How, then, did reporters acquire such comprehensive reports of the new work? And why such extraordinary interest in the conjectural new particle?

Enter J.G. Crowther, Science Correspondent of the *Manchester Guardian*. Having dropped out of Cambridge in 1919 after a term studying mathematics at Trinity College, Crowther had joined Oxford University Press in 1924 as a technical representative. Over the next two years he visited many colleges, universities and research laboratories, meeting the great and the good of British science. In his spare time he wrote some short articles on scientific developments for periodicals like the *New Statesman* and for newspapers, principally the *Manchester Guardian*.[11] In 1928, after an interview with the paper's editor, C.P. Scott (who had known Rutherford on the Senate of Manchester University), he was taken on as the *Guardian*'s scientific correspondent. Crowther had heard of Chadwick's work casually from J.D. Bernal at a meeting of Solly Zuckerman's Tots and Quots group in London,[12] and, with his journalistic interest aroused, arranged an invitation for himself to the Kapitza Club to hear Chadwick speak on his discovery. Crowther's account of Chadwick's work appeared in the *Manchester Guardian* on 27 February 1932 under the title "The Origin of Matter," an article which, in view of Rutherford and Chadwick's reluctance to speak to the rest of the press, was the only source of information for other newspapers.

Competing for column space with the Lindbergh kidnapping and the exploits of speed-king Malcolm Campbell, Crowther's story was one of heroic scientific endeavour and achievement, rather similar in tone, in fact, to the sensationalist accounts of Campbell's latest attempts at the land speed record.[13] Against a background of swingeing financial cuts

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[10] Cockcroft (1984), 75.
[11] Crowther (1970), 9-40.
[12] See Zuckerman (1978), 392-404.
[13] For an explicit link between atomic physics and the characteristically Modern trope of speed, see Rutherford (1932d), 183. Compare, also, Kern (1983).

in the University of Cambridge and elsewhere as economic slump turned to depression,[14] Crowther's favourable reports provided a striking justification of the work of the Cavendish Laboratory.[15] He soon began to report on Cambridge physics for other newspapers and popular periodicals, acquiring something of a reputation as a knowledgeable and reliable source.[16] And within weeks, Crowther took to his typewriter again to report more news from Cambridge: Cockcroft and Walton had succeeded in splitting atoms of lithium - the first disintegration of atoms by artificially accelerated particles.[17] "It never rains," Rutherford told Bohr, "but it pours."[18]

While Crowther's Marxist enthusiasm for science and technics exhorted him to ever greater heights of praise and admiration for the work of the Cavendish,[19] his synoptic history of the disclosure of the new particle, supplemented by a profile of Chadwick, "The Discoverer of the Neutron,"[20] was taken up by the *Times*, which elicited from Chadwick the comment that "the result of his experiments in search of the particles called 'neutrons' had not, at the moment, led to anything definite and the element of doubt still existed." There was however, "a distinct possibility that investigations were proceeding along the right lines. In that case a definite conclusion might be arrived at in a few days, and on the other hand, it might be months."[21] In fact, work on the neutron was going on at a furious pace,

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14 Cuts which directly affected the Cavendish Laboratory. Rutherford told Marsden in December 1931 that "There is a general impression that we shall get still worse during the next year unless the depression lifts very rapidly. The University hopes not to reduce salaries or wages but is quite properly reducing our general maintenance grants so as to be on the safe side." See Rutherford to Marsden, 21 December 1931, RP. For the background to the depression in England, see Aldcroft and Richardson (1969); Stevenson and Cook (1979); P. Williamson (1992). For its effects on science in the U.S.A., see Weiner (1970).

15 Rutherford told Bohr that he found Crowther "quite intelligent in these matters" (Rutherford to Bohr, 21 April 1932, RP).

16 See, for example, Crowther (1932c, 1932d, 1932e, 1934b, 1934c). Also see Crowther (1970), 89-114. With his long-standing interest in the work of the Cavendish, Crowther was later a natural choice of author for the Laboratory's celebratory centenary history. See Crowther (1974), but cf. Feather's (1975) review.

17 Cockcroft and Walton (1932a, 1932b). Although the initial qualitative observations of the disintegrations were made with the scintillation method, later data-gathering relied upon the cloud chamber (for photographic evidence) and electrical counters. Significantly, Rutherford told Bohr that the electrical techniques "completely confirm[ed] the scintillation method, *so that there is no doubt we are on safe ground*" (Rutherford to Bohr, 26 May 1932, RP, my emphasis).

18 Rutherford to Bohr, 21 April 1932, RP.

19 The technical achievements of the Cavendish also served as inspiration for those of other political persuasions: see, for example, Heard (1931) in Oswald Mosley's Fascist newspaper *Action*. See also Werskey (1978).

20 Crowther (1932b).

21 "A New Ray. Dr. Chadwick's Search for "Neutrons"," *The Times*, 29 February 1932, 9.

as Cambridge workers sought to keep ahead of the other laboratories which they were sure would soon join the fray (hoping to obtain more supplies of polonium, for example, Feather told Fred West at Baltimore that the Joliots were "working feverishly too" on the neutron[22]). Chadwick engaged Feather to continue the cloud chamber experiments investigating the interactions between the putative new particles and nitrogen nuclei.[23] Another graduate, Philip Dee undertook a series of (ultimately unsuccessful) cloud chamber studies to search for recoil electrons produced by neutrons, using Chadwick's polonium source overnight while Chadwick and Feather rested in preparation for the following day's labours.[24] The theoreticians contributed too, with Harrie Massey's investigation of the properties of the new particle in its passage through matter.[25]

With all these researches in progress, Rutherford and other researchers at the Cavendish Laboratory articulated what was, in effect, a labour theory of value, in which the neutron's worth was measured by its potential to make opportunities for further experimental and conceptual work. As Feather told West, "[a]n enormous field is opening up, Rutherford says five years' work at least."[26] This applied as much to theoretical as to experimental practice. According to Bohr, with the disclosure of Chadwick's neutron "[o]ne sees a broad new avenue opened, and it should soon be possible to predict the behaviour of any nucleus under given circumstances."[27] He invited Chadwick to give a first-hand account of his discovery at the forthcoming annual physics conference in Copenhagen, where the neutron would be top of the scientific agenda.[28] But Chadwick was too busy to attend, undertaking

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22 Feather to West, 16 March 1932; West to Feather, 1 March 1932, FEAT 11/4, NFP. In a certain sense, the novelty of the neutron hypothesis could be put to effective use as a lever to extract further supplies of polonium. Feather had sent a cablegram to West on 26 February stating: "Experiments using dead tubes suggest existence of new type of ultimate particle. Further supplies greatly appreciated." See Burnam to Tuve, 26 February 1932, Box 4, MATP.
23 Feather (1932, 1933b). Also see Feather to West, 16 March 1932, FEAT 11/4, NFP; Feather (1974); Cochran and Devons (1981), 269-270.
24 Dee (1932, 1984); Curran (1984), esp. 143-145.
25 Massey (1932b); Bates, Boyd and Davis (1984), esp. 451-452; Dee (1984).
26 Feather to West, 16 March 1932, FEAT 11/4, NFP. For a cogent analysis of the legitimation of research in terms of its potential to create opportunities for further work, see Lyotard (1984), 41-47.
27 Bohr to Rutherford, 2 May 1932, RP.
28 Bohr to Chadwick, 25 March 1932, BSC. See also Bohr to Heisenberg, 21 March 1932, BSC; Bromberg (1971); Weiner (1972); Aaserud (1990), 53.

his own elaborate confirmatory experiments which aimed to characterise the neutron more fully. The participants at Bohr's conference toasted Chadwick in his absence (see p. 292).

During March and April 1932, as it acquired substance and character, the neutron "became accepted in the Cavendish as a definite, almost familiar, entity."[29] The process of gradual habituation was nicely captured by Eddington, who, presenting the Physical Society's Duddell Medal to C.T.R. Wilson in March, found it "appropriate to mention that at the moment we have chosen for honouring the inventor, all the expansion chambers in Cambridge (and probably throughout the world) are working overtime on a new discovery," the facts of which appeared to be "that *something* gets through a thickness of lead which no kind of matter hitherto known could penetrate." The cloud chamber had become to the physicist, he said, "what the telescope is to the astronomer"[30] - praise indeed! By the middle of May, the Cambridge experiments had advanced to the point where Chadwick was able to send a comprehensive and detailed paper to the *Proceedings of the Royal Society*, where it appeared with remarkable speed in the June number. Entitled "Existence of a Neutron," it confidently put forward the evidence amassed in favour of the new particle over the previous two months. It was buttressed by two supporting papers from Feather and Dee, outlining the results of their investigations and further delineating the characteristics of the new particle in its interactions with matter, complete with cloud chamber photographs. There could now be no doubt about the neutron hypothesis. The neutron was as real as the polonium and the cloud chambers which manifested it. As we shall see, however, this was a truism which cut both ways.

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29 Dee (1984), 48. Much of that familiarity came from Chadwick's equation of his particle with that postulated by Rutherford in the Bakerian Lecture twelve years previously, giving it a firm historical foundation. Within days of the initial announcement, for example, Chadwick told the Times that his experiments were "the normal and logical conclusion of the investigations of Lord Rutherford 10 years ago." Indeed, it soon came to seem as if Rutherford had "predicted" the existence of the putative particle disclosed by Chadwick's experiments. But while Chadwick's historicisation of the particle endowed it with respectable parentage and did a great deal to embed it in culture, much of the neutron's immediate value arose from the fact that it allowed physicists to 'save' the long-standing conservation laws. See "A New Ray. Dr. Chadwick's Search for "Neutrons"," *The Times*, 29 February 1932, 9; Feather (1962); Kröger (1980); Six (1987).

30 Eddington (1932), 428.

2.2 A "Very Attractive Hypothesis": Early Neutron Research in Paris

When the 27th February, 1932, issue of *Nature* appeared in physics laboratories and libraries, many of its readers would have turned first, as they usually did, to the letters page. There, next to a letter about the Oldoway human skeleton, they would have read Chadwick's letter describing his recent work at the Cavendish Laboratory. In Paris, the Joliots were evidently perplexed to have been 'overtaken' by another laboratory (especially the Cavendish). As Joliot told Skobeltzyn, "[w]e have had to speed up the pace of our experiments, for it is annoying to be overtaken by other laboratories which immediately take up one's experiments. In Paris this was done straight away by M. Maurice de Broglie with Thibaud and two other colleagues. In Cambridge Chadwick did not wait long to do so either."[31] In the new competitive, reactive culture, such a situation was, perhaps, almost inevitable. At the same time, however, Joliot reacted with measured equanimity to Chadwick's preliminary neutron paper which arrived in France early in March, telling Skobeltzyn casually that:[32]

> Chadwick has, by the way, published the very attractive hypothesis that the penetrating radiation from Po (α) Be is composed of neutrons. I tell you this because you are in touch via C.R. and Nature with these experiments concerning the projection of atomic nuclei. We have recently been carrying out new experiments on the Po (α) Be radiation and the results will be published on Monday in the C.R. Here is a summary. Po (α) Be radiation is composed of at least two parts: one part is gamma rays of energy between 5 and 11 MeV and is scattered by the Compton effect. The other part is radiation of enormous penetrating power - about half is absorbed in 16 cm of Pb following collisions with the nuclei. This radiation is very probably composed of neutrons.

