Negotiating Progress: Promoting 'modern' physics in Britain, 1900-1940

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Contents

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Abstract

The first four decades of the twentieth century was a period of rapid development in physics. The late nineteenth century discoveries of X-rays, Becquerel rays and subatomic particles had revealed new properties of matter, and the early twentieth century quantum and relativity theories added to the notion that the discipline was undergoing a fundamental change in thought and practice. Historians and scientists alike have retrospectively conceived of a sharp divide between nineteenth century and twentieth century physics, applying the terms 'classical' and 'modern' to distinguish between these two practices.

However, recent scholarship has suggested that early twentieth century physicists did not see this divide as self-evident, and in fact were responsible for consciously constructing these categories and definitions. This thesis explores the creation of the terms 'classical' and 'modern' physics in Britain, and the physicists responsible. I consider how these terms were employed in 'public' arenas (lectures, books, newspapers, museums) influencing the wider reception of 'modern' physics. I consider not only the rhetorics employed by 'modern' physicists, but also those we would now consider to be 'classical', revealing a diverse range of potential definitions of 'modern' physics. Furthermore, even within the 'modernists' themselves, there was considerable disagreement over how their work was to be presented, as industrially applicable, or of value simply as intellectual knowledge in and of itself. There were also different notions of how scientific 'progress' should be portrayed, whether knowledge advanced through experimental refinement or theoretical work.

Early twentieth century 'modern' physics appeared to discard long held theories, rejecting much of the discipline's past. As such, physicists' connection to the legacy of Newton was under threat. Furthermore, the instability of science more generally was revealed: if physicists had shown the old theories to be wrong, then why should the new ones be any different? This had severe implications as to how the public placed 'trust' in science. I explore how physicists carefully managed the 'public' transition from 'classical' to 'modern' physics, regaining public trust during a period of scientific 'revolution' and controversy.

Declaration

No portion of the work referred to in the thesis has been submitted in support of an application for another degree or qualification of this or any other university or other institute of learning

Copyright Statement

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My Dad for not telling me to get a 'proper' job. My mum for using subliminal witchcraft to set me on the path to the history of science. My siblings because they'd probably like to see their names down here – Camilla, David and Sonia. [Camilla – the names are listed in descending order of preference] [Sonia – the names are listed in ascending order of preference] [David – the names are listed alphabetically, but my favourite's in the middle]

And finally Sam, who has always wanted to be formally acknowledged.

Note on references

The majority of my references are listed in the footnotes as [Name (Year)]. Further details can be found in the bibliography under ‘Primary Published Sources’ and ‘Secondary Sources’. For contemporary obituaries, which can be considered to fall into both categories, I have placed them in ‘Secondary Sources’.

For newspaper articles and archival sources, I include full details in the footnotes. The footnoted references to archival sources contain an abbreviation of the archival collection; a key to these is supplied at the beginning of the bibliography.

I have not included in the bibliography the published reports of the annual meetings of the British Association for the Advancement of Science, and instead listed them in the footnotes as [*Report of the British Association for the Advancement of Science* (19xx)].

Many articles in *The Times* are published anonymously, but the name of the author can be found in the marked copies, located at the News International Archive and Record Office. Where I have referred to such an author in the text, but not in a corresponding footnote, the author information was found in the marked copies and not detailed in the article itself.

Chapter One: Introduction and Literature Review

1.1. The 'public' reception of 'modern' physics

REVOLUTION IN SCIENCE.

NEW THEORY OF THE UNIVERSE.

NEWTONIAN IDEAS OVERTHROWN.

The words above appeared on the top right hand corner of page 12 of *The Times* on 7 November 1919.[1] The article which accompanied these dramatic pronouncements discussed a meeting held the previous day, during which various scientists had debated a possible experimental verification of Einstein's general theory of relativity. The topic was an esoteric physical theory, proposing that time and space were interdependent and relative to the motion of the observer, and was mostly incomprehensible to anybody without a considerable amount of mathematical training. But headlines such as the one above helped create a wider interest in this event, with reference to revolution and overthrow. In the wake of the Great War which had ended almost one year before, and the earlier Russian revolutions of 1917, these words had the potential to resonate far beyond the experiences of physicists. Indeed on the opposite side of this page, a larger headline referred to 'The Glorious Dead', and introduced an article about the first anniversary of the Armistice that had ended the First World War.[2] A message from King George V was printed, inviting the citizens of the British Empire to observe two minutes of silence in remembrance of those who had died in the war. The narrative constructed around Einstein's theory also involved remembrance, because in the aftermath of a 'revolt' there is destruction, the desertion of those who do not fit into a new regime. In the case of the 1919 'Revolution in Science', it became apparent that the victim would be Sir Isaac Newton.

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[1] 'Revolution in Science. New Theory of the Universe. Newtonian Ideas Overthrown.', *The Times*, 7 November 1919, p.12.

[2] 'The Glorious Dead. King's Call To His People., Armistice Day Observance., Two Minutes' Pause From Work.', *The Times*, 7 November 1919, p.12.

Newton's name represented far more than simply another deceased scientist. He had come to be regarded as 'the world's first scientific genius', and one which British physicists could lay particular claim to.[3] Newton was a national hero, but his influence extended far beyond Britain. His work was only of direct relevance to physicists, but he was regarded as a founding father of 'modern' science.[4] His legacy allowed physicists to frame their discipline as a foundational science, underpinning all others, with the actions and properties of all natural phenomena reduced down to Newton's fundamental laws of mechanics.[5] By 1919, however, much of this narrative was under question, with the emergence of new discoveries and theories which threatened to undermine the discipline's very foundations. The category of 'modern' physics was emerging, and coming to be partially characterised not by its continuation from Newton's work but by its departure. Physicists were in danger of losing their close connection with this hero of science. And the loss of Newton was representative of a much larger problem, concerning the relationship of modern physics to past theories. If modern physicists had indeed "overthrown" Newton's laws of mechanics, then this had unwelcome implications regarding the stability of the discipline and its ability to produce objective knowledge. If laws that had been held as true for nearly 300 years were now shown to be false, then why should anybody trust the new theories to be any more reliable? The transition from 'classical' to 'modern' thus needed to be very carefully managed if physicists were to maintain public trust in physics, and in science more generally. My thesis explores how physicists carefully constructed narratives of progress, tying the past to the present in order to "save" their practice from potential disrepute.

Furthermore, the categories of classical and modern physics were not self-evident but rather created for particular purposes. During the 1919 'Revolution in Science', Newton's work was synonymous with a notion of classical physics, while modern physics was represented by Einstein's new relativity theory. However, these were not the only ways that such terms could be defined. At the very beginning of the twentieth century, 'modern' physics might have described the study of any of the numerous extraordinary phenomena discovered at the end of the nineteenth century: X-rays, radioactivity, and subatomic particles. The experimental physicists engaged in this research were reducing matter down to its discrete components, in stark contrast with the ether physics which had dominated much of nineteenth century physical thought and

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[3] Fara (2002), p.xv.
[4] Bowler and Morus (2005)
[5] Morus (2005).

emphasised the ultimately continuous nature of matter. A movement away from continuity was further intensified by a theoretical development, the quantum theory, which proposed that energy was also discontinuous in nature. Experiment and theory, and discontinuous energy and matter, were combined in Niels Bohr's 1913 quantum model of the atom, which built upon Ernest Rutherford's 1911 nuclear model. Quantum theory was developed further between 1924 and 1927 with wave mechanics and the Copenhagen interpretation that appeared to reject causality and determinism. Quantum theory was also utilised by both chemists and physicists studying the properties of matter at very low temperatures, who confirmed Einstein's theory of specific heats. Meanwhile, various aspects of the 'reductionist' physics of the early twentieth century had been developed by experimental physicists. The technique of X-ray crystallography was established by the father and son duo of William and Lawrence Bragg, providing new experimental means to uncovering knowledge about the structure of matter. With the discovery of the neutron in 1932, Rutherford's 'nuclear' physics progressed. And thermionics, the study of the properties of charged particles, found applications in the wireless industry. In the midst of this, Einstein published his 1905 Principal of Relativity (which would later be termed special relativity) and his 1915 general theory.

It is thus immediately clear that there was not one coherent definition of 'modern' physics in the years between 1900 and 1940. This brief overview of types of 'modern' physics includes both the theoretical and the experimental, with physics advanced by mathematical refinement but also the development of experimental techniques and apparatus. Some of these subjects increased natural knowledge, while others had tangible applications. Many contributed to both theory and practice. And yet, scientists and many historians continue to apply the terms 'classical' and 'modern' physics retrospectively to this period with no recognition of the complexities involved.[6] In their textbook of *Twentieth Century Physics*, Brown, Pais and Pippard provide a fleeting overview of the development of 'what is referred to as Modern Physics':

> 'As the year 1900 approached, the splendid edifice of classical physics, founded on the ideas of Newton, Maxwell, Helmholtz, Lorentz, and many others, appeared to be reaching a state of near-perfection; but this very advanced state revealed some structural flaws, which turned out to be more than superficial. The experimental and theoretical discoveries of the years around the turn of the century

[6] See, for example, Kragh (1999); Knight (2006), Chapter 12; Büttner, Renn and Schemmel (2003) in their response to Kuhn (1978).

> led to revolutions that transformed the basic outlook of physicists: atomic structure, quantum theory, and relativity.'[7]

The terms 'classical' and 'modern' physics are used throughout this book, with the latter emerging in 1900 and representing a dramatic shift in thought. However, it is not at all apparent that the various physicists engaged in the study of a diverse range of topics in the early twentieth century thought of themselves as part of a cohesive group of 'modern' physicists. Similarly, those who remained focused on the ideas of the nineteenth century were not immediately labelled as 'classical'.

In recent years, historians have attempted to rectify this retrospective simplification, paying closer attention to contemporary uses of these categories. Darrigol and Needell have argued that the term 'classical' physics was constructed around 1910, and thus to employ it any earlier would be an anachronism.[8] It becomes clear that in order to define 'classical' physics, one must start in the twentieth century and work backwards. Staley has attempted such a task in an ambitious reconceptualisation of the roots of our notions of both 'classical' and 'modern' physics. Focusing mainly on the German case, he has argued that both categories were defined concurrently, and that the classical was 'constructed in the light of the modern and defined by proponents of the new'.[9] Staley situates the construction of our current definitions of these categories at the 1911 Solvay Congress, during which Max Planck crystallised emerging uses of 'classical' to differentiate this type of physics from quantum ideas and establish the year 1900 as the turning point. However, Gooday and Mitchell, responding to Staley, have shown that as late as 1918, Planck was using the term 'classical' to refer to particular theories, not an entire domain of physics.[10] Indeed, Staley's study has considerable limitations, focusing predominantly on German physicists and professional dialogues, and ending in the year 1911. In the British case, the situation was very different, and Gooday and Mitchell have suggested that here the category of 'classical physics' was first employed in 1927, by Arthur Stanley Eddington.[11] Notably, this was not for the benefit of an audience of physicists, but instead featured in his Gifford Lectures, which were intended to be more

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[7] Brown, Pais and Pippard (1995), p.ix.

[8] Needell (1988); Darrigol (2001).

[9] Staley (2005), p.542.

[10] Gooday and Mitchell (2012); This was in his Nobel prize speech, "Max Planck - Nobel Lecture: The Genesis and Present State of Development of the Quantum Theory". Nobelprize.org. 2 Sep 2012 http://www.nobelprize.org/nobel_prizes/physics/laureates/1918/planck-lecture.html.

[11] This was in Arthur Stanley Eddington's 1927 Gifford Lecture, published as Eddington (1928), and discussed in Chapter Four.

widely accessible.[12] Indeed, in their published form, *The Nature of the Physical World*, Eddington's lectures were hugely successful, selling nearly ten million copies in the first two years.[13] By extending Staley's study into the 'public' sphere and the years 1900 to 1940, my thesis examines how and why the categories of 'classical' and 'modern' physics were constructed in Britain.

In doing so I present a broader picture of the development of these terms. Staley argues that 'classical' physics was defined by the practitioners of the new, in order to situate their work within the history of the discipline. I move beyond this focus on the 'modernists', considering also those physicists who we would now consider to be 'classical', and I find that they were also responsible for creating these definitions. Physicists of the nineteenth century 'classical' tradition did not simply fade out of view with the emergence of 'modern' physics, and many retained positions of power and influence right up until the Second World War. Furthermore, following a lengthy career several of these 'classicists' had by the early twentieth century achieved a considerable degree of recognition beyond the professional realm of physics. In the face of 'modern' developments, Oliver Lodge campaigned vigorously for the ether throughout his life, but in 'public' came to be regarded as an authority not just on matters of physics but on 'modern' physics. From 1900, as Principal of Birmingham University, he was no longer an active researcher, but his lack of proximity to the practice of physics did not affect his status as an 'expert' in this regard.[14] The promotion of ideas of 'classical' and 'modern' physics was not only the result of 'modern' physicists trying to maintain their links with the past (as Staley argues), but also 'classical' physicists who wanted the older theories to plan an active part in the future of the discipline. Furthermore, just as the categories of 'classical' and 'modern' physics were under negotiation, it was also unclear where physicists themselves were situated within this apparent dichotomy.

There are many studies that explore the transition from classical to modern physics by taking a category of 'modern' physics as their starting point and then looking back to the origins of these ideas.[15] Andrew Warwick has, however, revealed the situation to be far more complex in his exemplary study of the reception of relativity theory in different Cambridge traditions from 1905 to 1911. He reframed the question as being not about

[12] On the history and purpose of the Gifford Lectures, see Jaki (1986) and Witham (2005).
[13] Whitworth (1996).
[14] On Lodge, see Hunt (1991), Rowlands (1990) and Wilson (1971).
[15] For example, Buchwald (1998); Darrigol (2000).

why Cambridge physicists ignored the aspects of relativity theory which would come to be important for future physicists, but rather how, in the context of their own work, they found the theory to be of use.[16] If we want to consider the actual practice of physics, and views of physicists, during the first half of the twentieth century, then the received differentiation between classical and modern has clearly become an obstruction. Instead, I treat these categories in their contemporary contexts, and consider how they were created, and what this can tell us about understandings of the discipline during the period.