The Joliots' rapid "conversion" to the neutron hypothesis speaks to the persuasive power of Chadwick's interpretation of the beryllium radiation. And, implicitly, it demonstrates that Joliot was already *au fait* with the concept of the neutron which, as I have shown, featured prominently in many of the nuclear models proposed in the late 1920s.

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31 Joliot to Skobeltzyn, 2 April 1932, in Goldsmith (1976), 42.
32 *ibid.*

Conversely, however, it also emphasises the preliminary and tentative character of the Joliots' own earlier interpretation, to which they were clearly not irrevocably committed (the Joliots' immediate acquiescence to the neutron interpretation was in any case tempered by their insistence that the beryllium radiation consisted at least in part of gamma rays, as per their original claim).[33] At the end of April, the Joliots broke off their work on the new particle in order to make their pre-arranged field trip to the high-altitude research station on the Jungfrau.[34] Taking with them a veritable battery of instruments from the Laboratoire Curie, they spent some time investigating the connections between the mysterious cosmic rays and the equally enigmatic neutron.[35] This was still an extremely plausible and promising line of investigation, for both types of ray were extremely penetrating, and neither was yet well-understood. Indeed, press reports persistently insinuated that the neutron, "a particle of extraordinary properties whose study must extend the knowledge of matter in hitherto unknown directions" opened the possibility that "[t]he famous cosmic rays might in fact be streams of neutrons."[36]

This was an interpretation firmly resisted by Millikan, champion of the electromagnetic cosmic ray, who told the New York Times that "[t]he proof as to whether such an entity [the neutron] exists is rather difficult to get, and up to the present I have seen no way of differentiating between the effects due to photons ... and these hypothetical neutrons."[37] The view - and the confusion - were shared by Maurice de Broglie, who offered a roving *Science News* reporter the opinion that:[38]

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[33] Curie and Joliot (1932d, 1932e, 1932f, 1932g); Curie, Joliot and Savel (1932). For the connection between the Joliot's emphasis on gamma rays and the instrumentation at their disposal, see Six (1987), 74-80; Six (1988).

[34] See I. Curie to M. Curie, 26 April, 1 May, 8 May 1932, in Ziegler (ed.)(1974), 338-340.

[35] Recall that the trip had originally been arranged to investigate the variation of the beryllium radiation with altitude (on the supposition that it was related in some way to the cosmic rays). See Joliot-Curie laboratory notebooks, 27 April - 9 May 1932, with list of apparatus; I. and F. Joliot-Curie to Bohr, 16 May 1932, IFJCP; Curie and Joliot (1933h).

[36] Crowther (1932a); "Neutron, Atomic Brick, May Solve Mystery of Cosmic Rays," *Science News Letter*, 5 March 1932, 143.

[37] "Millikan Likens Neutron to Photon," *New York Times*, 29 February 1932, 1. Also see Atchley (1991), 19-22.

[38] "European Scientists Study Neutron, Latest Atomic Part," *Science News Letter*, 9 April 1932, 230 [Cofman (1932)].

It is not certain at present whether we are dealing with material particles or with radiation. ... The facts so far known about the peculiar rays whose nature is being investigated, do not agree completely either with the 'quantum' or with the 'neutron' hypothesis. It is difficult to devise crucial tests that will distinguish between them. If it could be shown that the rays are even very slightly affected by an electro-magnetic field, that would definitely prove their material nature, because quanta could not be so affected.

At de Broglie's own laboratory, as Joliot's irritated comments to Skobeltzyn indicate, work had begun almost immediately on the replication of Chadwick's experiments and the characterisation of the properties of the new radiation. Jean Thibaud and Père F. Dupré la Tour, colleagues of de Broglie who had not previously been involved with Leprince-Ringuet's disintegration programme quickly joined the neutron work.[39]

As his laboratory joined the headlong rush to work on the neutron, de Broglie wrote an admiring tribute to Rutherford for *Nature*'s 'Scientific Worthies' feature in May 1932.[40] Within a few months, however, it became clear that de Broglie's laboratory would have to move away from the study of neutrons and their effects, due to its lack of a strong polonium source, the key necessity for such work (it was in order to overcome the shortage of polonium, in fact, that de Broglie later suggested using a radium-beryllium mixture as a source of neutrons). Leprince-Ringuet and various collaborators began to concentrate instead on cosmic ray research, which could be carried out with the same kinds of hardware but which did not require hard-to-come-by radioactive sources.[41]

This was also the trajectory followed by Francis Perrin and Pierre Auger of Jean Perrin's Institute for Physical Chemistry. Auger and Perrin (both of whom had attended the 1928 Cambridge conference) initially deployed cloud chambers to undertake work on the new radiation.[42] Auger had previously used the device both in disintegration experiments and in

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39 Thibaud and la Tour (1932a, 1932b); de Broglie, la Tour, Leprince-Ringuet and Thibaud (1932); de Broglie and Leprince-Ringuet (1932a, 1932b); Pestre (1984), 78-79.
40 de Broglie (1932).
41 Leprince-Ringuet (1933, 1934, 1960, 1982, 1983); Leprince-Ringuet (1991), 71 ff. Frédéric Joliot tried to entice Leprince-Ringuet, with his electronics skills, away from Maurice de Broglie's laboratory to the Institut Curie, which lacked a valve amplifier – an offer which Leprince-Ringuet graciously declined. See Leprince-Ringuet (1991), 69-70.
42 Auger (1932a, 1932b, 1933a, 1933b); Auger and Monod-Herzen (1933); Perrin (1932a, 1932b, 1932c). For the later work on cosmic rays, see Auger (1983), esp. 173-174; Pestre (1984), 85 and *passim*.

cosmic ray work[43] and, like Chadwick, had taken up the Joliots' original observations early in 1932. Like other French workers, Auger and Perrin were quickly persuaded of the virtues of the neutron interpretation. But, like de Broglie's group, they lacked access to sufficiently powerful radioactive sources, and soon turned to cosmic ray work. Nevertheless, for a considerable time, they were important contributors to experimental and theoretical work on the neutron.

2.3 Early Neutron Research in Germany

In sum, then, three Parisian laboratories made early forays into neutron research. In so doing, they increased the number of sites at which neutrons could be manufactured from one to four. Significantly, all three laboratories had shifted into such research as a result, direct or indirect, of the Cambridge conference in 1928. And it was for this reason that they found themselves (as it were) with the tools to take up Chadwick's work within days of its announcement. The same was true in Germany, where neutron research began in earnest almost immediately after the publication of Chadwick's paper.[44] In Germany, as in France, the groundwork had already been laid. Several German researchers had, of course, been working on the beryllium radiation before Chadwick's announcement. Polonium, cloud chambers and electrical counters were already in place for disintegration work, making it easy for researchers to take up the new investigations. What was required in these cases was that they now 'see' the beryllium radiation as 'neutrons.'

Like the Joliots, Bothe (who was called from Giessen to Heidelberg in March 1932, disrupting his experimental work for some time) was quickly won over to Chadwick's corpuscular interpretation of the penetrating beryllium radiation, perhaps through a consideration of the wider ramifications of the alternatives.[45] As Rutherford told an audience at the Royal Institution in March, only a few weeks after Chadwick's tentative

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[43] See, for example, Auger and Perrin (1922); Auger and Skobeltzyn (1929).
[44] Becker and Bothe (1932). For some remarks on Bothe's early neutron work, see Six (1987), 71-73; Six (1988). Also see Dostrovsky (1970); Maier-Leibniz (1985).
[45] Bothe to Meitner, 31 March 1932; Meitner to Bothe, 5 April 1932, MTNR 5/2, LMP.

announcement, for example, "[i]f the neutron hypothesis proved wrong and γ radiation was responsible, the principles of conservation of momentum and conservation of energy must be given up, and these constituted almost the last raft left to the poor physicist."[46] With Becker, Bothe employed the coincidence counting method in an attempt to measure the energy of the new radiation - a line of investigation also taken up by Franco Rasetti in Lise Meitner's laboratory (a source of conflict with Bothe over priority and propriety).[47] While Heinz Pose at Halle continued his work on resonance disintegration in collaboration with Kurt Diebner,[48] the European picture is completed by the *enfant terrible* of radioactivity, the Institut für Radiumforschung in Vienna, where Gerhard Kirsch and several others immediately began work on the production and properties of neutrons.[49]

All in all, then, the first few months of 1932 saw furious activity in those laboratories already involved with nuclear research, as researchers sought to exploit the resources at their disposal to maximum advantage in an attempt to gain an early foothold in the study of the new particle. By the middle of 1932, at least half a dozen European laboratories were working on various aspects of experimental neutron research. The neutron acted as a point of convergence for nuclear theorists, too (Heisenberg immediately sought to develop a theory of nuclear constitution using the neutron as a key component, telling Bohr that "[t]he basic idea is: to shove all principal difficulties onto the neutron, and to apply quantum mechanics to the nucleus"[50]). To suppose that the neutron secured immediate, universal and *unqualified* assent, however, would be a mistake. If, as Bromberg, Heilbron and Seidel and others have pointed out, theorists only gradually came to agree on the character and properties of the neutron,[51] the same was surely true of experimentalists. Even in the first

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46 Rutherford (1932c), 452.

47 Rasetti (1932a, 1932b); Meitner to Bothe, 5 April 1932, n.d. [April 1932]; Bothe to Meitner, 6, 14, 20 April 1932, MTNR 5/2, LMP. On propriety and priority, cf. the important paper by Iliffe (1992).

48 For the work at Halle, see Diebner (1932); Diebner and Pose (1932). See also Pose to Meitner, 18 March 1932; Meitner to Pose, 24 March 1932, MTNR 5/14, LMP.

49 Blau and Wambacher (1932); Kirsch and Rieder (1932); Kirsch and Trattner (1933); Kirsch and Wambacher (1933); Meyer (1932b); Przibram (1950); Rona (1978), 30-39. On the poor reputation of the Institut für Radiumforschung in the 1930s, see Frisch (1967), 43-44.

50 Heisenberg to Bohr, 20 June 1932, BSC; Heisenberg (1932a, 1932b, 1933). See also Brown and Moyer (1984); Brown and Rechenberg (1988); Aaserud (1990), 54.

51 Bromberg (1971); Heilbron and Seidel (1989), 147 n.130.

few months of 1932, the neutron was open to a range of interpretations and readings – manifested as debates about its mass, spin and other characteristics.[52] Doubtless, a detailed analysis of the controversies and negotiations surrounding the new particle in the early- and mid-1930s would reveal the complex ways in which the particle was shaped through the experimental and conceptual practices of the emergent and ever-expanding investigative community.

3. The Politics of Polonium and the Material Culture of Artificial Disintegration

I have now shown how the laboratories which replicated Chadwick's work on neutrons early in 1932 and which formed the institutional core of an emergent research community were precisely those laboratories which had entered the field of artificial disintegration in response to the Cambridge-Vienna controversy. At the Cavendish Laboratory, 'neutrons' were embedded in a very specific material and social culture. To reproduce neutrons elsewhere was also to reproduce that culture.[53] And, as I demonstrated in Chapters Four and Five, all the laboratories which were subsequently able to carry out neutron research had taken great pains to acquire very specific instruments, techniques and materials to enable them to participate in the disintegration experiments out of which the neutron gradually emerged as an independent entity. By mid-1932, as the new particles became embedded in laboratory routines and in conceptual practice, it was becoming clear that, because of its very universality, experimental neutron research was going to be an

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[52] Stuewer (forthcoming 1993).
[53] Latour (1987), *passim*; Schaffer (1989). For an excellent analysis of the ways in which phenomena can appear to be global in scope through the circulation and deployment of particular elements of material and social culture, see O'Connell (1993), and compare Latour (1988); Rouse (1987), 69-126; Rouse (1993). Stansfield (1990), Gooding (1989b) and Sibum (1992) consider the replication of experiments and material culture from an historical perspective.

important line of investigation in nuclear research.[54] In this section, therefore, I want briefly to extend my analysis to show how this redefinition of the evidential context of nuclear research itself acted as a catalyst, bringing many more researchers into what now seemed (at least from outside) like a stable and exciting research field.

Like those who had preceded them, researchers who chose to enter the field of artificial disintegration had first had to acquire the wherewithal to join the game. Three key elements were necessary to undertake neutron and disintegration research: strong sources of polonium, valve amplifiers and cloud chambers. Each of these elements was crucial to the production and detection of neutrons, yet each presented difficulties to the would-be experimenter in the way of acquisition or operation. Take first, for example, the case of Merle Tuve and his associates at the Carnegie Institution of Washington's Department of Terrestrial Magnetism (DTM), who entered the field of artificial disintegration on the basis of their assessment of the situation in European laboratories. Tuve had followed the Cambridge-Vienna controversy closely, and found Heinz Pose's work in particular "of most fundamental importance in nuclear physics."[55] After a comprehensive programme of instrument acquisition, disintegration research began at DTM in the spring of 1931.[56]

But the start was not promising. All of Tuve's efforts to acquire a cloud chamber for his work ended in failure, for example, and he was eventually forced to borrow one from L.F. Curtiss of the National Bureau of Standards.[57] The group experienced similar difficulties in obtaining the other crucial elements for nuclear research. In their efforts to duplicate the Wynn-Williams pattern valve amplifier so as to repeat work originally carried out at the Cavendish, Tuve's group (and other 'latecomers') were again frustrated by their inability to acquire the crucial D.E.V. valves necessary for the input stage of the device. In November

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[54] It is important to note that the multiplication of sites at which valve amplifiers, cloud chambers and so on were deployed also took place *within individual laboratories*. At the Cavendish, for example, there were at least four Wynn-Williams pattern valve amplifiers in use by 1933. See Lewis (1972), 63.

[55] Tuve, "Report for October 1931," quoted in Cornell (1986), 313. For Tuve's notes on the Cambridge-Vienna controversy, see Box 7, folder "Pettersson & Kirsch etc.," MATP. It is unclear whether these notes were writen during the course of the controversy in the mid-1920s, or whether they were compiled later.

[56] For nuclear research at DTM, see Cornell (1986, 1988, 1990); Dennis (1990), 134-247, esp.162 ff. On the group's acquisition programme, see "Special Report on the Experimental Work, October 9 1931," and the series of monthly laboratory reports in Box 4, MATP.

[57] Cornell (1986), 216-217.

1932 Tuve told Ernest Lawrence that the Washington group had "found no American tubes that were satisfactory for the input tube and resorted to a Marconi D.E.V. as used in Cambridge."[58] A year later, however, it was impossible to buy D.E.V. valves, the company having discontinued the line. "The only way we know of purchasing these tubes," DTM director J.A. Fleming told Frank Verwiebe of Chicago's Ryerson Physical Laboratory, "is by the good graces of radio amateurs, especially those residing in seaports."[59] Radio culture again became the controlling context for nuclear research.

As far as polonium was concerned, Tuve rapidly became an assiduous correspondent and collector, courting several hospitals and institutes for supplies of the precious polonium-containing radon tubes. To Tuve's chagrin, the most likely source of tubes, the Kelly Hospital in Baltimore, had already sent all their dead tubes to Feather in Cambridge. Nevertheless, a small number of tubes had accumulated since Feather's departure, and Tuve was able to acquire these for DTM, earning him congratulations from Arthur Ruark for "obtaining such large amounts of polonium."[60] Tuve was still not satisfied, however. While he had, he said, "the greatest respect and warm regard for the Cambridge physicists," and felt "like a small boy speaking up in church when I deflect anything toward myself which might have helped them," he emphasised that any further tubes the Kelly Hospital could supply would be "put to good use" at DTM.[61]

The appearance of Chadwick's neutron paper in February 1932 did much to galvanise the DTM group's efforts to procure the material bases for disintegration research. Appealing to another institution for yet more tubes in April 1932, Tuve explicitly drew attention to Chadwick's recent work and his 25 mC polonium source, and noted that "if I want to equal

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[58] Tuve to Lawrence, 4 November 1932, Box 8, MATP.

[59] Fleming to Verwiebe, 10 November 1933, Box 8, MATP. The DTM group were "willing to send [Verwiebe] our only spare DEV to use for comparison with other tubes in your instrument for a week or two." Also see Dempster to Lawrence, 4 November 1933; M.C. Henderson to Dempster, 9 November 1933, Box 6, EOLP. Cornell (1986), *passim*, stresses the constitutive role of Tuve's early interest in radio in his later laboratory practice. For the DTM group's amplifiers, see the series of circuit diagrams and calculations in 'Greinacher Amplifier' file, Box 6, MATP. DTM also obtained blueprints for Wynn-Williams' original circuit for a mains operated counter. See M.J. Stebbing [Secretary of the Cavendish Laboratory] to Tuve, 13 July 1933; Tuve to Stebbing, 1 August 1933, Box 4, MATP.

[60] Burnam to Tuve, 10 September 1931; West to Tuve, 1 March 1932, Box 4, MATP; Ruark to Tuve, 4 February 1932, Box 6, MATP..

[61] Tuve to West, 4 March 1932; West to Tuve, 17 March 1932, Box 4, MATP.

Chadwick's source I need to dun all of my friends for radon tubes."[62] He was supported by Fleming, who told one hospital that Tuve's request was "not a wholly selfish one," for "[i]t happens the most important experiments in this field require the strongest possible sources, so that a given quantity of Radium-D is much more fruitful of results if it is concentrated in one place than if it is distributed among a number of laboratories."[63] This was a persuasive line of argument. By the beginning of 1933, a DTM report was able to note that "[w]e have the technique and instruments for neutron studies completely in hand ... No other laboratory in the U.S. (and only 2 or 3 in the world in addition to the Cavendish) have this technique and experience." It continued:[64]

> Chadwick's (Cavendish) neutron source is the practical maximum possible with radioactive sources. By actual test we can detect 1/100 of this neutron intensity. The polonium neutron source we are using gives about 1/3 of Chadwick's intensity, but with this we have verified (September 1932) the major observations of Chadwick and have obtained completely independent proof of the existence of the neutron.

Not everyone was as fortunate as Tuve and the DTM group. When Millikan, a late convert to the cause, attempted to acquire some polonium from the Kelly Hospital in July 1932, for example, he was dismayed to find that all the available material had been siphoned off for use elsewhere. Burnam replied apologetically:[65]

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62 Tuve to Failla, 23 April 1932, Box 4, MATP. On Tuve's attempts to garner polonium, see also Tuve to Weatherwax, 15 February 1932; Tuve to Failla, 29 February 1932; Tuve to West and Burnam, 4 March, 15 June 1932, all in Box 4, MATP; Tuve to Failla, 5 December 1932, Box 8, MATP; "Department of Terrestrial Magnetism," *Carnegie Institution of Washington Yearbook* **31** (1931-32), 223-277, on 233.
63 Fleming to R.W. Teahan, 15 December 1932, Box 8, MATP.
64 "Memorandum on the Emergency Necessity for Immediate Housing of the Two-Meter Generator," Box 5, file 'Original MSS,' MATP. See also Tuve to Lawrence, 4 November 1932, Box 8, MATP. Even when they had acquired sufficient supplies of dead radon tubes, however, further problems awaited in extracting the polonium. Fleming invited Otto Hahn to Washington in May 1933 specifically so that workers at DTM could learn the relevant technique, for "there appear to be pitfalls in the chemical procedures which cause very serious troubles for anyone who has not been working in this field for many years. There is no one in this country who is specially qualified in this kind of radioactive chemistry." See Fleming to Hahn, 4 May 1933, Box 8, MATP.
65 Burnam to Millikan, 27 July 1932, RAMP. As this extract suggests, the Kelly Hospital seems to have acted as an unwitting clearing house for information about the distribution of polonium (and therefore of nuclear research) - an important factor given the increasingly secretive character of nuclear research.

> I am sorry to say that we have no polonium on hand. We supplied the Cavendish Laboratories and, also, the Curie Institute with the polonium with which they have been working recently. All of our remaining supply, at this time, is with Dr. Tuve in Washington. Until lately our technique was the one which gave us the larger amount of polonium in the world. We had saved every bit of it for a number of years and it was this that went to Europe.

Burnam and West had, in fact, been swamped with requests for old tubes. A complex system of patronage rapidly developed, West telling Norman Feather that "[t]he Rare metals group from the Bureau of Mines is anxious to have our dead tubes. Dr. Flexner wants them, both for himself and Madame Curie [and we] have had requests from Philadelphia and Canada."[66] But this polity of polonium did not last long. By the autumn of 1932, the Kelly Hospital had changed its radiological procedures, reducing the number of tubes in use and eliminating the steady supply of dead tubes. West apologised to Feather: "Millikan, Rockefeller, Bureau of Standards, Hopkins, and a host of others, have been requesting bulbs, and I feel a snug satisfaction that I was able to switch our last big supply to you, where they were put to so much good work."[67]

In a sense, Tuve and his DTM colleagues were unusual in that they could command the contacts and resources of a private research institution in their attempts to enter the field of disintegration research.[68] From the academic perspective, things looked a little different. Consider, secondly, then, the case of J.A. Gray, Chown Research Professor in Physics at

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66 West to Feather, 1 September 1931, FEAT 11/4, NFP. This system of patronage required generous acknowledgement on the part of the recipient of old radon tubes. West complained to Feather that "we sent a large quantity to Flexner and a big supply to Moore of the Bureau of Mines and found later that they had been reconsigned to Madame Curie without even getting a "thank you" from her." See West to Feather, 1 March 1932, FEAT 11/4, NFP.