Before further detailing the aims and structure of my thesis, I lay out a review of the relevant existing literature. In doing so, I consider the methodological approaches I will be using in my own study, and the gaps in knowledge that I hope to fill. This literature review is divided into three parts. I begin with an analysis of the historiography of early twentieth century physics in Britain, and the ways in which this either adds to, or is limited by, conceptions of 'classical' and 'modern' physics. I then consider the influence of the Sociology of Scientific Knowledge and culturally informed approaches to the history of physics. Finally I explore methodological approaches to the 'public', studies of science communication, and the context of 'public' and 'popular' physics in Britain during this period.

1.2 The historiography of early twentieth century physics

I begin by considering accounts, in addition to those cited above, of the discipline during the late nineteenth and early twentieth century in Britain. I explore the contextual and methodological questions they raise, and how such accounts contribute to solving the problems detailed above. For a starting point, Morus has provided a broad overview of the development of physics as the 'king' of sciences in the nineteenth century, while Nye has considered the interrelations between chemistry and physics from 1800 to 1940.[17] Kevles' descriptive account of the emergence of a physics community in the US reveals the contingency of national contexts on such a study.[18] In this section, I consider the particularities of the British case.

[16] Warwick (1992, 1993a, 2003).
[17] Morus (2005); Nye (1996).
[18] Kevles (1995).

Here, an occasion which has attracted particular attention is J. J. Thomson's 'discovery' of the electron in 1897. This event is revealing not only of the practice of physics, but how its history was written by its practitioners. It is now well known that Thomson did not conceptually connect the 'corpuscles', for which he found experimental evidence, with theories of the electron. Smith has explored the purposes behind Thomson's own research which led to his discovery of 'corpuscles', revealing that Thomson was not motivated by questions related to the electron, and did not view his work or results in this light.[19] Similarly, Falconer has considered the distinctions between the 'electron' question, related to finding links between ether and matter, and Thomson's interest in corpuscular atomic theory.[20] Falconer has also questioned why historians and physicists alike came to conceive of Thomson's work in the framework of a discovery narrative, arguing that it was the retrospective work of Oliver Lodge, in 1902, which conceptually tied together these two divergent research topics.[21] Lodge was here defining the physics of the present in relation to the past. Such an approach was used by the physicists who appropriated Newton's name to acquire professional prestige, and is a theme which runs through my thesis.

J. J. Thomson was Director of Cambridge University's Cavendish Laboratory from 1884 to 1919, and the historiography of this particular institution also provides an example of the appropriation of history for certain purposes. As Hughes has suggested, our conception of 'modern' physics in early twentieth century Britain has been influenced by an emphasis on the work undertaken in, and results produced at, the Cavendish.[22] As a result, British 'modern' physics has to some extent been equated with 'microphysics', and its history designated as arising from the regime of experimental explorations into the particulate structure of matter established by J. J. Thomson. This account is the result of both retrospectively viewing the activities of the Cavendish Laboratory in the context of the importance of post-war nuclear physics, and uncritically accepting the active promotion by many of its proponents at the time.[23] Many recent studies have, however, suggested the need for a more complex understanding of the Cavendish Laboratory and its wider contexts. Hughes has responded to the idea of the early twentieth century Cavendish as defined by a 'sealing-wax and string' approach, indicating small-scale,

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[19] Smith (2001).
[20] Falconer (1987).
[21] Falconer (2001).
[22] Hughes (2002a), pp.350-1.
[23] Hughes (2009a) has considered the promotion of the Cavendish laboratory by science journalist J. G. Crowther.

benchtop laboratory work. He has countered this myth (propagated by the Cavendish's own inhabitants), pointing out the many connections the Laboratory had with engineering and the radio industry.[24] Warwick has considered pedagogical practices at Cambridge University, and the types of physicists this created.[25] His study of the early reception of relativity theory revealed how two divergent traditions at Cambridge, the mathematical and the experimental, interpreted the theory in different ways.[26] He showed that even within the Cavendish Laboratory there were differences, with the older tradition of precision measurement, maintained by physicists such as Richard Glazebrook, co-existing with Thomson's new regime.

Furthermore, as the twentieth century progressed, different traditions emerged in the Laboratory. The breed of experimentalists trained under Thomson established a distinct research ethos of their own. Ernest Rutherford and William Henry Bragg (and later his son William Lawrence Bragg), were trained at the Cavendish, before becoming directors of their own laboratories, where they promoted an adapted version of Thomson's experimental style.[27] Rutherford himself went on to become Director of the Cavendish in 1919, sharing an institutional space with Thomson, who remained in a separate section of the Laboratory. Thomson's experimental work was supported by mathematical analysis and his research into particulate matter was conceived within a broader theoretical framework of ether physics. The newer Rutherford tradition was less mathematical (as the training of experimental physicists in Cambridge became distinct from the training of mathematicians) and dedicated towards the study of microscopic particles themselves, rather than their relations with an ether, which was now of little interest experimentally.[28] The case of Thomson has implications as to changing meanings of 'modern' physics represented at Cambridge, with his apparent shifting from 'modern' to 'classical' in a relatively short time period. By treating the categories of 'classical' and 'modern' as being under construction during the early twentieth century, the case of J. J. Thomson's speedy fall from grace can be seen as the consequence of a retrospective interpretation of events from the viewpoint of current, and thus anachronistic, definitions of 'classical' and 'modern' physics. A more complex and

[24] Hughes (1998).
[25] Warwick (2003).
[26] Warwick (1992, 1993a, 2003).
[27] Heilbron (1968) has explored how Rutherford in particular (but also Bragg) diverged from Thomson in his interpretation of alpha and beta-ray scattering, while Wynne (1976) has considered the place of C. G. Barkla as something of an outsider, aligned with the older Thomson tradition.
[28] Falconer (1989), p.109.

varied characterisation of both the Cavendish Laboratory and the characterisation of 'classical' and 'modern' physics has thus emerged.

In addition to this, institutions and contexts outside of Cambridge have only recently been assessed on their own terms, rather than as comparative 'failures' in relation to the apparently more successful Cavendish Laboratory. As Gooday and Fox have argued, historians have been guilty of retrospectively applying modern day values to accounts of 'successful' physics; they point out that what we now consider to be physics was often researched in other disciplinary spaces, particularly chemistry, and that research did not necessarily take priority over teaching.[29] Such disciplinary uncertainty and variety is overlooked in Forman, Heilbron and Weart's otherwise impressive presentation of the institutional landscape of physics in Britain circa 1900.[30] Their study of academic physicists omits those housed in different disciplinary spaces or independent laboratories, and workers in industry. However, more recent scholarship has created a broader sense of the context of physics in the late nineteenth and early twentieth centuries. Building on Sviedrys' work, Gooday's study of physics laboratories treats the research-focused Cavendish as an anomaly, arguing that the majority of laboratories were established as teaching institutions, producing workers for the burgeoning electrical communications industry and science teachers who were now required in the training of a wide variety of professions.[31] Fox and Guagnini have revealed the difficulties faced by experimental physicists in carving out a disciplinary space for themselves, as they came into conflict with electrical engineers at the end of the nineteenth century over the practical use of academic knowledge.[32] The need for a consideration of the wider context of industry is evident in the 1900 establishment of the National Physical Laboratory.[33] Furthermore, moving forward in time, Edgerton has argued that studies of the interwar period (and beyond) should take into account military dimensions, which directed much of the funding and topics of science.[34] Looking beyond the Cavendish, this wider landscape of British physics reveals considerable diversity in the practice of the discipline, and thus numerous intellectual and institutional spaces into which 'modern' physics could be adopted.

[29] Gooday and Fox (2005).
[30] Forman, Heilbron and Weart (1975).
[31] Sviedrys (1976); Gooday (1990); for additional detail, see Gooday (1989).
[32] Fox and Guagnini (1999).
[33] Fox and Guagnini (1999); Moseley (1978).
[34] Edgerton (2005).

However, the physicists trained by Thomson at the Cavendish did have a significant influence over the direction of the discipline throughout the early twentieth century. Lelong has described a Cavendish 'diaspora' of former students going on to establish their own research schools.[35] One methodological approach to analysing such a diaspora can be found in Servos' study of how the discipline of physical chemistry came to be established in the US by physicists trained at Ostwald's laboratory in Leipzig.[36] Servos considers the individuals and institutions involved, and their interactions with both industry and other sciences, to reveal the roles played by particular people and places in the adoption and propagation of the principal characteristics of a discipline. My aim is thus to explore how the influence that a particular kind of Cavendish physicist had on the emerging definitions of 'classical' and 'modern' physics, and examine the competing definitions emerging from other institutions. I acknowledge the influence of the Cavendish during this period, while also considering its heterogeneous nature and place in the wider context of British physics.

1.3 Broadening the context: sociologically and culturally informed studies

The studies detailed above have benefited from a contextually rich approach, moving beyond internalist studies of the development of scientific theories. The roots of such work can be found in the Sociology of Scientific Knowledge (SSK) movement, which emerged in the 1970s and explores the notion of scientific knowledge as a product of social conditions.[37] Bloor proposed leaving aside the contentious issue of what is 'true' or not, and instead reserving the word 'knowledge' for that which is 'collectively endorsed'.[38] In considering how such endorsements come to be, Collins explored the interpretation of experimental results as a socially contingent practice.[39] Latour, building on, and departing from, SSK, emphasised the role of networks in achieving consensus.[40]

Broadly, this methodology can be, and has been, used by historians to consider how scientific knowledge is produced, promoted and accepted as 'fact'. Shapin and

[35] Lelong (2005), p.212.
[36] Servos (1990).
[37] Barnes, Bloor and Henry (1996); Bloor (1976/1991); Collins (1985); Collins and Pinch (1993); Pickering (1992); A helpful summary of developments can be found in Golinski (1998).
[38] Bloor (1976/1991), p.5.
[39] Collins (1985).
[40] Latour (1985); Latour and Woolgar (1986); See also Shapin (1988) for a consideration of how this work differed from standard SSK texts.

Schaffer's exemplary study explored how seventeenth century natural philosophers came to define acceptable methods of knowledge production.[41] Similarly, Rudwick and Secord both examined the role played by debates between experts in the acceptance of nineteenth century geological ideas.[42] Shapin's *Social History of Truth* explored the relationships of 'trust' between scientists, based, in his seventeenth century context, on gentlemanly values.[43] In my study, I consider not the development and acceptance of theories, but rather of subdisciplinary categories. I consider how the categories of 'classical' and 'modern' physics came to be defined and how notions of 'progress' from the past to the present were constructed. I will show the importance of networks in reinforcing how the discipline of physics should be practiced and what should be counted as a 'valuable' contribution.

While I have so far considered the direct influence of SSK on studies outside of the specific context of early twentieth century physics, the sociologically-aware approach has been applied to such histories. Although pre-dating much of the SSK scholarship, Paul Forman's celebrated study of early German receptions of quantum mechanics also considers the influence of wider cultural context on the adoption of scientific theories. In his case study, he reveals German physicists as interpreting quantum mechanics favourably in the context of a Weimar culture that emphasised acausality, individuality and visualizability.[44] Maria Beller has explored the development and establishment of the Copenhagen interpretation of quantum mechanics as the result of dialogues and debates among physicists.[45] Galison has considered the work of both Einstein and Poincaré on the nature of time as a response to practical demands of determining longitude and establishing precise railway networks.[46] His study presents abstract thought and practical application as part of the same process, producing a history of scientific thought in which the context is a crucial part of the story. By stripping down a scientific problem to the core philosophical concepts involved, and then relating them to the 'real' world, Galison's study is suggestive of the possibility to consider relations between science and other forms of culture. Staley proposes such a consideration in his 'co-creation' study. He suggests that framing 'modern' physics in competition with

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[41] Shapin and Schaffer (1985).
[42] Rudwick (1985); Secord (1986).
[43] Shapin (1994).
[44] Forman (1971).
[45] Beller (2001).
[46] Galison (2003).

'classical' gave it a sense of heightened drama, relating it to broader cultural changes.[47] However, this is merely a suggestion, and he provides no historical evidence, leaving this task to other historians. I hope to contribute to such a project, and explore the relations between 'revolutions' in physics and other modes of thought in Chapter Three.

Particular interest has been paid by historians to the interplay between 'modern' physics and 'modern' art and literature. In perhaps the most ambitious account, Everdell attempts to fit a vast array of cultural and scientific ideas into one neat definition of 'modernism', characterised by a move towards discontinuity.[48] In physics, this discontinuity was represented by the atomism of matter, a pre-existing issue intensified by the discovery of subatomic particles, and the notions of discontinuous energy emerging with quantum theory. A number of more localised studies consider the literary reception of new physical theories emerging in the early twentieth century. Whitworth has explored how metaphors employed by 'modern' physicists were used by contemporary writers to discuss the modernism emerging in their own field.[49] The work of Price has revealed a multiplicity of cultural interpretations of aspects of relativity theory, while Friedman and Donley have emphasised how Einstein's worldwide fame contributed to the theory's literary reception.[50] Beyond relativity theory, Henry has considered how the 'new cosmology', popularised by James Jeans, directly influenced the work of Virginia Woolf.[51] Such studies, however, are unidirectional, considering only the influence of science on other forms of culture, and not how the science itself may have been contingent on cultural factors. Miller's more symmetrical approach considers how Picasso and Einstein shared a common influence in the writings of Poincaré on time and simultaneity.[52] Miller, as with many of these cultural studies, considers only 'modern' physics, assuming that after 1900 this was the only physics of interest in Britain. Clarke and Henderson have warned us not to focus exclusively on atomic, quantum and relativistic theories, as ideas of the ether still held considerable cultural influence.[53] In Chapter Three, I consider the broader cultural context as the location of discussions about both the 'old' and the 'new' physics. This context

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[47] This is noted in Staley (2005), and further emphasised in Staley (2008c).
[48] Everdell (1997).
[49] Whitworth (2001).
[50] Price (2005, 2008, 2012); Friedman and Donley (1985).
[51] Henry (2003).
[52] Miller (2002); The relations between Einstein's relativity theory and Picasso's abstract cubism have been explored since 1921, with the first mention was in a book on cubism written by Czech historian of art Vincenc Kramár. This reached a wider audience in the 1940s, and is described in Gamwell (2002).
[53] Clarke and Henderson (2002).

influenced the focus of debates, pitting Everdell's discontinuity against the continuity of the ether. As I shall show, defences of the ether were not simply the result of a commitment to this physical principle, but also a reaction to wider, similarly 'revolutionary', changes in Britain at this time. Debates about the particulate nature of matter and the quantum were related to a wider issue of discontinuity in thought, of physics progressing through a 'revolution' that discarded long held ideas. While Forman's German physicists were preoccupied with notions of acausality and indeterminism, in the British case I find that the concepts of continuity and discontinuity were far more relevant.