67 West to Feather, 13 September 1932, FEAT 11/4, NFP.

68 Though, of course, such resources still had to be fought for, as Dennis (1990) convincingly and comprehensively shows. It should also be noted that the chief concern of Tuve and company was the development of an artificial accelerator. The DTM group sought to deploy a polonium-beryllium neutron source so as to

> carry along simultaneously a set of special investigations by the older radioactive methods, both to insure that we shall not be misled by interpreting our results with the new (artificial) source, and because vital new discoveries and measurements of overwhelming importance (neutrons and resonance disintegration) have just been made using the old sources (polonium), whose verification, extension, and application to our own investigations is in no sense of secondary importance.

See Tuve, "Report for June 1932," Box 4, MATP.

Queen's University, Kingston, Canada. A pupil of Rutherford from the Manchester days (and, as we have seen, a participant at the 1928 conference), Gray's research in the 1920s had been largely on β- and γ-rays, 'traditional' lines of investigation in radioactivity.[69] Like Tuve, Millikan and others, Gray quickly realised in 1931 that to participate in the emergent field of artificial disintegration, he would require a good polonium source. Like Tuve, he started from scratch, assiduously cultivating hospital physicists (most of whom had also been approached by Tuve and others) in the hope of relieving them of their dead radon tubes.[70] In 1933, Gray spent a term in Cambridge to learn of the latest developments in nuclear research. During his time in Cambridge he was "given every facility to learn what I required for the work I have in mind." Writing back to Queen's to order supplies to enable him to take up and repeat the Cavendish work in Canada, he provided a very full list of the *matériel* necessary for work in transmutation:[71]

From J.G.R. Lilienthal, Wendictendorf, Thuringen, Germany
2 lbs Weiss Kittlack 849A

From H.G. Everett, Park Gate Pharmacy, Salisbury Row, Park Gate
1 lb No.1 hard wax
1 lb No.1 soft wax

From Technical Products Ltd., 31 Great St. Helen's, London EC3
2 4oz jars of apiezon grease M
4 $1^1/_2$ lb tins of sealing compound Q

From the Westinghouse Brake & Saxby Signal Co, King's Cross, London
2 H.T. 8 and 2 H.T. 9 metal rectifiers

From the General Electric Company, Magnet House, Kingsway, London
2 Osram D.E.V. valves for Wynn-Williams α-particle counter
2 Osram H.L. 2 valves

From the British Thomson-Houston Co., Rugby, England
2 Thyratrons B.T. 1
1 Mazda AC/pen
1 Mazda AC2/HL

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69 On Gray, see Lewis (1967); Rutherford to Gray, 22 March, 18 July 1933, Box 1, JAGP.

70 See, for example, Reinhard [New York State Institute for the Study of Malignant Disease] to Gray, 6 July 1931; Beasley [Cleveland Clinic Foundation, Ohio] to Gray, 18 July 1931; Macdonald [Cancer Relief and Research Institute, Winnipeg] to Gray, 30 May 1932; Failla to Gray, 17 June 1932, all Box 8, JAGP. On Gray's attempts to buy beryllium, see also Kemet Laboratories Co. Inc. to Gray, 4 June 1931, Box 1, JAGP.

71 Gray to A.L. Clark, 25 August 1933, Box 8, JAGP. The list was supplemented by two further letters asking for various electrical components, tubing and other laboratory materials. See Gray to Clark 29 August 1933, 7 September 1933, Box 8, JAGP.

This remarkable list was just a small part of what it cost to participate in disintegration experiments and to make neutrons. Reproducing the exciting new experiments carried out in the Cavendish and other European laboratories demanded the reproduction of a very specific material culture, down to the *exact* kinds of valves for the electronic amplifiers, even down to the particular brands of wax and grease used as sealants.[72]

As Gray's experience illustrates, setting up a laboratory for nuclear research was a rather different kind of enterprise in 1932 than it had been only a decade earlier. Indeed, in the early 1930s, nuclear research was an entirely different kind of enterprise than it had been when Rutherford arrived at the Cavendish in 1919, or when Pettersson and Kirsch had begun disintegration research a few years later. The displacement of responsibility for registration of phenomena away from the experimenter and onto cloud chambers and electronic counting equipment was accompanied by a simultaneous displacement of what it meant to do an experiment. Much of the work of experiment now involved setting up and maintaining elaborate valve systems and analysing their output. As Patrick Blackett told Julian Huxley in a 1934 interview, Cavendish experiments now required "apparatus of extreme complexity: innumerable valves and rows of thyratrons flashing, relays clicking, and so on," looking "rather like a cross between the advertisement lights in Piccadilly and the transmitting station of a modern battleship" (fig. 6.1). "Modern physics," he stressed, "uses all the technical assistance it can get."[73]

In this extremely (and increasingly) complex socio-technical milieux, it was easy to look back from the late 1930s through the rose-coloured spectacles of the historical gaze to see a straightforward, teleological picture of technical progress. Electrical counting and

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[72] This is not to suggest, of course, that the reproduction of the material culture of the Cavendish laboratory was *of itself* sufficient to guarantee the successful replication of experiments. Certainly, acquisition of the basic materials for laboratory work was supplemented by frequent communication with various researchers in Cambridge (Gray's own former students proving particularly useful in this regard: see, for example, Wynn-Williams to Gray, 13 November 1933; W.J. Henderson to Gray, 11 May 1933, 11 November 1934, 13 March 1935; W.E. Bennett to Gray, 8 February 1935, Box 1, JAGP.). As I have suggested, it would be interesting to know how the experimental data underlying the extensive debates in the 1930s about the mass and other characteristics of the neutron (Stuewer (forthcoming 1993)) reflected different experimental styles or different forms of laboratory practice. For a suggestive analysis in a different research field, see Harwood (1993). Cf. also Fruton (1990); Daston and Otte (1991). For some general and informative remarks on the significance of the material culture of the laboratory, cf. Stansfield (1990).

[73] Blackett, in Huxley (1934), 209.

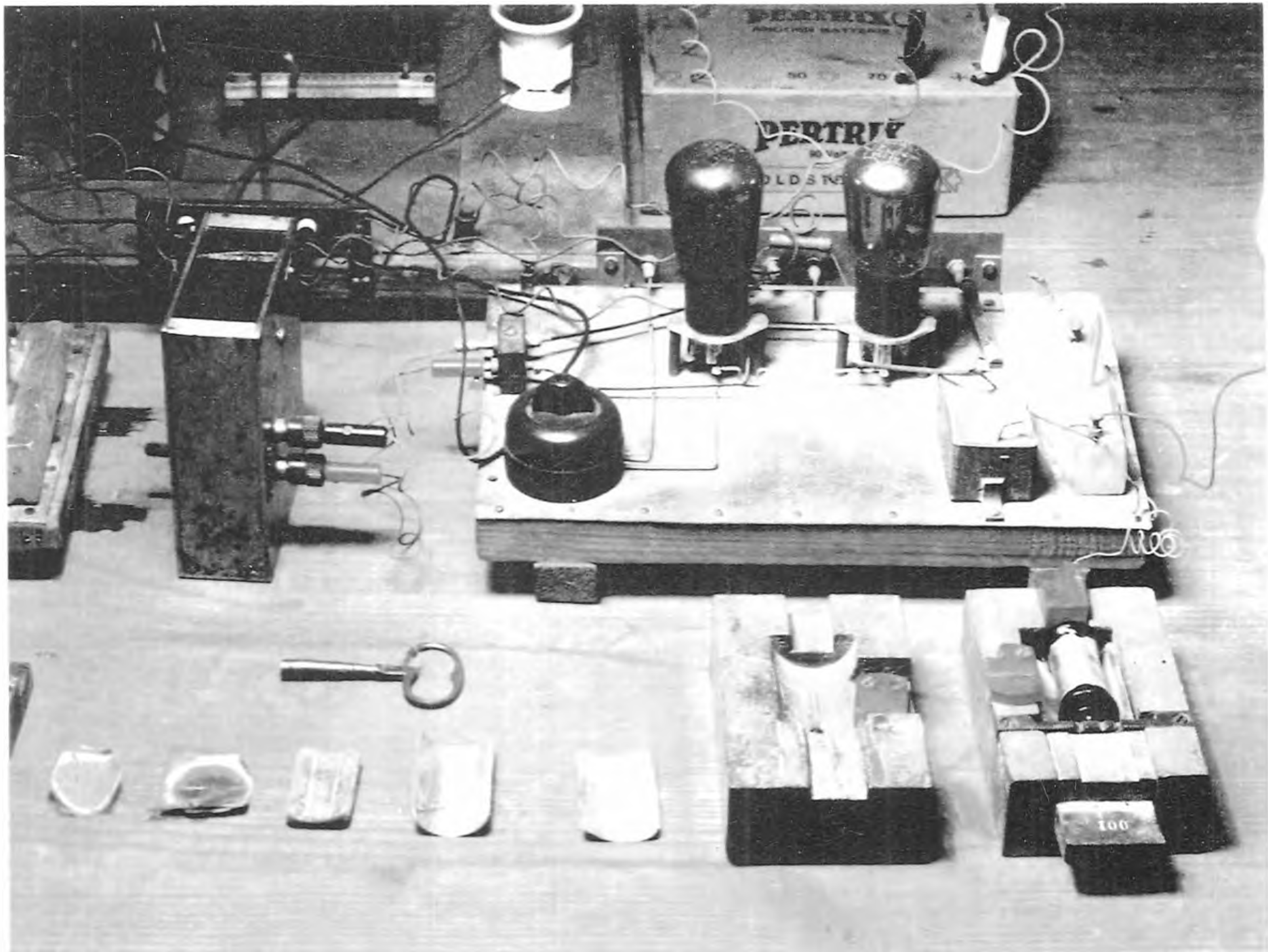

Fig. 6.1 Geiger-Müller counter with lead shield, valve amplifier and mechanical counter, as used in the mid-1930s.

Source: Deutsches Museum.

amplification methods capable of dealing with particles arriving at rates of up to 10,000 a minute were in use at the Cavendish Laboratory by 1938, with yet further development in progress, all "made possible by the great advances in the manufacture of wireless valves and other electrical apparatus." As one insider put it, "from a glance at the present-day equipment of the Cavendish Laboratory one would certainly conclude that the thermionic valve has come to stay in the field of nuclear physics."[74] Precisely *because* of the work that had to be done to make electrical techniques robust and reproducible, and because of the investment of time and labour such work represented, it would now be very difficult to challenge these technologies in the way that scintillation counting had been challenged. In retrospect, stories could be told about the scintillation technique which emphasised its limitations and difficulties, conveniently forgetting that the technique had served as the basis for over a decade's work in artificial disintegration.[75]

With the development of complex electronic counting equipment and the emergence of large-scale electrical engineering experiments requiring entire laboratories to themselves, physics laboratories started to look completely different than they had done only a few years previously. In 1921, state-of-the-art technology had been Rutherford's scintillation screen experiments. In 1931, huge electrical machines developing massive potentials were the order of the day. A visitor to the Cavendish in 1930 seized on the novelty of this form of organisation: "At Prof. Kapitza's laboratory, you [have] to ring to be admitted by a 'flunkey' and [are] confronted with Prof. Kapitza seated at a table, like the arch-criminal in a detective story, only having to press a button to do a gigantic experiment."[76] It was a theme echoed by Rutherford himself. "At Cambridge," he remarked a few years later, "a great hall contains massive and elaborate machines rising tier on tier, to give a steady potential of about two million volts. Near by is the tall accelerating column with a power

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[74] Devons (1938), 41. For subsequent developments, see Lewis (1942); Korff (1946).
[75] See, for example, Aston (1935b), 25.
[76] In "A History of the Cavendish Dinner," entry for 1930, CAV 7/1, CUL.

station on top, protected by great corona shields - reminding one of a photograph in the film of H.G. Wells' 'The Shape of Things to Come.'"[77]

By 1935, particle accelerators and cyclotrons were becoming laboratories in their own right (compare, for example, figs. 6.2 and 6.3), with their own work regimes, division of labour and social organisation.[78] In this brute-force approach to the investigation of the internal structure of matter, "atom-smashing" became the dominant descriptive idiom. As usual, Eddington captured the prevailing sentiment perfectly when he wrote: "In a contest between the sun and the Cavendish Laboratory as to which could do the most violence to a single atom, I would back the Cavendish Laboratory."[79] Here, in this characteristically modern, machine-age type of experiment, progress (and perhaps even the notion of discovery itself) became defined largely in terms of technical criteria – ever-higher voltages, higher disintegration yields and so on. An internal dynamic developed in which the technics themselves became an exciting and important part of the work.[80] Yet the analysis developed in this dissertation raises the intriguing suggestion that particle accelerators, like electrical counting methods, acquired much of their epistemological warrant in the wake of the Cambridge-Vienna controversy.[81]

In the new, particle-oriented culture of the early 1930s,[82] other forms or combinations of

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77 Quoted in Wood (1946), 48. For an introduction to science, fiction and film, see Lambourne, Shallis and Shortland (1990); Weart (1988a), esp. 55-74.

78 On the rise of the particle accelerator as experiment, see *inter alia* Aaserud (1990); Bugos (1992); Galison (1985, 1987); Heilbron, Seidel and Wheaton (1981); Heilbron and Seidel (1989); Hermann, Krige, Mersits and Pestre (1987); Hughes (1992); McMillan (1979); Pestre (1992); Pickering (1984a); Seidel (1992a, 1992b); Traweek (1988).

79 Eddington (1935b), 144.

80 Compare, for example, A.H. Compton (1931); K.T. Compton (1933); Devons (1938); Solomon (1945)[1940]. See also "Smashing the Atom," *Nation* **134** (1932), 587-588; Hughes (1992). Marquis (1986), Galison (1990) and Carey (1992) offer interesting insights into Modern culture.

81 I am *not* suggesting that particle accelerators were developed *because of* the Cambridge-Vienna controversy. Rather, the thought is that such devices acquired their meaning and significance, and were attributed the capacity to yield evidence concerning atomic disintegration, in a situation where conventional disintegration methods using radioactive sources were locked in a sceptical regress. The development of accelerators thus offered a semi-independent means of approaching these experiments. At the same time, however, the development of accelerators of various descriptions at Cambridge, CalTech, Berkeley and Princeton depended a great deal upon local circumstances and contingencies. Much further work is required to show how these instruments' evidentiary status developed during the 1930s.

82 Frank Spedding confessed to Gilbert Lewis in 1934 that "[t]his field is moving so rapidly that one becomes dizzy contemplating it. With talk of the experimental properties of H^3, He^3, He^5, the new artificial radioactive elements, the neutron and positron, and the predicted properties of the neutrino and proton of minus charge, one who has been brought up on the old naïve picture of protons and electrons in the nucleus feels bewildered" (Spedding to Lewis, 1 December 1934, GNLP). Spedding went on: "There was one rather

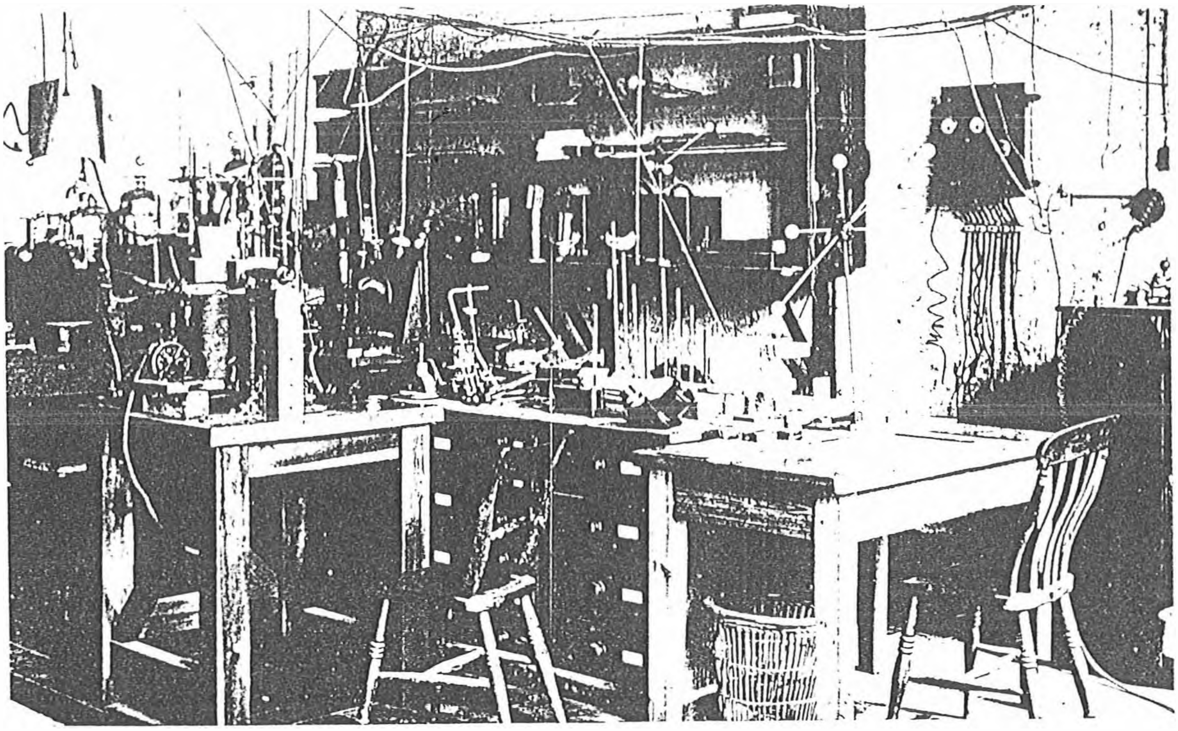

Fig. 6.2 Rutherford's research room, Cavendish Laboratory, early 1920s. Compare with Fig. 6.3.

Source: Cavendish Laboratory.

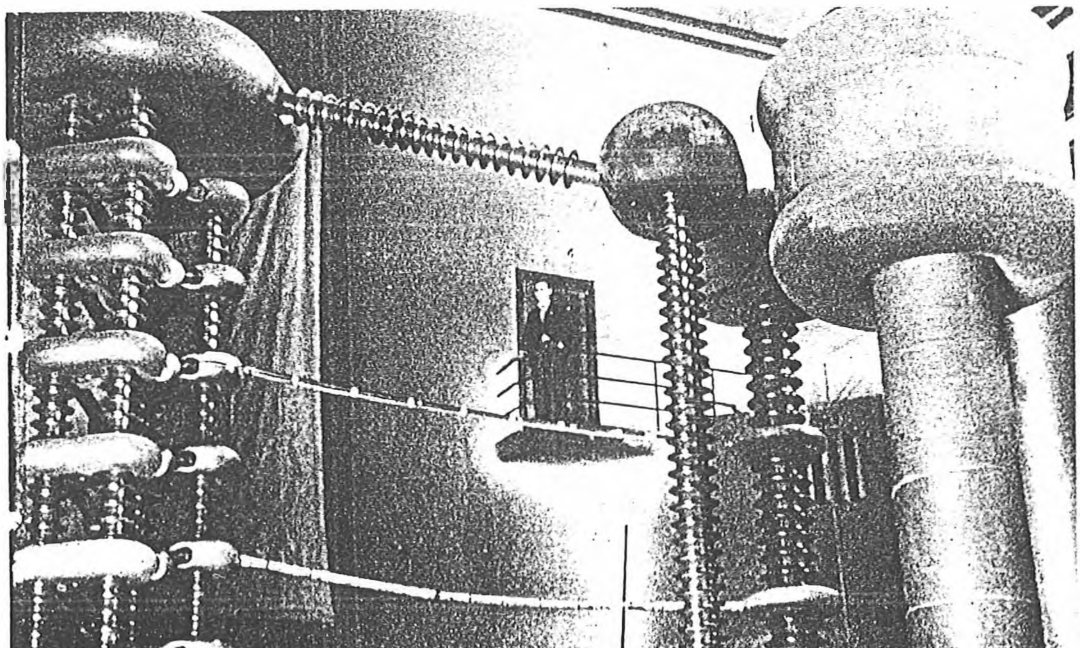

Fig. 6.3 The new high-tension laboratory at the Cavendish, late 1930s. Compare with Fig. 6.2.

instrumentation, too, acquired (or, rather, were attributed) the capacity to yield evidence. The cloud chamber in particular assumed a new prominence, not only because of its central role in neutron work, but also in cosmic ray research. By 1934 the cloud chamber had become, according to Rutherford (who usually regarded himself as judge and jury in such matters) the "final court of appeal by which the validity of our explanations can be judged."[83] At the Cavendish, Blackett and Occhialini linked the Geiger counter to a cloud chamber with a magnetic field and "other luxuries" to produce "no end of queer things," among them the positron.[84] And it was the series of increasingly exotic particles disclosed during the 1930s, the products of what Feather called the 'New Physics of the Nucleus,' which invariably attracted attention, rather than the processes which had given rise to and sustained them. Indeed, this obsessive emphasis on the products, rather than the processes, of science elicited a caustic commentary from Feather:[85]

> To those instructed and kept informed almost entirely through the medium of the popular press, 1932 must doubtless have appeared as the first year of a new era in physical science. There is, of course, a measure of truth in this judgement, but there is a great deal more of exaggeration ... [which] ... lies in the over-emphasis on novelty and the failure to appreciate the extent to which successful scientific research, in almost all cases, draws upon the past for its inspiration. New discoveries appear in the course of time, very rarely with a "Lo here!" or, "Lo there!", but rather by diffused advance from many sides, until somewhere or other the offensive breaks through and all available forces are thrown in to consolidate the position newly gained.