In order to paint a broad contextual picture, I also consider the changing relationship between science and religion. British attitudes toward religion in the early twentieth century have been explored extensively by Bowler.[54] He reveals a conflict between past and present authority, as 'modernist' religious formers altered their practice in order to make it more compatible with science. As I shall consider, such reconsiderations of the role of the past were also endemic in art, literature and physics. Alongside religion, we find the practice of spiritualism, which Oppenheim has situated in the context of contemporary intellectual concerns.[55] Seriously considered by only a minority of Victorians, a significant proportion of this minority were physicists, many of whom were members of the Society for Psychical Research. For these physicists, their psychical and physical beliefs were often closely connected: as Noakes has shown, a number of fin de siècle physicists were interested in using X-rays and the electrical theory of matter to study occult effects scientifically.[56] As artists and writers were influenced by both the 'old' and 'new' physics, so too was spiritualism used in conceptions of not just late nineteenth century discoveries, but also the older idea of the ether. Wynne has situated commitments to the ether in the context of the conservatism of late-Victorian Cambridge, although Noakes has since argued that Wynne's characterization of Cambridge physicists is too homogeneous, overlooking the variety of different concepts of the ether held.[57] In the case of Oliver Lodge, the ether needed to be material, forming the basis of his physical and psychical model of nature depended on the existence of an ether.[58] Spiritualism was part of a larger debate, continuing on from the nineteenth century, concerning whether the natural world was ultimately materialistic, or whether

[54] Bowler (2001).
[55] Oppenheim (1988). See also the essays in Bown, Burdett and Thurschwell (2004).
[56] Noakes (2008a).
[57] Wynne (1979); Noakes (2005).
[58] Wilson (1971); Root (1978); Noakes (2005).

there were ‘vitalist’ forces at work.[59] As I shall explore in Chapter Three, debates about ‘materialism’ and ‘vitalism’ were also considerations of the limitations of scientific disciplines.

Furthermore, in the case of spiritualist scientists there are parallels with the development of ‘modern’ physics. Noakes’ overview of the historiography of psychical research emphasises the importance of treating this practice as one would any other emerging scientific discipline.[60] By considering spiritualism in its historical context, and not as the marginal and problematic area that it has become, this topic can be interpreted as a case study in the professional reception of new scientific theories and approaches. Similarly, ‘modern’ physics was not always destined to ‘supersede’ the older approach, but instead was practiced and considered alongside ‘classical’ physics throughout the first half of the twentieth century. Throughout my thesis, I approach these two categories symmetrically, interpreting them within the wider frameworks of the scientific, cultural, social and political context of early twentieth century Britain.

Considerations of the broader context of early twentieth century physics thus take us from professional networks and relationships of trust, to the interplay of science with art and literature, ending in the spiritual realm. I propose that this wider context affected the formation of definitions of ‘classical’ and ‘modern’ physics and directed physicists’ commitments to certain theories. Furthermore, such considerations affected not only how physicists interpreted scientific ideas, but also how their work and discussions were received by the ‘public’. As seen in the case of the 1919 ‘revolution in science’, wider issues influenced the terms and concepts used to present developments in physics to the ‘public’.

1.4 Science and the ‘public’

I end my literature review with an overview of the various considerations that need to be taken into account when discussing the ‘public’ reception of science. Before analysing the relevant literature, I return to November 1919 and explore the issues raised by this particular event in the history of science communication. The origins of this episode lie in a prediction made by Einstein, that according to his general theory of relativity

[59] This has been explored by Bowler (2001).

[60] Noakes (2008b).

starlight was deflected by the sun's gravitational field. In order to test this, observations needed to be made during a solar eclipse, when the visibility of the stars was not obscured by the sun. Arthur Stanley Eddington, one of the first British converts to relativity theory, worked with Astronomer Royal Frank Dyson to organise an expedition to two cities, Príncipe in Africa and Sobral in Brazil, where an eclipse was expected in May 1919.[61] The results were announced at a Joint Meeting of the Royal Astronomical Society and the Royal Society on 6 November 1919, the *Times*' dramatic headline appeared the following day, and a number of newspapers quickly followed suit.[62]
The *Times'* reference to a 'revolution' having 'overthrown' Newton was reinforced the following day, with another 'Revolution in Science' article accompanied by the subheading 'Einstein v. Newton'.[63] This depiction of conflict was a response to the way that scientists had presented the expedition, both to their peers and the media. As Earman and Glymour have noted, Eddington presented a 'trichotomy' of possible results from the expedition: no deflection; the full deflection, derived from relativity theory; or a value half that amount, in accordance with Newtonian mechanics. They argue that this gave the event heightened drama, and the character of a crucial experiment.[64] They also find fault with the derivation of the Einsteinian deflection in the first place, depicting it as a perfect example of Harry Collins' 'experimenter's regress'.[65] This involves mutual dependence between theory and experiment, where both are judged on the basis of their accordance with the other. Earman and Glymour argue that Einstein's derivations and the photographs obtained were both problematic, and each reinforced the validity of the other. They go so far as to suggest that Eddington's scientific ethics were compromised by his bias towards relativity theory.

Stanley has challenged some of Earman and Glymour's conclusions, arguing that, when his practices are assessed according to the disciplinary conventions of early twentieth century astronomy, Eddington can not be accused of unethical behaviour.[66] However, he agrees with Earman and Glymour that Eddington's subsequent popularisation work retrospectively framed the expedition as being far more conclusive than it actually was.

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[61] For Eddington, see Stanley (2007a).
[62] 'The revolution in Science', *The Times*, 7 Nov 1919, p.12; 'Upsetting the Universe', *Daily Express*, 8 Nov 1919; 'The Baseless Fabric of the Universe', *Observer,* 9 Nov 1919; 'Bloodless Revolution', *Daily Herald*, 8 Nov 1919; 'Light caught bending', *Daily Mail*, 7 Nov 1919.
[63] 'The Revolution in Science. Einstein v. Newton. Views of Eminent Physicists.', *The Times*, 8 November 1919, p.12.
[64] Earman and Glymour (1980).
[65] Collins (1985).
[66] Stanley (2003).

Sponsel has considered this further, exploring the ways in which Eddington and the Joint Permanent Eclipse Committee (JPEC), which was set up to organise the expedition, promoted their purpose, expected outcome and results.[67] Through close contact with *The Times*, made possible by the dual position of Henry Park Hollis as *Times* astronomical correspondent and JPEC member, they ensured that the expedition was framed as a crucial experiment. The expedition was carefully communicated to the 'public' by the JPEC through collaboration with journalists and editors at *The Times*. Furthermore, Sponsel argues that Eddington also primed other physicists to accept the results *before* the official announcement.

These studies of the eclipse expedition reveal the complexities involved in the popularisation of scientific ideas and events. Here, scientists worked to publicise the expedition and its results in a certain way, emphasising the trichotomy and depicting the results as an uncontested 'proof' of Einstein's theory. Publishers, editors and journalists, meanwhile, were motivated by different aims. They were driven by what Gregory and Miller have termed 'news values', required to attract readers and sustain wider interest. These include relevance to the reader's life, regular frequency, and an element of unexpectedness.[68] These 'news values' could differ considerably from the values that governed how scientists wanted their work presented. There were thus careful negotiations underway between scientists and the media, with each party working to achieve a different result. In the case of the eclipse expedition, which I explore further in Chapter Four, the media took the 'news value' potential of the trichotomy too far in the eyes of many scientists. The rhetoric of 'revolution' had unwelcome implications, suggestive of the destruction of Newton's theories and thus separating 'modern' physicists from their prestigious heritage. Physicists, Eddington included, subsequently struggled to minimise this damage, publicly insisting that Newton's legacy was safe. Hughes has explored a different case study, which also reveals the importance of these negotiations, discussing how the Cavendish Laboratory, in the late 1920s and 1930s, developed and maintained a relationship with the *Manchester Guardian*.[69] Cavendish physicists achieved this through cooperation with J. G. Crowther, science correspondent, with whom they worked closely to ensure their work was presented in certain ways.[70]

[67] Sponsel (2002).
[68] Gregory and Miller (1998).
[69] Hughes (2009a).
[70] For Crowther, see Hughes (2007). There is also his autobiography, Crowther (1970), but, as Hughes has shown, this is a particularly unreliable source.

However, Hughes has also shown how Crowther had to negotiate with his editors, who had their own ideas about what their readers wanted.[71]

Studies of science communication are further complicated by the fact, as evident in the popularisation of the eclipse expedition, that there was not always a clear distinction between the 'public' and professional presentations of science. Here, the 'popular' communication took place only one day after the presentation of the results to scientists, and *The Times* received, and printed, responses from physicists. This included Oliver Lodge, who had been silent in the meeting and instead used the newspaper as one of many vehicles for his objections to relativity theory.[72] As Duncan has noted, *The Times* had a strong relationship with established scientists, and they often used the paper to present their views to a wider 'public' including politicians and policy makers.[73] Indeed, the initial *Times* article was written by a biologist, Peter Chalmers Mitchell, a seasoned contributor to the paper, who had recently been hired as their scientific correspondent. As a Fellow of the Royal Society, Mitchell had both 'expert' status and immediate access to events, such as the November 1919 joint meeting, not available to non-scientific journalists. But as a biologist, he was also a 'layman' with regards to new physical theories, and relied on others, including Oliver Lodge, to communicate these ideas to him.[74] Mitchell was thus both a scientist and a member of the 'public'.

It is thus immediately clear that the term 'public' cannot be employed to describe one homogeneous group of 'nonscientists', and a more complex understanding is required. The starting point of our concepts of the 'public sphere' is the work of Jürgen Habermas, dating from the 1960s, but first translated into English in 1989.[75] Habermas described the emergence of a 'bourgeois public sphere' separate to the public sphere of the state. In this new sphere, there was no influence from the standard figures of authority, allowing for a freedom of debate directed, according to Habermas, by purely rational considerations. However, subsequent historical examinations revealed the elitist nature of Habermas' public sphere, excluding many members of society.[76] As a result,

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[71] Hughes (2007).
[72] Lodge's reticence was noted in the original article, and his response was published the following day, Oliver Lodge, 'The Ether Of Space. Sir Oliver Lodge's Caution', *The Times*, 8 November 1919, p.12.
[73] Duncan (1980), pp.74-5.
[74] Mitchell is discussed in Duncan (1980), Crook (1989), and Hindle (1947). He also has an autobiography, Mitchell (1937).
[75] Habermas ([1962], 1989).
[76] For example, see Ryan (1992) and Eley (1992).

historians and sociologists have moved toward a conception of a multiplicity of publics and discourses, which are local and impermanent in nature.[77]

By the early twentieth century, the nature of these various 'public' spheres had been affected by the nineteenth century population explosion. The end of the century saw legislation introducing universal elementary education, resulting in a huge literate 'public'.[78] For this new 'mass' public, new modes of communication were created. There was a revolution in newspaper publishing, as new production techniques allowed publishers to sell papers in vast quantities and at low prices. The 'popular' daily press was a commercial success, and the *Daily Mail* held the title of best-selling newspaper in Britain from 1896 until the 1930s.[79] Burnham, in an analysis of the popularisation of science and health in America, has drawn a clear line between how science was communicated to a more educated 'elite' and this literate 'mass'. He argues that popularisation was a two-tiered activity, with professional scientists presenting their work in popular books, lectures and more prestigious newspapers, while commercialised science popularisation was produced, and read, by the uninformed, and written in a sensationalist manner.[80] More recently Broks has argued that this is less a taxonomy, more a value judgement, sorting the 'good' from the 'bad'.[81] Such negative assessments of a 'lower' form of science popularisation are perhaps partly to blame for the disproportionate focus on elite audiences in much of the literature, as noted by Cooter and Pumfrey in 1994.[82] Broks himself has attempted to rectify this gap in the literature by studying mass-circulation magazines in late-Victorian and Edwardian Britain, looking for science in what was popular, rather than the popular in science.[83] Similarly, LaFollette has looked at science articles in American general interest magazines in the first half of the twentieth century.[84] Such studies consider a rather different 'public' sphere than those that address only elite audiences, such as the numerous researches into the reception of 'modern' physics by writers and artists.

However, it is not always necessary to draw such a sharp distinction between the 'mass' and the elite when considering the reception of science. This is true in the case of best-

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[77] Sturdy (2002), p.3.
[78] Carey (2002).
[79] Williams (1957).
[80] Burnham (1987), p.152.
[81] Broks (2006).
[82] Cooter and Pumfrey (1994), p.245
[83] Broks (1993, 1996).
[84] LaFollette (1990).

selling popular science books which reached wide audiences. As I have already noted, Eddington's 1928 book *The Nature of the Physical World* sold ten million copies in its first two years of publication. In the wake of this success, *The Mysterious Universe,* written by mathematical physicists and astronomer James Jeans was published in 1930.[85] As Whitworth has shown, both of these books were published and marketed to numerous different 'publics', in both expensive and affordable editions.[86] While the outward appearance of these various editions varied considerably, the content remained the same, in stark contrast to Burnham's simplistic division of audiences. Recognising the problems of such a dichotomy, Bowler's overview of early twentieth century science popularisation in Britain considered an additional category of 'public', the self-educators who used a modest income to buy affordable books and magazines written by scientific 'experts'.[87] In exploring science communication during this period, one must thus consider a multiplicity of overlapping 'publics'.

Alongside considerations of who exactly constituted the 'public', questions are also raised about the nature of the communication itself. Bowler has suggested that the books and articles written for his self-educators, actively seeking an 'expert' account, are characteristic of the 'deficit' model of science communication.[88] While this traditional model, of a one-way transmission of information from experts to the public, might indeed be appropriate in some cases, it has been replaced by a more complex understanding.[89] Brian Trench has described developments in the theory of science communication as moving from 'deficit' to 'dialogue', from one-way to two-way communication, where public information and experience is also transmitted to the experts.[90] Similarly, Broks has suggested that the media should not be viewed as a means to simply transfer messages from scientists to the public, but rather a forum for negotiations between that which was popular and that which was scientific.[91] And McQuail has questioned whether newspapers are controlled by a dominant class, or, for commercial purposes, respond to demand from below.[92] In the case of the 1919 eclipse expedition, where the 'popular' and scientific receptions occurred simultaneously, a

[85] Jeans (1930).
[86] Whitworth (1996).
[87] Bowler (2009).
[88] Bowler (2009), p.77.
[89] See Bauer, Allum and Miller (2007) for a summary of developments within the Public Understanding of Science movement throughout the last twenty five years.
[90] Trench (2008), p.119.
[91] Broks (1996); Broks (2006).
[92] McQuail (1987).

dialogue model can be of use, as physicists responded to issues raised in the wider publicity, which itself was created by scientists and non-scientists alike. As I explore in Chapter Four, physicists used both their ‘popular’ and scientific publications to counter accusations of a destructive ‘revolution’, and establish scientific consensus.

The concurrent production of both scientific and ’popular’ accounts of relativity theory is supportive of the continuum model suggested by Cloitre and Shinn. They have proposed four categories of scientific texts: specialist, inter-specialist, pedagogical and popular articles. They present a continuum, with different categories of texts often intertwined, thus arguing that ‘popular science’ is not fundamentally different from that which appears in specialist scientific journals.[93] Summarising such proposals, Bucchi suggests that the differences between types of science communication are in degree, not kind. At the popular level, all doubts and disclaimers have been removed, resulting in simple facts.[94] However, Peters has noted that the process of science communication can not be seen simply as a ‘translation’: there is no equivalence between scientific and everyday language.[95] This is certainly the case in the highly complex mathematics characteristic of many, although not all, of the new physical theories of the early twentieth century, and suggests flaws with any continuum model, and fundamental differences in the content of science at different levels.

Furthermore, the continuum model pays little attention to the motives of scientific authors, assuming instead a straightforward attempt to present the facts of science to different audiences. As Bowler has noted, the scientific community ‘did not speak with one voice’, and some used ‘public’ arenas to promote a particular viewpoint. This could be an adherence to a contested scientific theory but might also be a larger commitment, for example to a philosophy of materialism (or conversely vitalism). Similarly ‘public’ communication could be used to promote the purpose of science, as the means to new technologies, or a path to moral and intellectual development.[96] These notions were particularly important following the First World War and responses to technological advancement. The Bishop of Ripon delivered a sermon at the 1927 British Association meeting in Leeds calling for a ‘science holiday’ in order that the necessary moral

[93] Cloitre and Shinn (1985), p.32.
[94] Bucchi (2008).
[95] Peters (2008), p.139.
[96] Bowler (2009), p.11.

development could take place in response to scientific progress.[97] In 1931, partly as a reaction to a perceived public concern with the rapid advance of science, the Social Relations of Science movement emerged, as scientists became concerned with issues involving the relationships between science and society.[98] Collins has explored this movement within the institutional setting of the British Association, noting how a perceived loss of public trust in science (related to negative consequences of technological advancements, such as unemployment, and the production of weapons) had led it to become a 'public apologist' for science.[99] In a similar case in America, Tobey has explored how scientists actively tried to present use of the scientific method as guaranteeing certain values of individualism, political and economic democracy, and progress.[100] The 'public' communication of science could thus serve a wider purpose. It was not straightforward education, but instead a means to promoting particular viewpoints, depicting a consensus that did not exist.

In this thesis, I consider the utilisation of 'public' communications of science in promoting and establishing emerging definitions of 'classical' and 'modern' physics. I explore the various approaches taken to represent the discipline as stable and secure during a period of rapid change. While many physicists were considering the place that older theories were to take in professional practice, they were constructing narratives of 'progress' from 'classical' to 'modern', obscuring this problem in their 'public' communications. Attempts were frequently made to conceal any uncertainty in the 'public' presentation of 'modern' physics. I examine how physicists of the early twentieth century maintained public trust in the midst of a 'revolution in science'.

1.5 The structure of the thesis

Chapter Two serves as an introduction to the institutional and intellectual context of early twentieth century physics. I begin with the establishment of the discipline in the nineteenth century, tracing its development up until the year 1895. From this point, I explore how the 'new' discoveries of X-rays, the electron and radioactivity were appropriated by British physicists, and the spaces in which they were situated. I consider the role of Cavendish-trained experimental physicists in the propagation of 'new'

[97] Pursell (1974).
[98] McGucken (1984).
[99] Collins (1981).
[100] Tobey (1971), p.xi.

physics, but also explore how older ideas and approaches were maintained. I also propose an alternative 'modern' physics to this, the study of low temperature research, which had links with both industry and chemistry. I end in 1911, designated by Staley as the year in which definitions of 'classical' and 'modern' physics were simultaneously created, and reveal that in the British case this construction was far from complete.

The remainder of the thesis thus considers the development of notions of 'classical' and 'modern' physics, and how the two were defined in relation to each other in the British context. In Chapter Three, temporally located around the year 1913, I explore uses of the notions of continuity and discontinuity in such characterisations, and the variety of different concepts that they represented. While these terms could simply refer to the nature of matter and energy, I consider broader meanings concerning the nature of disciplinary change: continuity or discontinuity between the past and the present. In doing so, I analyse the relations between scientific discussions and contemporaneous debates in art and literature. I place the concepts of continuity and discontinuity in the context of a wider sense of revolution, exploring how this emphasised the problem of the role of past authorities in the new physics. I analyse the usage of the notions of continuity and discontinuity, exploring their multiple meanings across different disciplinary debates, exploring the effect that this had on the wider interpretation of debates about 'modern' physics.

In chapters Four and Five, I move forward to the 1920s. Chapter Four places a broadly defined 'public' at the centre of the study, exploring the wider reception of 'classical' and 'modern' physics. I focus mainly on relativity theory, moving beyond the existing literature that tends to focus on the role of Eddington and the JPEC. Where these accounts depict the popularisation of 'modern' physics by 'modern' physicists (and practical astronomers who can perhaps be considered as 'neutral' in this respect), I approach this from an alternative perspective, to provide a more symmetrical understanding. I thus consider the 'public' communications of Oliver Lodge, self-proclaimed 'conservative' physicist and vigorous defender of the ether, and how he responded to an apparent threat to 'classical' physics, both in discussions of relativity theory and his broader popularisations of 'modern' physics. I consider the responses of both 'classical' and 'modern' physicists to the rhetoric of 'revolution' promoted in the media, and explore the different concepts of 'progress' that they constructed and promoted.

In Chapter Five, I explore how beliefs in the role of the 'classical' in modern physics affected the content of the Royal Society's *Proceedings*. I consider the networks and relationships of trust involved in making judgements over what was 'valuable' and what was not. I ask which physicists still saw 'classical' physics as valuable, and which felt the future lay in discarding such approaches. I propose the *Philosophical Magazine* as a more inclusive journal than the Royal Society's output, on account not necessarily of its informal editorial management, but rather the 'classical' physicists who were in charge there and responded positively to both 'classical' and 'modern' papers. Conversely, I consider the Royal Society as managed by a network of decidedly 'modern' physicists, although not exclusively so, and explore the consequences that this had on the journal's output. Throughout, I reveal tensions arising from a lack of consensus over what was 'credible' physics and what was not.

In Chapter Six, shifting to the 1930s, I consider how 'modern' physics was displayed at the Science Museum, in South Kensington, London. Here there were competing interests: governmental pressure to promote industrial work, and the need by physicists to present their research as applicable to this; and the desire of Director Henry Lyons, and physics keeper F. A. B. Ward, to display the 'pure' research underway at the Cavendish. I explore two explicit definitions of 'modern' physics, representing these two opposing sides, and consider how they were put on display at the Museum. I reveal that, even as late as the 1930s, there was not one clearcut definition of 'modern' physics, but rather a variety of ways in which physicists chose to present their discipline.

Throughout, I treat the categories of 'classical' and 'modern' physics as far from self-evident, and explore the changing usage of these concepts across a period of four decades. I reveal the distinctions between how physicists used older ideas in their work and how they discussed the 'value' of 'classical' physics when communicating to the 'public'. Analysing a period of apparent 'revolution', I ask how physicists worked to regain 'trust' in science, while seemingly abandoning long-held theories. I examine how physicists maintained their connections with history, presenting 'modern' physics as the result of a constant stream of steady progress. How did 'modern' physicists 'overthrow' Newton's mechanics, but retain his legacy?

Chapter Two: The Emergence of 'Modern' Physics, 1895-1911

2.1 Introduction

This chapter explores the development of physics in Britain from the mid nineteenth century until 1911, the date of the first Solvay Congress. I consider the institutional, social and intellectual state of physics in 1895, laying out the context in which the 'new' physics emerged and was adopted. I show that while the work undertaken in the Cavendish, and the laboratories developed by its alumni, has come to dominate our ideas of early twentieth century physics, it was not the only 'modern' physics on offer. Instead, a tradition of industrially relevant precise experimental physics continued, and was developed, at the same time as the Cavendish 'microphysics' of subatomic matter. Another alternative 'modern' physics can be found in the emergence of low temperature research, which again had strong connections to industry, but also utilised aspects of quantum theory. Furthermore, the Cavendish researchers were not a homogeneous group, and many of the areas of physics investigated by its alumni also fall into the category of industrially relevant research. Thermionics, researched by O. W. Richardson, had applications in wireless technology, while X-rays and radiation were studied at the practically-focused National Physical Laboratory. Against this backdrop, I consider how British physicists were beginning to define the category of 'modern' in the early years of the twentieth century.

In laying out the context of late nineteenth and early twentieth century physics, I shall consider how its practitioners were attempting to carve out a disciplinary space for themselves, separate from the disciplines of engineering and chemistry. This space was not clear cut, and both tensions and collaborations between disciplines were characteristic of late nineteenth and early twentieth century physical sciences. Gooday has noted that physicists in the 1870s and 1880s promoted their discipline as containing a strong theoretical foundation, but also exacting experimental techniques.[1] While the laboratory practice of physics was a clear continuation of the older traditions of chemistry and natural philosophy, physicists' establishment of the science of energy allowed them to depict their discipline as a 'foundational' science, constructing a

[1] Gooday (1990), pp.36-7.

hierarchy of sciences with physics at the top. The role of theory was also used in conflicts with engineers over the disciplinary home of practical electrical research and application.

In order to explore the professional identities of physicists, I begin by detailing the institutional landscape of laboratories, developed through the second half of the nineteenth century. Here, the focus was on teaching and industrially applicable precision measurement. I then consider the theoretical frameworks being laid down, of thermodynamics, Maxwellian electrodynamics and the ether, before considering disagreements between the experimental and theoretical approaches. There were also disagreements over the ultimate nature of matter: while the ether provided the foundation for a continuous view of nature, other developments suggested that both matter and energy were instead discontinuous. Returning to the boundaries between physics and chemistry, I look at the development of physical chemistry and the idea of electrolytic dissociation. As I shall show, J. J. Thomson utilised the concept of ions in his work at the Cavendish Laboratory, subsequently creating a research school of ion physics. However, Thomson's approach was not destined to become our idea of archetypal 'modern' physics. I end my survey of physics up until 1895 with consideration of low temperature research, which originated in a chemical context but would, in the twentieth century, adopt Einstein's quantum theory of specific heat.

After discussing the state of physics circa 1895, I explore the ways in which late nineteenth and early twentieth century developments slotted into the existing intellectual and institutional spaces. I consider Benoit Lelong's concept of a Cavendish 'diaspora', whose work came to characterise one particular notion of 'modern' physics. However, many lines of physical research had their origins in late nineteenth and early twentieth century Cavendish practice, and thus fit into this characterisation, but then diverged, each becoming part of different histories of 'modern' physics and 'modern' science more generally. I will also suggest another candidate for the 'modern' physics label, low temperature physics, which did not have its roots in Cavendish research. Having discussed the wide variety of physics being practiced in Britain during this period, I end my study at the 1911 Solvay conference. It was here that, according to Richard Staley, German physicists constructed definitions of both 'classical' and 'modern' physics. I shall argue that, in the British case, 1911 did not signal the end of the development of definitions of ‘modern’ physics. Instead, as the remainder of my thesis shall explore, this was just the beginning.

2.2 The institutional landscape of physics: laboratories, teaching and precision measurement

At the beginning of the nineteenth century, a laboratory was defined as a space for specifically chemical research. By 1885, however, physics laboratories had become fully established and were an integral part of the construction of new universities.[2] As Gooday has shown, the establishment of these laboratories can be understood in the contexts of the existing laboratory culture of chemistry and the industrial relevance of the new physical methods of precision measurement.[3] In this section, I shall explore how experimental physicists carved out a disciplinary space for themselves, which for many was quite separate from the conceptual space of theoretical physics. The first physics laboratory to receive formal recognition from a higher learning institute was William Thomson’s at Glasgow University. Thomson (Lord Kelvin from 1892) played a fundamental role in laying down the conceptual foundations of physics in the second half of the nineteenth century. However, with his Glasgow laboratory, he also contributed significantly to the construction of physics as an *experimental* discipline.[4] I shall explore the theory of physics only after establishing the development of the institutional context of the discipline, a context in which experiment, and practical application, was key.