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amusing incident that occurred here. Prof. Born had prepared a rather involved paper on the quantum theory of the nucleus ... He wrote the paper longhand labelling it "For the Conference on Nuclear Physics." He made his "n"'s and "u"'s much alike so that his stenographer in copying it wrote "For the Conference on Unclear Physics." See Weiner (1972), 46-47. Wheeler (1979), 235, gives a slightly different version of the story.

83 Rutherford (1935c),14.

84 C.T.R. Wilson to Schonland, 9 June 1932, 30 January 1933, BFSP; Blackett and Occhialini (1932, 1933); Chadwick, Blackett and Occhialini (1933). Also see Anderson (1932b, 1933); Hanson (1963), 135-165; Brown and Hoddeson (eds.)(1984); Sekido and Elliott (eds.)(1985); De Maria and Russo (1985); Galison (1987), 75-133.

85 Feather (1934), 193. For similar comments, see Blackett (1933), 67 [epigraph to this dissertation]. J.G. Crowther was prominent among those who celebrated the new particle culture. See Crowther (1932e, 1934b); Crowther (1934a), 122-179.

This view and its attack on the "falsity of epochism" were echoed and endorsed by Rutherford himself in a mid-1930s radio broadcast:[86]

> To the average man with no special knowledge, the progress of Science often appears to be governed by more or less accidental discoveries of new facts or by the catastrophic demolition of old theories. The scientific man appears rather like a conjurer who lifts the hat and shows the rabbit underneath. This is very far from the truth, for while in a sense the investigator in the course of his researches may bring to light an unexpected fact or an unsuspected relation, yet this new discovery depends on the work of many others who have developed the methods and advanced the ideas that made the successful experiment possible ... If we examine closely the history of science we find that progress shows a certain orderly advance, though with occasional ups and downs, and that the outstanding discoveries of new facts or the development of new theories have in general a long history behind them and have depended on the previous work of hundreds and maybe thousands of scientific men.

Rutherford knew very well that the experimental achievements in the first few months of 1932 – the disclosure of the neutron and the 'splitting of the atom' – were contingent upon the changes in laboratory practice over the previous three years, a point made explicitly by P.M.S. Blackett, who noted that "the discovery of the neutron, a discovery of the utmost importance, has depended technically upon the preparation of large sources of polonium and on the development of the valve amplifier and Hoffmann electrometer to detect the ionisation due to single particles."[87] I hope I have gone some way towards vindicating that view and showing its relevance for our understanding of the development of nuclear physics.

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[86] Rutherford, "Science in the Making," PA 33, RP. Citing the work of Wynn-Williams, Greinacher, Geiger, Tuve and C.T.R. Wilson, Rutherford (1935c), 14, noted that "[t]he rapidity of advance in the last few years has been in large part due to the great improvement in the technical methods of attack." Cf. his remarks in similar vein at the B.A.A.S. in 1923, quoted in Chapters Two and Three. See also Soddy to W.A. Noyes, 22 February 1936, FSP.
[87] Blackett (1933), 82.

4. Conclusion: The Social Origins of 'Nuclear Physics'

It has become an article of faith in the history of nuclear physics that the disclosure of the neutron by Chadwick at the Cavendish Laboratory in 1932, the "*annus mirabilis* of nuclear physics," was merely the correct interpretation of a series of misinterpreted observations by Bothe and the Joliot-Curies over the preceding few months. The account I have given here challenges that orthodoxy. Against a dominant historiography which sees the transition from scintillation counters to electrical methods as inevitable technical progress, and which casts the the discovery of the neutron as a decisive step forward in the elucidation of the structure of the nucleus, I have emphasised construction and contingency, arguing that the neutron and, indeed 'nuclear physics' itself must be understood as the products of a very specific social, material and intellectual culture. The neutron emerged in 1932 into a community which was already taking shape in the wake of the Cambridge-Vienna controversy, a community already defined and delineated by shared regimes of practice. Embodied in those practices itself, the neutron was therefore assimilated relatively easily into the disintegration experiments which had occupied experimentalists for the previous three or four years.[88]

Much as disintegration work with heavy hydrogen after 1933 depended upon the circulation of the rare samples of heavy water supplied on request by Berkeley's Gilbert Lewis,[89] the muliplication of sites employing electrical counters, polonium, cloud chambers and other material technologies established a network of laboratories and a trans-national

[88] Nor did the neutron mark a radical break in the theoretical understanding of the nucleus. Writing to Bohr in November 1932, Samuel Goudsmit commented on Heisenberg's lectures at a recent summer school in theoretical physics: "We followed with great interest his new ideas about the nucleus but everyone feels that there still are great difficulties. It is strange and regrettable that the discovery of the neutron did not give some more fertile clues for progress. In many respects the situation has not changed much from what it was at the Rome meeting a year ago, except that the difficulties can now be formulated more sharply." Goudsmit to Bohr, 4 November 1932, quoted in Weiner (1972), 43. See also Bohr to Goudsmit, 28 December, 1932, BSC. For early theoretical work on the neutron, see Bromberg (1971).

[89] As K.K. Darrow put it, there was a period in which "nearly every paper ... began with an acknowledgement to Lewis for a small amount of water rich in heavy hydrogen which the fortunate author had received from him" (Darrow (1934c), 108). Even the Cavendish relied upon Lewis for such a sample. See Fowler to Rutherford, 5 April 1933; Lewis to Rutherford, 15 May 1933, RP; Fowler to Lewis, 9 May 1933; Rutherford to Lewis, 30 May 1933, Box 3:64, GNLP; Oliphant, Kinsey and Rutherford (1933). On the discovery of heavy hydrogen, key sources include Urey, Brickwedde and Murphy (1932a, 1932b, 1932c); Brickwedde (1982); Stuewer (1986c).

community of researchers addressing similar issues with similar tools and resources. It is extremely significant, I think, that contemporary commentators recognised and reflected upon the emergence of precisely such a community, and commented upon its reactive, opportunistic character. K.K. Darrow, for example, noted that the manner in which the neutron was disclosed was "abnormal":[90]

> It is natural to expect that when a physicist has made and published the first advances in a field till then untrodden, those who wish to follow will have to spend so long a time in gathering resources like to his, in imitating his equipment and in learning his technique, that in the meantime he will go the rest of the way. In the case [of the neutron] there would have been good reason to expect it, since in the closely allied field of transmutation all research was confined to a single laboratory [Cambridge] for full three years after the first announcement, and to that laboratory and one other [Vienna] for fully another five, - this despite the fact that controversy soon flared up between the two, so that decisive word from some third institute was ardently desired. Yet, when from the Reichsanstalt in the last month of 1930 it was made known that alpha-particles elicit penetrating rays from such elements as lithium, beryllium and boron, the Institut du Radium and the Cavendish Laboratory *were equipped and were alert.* Their contributions came within intervals of months, not years; and progress could scarcely have been swifter, had the work all been ordered by a sole far-seeing mind.

But it was no "sole far-seeing mind" that shaped the community which gave birth to and sustained the neutron. That community was forged though the contingencies of the Vienna and Columbia heresies and through the availability – or otherwise – of polonium and the other resources necessary to participate in experimental work. In particular, the strategic decision of the actors in the Vienna controversy to keep the outcome of Chadwick's visit to Vienna private must, I think, be seen as central to any understanding of the development of nuclear research.[91] The widespread perception (in no way discouraged by the regular appearance of papers from Vienna subsequent to 1928) that the deadlock was ongoing did much to structure the development of the discipline for, as I have tried to demonstrate, at least five groups of researchers entered the field of disintegration research between 1927

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90 Darrow (1933a), 58-59, my emphasis.
91 Cf. Morus (1992b), esp. 27-28.

and 1931 in an attempt to arbitrate between Cambridge and Vienna, or at least to shed light on the contested issues (as Darrow's commentary implicitly recognises).

The rapid expansion in the number of laboratories and workers involved in nuclear research in the early 1930s produced an implicit conflict of disciplinary identities. Having established their careers in the subject, Rutherford, Chadwick, Meyer, Geiger and Bothe consistently placed themselves squarely in the tradition of radioactivity. The new workers, however, came from a variety of different backgrounds. Crucially, they lacked that same sense of identification with the discipline of radioactivity. They sought a disciplinary identity elsewhere, gradually appropriating and domesticating the term "nuclear physics" (a term which emerged in English only in the early 1930s, becoming established by about 1934) to describe their enterprise.[92] In due course, the role of the Cambridge-Vienna controversy lost its original significance as new concerns (like the neutron, the positron and the diplon) came to the forefront and laboratories acquired an inertia of their own.

Yet "nuclear physics" was not a term which sat comfortably with those who had long worked in radioactivity and who clearly continued to situate themselves within the older discipline. After a visit to Münster with Rutherford in April 1932, Chadwick wrote to Meitner noting how pleasant it had been "to see all the radioactivists gathered together."[93] A glance at fig. 6.4 shows clearly that the "radioactivists" were precisely those who had worked in radioactivity since before the war and who shared – and considered themselves as sharing – a disciplinary past. Throughout the 1920s, laboratories like Rutherford's Cavendish and Meyer's Institut für Radiumforschung still regarded themselves as engaged fundamentally in radioactivity research. Indeed, the term 'nuclear physics' was alien to the

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[92] Unknown in 1920, the category emerged in the late 1920s and early 1930s (through the German *kernphysik* and French *physique nucléaire*) as a general descriptive term for several kinds of nuclear research. In German, the term most often connoted theoretical work, while in France it was generally applied to experimental research. The term still appeared in quote marks in Anderson (1935). On the notion of a disciplinary identity, see Barkan (1992); Graham, Lepenies and Weingart (eds.)(1983); and cf. Hunt (1991). For a more general introduction to the function of 'tradition,' see Hobsbawm and Ranger (eds.)(1983); Wright (1985).

[93] Chadwick to Meitner, 5 June 1932, MTNR 5/3, LMP. See also Rutherford to Bohr, 26 May 1932, RP. The term "radioactivists" thus emerges in a rather satisying way as an actors' category, and as a complement to the emergence of "nuclear physics."