The development of William Thomson’s laboratory was inextricably linked to the electrical communications industry. From the 1840s, privately funded telegraph networks were built in Britain, while the government financed the development of telegraphs abroad.[5] In 1856, Thomson became a Director of the Atlantic Telegraph Company, and with the failure of the first Atlantic submarine cable, from 1857 to 1858, he promoted precision measurement as an alternative to the methods of ‘trial-and-error’ used by electricians. In 1861, Thomson became involved with the Committee on Electrical Standards, set up by the British Association for the Advancement of Science. In these practical endeavours, he constructed a disciplinary space for experimental

[2] Gooday (1990), p.29.
[3] Gooday (1990).
[4] Sviedrys (1976).
[5] Sviedrys (1976), p.409.

physics and his new laboratory, which had been opened up to students in 1855. As Gooday has shown, Thomson used his involvement with the telegraph industry to endorse the use of the technology of precision measurement in industry and education.[6]

In Cambridge, laboratory physics was also originally developed on the basis of precision measurement and its applications. James Clerk Maxwell, the first Director of the Cavendish Laboratory in 1874, provided training in new instruments, experiments and techniques of measurement.[7] His experimental teaching was separate from his theoretical work on electromagnetism, explored later in this chapter. A decade after opening, the Cavendish had become a centre of electrotechnical metrology. While the main research topic at this point was electrical standards, researchers at the Cavendish were mostly free to pursue their own interests. Under its second Director, Lord Rayleigh, the Cavendish was able to generate an income through verifying and testing electromagnetic standards. Rayleigh was also, however, adamant that there be a division of labour between experimentalists and mathematicians.[8] He did not want the Laboratory to be devoted solely to the practical work of standardisation, but also contribute to theoretical physics.

There were tensions between the experimental physics practised in the Cavendish and Cambridge's long-standing tradition of 'mixed mathematics'. By the mid nineteenth century, the academic centre of Cambridge University was the Mathematical Tripos, a series of gruelling examinations that tested a student's use of analytical technique in solving physical problems. Students who achieved the highest scores were given the title of 'Wranglers', and upheld as the 'ideal intellectual'.[9] Their training in rational thinking was not necessarily viewed as a direct route to scientific research, but rather a moral pursuit, part of a liberal education.[10] However, wranglers did go on to hold many of the highest physics posts in Britain, with Maxwell, Rayleigh and Kelvin all graduating from this tradition.

It was against this backdrop of rational thinking and a liberal education that the Cavendish was founded, appearing to represent the rather different ideals of a Victorian workshop. In 1871, Maxwell warned that the lure of experimental physics might distract

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[6] Gooday (1990), pp.32-6.
[7] Warwick (2003), pp.286-356.
[8] Schaffer (1992).
[9] Warwick (2003), p.218.
[10] Warwick (2003).

students from developing the intellectual rigour required to acquire wrangler status. As Schaffer has shown, the two Cambridge physical traditions were reconciled by the 'moralistic' nature of public commentaries on precision measurement. Victorian ideals of hard work and punctuality were reinforced by research on standardisation, revealing a continuity between the actions of moral men and physical space and time.[11] Furthermore, the Mathematical Tripos and Cavendish were connected somewhat, with researchers at the latter being graduates of the former. The Cavendish under Rayleigh required accomplished mathematicians with an experimental style based on precision and diligence.[12]

As Gooday has shown, both Thomson's Glasgow laboratory and the Cavendish are anomalous cases in the story of nineteenth century physics laboratories in Britain. An additional context was crucial to the development of physics laboratories that appeared across Britain, following the official recognition of Thomson's in Glasgow. Gooday has detailed the co-creation of both a new clientele of science teachers and the laboratories in which they received their training. In 1868, the Society of Arts advised that practical experience in a physics laboratory was essential in the training of a multitude of professions: agriculture and gardening; chemical manufactures; metallurgy; mining; civil engineering; naval architecture and marine engineering; mechanical engineering and machining; and architecture. While Sviedrys has described the training of telegraphists as the predominant role of the new laboratories in London, Gooday argues that instead their main purpose was to train teachers of these professions, and graduates were subsequently employed in the Department of Science and Art.[13]

This can be seen in the case of the two London physics laboratories established in 1866 and 1868: the chemically trained George Carey Foster founded a laboratory at University College; and Cambridge graduate William G. Adams established a laboratory at King's College. While they originally served as training grounds for civil service candidates, particularly those wanting to enter the Indian Telegraph Service, this was not their main function, and only a temporary one. In the 1870s, this aspect of their teaching was replaced after specialised private schools began to fulfil this purpose.[14] Instead, the two London laboratories, joined by Frederick Guthrie's new laboratory at the Royal

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[11] Schaffer (1992).
[12] Falconer (1989), pp.105-6.
[13] Gooday (1990), pp.43-50.
[14] Sviedrys (1976), pp.416-432.

School of Mines (later Royal College of Science) adopted a focus towards the training of science teachers.[15]

With this new purpose for practical training in physics, laboratories were established across the country. Many of the new technical colleges, such as those in Bristol, Birmingham, Liverpool and Bangor, had a laboratory from their very inception.[16] While the focus was pedagogical, many professors involved their students in their own research. In Edinburgh, Peter Guthrie Tait, modelling his laboratory on Thomson's, took on able students to assist in his research into thermo-electricity.[17] Balfour Stewart used his laboratory at Owen's College, Manchester to establish meteorology as an exact branch of physics, an aim achieved by his successor Arthur Schuster. He did, however, also teach standardised measurement practices, in accord with the new tradition of laboratory pedagogy.[18]

While research could be conducted in these laboratories, it was difficult to attract a workforce with little funding for research students and no formal research degrees. This changed towards the end of the nineteenth century with the creation of research degrees and dedicated sources of funding. The University of London was the first in Britain to offer such qualifications, establishing a D.Sc. in 1880, and the provincial colleges quickly followed suit. The following year, Owens College, Manchester founded the Bishop Berkeley Fellowships to fund students pursuing research. Between 1881 and 1895, five physics students received these awards. In 1890, the Commissioners for the Exhibition of 1851 created scholarships to fund science students in two years of research at the institution of their choice.[19]

Finally, in 1895 the Cavendish began offering formal postgraduate qualifications to research students from other institutions.[20] This resulted in a much larger proportion of Exhibition students choosing to take up their awards at the Cavendish.[21] From 1884, the Laboratory had been transformed since coming under the directorship of J. J. Thomson who, as we shall see, had a theory-driven approach to experimental physics. The first to

[15] Sviedrys (1976), p.420.
[16] Gooday (1990), p.29.
[17] Gooday (1989), pp.3.26-30
[18] Gooday (1989), p.7.52-54
[19] Fox and Gaugnini (1999), pp.65-8.
[20] Sviedrys and Thackray (1970), p.72.
[21] Simpson (1983), p.38.

take up the new opportunity at the Cavendish were Ernest Rutherford (who would go on himself to be Director in 1919) and J. E. Townsend (later Wykeham Professor of Physics at Oxford). As I shall explore later in this chapter, towards the end of the nineteenth century laboratory tradition in Cambridge began to move away from precision measurement and towards Thomson's own research interests and practices.

By 1895, the discipline of experimental physics had been institutionalised in university laboratories. Focus here was on teaching (for various reasons) and precision measurement (for industry).Concurrent with this institutionalisation of experimental physics, however, there were developments in the theoretical side of physics.[22] While physicists were carving out an institutional space for themselves, they were also situating their work conceptually, establishing their science as 'foundational'. The following section will explore how physicists created theoretical frameworks for the discipline using the new science of energy and Maxwellian electrodynamics.

2.3 Theoretical developments: energy and the ether

As physics became established as a discipline in the nineteenth century, it came to include those elements of natural philosophy that had not been absorbed by the older disciplines of chemistry and natural history.[23] Many phenomena that would come under the domain of physics were originally seen to fall more into the category of chemistry: until the mid nineteenth century this was true of heat and electricity.[24] Indeed, many chemists at the beginning of the nineteenth century saw the practice of physics as being simply mechanics, while their own discipline covered far more ground.[25] Physicists, using mathematical methods to study the dynamics of natural entities, described a world of reversible and predictable motion. By contrast, chemists were interested in irreversible processes: the focus was change, not stability.[26] The new science of thermodynamics, which began to take form in the middle of the century, provided physics with a theoretical framework, whilst also bringing it closer to chemistry.

[22] For an overview see Morus (2005) and Nye (1996).
[23] Silliman (1974).
[24] Hiebert (1982), p.99.
[25] Hiebert (1996), p.101.
[26] Hiebert (1996), p.110.

Thermodynamics allowed a variety of disparate phenomena to be brought under one conceptual idea. Where physicists had previously been concerned with force, the central idea in Newton's laws of motion, thermodynamics replaced this with energy. Physics could then be defined as the study of the transference of energy. Thermodynamics was an attempt to provide a strong conceptual framework for physics, but it was also closely linked to industry and engineering. In the *Treatise on Natural Philosophy*, written in 1867 by the two Scottish physicists William Thomson and Peter Guthrie Tait, the science of energy was interpreted as being part of a long mathematical lineage dating back to Newton, and thus due the same prestige.[27] However, it was also depicted as an outcome of the work of James Joule on energy efficiency. Thermodynamics also had practical uses, in minimising the amount of heat lost by steam engines.[28]

Thermodynamics could be used to unify not just science and engineering, but also physics and chemistry. In 1851, William Thomson discussed the 'dissipation' of energy, describing how the flow of heat in natural processes was directional and irreversible. This idea became the second law of thermodynamics, that of entropy, and brought elements of physics more conceptually in line with chemistry. Meanwhile chemical ideas of equilibrium directed some chemists towards ideas of natural stability, reactions with no clear end. Proponents of the two disciplines could now be united over a common interest in both reversibility and irreversibility. Thermodynamics, combined with thermochemistry, would come to be one of the defining features of late nineteenth century physical chemistry, a topic I shall explore later in this chapter and which was crucial in the development of late nineteenth century physics at the Cavendish.

Heat was not the only phenomenon starting to have particular significance in the work of physicists. As discussed in my overview of the development of laboratories, electricity was a subject of considerable practical importance in the second half of the nineteenth century. Here I explore its role in the theoretical side of the discipline. The concept of an electromagnetic ether, with a real mechanical existence, provided a fundamental framework of physics that, for some, would persist long into the twentieth century. Furthermore, we here find another case of the merging of theoretical and practical concerns. Building on Michael Faraday's exploration of the relationships between

[27] Thomson and Tait (1867). p.vi.
[28] Smith (1998); Smith and Wise (1989).

electricity and magnetism, and the new doctrine of energy, physicists in the second half of the nineteenth century began to develop an 'electromagnetic worldview'. Here, they considered the properties of the ether, an all-pervading substance viewed as fundamental by Newton, and its relations to matter.

William Thomson was one such investigator, producing mechanical models of the ether based on direct sensory perception: these were models grounded in the physical world rather than the abstract world of mathematics.[29] In 1873, James Clerk Maxwell's *Treatise on Electricity and Magnetism* was published.[30] In this work, Maxwell, another Cambridge graduate of the Mathematical Tripos, laid out the results of 27 years of experimenting and theorising on the subject of electromagnetism. His book gave a mathematical basis to existing experimental research. As Warwick has explored, Maxwell's *Treatise* was used in the pedagogical foundations of a Cambridge mathematical physics research school in electromagnetic field theory. However, Maxwell himself was not responsible for setting this up, nor did he direct his students at the Cavendish towards experimental research in electromagnetism.[31]

The content of the *Treatise* was initially further developed by physicists outside of Cambridge. After Maxwell's death in 1879, a generation of 'Maxwellians' expanded on the electromagnetic worldview: Oliver Lodge, then Professor of Physics at University College, Liverpool; George Francis Fitzgerald, Professor of Natural and Experimental Philosophy at Trinity College Dublin; and Oliver Heaviside, a telegrapher with no university affiliations. Together, these men worked to develop Maxwell's theoretical work, formulating the now famous four Maxwell equations of electromagnetism. By the end of the nineteenth century they had reformulated the science of electricity and magnetism, in an attempt to bring all chemical and physical phenomena together in an electromagnetic theory of the ether. Crucially this included light, thus bringing the study of optics into the science of electromagnetism.[32]

For the 'Maxwellians', the ether lay at the centre of their scientific vision. As Cantor and Hodge have shown, conceptions of the ether varied considerably over time and among

[29] Smith and Wise (1989), pp.464-7.
[30] Maxwell (1873).
[31] Warwick (2003), pp.286-356.
[32] Hunt (1991).

different physicists.[33] For Oliver Lodge, it was a material substance, capable of carrying electromagnetic energy and transmitting all known forces; his ultimate aim was to express it in mechanical terms.[34] Noakes, both building on and challenging the work of Wynne, has suggested that the ether was used actively by a wide range of physicists to express various religious and political views.[35] From the purely physical point of view, the ether provided a means to conceiving of all of physical reality as continuous. However, as the next chapter will explore, the ether's role in continuity was about much more than the nature of matter and energy, and was part of a broader debate about the nature of scientific progress. For Joseph Larmor, a lecturer in mathematics at the University of Cambridge, the ether was non-material and purely dynamical, to be expressed in mathematical terms.[36] In 1895, he began to develop what came to be known as the Electronic Theory of Matter.[37] Larmor began working on the problem of the dynamics of the ether in 1893 and, on the suggestion of Fitzgerald, introduced into his theory the concept of subatomic particles he called electrons.[38] As we shall see, after 1895 this theory was developed, while the experimental work of J. J. Thomson was interpreted as providing further evidence of the discrete nature of the world.

As well as laying out theoretical frameworks, the Maxwellians also worked experimentally to prove the existence of electromagnetic waves, and practically to apply their research to problems in electrical communications. Here, tensions arose with the new profession of electrical engineers, who saw no use for complicated theories of how electricity worked. This battle was one of disciplinary authority: the ether physicists sought to show that their theory was fundamental to the practical work of the electrical engineers.[39] Meanwhile, dedicated electrical engineering laboratories were being created; as I have noted, physicists were no longer responsible for their training, focusing instead on the education of science teachers.