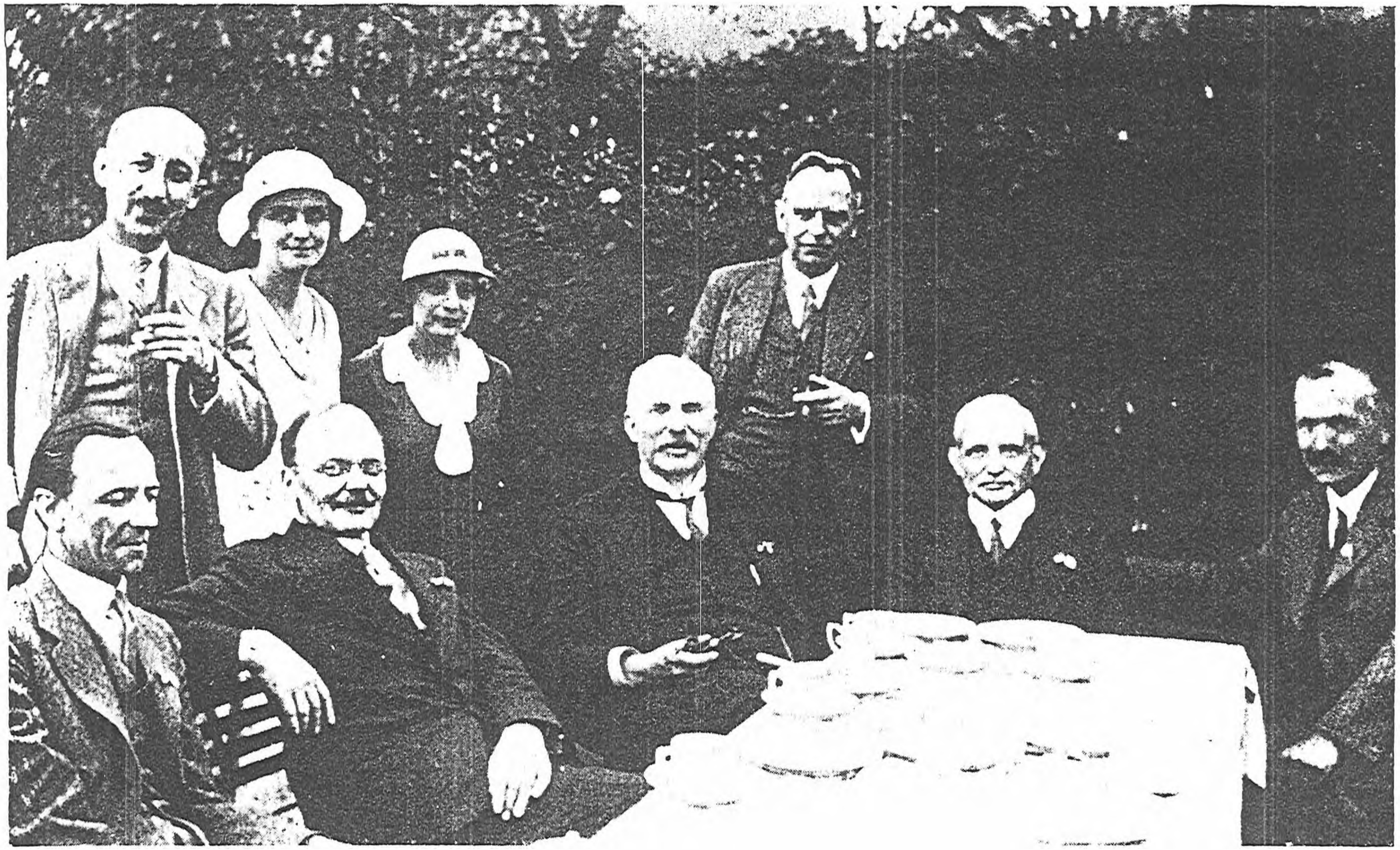

Fig. 6.4 The Radioactivists - what Chadwick called an "informal group of old friends on the occasion of a meeting of the Bunsengesellschaft in Münster, 1932." Left to right: Chadwick, Hevesy, Frau Geiger, Geiger, Meitner, Rutherford, Hahn, Stefan Meyer, Przibram.

Source: Snapshot taken by F. Paneth, in CPR 3, facing 288.

Cavendish Laboratory until about 1934, when (significantly) the younger members of the research staff began so to describe their work.[94]

Clearly, then, we cannot take the term 'nuclear physics' for granted. It was a term which acquired meaning in and through the emergence and development of the investigative community described in this dissertation. As we have just seen, it may even have meant different things to different members of that community. I have therefore tried to characterise the emergent nuclear physics community through the spread of instruments, techniques, practices and people from site to site. One of the elements crucial to this multiplication of the number of places where particular material technologies could be put to work was the social technology of the *visit*. Visits and personal contact are necessary for the acquisition of skills and the replication of experiments, certainly – that, after all, has been one of the guiding principles of the sociology of scientific knowledge over the last decade – but they are equally important for the denigration of particular forms of laboratory practice and the destruction of experimental claims.[95] The integrity of the site of knowledge production was therefore foundational to the production, warranting and circulation of knowledge of the atomic nucleus.

The laboratory visit displays clearly the dual nature of the emergent culture of nuclear physics, in which a tension existed between the normative and ideological demands of openness, on the one hand, and the pragmatic requirement for privacy, even secrecy, on the other. In that sense, visits emerge in this dissertation as complex sociological episodes, constituted in large part through the categories of trust, openness and integrity. But the analysis presented here also draws attention to instruments, to the details of laboratory practice and to the local creation and maintenance of such intangible but constitutive

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94 Compare Feather (1934, 1936). The transition is also illustrated by the change in title between the first and second editions of Gamow's monograph: *The Constitution of Atomic Nuclei and Radioactivity* (1931), and *Structure of Atomic Nuclei and Nuclear Transformations* (1937). As late as 1934, after a trip to the West Coast during which he had discussions with Lawrence at Berkeley and Lauritsen at Caltech, Tuve admitted to Hafstad: "Am nearing the end of a hell of an uncomfortable trip. Nuclear physics isn't physics yet, and symposiums on a subject that isn't born yet are premature ... this is just a tardy warning that we haven't got the world by the tail yet." See Tuve to Hafstad, 2 July 1934, Box 16, MATP; Cornell (1988), 62. For the radioactivists, "modern alchemy" or some cognate became the dominant description of their work in the 1930s. Compare, for example, Andrade (1936); Rutherford (1935a, 1937).

95 Compare Collins (1983, 1988b); Collins and Pinch (1982).

considerations as credibility and certitude. These are the categories which concerned nuclear researchers in their day-to-day practice. They are also the categories which should concern the historian.

The social, technical and intellectual histories of radioactivity and nuclear physics are intimately and inextricably intertwined through the complex processes by which facts, instruments and theories came to acquire meaning and evidential significance. To understand radioactivity and nuclear physics in the interwar period is therefore to understand the changing social and intellectual geography within and between the institutions involved in the field of nuclear research, for it was in and through institutions that nuclear researchers sought to make themselves and their work credible. In adopting such an approach, have perhaps raised more questions than I have answered. But if I have at least succeeded in showing that questions concerning practice, evidence and certitude are relevant to our understanding of the history of 'nuclear physics,' then this essay will have served its purpose.

Finale

Apotheosis of the True Neutron

from

The Blegdamsvej Faust

Performed at the Spring Conference on Theoretical Physics,
Copenhagen, April 1932[96]

WAGNER [Chadwick] *appears as the personification of the ideal experimentalist, balancing a black ball on his finger, and says with pride*:

The Neutron has come to be.
Loaded with Mass is he.
Of Charge, forever free.
Pauli, do you agree?

MEPHISTOPHELES [Pauli]:

That which experiment has found -
Though theory had no part in -
Is always reckoned more than sound
To put your mind and heart in.
Good luck, you heavyweight Ersatz -
We welcome you with pleasure!
But passion ever spins our plots,
And Gretschen is my treasure!

MYSTICAL CHORUS [omnes]:

Now a reality,
Once but a vision.
What classicality,
Grace and precision!
Hailed with cordiality,
Honored in song,
Eternal neutrality
Pulls us along!

FINIS

[96] The complete script of the 'Blegdamsvej Faust' and some introductory remarks by Gamow are given in Gamow (1985)[1966], 165-214. 'Gretschen' was Pauli's 'neutron,' subsequently re-named 'neutrino.' See Brown (1978); Atchley (1991).

Appendix 1

1851 Exhibition Science Research Scholars in Experimental Physics at the Cavendish Laboratory, 1919-1936

Name	Nominating University	Tenure	Ph.D.	Subsequent Appointment
J. Chadwick	Manchester	1919	1921	Assistant Director, Cavendish Laboratory
P.W. Burbidge	New Zealand	1919-1921	-	Auckland University College
G. Shearer	Edinburgh	1919-1921	1923	University College London
G.H. Henderson	Dalhousie	1919-1922	1922	University of Saskatchewan
E.S. Bieler	McGill, Montreal	1920-1923	1923	McGill University, Montreal
J.K. Roberts	Melbourne	1920-1922	1923	NPL, Teddington
J.S. Rogers	Melbourne	1922-1924	-	University of Melbourne
J.F. Lehmann	Alberta	1923-1926	1927	Research Physicist, I.C.I. Billingham
W.L. Webster	Toronto	1923-1925	-	1851 Senior Scholarship, Cambridge
L.H. Martin	Melbourne	1923-1926	1928	Rockefeller Fellowship, Cambridge

D.C. Rose	Queen's, Kingston	1924-1927	1927	1851 Senior Studentship, Cambridge & Bristol
S.W. Watson	South Africa	1925-1927	1932	Rhodes University College, Grahamstown
H.M. Cave	Queen's, Kingston	1926-1929	1930	Queen's University, Kingston, Ontario
R.R. Nimmo	New Zealand	1926-1929	1929	Univ. Western Australia, Perth
G.C. Laurence	Halifax, Nova Scotia	1927-1929	1930	National Research Council, Ottawa
F.R. Terroux	McGill, Montreal	1927-1930	1931	McGill University, Montreal
M.L. Oliphant	Adelaide	1927-1929	1929	1851 Senior Studentship, Cambridge
B.F. Schonland	Cape Town	1927-1928	-	Cape Town
E.T.S. Walton	Dublin	1927-1930	1931	Cavendish Laboratory; Dublin
B.W. Sargent	Queen's, Kingston	1928-1930	1933	Queen's University, Kingston, Ontario
H.C. Webster	Melbourne	1928-1930	1932	Bristol; Queensland
J.D. McGee	Sydney	1928-1931	1931	Research Physicist, E.M.I. Hayes
W.A. Macky	New Zealand	1928-1931	-	Meteorolgical Service, Malaya
C.B.O. Mohr	Melbourne	1930-1933	1933	D.S.I.R. Senior Award, Cambridge
F.H. Nicoll	Saskatchewan	1931-1933	1934	Physicist, E.M.I. Hayes
J.L. Pawsey	Melbourne	1931-1934	1935	Research Physicist, E.M.I. Hayes
E.C. Halliday	Cape Town	1932-1934	-	Witwatersrand

E.H.S. Burhop	Melbourne	1933-1935	1938
W.E. Bennett	Queen's, Kingston	1934-1937	1937
D.P.R. Petrie	Melbourne	1934-1937	-
J.S. Marshall	Queen's, Kingston	1935-1938	1940
J.C. Bower	Melbourne	1935-1938	1940
A.D. Misener	Toronto	1936-1938	1939
C. O'Ceallaigh	Nat. Univ. Ireland	1936-1938	-