As the discipline of physics was being established both conceptually and institutionally, it was also building up a textual home. The *Philosophical Magazine*, owned by the

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[33] Cantor and Hodge (1981).
[34] Goldberg (1970); Hunt (1991).
[35] Noakes (2005); Wynne (1979).
[36] Goldberg (1970), p.106.
[37] The development and propagation of this theory is discussed at length in Warwick (2003).
[38] Warwick (1991).
[39] On the development of the discipline of electrical engineers see Fox and Guagnini (1999); On the interactions between the 'Maxwellians' and electrical engineers see Hunt (1991) and Morus (2005), pp.166-174.

publishing company Taylor & Francis, was originally intended to cover all scientific disciplines, but by the twentieth century had become a publication exclusively for mathematical and experimental physics.[40] William Thomson took the role of editor from 1871 until 1907. Meanwhile the Royal Society was formally separating physics from other sciences, and in 1887 the prestigious *Philosophical Transactions of the Royal Society* was split into Section A, publishing mathematical and physical papers, and Section B, for biological papers. The Royal Society's other output, the *Proceedings of the Royal Society*, was originally a medium for society news and abstracts of papers appearing in the *Transactions*, but by the end of the nineteenth century had become a scientific journal in its own right. It too was split into two sections, in 1905.[41] As I shall explore in Chapter Five, there were debates in the 1920s among physicists about what kinds of physics should be published, particularly in the *Proceedings*. These discussions centred on considerations over what constituted a 'valuable' contribution to the field, and this was closely linked to definitions of 'modern' physics, and notions of the use of older ideas in scientific progress.

For many experimental physicists, concerned with the development of precise techniques, the *Philosophical Magazine* and the Royal Society's output were not sufficient. Frederick Guthrie began plans to establish a physical society in 1873, arguing that the Royal Society's *Proceedings* wasn't publishing the kind of physics of interest to many of the physical community, including those in his laboratory at the Royal School of Mines. While the *Philosophical Magazine* did publish many of Guthrie's papers on the development of experimental apparatus, this entirely textual medium was not the best way to demonstrate experimental physics. At the meetings of the Physical Society, members delivered reports of their work and also practical demonstrations of their experiments. Initially there was a mutual relationship with the *Philosophical Magazine*, with the older journal publishing papers delivered at the Physical Society. However, the Physical Society quickly established its own journal in which experimental physicists could publish papers relevant to their work in precision measurement and instrumentation. Many of the founding directors of the new physics laboratories, which promoted this style of physics, served as President of this new Society in its early years: Foster, Adams, Thomson, Clifton and Guthrie. Maxwell was notably absent from this society, and Gooday has pointed out that five weeks after his death, two researchers at

[40] Brock and Meadows (1998).
[41] For Royal Society publications see Atkinson (1999), Chapter 2.

the Cavendish joined up. The Physical Society provided a home not just for prestigious professors, but also science teachers and professional telegraphists.[42]

I have shown that physicists at the end of the 19th century could not be defined as one neat homogeneous group. While some were concerned with the development of a mechanical model of the ether, others were engaged in metrology and the refinement of precision techniques. Many were now based in newly built dedicated physics laboratories, while others held mathematical posts. As well as mathematics, physicists found themselves sharing disciplinary boundaries with other sciences, including engineering and chemistry. In the following section, I shall explore how the relationship between physics and chemistry further developed at the end of the nineteenth century, in the context of a growing notion of discontinuous matter.

2.4 On continuity and discontinuity: atoms, ions and J. J. Thomson's Cavendish

In 1808, John Dalton published his *New System of Chemical Philosophy*. Building on Lavoisier's work on elements and Newton's notion that indivisible particles were the building blocks of matter, Dalton proposed a corpuscular theory of matter. As the nineteenth century progressed, the chemical atom served a useful purpose as a unit of reaction with a unique weight. However, until the end of the century, not all chemists were willing to commit to the concept of the atom as a physical reality.[43] This was retrospectively noted by Oliver Lodge, speaking in 1912: 'chemists have always been careful to say that these pictorial representations were not to be taken literally or supposed to correspond with actual fact, but that they were to be treated in a more or less metaphorical or allegorical manner rather than as statements of reality.'[44]

Meanwhile, alternatives to atomism could be found in the newly created discipline of physical chemistry. Promoted by the German chemist Wilhelm Ostwald from 1887, physical chemistry involved a new way of thinking about the relations between chemistry and physics. Fundamental to the work of Ostwald and his colleagues was the

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[42] Moseley (1977) characterized the early Physical Society as the home of 'second order' experimental physicists, and of little interest to senior academic physicists. This has been contested by Gooday (1989), pp.8.35-8.51; Further information on the physical society can be found in Lewis (1999), although this is an uncritical institutionally sponsored history.
[43] Nye (1996), pp.28-56.
[44] Lodge (1912a), p.2027.

application of physical methods and techniques to chemical problems. Physical chemistry originated primarily from three areas of research: electrolytic dissociation; the osmotic theory of solutions; and thermodynamics. The latter of these was adopted by Ostwald as an alternative to atomism, and took on the name 'energetics'. He used as its basis the idea that everything was energy, and thus that this, not matter, should be the focus of study for physicists and chemists.[45] While this theory was carefully considered by continental physicists, such as Ludwig Boltzmann and Max Planck, in Britain, it was electrolytic dissociation which had a significant impact. This was the theory, developed by the Swedish physicist Svante Arrhenius, that certain compounds (electrolytes) broke up, or 'dissociated', in solution to produce discrete particles of electric charge known as ions. It would come to play a significant role in the reception of much of the 'new' physics by Cavendish researchers at the end of the nineteenth and beginning of the twentieth centuries.

John Servos has explored how the discipline of physical chemistry was quickly established by chemists in the United States. Ostwald created an international journal, *Zeitschrift für physikalische Chemie*, wrote textbooks and welcomed researchers to his laboratory in Leipzig. After spending some time in the laboratory, Ostwald's American students returned to their home country and set up their own successful research schools in physical chemistry. As Servos has noted, and Dolby has detailed, the reception of physical chemistry by chemists in Britain was nowhere near as positive.[46] Henry Edward Armstrong, Professor of Chemistry at the Central Technical College in London, and an 'extreme individualist', led the attack and argued against the use of physical methods and considerations to solve what he saw as purely chemical problems.[47] As Sinclair has noted, Armstrong, like many British chemists, was fundamentally opposed to any notion of atomic dissociation, and 'attacked in succession, with relish, Arrhenius' theory of electrolytic dissociation, the existence of corpuscles, or bodies smaller than atoms and the transformation theory of radioactivity'.[48] He was firmly against the intrusion of reductionist physics into chemistry.[49] On the opposite side of the debate, William Ramsay, Professor of Chemistry at University College, London, was a supporter of ionic theory and welcomed former students of Ostwald into his laboratory.[50] The heated

[45] For Ostwald and energetics, see Hiebert (1971) and Hakfoort (1992).
[46] Servos (1990), p.51; Dolby (1976).
[47] Dolby (1976), p.311.
[48] Sinclair (1988a), p.97.
[49] Dolby (1976).
[50] For the life and work of Ramsay, see Travers (1956).

debates that surrounded the subject of physical chemistry in Britain ensured, however, that the discipline took much longer to be institutionalised there than in America.

However, in the reception of ionic theory among certain British *physicists*, we can perhaps find a parallel with Servos' American narrative, by considering J. J. Thomson. In his biography of Ernest Rutherford, Wilson argues that the concept of ions, as developed by Arrhenius, was crucial to the late nineteenth century Cavendish work on the conduction of electricity through gases.[51] As Falconer has shown, Cavendish research at this time was directed by the interests of its Director J. J. Thomson, who succeeded Rayleigh in 1884.[52] She has also detailed the development of Thomson's interest in gas discharge, noting that he began to use the concept of ions in his early work on X-rays.[53] The literature on physical chemistry pays scant attention to Thomson, and we learn merely that he was critical of Arrhenius' dissociation theory.[54] However, in the historiography of Thomson's research in the late nineteenth century, Sinclair has speculated that Thomson may have been influenced by existing debates on ionisation, and Charcut has more concretely established Thomson's engagement with contemporary chemical research.[55] Evidence for a direct relationship between Thomson's interest in ions and that of the physical chemists appears to be insubstantial, but we know that Thomson was certainly engaged in the ionic debates.[56] Regardless of the source of Thomson's commitment to ions, a parallel with Servos' account still stands if we place Thomson in the role of Ostwald. Here we find the influence of one man directing the research of his students, who then went on to establish their own research schools, in Britain and around the world, thus both creating and defining a subdiscipline of 'modern physics'. And, as with Ostwald's physical chemistry, ionic theory played a fundamental role.

J. J. Thomson's appointment as Cavendish Professor of Experimental Physics initiated a fundamental change in the nature of laboratory physics research at Cambridge.[57] Rayleigh had promoted precise measurement of physical constants, achieved through hard work and skill, not imagination. Researchers were not expected to make wild

[51] Wilson (1983), p.112.
[52] Falconer (1989).
[53] Falconer (1987).
[54] Dolby (1976), p.325.
[55] Sinclair (1987); Charcut (1991).
[56] Dolby (1976).
[57] Thomson's reign at the Cavendish has been explored by Kim (2002).

speculative leaps, but rather achieve ever more precision, against which the models and mathematics of theoretical physics could be confirmed. Under Thomson, the focus turned to his own research interests and practice: investigations into the fundamental structure of matter, theoretical speculation, and the favouring of visual assessments of experiments over metrical precision. His interest in electrical discharge in gases soon became an essential area of focus.[58]

It was during his investigations into electrical discharge that Thomson became acquainted with William Crookes, who was studying the cathode rays found in gas discharge tubes. Crookes was a very different kind of scientist to that found in Cambridge, a businessman funding his own private laboratory through 'commercial science'.[59] He and the other discharge researchers could be defined as 'amateur' scientists, and had little mathematical training. Thomson's work with the discharge researchers exposed him to a very different way of doing of physics from that found in his mathematical training, and this new qualitative, imprecise style shaped his scientific development.[60] In 1879, Crookes had argued that the rays were molecules of gas, and thus particles, while a number of German physicists proposed that they were not particles but an ethereal disturbance. Arthur Schuster, Professor of Mathematics at Manchester, conducted experiments that led him to believe that the rays were charged atoms.[61] Schuster was, however, an exception, and these rays attracted little attention in Britain until after the discovery of X-rays, at which point Thomson turned his attention to the cathode rays, and they began to play a fundamental role in Cavendish physics.

Thomson's studies of gas discharge were thus unconnected to the mostly continental debate over the atomic or ethereal nature of the cathode rays. Following on from Maxwell, Thomson's original interest lay in the unification of physical theories, and his early research on gas discharge was a means to understanding the relations between the disruption of molecules, the electric field and the ether. From around 1890, however, Thomson's views changed as he began to consider the consequences of Faraday's laws of electrolysis, where electrical charge was transferred in discrete units. This was an attempt to reconcile Maxwellian theory with experimental results. Now, ions were

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[58] Falconer (1989).
[59] Brock (2008).
[60] Falconer (1989), p.108.
[61] Falconer (1987).

entering his work.[62] Sinclair has stressed a continuity in Thomson's theories of matter from 1880 to 1906 and, as we shall see, his interest in ions crucially affected his post-1895 work on cathode rays.[63] Furthermore, his particular interests and methods of approach influenced how the various discoveries at the end of the nineteenth century were adopted by researchers at the Cavendish. A significant aspect of his new approach was a move towards the study of the *discontinuous* nature of matter, as opposed to the continuity found in ether physics. In the early twentieth century, this notion of discontinuity was intensified by the consideration in 'modern' physics of quantum theory, which proposed that energy was also discontinuous. As I shall explore in Chapter Three, the problem of discontinuity became an important issue for certain physicists, and was related to wider notions of revolutionary change and modernism.

In Thomson's Cavendish at the end of the nineteenth century, we find a move from the macroscopic to the microscopic. The Maxwellian approach to atomism, evident in Larmor's work, involved the investigation of how molecules of matter interacted with the ether, altering its mechanical properties. There was no need to uncover the structure of the molecules themselves. Thomson's gas discharge research, however, began to be directed towards the microscopic, the internal structure of rays and atoms. A new 'microphysics' was emerging.[64] By 1895, much of the research (although not all) at the Cavendish was grounded in a commitment to the existence of ions, a reductionist research focused on these and other 'fundamental' particles, and Thomson's speculative theory-based approach to experiment.[65]

J. J. Thomson did not, however, dominate all of Cambridge physics, and we can characterise three different traditions co-existing as the nineteenth century came to an end: mathematical physics, precision measurement, and the Thomson school. In the mid nineteenth century, a Natural Sciences Tripos had been established, and in 1873 began to include physics as a separate subject. From about 1890, high achieving physicists were taking not the Mathematical Tripos, but rather the Natural Sciences Tripos, in which the physics was primarily experimental and involved training in precision measurement at the Cavendish. Physics in the Mathematical Tripos was instead highly mathematical, taking the form of analytical dynamics. The third tradition was Thomson's own brand of

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[62] Falconer (1987).
[63] Sinclair (1987).
[64] Darrigol (2000), pp.265-313.
[65] Falconer (1989); Kim (2002).

reductionist experimental physics, sharing a home at the Cavendish with the older style of experimental physics.[66]

As I shall show in the remainder of this chapter, in the early twentieth century the Cavendish Laboratory came to be seen as a centre of the emerging 'modern' physics of X-rays, radioactivity and the electron. Its researchers responded to these phenomena as guided by their training under Thomson, with ionic theory and the study of electrical discharge in gases dominating their approach. I end my overview of physics up to 1895, however, by exploring the origins of another plausible candidate for 'modern' physics, low temperature research. Unlike the microphysics of Thomson's Cavendish, research into low temperatures often required expensive equipment, and had strong industrial applications. However, there were similarities, as it also contributed to knowledge about the fundamental properties of matter. We also find tensions arising between physicists and chemists as to the validity of each other's methods and approaches.

2.5 The origins of low temperature research: the chemical beginnings of an alternative 'modern physics'

In 1848, William Thomson constructed an absolute scale of temperature, derived from the laws of thermodynamics. With this, he postulated the concept of an 'absolute zero', a point at which no more heat could be removed from a system. In 1882, Heike Kamerlingh Onnes was appointed Professor of Physics at the University of Leiden, where he completed development of his Cryogenic Laboratory in 1904. Here, using precise and often expensive equipment, Onnes and his colleagues conducted research into the liquefaction of gases.[67] In Britain, similar work was underway, with James Dewar at the forefront. While research into the properties of matter at very low temperatures would, in the 1920s and 1930s, become a major field of physical research, in nineteenth century Britain it developed predominantly (although not exclusively) in the domain of chemistry, under Dewar. In 1875, Dewar became Jacksonian Professor of natural experimental philosophy at the University of Cambridge. Two years later, he also took the position of Fullerian Professor of Chemistry at the Royal Institution, but retained his Cambridge post.[68]

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[66] Warwick (1993a), Wilson (1982).
[67] Mendelssohn (1977), pp.74-90.
[68] For Dewar and low temperature research, see Brock (2002).

At the Royal Institution, Dewar became interested in the properties of matter near absolute zero. His research into low temperatures quickly became directed towards the liquefaction of gases. In 1877, nitrogen, carbon monoxide, methane and nitric oxide were all liquefied, leaving oxygen and hydrogen as the only gases to not yet have received this treatment. In 1886, Dewar solidified oxygen, and in 1898 and 1899 he liquefied and solidified hydrogen. While Dewar was originally stimulated by questions concerning the properties of matter, research into the practical work of liquefaction and refrigeration had considerable commercial relevance, as summarised by Brock: 'the improvement of brewing, extractive techniques for oils and drugs, the potential to fix nitrogen and prevent potential food shortages, the extraction of oxygen for use in welding and hospitals, the manufacture of dry ice for the fishing and meat industries, and refrigeration for the South American and Australian meat steamships.'[69]

As well as being industrially applicable, cryogenic work was also expensive: Dewar spent nearly £16,000 between 1882 and 1908. He was assisted by the development of the Davy-Faraday laboratory at the Royal Institution, completed in 1896, and under the joint Directorship of Dewar and Rayleigh, who was also Professor of Natural Philosophy. This was thus a laboratory for both physics and chemistry, and the spatial closeness of physics and chemistry caused some difficulties: Rayleigh resigned from his Professorship (although retained his position as Director) in 1905, complaining that Dewar's compression engines were interfering with his precise physical measurements.[70]

Furthermore, there were larger conceptual problems arising from physicists and chemists studying the same phenomena. This is most clearly evident in the debates surrounding the properties of the noble gases, whose discovery from 1895 revealed the work on liquefaction of gases to be incomplete. Argon was isolated in 1894 by Lord Rayleigh and William Ramsay, Professor of Chemistry at University College London. Subsequently debates arose over fundamental principles of chemistry and physics: the periodic table and the kinetic theory of gases.[71]

The inert nature of Argon meant that it could not be analysed by traditionally 'chemical' methods, which relied on the study of reactions. Instead, Ramsay and Rayleigh explored

[69] Brock (2002), p.181
[70] Brock (2002), p.171
[71] Hirsh (1981).

its properties physically, determining the gas's boiling, critical and freezing points, and attempting to examine its structure using spectroscopy. The physical tradition of precision measurement was here being incorporated into chemical study. For the spectroscopic analysis, Ramsay and Rayleigh enlisted the help of Crookes, who had been involved in the early development of the technique and instrumentation. In the mid nineteenth century it had been found that each element had unique spectral lines, produced from the diffraction of light, and this technique could thus be used to detect new elements.[72] Initially, spectroscopy was used primarily by chemists, although it would later become fundamental to quantum physics, and is thus indicative of the move among some chemists away from reactions and towards structure.

Rayleigh and Ramsay did not only use physical techniques to study chemical phenomena; they also applied physical theory to the problem, considering their results in relation to the kinetic theory of gases. Developed in response to the principle of energy conservation, this was the notion that gases consisted of large amounts of rapidly moving particles, and this motion created the phenomenon of heat. It was a *physical* rather than *chemical* theory. Ramsay and Rayleigh determined that Argon had an atomic weight of about 40, which caused difficulties with chemists. A weight of 40 meant that it would have to be situated in the periodic table between potassium and calcium both of which were highly active. This was a problem because Argon was inert, and elements in the periodic table were supposed to be grouped with others having similar properties. Chemists and physicists were faced with a conflict between the periodic law and the kinetic theory of gases.[73]

Unsurprisingly there was opposition from Henry Armstrong. As well as being fundamentally against the use of physical methods in chemistry, Armstrong was a close friend of Dewar's. He argued that the kinetic theory was not valid, and accordingly the weight of 40 was incorrect. Dewar, meanwhile, contended that Argon was not a new element, but rather an allotropic form of Nitrogen. The issue was eventually resolved, after the discovery of further inert gases allowed Ramsay to suggest that these could together form a new group of elements. Notably, Ramsay conducted this later research without Rayleigh. The physicist's son later suggested that his father had remarked: 'I want to get back again from chemistry to physics as soon as I can. The second-rate men

[72] Brock (2008), pp.61-2.
[73] Hirsh (1981).

seem to know their place so much better.'[74] This was a problem of disciplinary differences: in Rayleigh's view, many of the objections made by chemists were fundamentally invalid, while from the perspective of chemists such as Dewar and Armstrong, the physical methods employed by Rayleigh and Ramsay didn't hold sufficient authority. Rayleigh and Ramsay received Nobel Prizes in physics and chemistry, respectively, for their discovery of Argon. This recognition of a successful collaboration between a physicist and chemist obscures the tensions and disagreements which characterised much of Rayleigh's involvement with the chemists. Early low temperature research in Britain was situated on an awkward disciplinary boundary between chemistry and physics. With Rayleigh's decision to distance himself from this interdisciplinary work, the research came to fall ever more under the domain of chemistry. At this point, very low temperatures did not seem an obvious candidate for an alternative 'modern' physics to the Cavendish work.

This episode of conflict had some consequence as to the development of facilities for low temperature research. Ramsay, with his assistant Morris Travers, continued to study the noble gases at low temperatures. However, hostilities with Dewar were intensified by a heated dispute with Ramsay over the Presidency of the Chemical Society, and subsequently Dewar refused to supply him and Travers with liquid air. Instead, Ramsay turned to William Hampson who, while working at Brin Oxygen Company in Westminster, had developed an air liquefaction machine. With the help of Hampson, Travers built a hydrogen liquefier that, in direct opposition of Dewar's claims of cryogenic research as costly, could be constructed for only £35. Brock has suggested that the low temperature equipment at University College London was consequently superior to that at the Royal Institution.[75]

As the twentieth century began, there were thus facilities in both University College London and the Royal Institution for studying the new science of low temperatures. By the early twentieth century, the laboratory at Manchester also had its own liquefier, although low temperatures never became a major research topic.[76] However, in Mendelssohn's history of low temperature research, he declares that following Dewar's move away from this area around 1908, 'low-temperature work in Britain came

[74] Quoted in Hirsh (1981), pp.129-30 note 32.
[75] Brock (2002), p.184.
[76] *The Physical Laboratories of the University of Manchester: a record of 25 years' work prepared in commemoration of the 25th anniversary of the election of Dr. Arthur Schuster, F. R. S., to a professorship in the Owens College, by his old students and assistants* (1906), p.36.

effectively to an end'.[77] Certainly, Dewar left no research school; he refused to work with anybody who was not sufficiently skilled, and in particular avoided undergraduates. Watson has described the Davy-Faraday laboratory in the first quarter of the twentieth century as housing a disparate group of scientists, many of whom only stayed for a short while; this environment fostered individual, not group, research.[78] At University College London, meanwhile, the use of low temperature techniques had simply been a means to finding out more about the inert gases; it was not of interest in and of itself.[79] Travers apparently did plan to continue low research work, hoping it would help him obtain a better professional position. However, when in 1904 he was appointed Professor of Chemistry at University College, Bristol, he took his liquid hydrogen apparatus there, but didn't use it.[80] In 1906, he left England to accept an appointment as Director of the Indian Institute of Science, and remained there until 1914.[81] As I shall show, in the twentieth century low temperature research began to develop outside of London, under the influence of German physicists and in response to the quantum theory of specific heat. Here, low temperature research would move more into the realm of physics.

2.6 Beyond 1895: the 'completeness' of physics and a Cavendish 'diaspora'

Fin de siècle physics, covering the last decade of the nineteenth century, was traditionally characterised by a sense of completeness, the idea that the fundamental ideas were in place, and all that remained was to fill in the details. This view has, however, been in contention for some time.[82] What does my overview of nineteenth century physics contribute to the completeness narrative? Some of the practitioners of precision measurement may have held the view countered by Maxwell, that the future of physics lay in the refinement of measurements. This was certainly *their* future of physics, and it had been vindicated by the successful discovery of the rare gases using these methods. As for the more theoretically engaged physicists, it does not seem as though they believed their work to be nearing completion. The Maxwellians were still looking for a successful model of the ether, with proof, and Larmor's Electronic Theory

[77] Mendelssohn (1977), p.73.
[78] Watson (2002).
[79] In Travers' (1956) biography of Ramsay, the focus is on the gas, and there is little mention of low temperature work.
[80] Bawn (1963), p.305.
[81] Kostecka (2011), pp.80-6.
[82] As detailed in Badash (1972).

of Matter was being developed in line with this. Cavendish experimentalists of the new Thomson school were working in the new field of ionic theory, which can't have seemed like the end of physics. Notably, Thomson retrospectively remarked that there was a 'pessimistic feeling' around at the end of the nineteenth century that 'all that was left was to alter a decimal or two in some physical constant'.[83] This, perhaps, may have been a reflection on the state of the Cavendish Laboratory at that time, when Thomson's 'modern' researches co-existed with the older tradition of precision measurement established by Maxwell and Rayleigh.

This older tradition was maintained in part by the presence of Richard Glazebrook, a Cambridge wrangler who worked as a demonstrator at the Cavendish under Rayleigh and conducted research on electrical standardisation. He applied for the directorship, but was passed over in favour of Thomson, and in 1891 became Assistant Director of the Laboratory. Here he continued Rayleigh's tradition of precision measurement and published papers on optical and electrical problems.[84] In 1896, he wrote a book on the life and work of James Clerk Maxwell, which he titled *James Clerk Maxwell and Modern Physics*. The final chapter explored the development of Maxwell's theory, with Glazebrook bringing in the various experimental confirmations made since his death. The book ended with a brief discussion of the final piece of the puzzle, still being sought: a theory of the ether that could incorporate all the facts of electricity, magnetism, luminous radiation and even gravitation:

> 'Meanwhile we believe that Maxwell has taken the first steps towards this discovery, and has pointed out the lines along which the future discoverer must direct his search, and hence we claim for him a foremost place among the leaders of this century of science'.[85]

Not only does this book provide a definition of 'modern' physics as based around Maxwellian electrodynamics, it also presents the physics of 1896 (shortly before the impact of new discoveries would be felt) as nowhere near complete. While there was only one 'piece of the puzzle' remaining, it required a substantial amount of work to be uncovered. In general, late nineteenth century British physicists did not have a 'complete' concept of the natural world which was to be disrupted, and even overthrown, by new developments. Rather, they had certain conceptual commitments, such as the ether, but these were flexible in their incompleteness. Rather than 'modern'

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[83] *Report of the British Association for the Advancement of Science* (1909), p.29.
[84] Rayleigh and Selby (1936); Rayleigh's Cambridge career is discussed in Kim (2002).
[85] Glazebrook (1896), p.221.

physics overthrowing the ’classical’, instead old ideas provided the context into which the new would be absorbed, or rejected.

Commitments to old ideas were generally contingent on a physicist’s pedagogical and institutional background.[86] With so many of the new ideas arising from research undertaken in the Cavendish Laboratory, and then being adopted by these Cambridge experimentalists, the nature of Cavendish physical research had an effect on the emergence of many ‘modern’ areas of physics. By 1895, the Cavendish Laboratory had become the pre-eminent centre of experimental physics in Britain. While the tradition of precision measurement continued, this occurred side by side with the new approach utilised by Thomson. Many researchers here responded to new developments in the context of the ‘new’ Cavendish style of physics: ionic research, explorations of the fundamental nature of matter, and less precise methods of experimentation. New discoveries were made in the Cavendish, most notably Thomson’s ‘corpuscle’, but so too were new physicists. Experimenters such as Ernest Rutherford and William Bragg received their training from Thomson before leaving the Cavendish and going on to establish their own research schools, perpetuating their own particular Cavendish style of experimental microphysics. Lelong has described this as an ‘international diaspora of Cavendish physicists’, and in the remainder of this section I shall consider the effect that such a diaspora had on the development of physics in Britain.[87]

I begin my account of physics after 1895 with a consideration of how J. J. Thomson and his students responded, and contributed, to the new discoveries. However, I will subsequently emphasise that this was not the only type of physics being practiced in Britain, and the institutional homes of the Cavendish diaspora were not the only sites where ‘modern’ physics could be established. Precision measurement continued to find itself in demand by industry, while the science of low temperatures, also industrially relevant, was becoming a plausible candidate for an alternative ‘modern’ physics.