Bristol; Toronto

University College, Cork

1851 Exhibition Senior Research Students in Experimental Physics at the Cavendish Laboratory, 1919-1936

Name	Nominating University	Tenure	Subsequent Appointment
H.W.B. Skinner	Cambridge	1925-1927	Bristol
W.L. Webster	Cambridge	1926-1928	London Schoolof Economics
D.C. Rose	Cambridge	1927-1929	Queen's University, Kingston, Ontario
E.J. Williams	Swansea	1927-1929	Manchester
T.E. Allibone	Cambridge	1928-1930	Metropolitan-Vickers Electrical Co., Manchester
C.E. Wynn-Williams	Bangor	1928-1931	Cambridge; Imperial College, London
M.L. Oliphant	Cambridge	1929-1932	Cambridge; Birmingham
E.C. Childs	King's, London	1931-1934	School of Agriculture, Cambridge
C.B.O. Mohr	Cambridge	1934-1936	Cape Town
D. Shoenberg	Cambridge	1936-1939	Cambridge

Appendix 2

Ph.D. Dissertations in Experimental Physics, University of Cambridge, 1921-1936

1921

20 June	J. Chadwick	The Charge on the Atomic Nucleus

1922

17 February	H.P. Waran	Effect of the Magnetic Field on the Intensity of Spectrum Lines
16 June	D.A. Keys	Rate of Reaching Equilibrium Distribution of Potential in a Discharge Tube
17 June	G.H. Henderson	Passage of α Particles through Matter

1923

20 January	B.N. Banerji	Phenomena of Discharge in Pure Gases
25 January	J.K. Roberts	Conservation of Energy in Hydrogen Discharge
8 May	G. Shearer	Emission of Electrons by X-Rays
15 June	P. Kapitza	Passage of α-Rays through Matter
18 June	E.S. Bieler	Law of Force … of the Atomic Nucleus
12 October	C.D. Ellis	The β-Ray of Disintegration

1924

16 January	H.D. Smyth	New Method for Studying Ionizing Potentials
7 May	B.F.I. Schonland	Scattering of β Particles
5 June	H. Robinson	Secondary Corpuscular Rays
18 June	T. Alty	Cataphoresis of Bubbles of Various Gases in Water
19 June	L.F. Bates	On Particles of Long Range
17 December	E.C. Stoner	Absorption of High Frequency Radiation

1925

28 May	N. Ahmad	Absorption and Scattering of X-Rays

1926

31 March	K.G. Emeléus	Methods for Studying Single Ionizing Particles
22 June	R.W. Gurney	Alpha- and Beta Particles
22 June	H.W.B. Skinner	Some Experiments in Magneto-Optics
23 June	W.L. Webster	Some Magnetic Properties of Single Crystals of Iron
25 June	D.H. Black	The β-Ray Spectra of Some Radioactive Bodies
9 August	K.B. Blodgett	A Method of Measuring the Mean Free Path of Electrons in Ionized Mercury Vapour
15 December	G.H. Briggs	The Decrease of Velocity and the Straggling of Alpha Particles

1927

20 January	R.J. Clark	Experiments on Atomic Rays
18 March	R.A.R. Tricker	The Inertia of the Electron at High Velocities
22 April	E. Madgwick	The Passage of β-Rays through Matter and the Continuous β-Ray Spectra of Ra B+C, Ra E and Th B+C
11 June	J.F. Lehmann	The Total Ionisation due to the Absorption of Slow Cathode Rays
17 June	M.A.F. Barnett	An Experimental Proof of Large-Angled Deviation of Wireless Waves in the Upper Atmosphere
20 June of	D.C. Rose	The Scattering of Alpha Particles and the Reflection Electrons from a Crystal
8 December	D.M. Morrison	A Study of the Chemical Activity of Helium
8 December	T.H. Osgood	The Total Ionisation Produced in Gases by Slow Electrons
8 December	W.A. Wooster	β and γ Rays: Their Part in Radioactive Disintegration

1928

10 February	L.H. Martin	The Absorption of X-Rays
11 February	R.W. Ditchburn	The Continuous Absorption of Light in Potassium Vapour
25 June	J. Chariton	Some Experiments Concerning the Counting of Scintillations Produced by Alpha Particles
25 June	J.D. Cockcroft	The Condensation of Molecular Streams
25 June	M.C. Henderson	The Scattering of Beta Particles and the Heating Effect of Radium and Thorium Products
25 June	F.C. Sharman	On the Secondary Electronic Emission from Solid Metal Surfaces

1929

25 May	R.R. Nimmo	Some Investigations with an Expansion Chamber
5 July	T.W. Wormell	Observations on Electrical Phenomena Produced by Thunderstorms and Showers
5 July	C.E. Wynn-Williams	The Production of Short Electric Waves, and their Absorption by Matter
8 November	C.F. Powell	The Condensation of Water Vapour
8 November	E.J. Williams	Passage of β-Particles through Matter
13 December	M.L. Oliphant	The Neutralisation of Positive Ions at Metal Surfaces, and the Emission of Secondary Electrons

1930

12 June	N.A. de Bruyne	The Emission of Electrons from Metals under the Action of Intense Electric Fields
13 June	F.A. Arnot	Collision Problems in Gases
13 June	H.M. Cave	The Rate of Disintegration of Radium
13 June Rise	C.A. Lea	Some Experiments Connected with Scintillations - An Attempt to Detect the Alpha Particles which Give to the Branch Product Radium C
13 June	P. White	On Certain Phenomena Connected with the Passage of Fast Electrons through Matter, with an Appendix on the Upper Limit to the Energy of the β-Rays of Radium B+C
13 November	T.E. Appleyard	Light Excitation by Slow Electrified Particles

13 November	E.E. Watson	Single Scattering of Electrons in Helium
5 December	N. Feather	A Study of Certain Corpuscular Radiations of the Active Deposits of Radium and Thorium by the Expansion Chamber Method
5 December	G.C. Laurence	Velocities and Ranges of Alpha Particles

1931

4 February	J.A. Chalmers	Some Problems in Radioactivity
4 February	J.L. Hamshere	The Mobility of Ions in the Air
4 February	F.A.B. Ward	The Application of a Valve Amplifier to the Detection of Single Ionising Particles
27 March	G.H. Aston	Intensities of the Radiations Emitted by β-Ray Disintegrations
12 June	L.H. Gray	The Absorption of Gamma-Rays
12 June	W.H. Watson	The Emission of Positive Electricity from Metals - The Symbolism of Electricity
16 July	E.T.S. Walton	The Production of Fast Particles. Galvanometer and Oscillograph Design
12 November	J.E.I. Cairns	The Electrical Conductivity of Thin Metallic Films
12 November	J.D. McGee	Some Investigations of the Properties of α-Ray Recoil Atoms
12 November	F.R. Terroux	Applications of the Expansion Chamber to the Study of Fast β-Rays

1932

16 February	S.W. Watson	The Heating Effects of Radium and Thorium Products, Metastable States and Afterglows
8 April	H.J.J. Braddick	The Detection of the Rate of Emission of Particles from Radium Products, and the Energy Changes in Radioactive Disintegration
8 April	J.E.R. Constable	Disintegration in the Nuclei of Light Elements Produced by their Interaction with Alpha Particles
28 June	R.L. Aston	On Artificial Seismic Disturbances
28 June	F.C. Champion	On Some Applications of an Automatic Expansion Chamber to the Investigation of the Collisions of α-Particles with Helium, and of Fast β-Particles with Electrons

28 June	E.C. Bullard	Electron Scattering ...
17 August	E.A. Stewardson	The Thermal and Electrical Dissociation of Gases
16 November	H.C. Webster	Some Phenomena Connected with the Interaction of α-Particles with Electrons and Atomic Nuclei
7 December	R.M. Chaudri	The Action of Positive Ions in the Electrical Discharge Through Gases
7 December	W.R. Harper	On the Ionization of Hydrogen by X-Rays
7 December	G.T.P. Tarrant	The Interaction of High Energy γ Radiation with Atomic Nuclei

1933

11 February	P.B. Moon	Positive Ions and Metal Surfaces
11 February	E.C. Pollard	Artificial Disintegration of the Nuclei of Light Elements
11 February	B.W. Sargent	The Disintegration Electrons
29 July	C.B. Mohr	Atomic Collision Phenomena

1934

2 January	W.E. Duncanson	Investigations in the Structure of Light Nuclei
2 January	J.P. Gott	Measurement of the Charge Carried by Beta-Particles with Application to Special Problems - Experiments Relating to Thunderstorms
2 January	H.O.W. Richardson	The Application of the Wilson Expansion Chamber to Investigate β-Rays of Low Energy in Radioactive Decay
2 January	F.W.G. White	Some Studies of the Propagation of Wireless Waves
25 April	R.W. Revans	The Oscillations of an Ionised Gas
24 October	B.V. Bowden	The Analysis of Alpha-rays and the Structure of Radioactive Nuclei
24 October	E.C. Childs	The Scattering of Slow Electrons by Atoms and Molecules
24 October	R.C. Evans	The Evaporation of Ions and Atoms of the Alkali Metals from Hot Surfaces

24 October	W.B. Lewis	The Accurate Analysis of α-Particles Emitted by Radioactive Substances
24 October	F.W. Nicoll	The Scattering of Electrons by Atoms and Molecules
24 October	E.L.C. White	An Experimental Investigation of the Ionosphere by means of Electromagnetic Waves
24 October	P. Wright	Applications of Valve Methods of Recording Ionising Radiations

1935

29 May	H. Miller	The Transmutation of Light Elements by Alpha Particles
29 May	J.L. Pawsey	An Experimental Study of the Intensity Variations of Downcoming Wireless Waves
13 November	W.J. Henderson	The β-Ray Disintegration
13 November	D.E. Lea	Secondary Gamma Rays Excited by the Passage of Neutrons through Matter

1936

7 April	N.S. Alexander	The Beta and Gamma Rays of Thorium Bodies
7 April	D. Shoenberg	The Magnetic Properties of Bismuth
7 April	R.A. Smith	A Study of Collisions between Heavy Atomic Particles with Special Reference to the Properties of Positive and Negative Ions
18 December	H. Carmichael	The Development of a New Electrometer and an Investigation of Cosmic-Ray Ionization-Bursts
18 December	B.M. Crowther	The Separation of Isotopes
18 December	M. Goldhaber	Some Experiments on the Structure of Light Nuclei
18 December	A.N. May	The Disintegrations of the Light Elements by Alpha Particles
18 December	G.E. Pringle	Some Problems in Nuclear Physics, Especially the Anomalous Scattering of Alpha Particles
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2. Sound recordings
3. Unattributed books and articles published before 1945
4. Books and articles published before 1945.
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