As I have already noted, Thomson’s interest lay in ions and unifying theories of matter. What changed following the discovery of X-rays were some initial speculations as to the existence of divisible atoms. When, in 1896, Thomson began to experiment on cathode rays, he turned such speculations into a more concrete suggestion. In 1897, he reported

[86] Warwick (2003).
[87] Lelong (2005), p.212.

the results of these experiments, postulating that cathode rays were negatively charged particles, which he termed ‘corpuscles’, from which atoms were constructed.[88]

As is now well known, Thomson was subsequently hailed as the ‘discoverer’ of electrons, the subatomic particles postulated by Larmor. By 1897, Larmor had developed his theory more fully, proposing an ether occupied by positive and negative electrons. Thus, matter consisted of discontinuities in the ether. This was compatible with Maxwellian electrodynamics if one thought of the mass of matter as being simply the electromagnetic mass of the electrons.[89] The standard ‘discovery’ history linking Thomson’s corpuscles to Larmor’s electrons has been rejected. There was a crucial difference between these two particles, as noted by Falconer: the ‘electron’ linked the ether and matter, whereas ‘corpuscles’ were part of an atomic model.[90]

While Thomson continued to develop theories of atomic structure, his students at the Cavendish also responded to new discoveries within the ionic context he promoted. Ernest Rutherford arrived at the Cavendish from New Zealand in 1895. He was not trained in mathematical physics and Thomson provided much of the theoretical and mathematical framework for Rutherford’s experimental work at the Cavendish. With Thomson’s help, Rutherford interpreted his work on X-rays as providing experimental evidence supporting an ionic theory of gases. He then turned his attention to a different newly discovered radiation, the Becquerel rays emitted by uranium. Now presumably working separately from Thomson, who was then engaged in his cathode ray work, Rutherford originally continued to focus his experimental work on the concept of ions, conceiving of the radiation as electromagnetic disturbances. However, gradually the ionisation of gases became merely a tool used to measure the properties of uranium radiation.[91]

In 1898, Rutherford moved to Montreal, Canada, where he had been appointed Professor of Physics at McGill University. Here, he continued his work on radioactivity, now of both thorium and uranium, with the help of the chemist Frederick Soddy. Notably, Soddy differed from the Cavendish school in one fundamental area: like many chemists, he rejected both the electron theory of matter and Thomson’s corpuscular atomic

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[88] Falconer (1987), pp.264-6.
[89] Warwick (1991); Warwick has noted that while Larmor believed in a further reduction of the electromagnetic equations (using the Principle of Least Action), in practice he too relied on the equations.
[90] Falconer (1987), pp.268-9.
[91] Wilson (1983), pp.125-6.

theory.[92] Soddy and Rutherford published their theory of atomic disintegration in 1902. While Soddy framed their development of ideas as having arisen purely out of chemical investigations, Sinclair has noted that Rutherford continued to correspond with Thomson throughout this period on the subject of their respective radioactive researches and theories.[93]

Indeed, when Rutherford returned to England in 1907, his work reflected Thomson's interest in atomic structure and unifying hypotheses. However, he differed from his former teacher, having a less mathematical approach and lacking the theoretical commitment to the ether instilled in Thomson during his Mathematical Tripos training.[94] Back in England, Rutherford passed his particular version of Cavendish experimentalism onto his students. As Professor of Physics at Manchester, he set up a radioactivity research school, training both undergraduates and postgraduates in the techniques necessary to pursue this area of physics.[95] Third year students were subjected to an examination on 'modern physics', particularly radioactivity, for which they prepared by reading not textbooks but current periodical literature. As one former student noted, Rutherford's emphasis on 'modern' physics was often at the expense of providing a broad training in the older 'classical' foundations.[96]

At Manchester, Rutherford was defining radioactivity research as an important part of 'modern' *physics*. However, its disciplinary place was by no means clear cut. Soddy was appointed Lecturer in Physical Chemistry and Radioactivity at the University of Glasgow, where he continued to separate radioactivity from corpuscular theories of matter.[97] In Paris, the Curies were seen as chemists, not physicists.[98] And, as with Argon, the interdisciplinary nature of radioactivity was reflected in the distribution of Nobel Prizes. Becquerel and the Curies were jointly awarded the 1903 prize in physics for the discovery of, and early research into, radioactivity. In 1908, Rutherford also received a Nobel Prize for radioactivity research, but his was in chemistry.

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[92] Sinclair (1988a), p.97.
[93] Sinclair (1988a).
[94] Falconer (1989), p.109.
[95] Falconer (1989), p.113; Hughes (2002a), p.356; Wilson (1983), pp.268-276.
[96] Robinson (1954), p.10.
[97] Sinclair (1988b), pp.116-7.
[98] See Malley (1979) for the Curies' early work on radioactivity and how this differed from Soddy and Rutherford's approach.

Alongside his research in McGill and Manchester, Rutherford worked tirelessly to promote his work and the burgeoning field of radioactivity to both his scientific peers and a wider 'public'. While at McGill, he spoke frequently at colleges and universities in the United States, as well as lecturing before professional societies (including the American Physical Society), and in 1906 taught a summer course at the University of California. He lectured at the 1903 meeting of the British Association for the Advancement of Science, and the 1904 International Congress of Arts and Science in St. Louis.[99] He also deliberately sought out a more general audience, writing articles for *Harper's Monthly Magazine*, an American general interest publication.[100] Indeed, in 1904, Oliver Lodge expressed concern that Rutherford might be wasting too much time lecturing, at the expense of his experiments.[101] Notably, by then Lodge was more of a lecturer than a researcher, and much of his time was also occupied with his administrative work as Principal of Birmingham University.[102] With his 1903 Fellowship to the Royal Society of London, and 1908 Nobel Prize, Rutherford was developing a reputation as a prestigious scientist, enhancing his image as an 'expert' communicator and teacher of radioactivity physics. With these activities, combined with his pedagogical work in the establishment of his Manchester school, Rutherford was able to successfully promote the new science in his own terms. While the new element of radium attracted 'public' attention for its potential medical applications, Rutherford espoused an alternative narrative of the utility of such research for furthering natural knowledge.[103] As I shall show in later chapters, Rutherford's emphasis of research into the structure of matter as knowledge 'for its own sake' continued throughout his career.

While Rutherford was establishing his reductionist radioactivity research school at Manchester and promoting the work more widely in 'public' and professional spheres,

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[99] Badash (1979), p.29.
[100] Rutherford (1904b); Rutherford (1906).
[101] Lodge to Rutherford, 4 January 1904, quoted in Badash (1979), p.29.
[102] In 1924, Lodge admitted to Edward Andrade that he had found it difficult to keep up with early twentieth century developments in physics as he had been 'occupied with University management, - perhaps unduly occupied therewith', Lodge to Andrade, 13 August 1924, LODGE.
[103] For example, the *Daily Express* frequently published articles on radium's potential medical applications: 'The Mystery of Radium / Blind men said to have regained sight', *Daily Express,* 8 June 1903, p.1; 'Radium rays cure cancer', *Daily Express*, 4 July 1903, p.1; 'Radium Germicide', *Daily Express*, 16 July 1903, p.1; 'Radium and Blindness / London surgeons doubt the reported cure', *Daily Express*, 25 August 1903, p.8; 'Five grains of hope / Anonymous gift of radium for cancer patients', *Daily Express*, 16 November 1903, p.5; 'Radium cancer treatment', *Daily Express*, 17 November 1903, p.5; 'Run on Radium / Fabulous Prices for Grey Dust / Cancer Experiemnts', *Daily Express*, 30 November 1903, p.1; 'Radium at Work / Many hopes and a false statement / Luxuries of Light', *Daily Express*, 9 December 1903, p.6; For the popular reception of radium and radioactivity in America see Badash (1978) and Lavine (2008).

other Cavendish alumni were going on to set up their own experimental schools. William Henry Bragg was a graduate of the Mathematical Tripos, but spent less than a year in Thomson's Cavendish before being appointed Elder Professor of Mathematics and Experimental Physics at the University of Adelaide in 1886.[104] However, in 1898, he paid a brief visit to England, spending a few weeks in Cambridge where he met with both former friends and new acquaintances to discuss recent developments in physics. This clearly had a significant impact, as on his return to Adelaide, Bragg decided to focus his attention on X-rays, radioactivity, the electron, positive ions and the ionization of gases.[105]

For Bragg's early work on radioactivity, he corresponded regularly with both Rutherford and Soddy, although the chemist disagreed with Bragg's particle concept of radiation.[106] By 1907, this work had made him sufficiently reputable to be elected a Fellow of the Royal Society, and in 1909, with the help of Rutherford, he was appointed Cavendish Professor of Physics at Leeds in 1909.[107] On arrival in England, Bragg immediately travelled to Manchester to visit Rutherford, and the two maintained a strong friendship throughout their career.[108] Bragg also continued to hold institutional links with Cambridge, where his son and collaborator, William Lawrence, was now based.

Bragg began directing research into X-rays and radioactivity in his Leeds laboratory. In Australia, he had hoped his work on radioactivity would help him to answer questions about molecular and atomic structure.[109] In England, he and his son began to explore these questions with X-rays. Together, the Braggs were founding a new field of physics, X-ray crystallography. Here X-rays were directed through matter, and their subsequent diffraction analysed to determine molecular structure, a technique developed by the Braggs from 1912. Notably, this was taken up very quickly by members of Rutherford's Manchester school, including Henry G. J. Moseley, who had joined the lab as a demonstrator in 1910 after graduating from Oxford.[110] Here he collaborated with Lecturer in Mathematical Physics and former Cambridge Wrangler, Charles Galton Darwin, who applied mathematical theory to Moseley's experimental work.[111] Another

[104] For biographical details, see Caroe (1978) and Jenkin (2008).
[105] Jenkin (2004).
[106] Jenkin (2004).
[107] Hughes (2005), p.276.
[108] Jenkin (2008), p.301; Wilson (1983), pp.299-300.
[109] Jenkin (2004), p.77.
[110] Jenkin (2008), pp.345-9; For Moseley see Heilbron (1974).
[111] For Darwin see his obituary, Thomson (1963); Navarro (2009) has explored his work in the 1920s.

Cavendish researcher, Norman Campbell, moved to Leeds in 1910, keen to work in Bragg's 'modern' laboratory.[112] Both Rutherford and Bragg's schools were providing homes for other Cavendish men to continue the reductionist research established by Thomson.

2.7 Looking beyond the Cavendish: alternative traditions in experimental physics

Bragg and Rutherford created research schools in Leeds and Manchester which enthusiastically adopted the new discoveries into a Cavendish framework of experimental reductionist physics. However, not all institutions offered a suitable environment for such a venture. John Townsend spent five years at the Cavendish under Thomson before being appointed Oxford University's first Wykeham Professor of Physics at in 1901. Here, at a physics department concerned more with teaching than research, Townsend's role was to teach electricity and magnetism, while the Professor of Experimental Philosophy, Robert Clifton, was responsible for teaching light, heat and sound. Townsend, however, brought with him a commitment to original research, specifically of the Cavendish style. He set out to establish ion research in his laboratory, with the focus on verifying theoretical hypotheses rather than producing precise measurements. Townsend succeeded to some extent, but, as Lelong has shown, this was not without difficulties. Townsend came into conflict with Clifton, with whom he competed for resources, over their differing styles of experimental physics.[113] The tradition of precision measurement and teaching established in Oxford, and upheld by Clifton, was not entirely conducive to the development of a Cavendish style 'modern' research laboratory.

Clifton was most interested in research into the structure of matter, but his Electrical Laboratory was also responsible for the teaching of electrical engineering and applied electricity and magnetism. The actual teaching was undertaken by Clifton's demonstrators, but Lelong argues that this does not mean the Professor had no interest in practical matters.[114] The relation of Cavendish reductionist physics to industrial or commercial applications is also evident in the work of another former student of

[112] Jenkin (2008), pp.305-6.
[113] Lelong (2005).
[114] Lelong (2005), p.225.

Thomson's. Owen Willans Richardson began studying at Trinity College in 1897, the same year that Thomson produced experimental evidence of his corpuscles. After graduating from the Natural Sciences Tripos, Richardson worked at the Cavendish until 1906, when he was appointed Professor of Physics at Princeton University. On Thomson's suggestion, Richardson explored the idea that electric currents were carried by corpuscles, which would cause a wire carrying an alternate current of high frequency to emit either radioactive or Röntgen radiation. He then turned his attention to the thermal emissions of electrons, caused by heating a wire.[115] While Richardson's interest was intellectual, there were practical consequences of such work. The electrical engineer John Ambrose Fleming, modified the Edison vacuum tube used by thermionic researchers to construct an effective rectifier of low frequency and high frequency currents.[116] His 'Fleming valve' was then used in radio communications, and the history of thermionics became connected to the history of practical wireless technology.[117] As I shall show in Chapter Six, Richardson's thermionics work could thus be used to provide a direct link between Thomson's 'discovery' of the electron and the wireless industry. As with Clifton, there is a link in Richardson's post-Cavendish career between 'pure' research and practical application.

While Thomson's protégés were beginning to spread across Britain, in many institutions research was developing along different lines. Even in Rutherford's 'modern' Manchester laboratory, the research topics which had been established by Arthur Schuster (optics, spectroscopy and thermodynamics) continued to be studied.[118] A similar co-existence of research styles could be found in Liverpool. Lionel Wilberforce came here from the Cavendish in 1900 but, having been a demonstrator there since the 1880s, had not adopted Thomson's style of experimental research. Even at the Cavendish, the 'old' style of precision measurement and teaching still had a place. Furthermore, while as Director of the new George Holt Physics Laboratory in Liverpool Wilberforce focused on teaching, he fostered an environment in which research could also develop. As a result, Charles Glover Barkla, another Cavendish researcher, found it a suitable Laboratory for his research on X-ray scattering.[119]

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[115] Knudsen (2004).
[116] Eccles (1945).
[117] For example, the South African electrical engineer and industrialist Hendrik van der Bijl traced the origins of the thermionic vacuum tube back to Cambridge research on electrons: van der Bijl (1920).
[118] Hughes (2005), p.275.
[119] Hughes (2005), pp.271-3. For physics in Liverpool see Rowlands (1990); for Barkla's work on X-rays see Wynne (1976).

In many laboratories, the Cavendish influence was simply not relevant. William Edward Ayrton was Professor of Physics at the London City and Guilds Central Institution, where the third year teaching emphasised electrical engineering and optical instrument making.[120] At University College, London, Hugh Callendar promoted the study of industrially relevant optical and thermodynamic problems, and collaborated with the departments of chemical technology, mechanical engineering and aeronautics.[121] The need for this kind of research is evident in the establishment of Imperial College and the National Physical Laboratory. In 1907, City and Guilds, the Royal School of Mines and the Royal College of Science were merged to form Imperial College. This new institution was given a 'utilitarian mandate' from its inception, pushing the research there in a particular, industrially led, direction.[122] The National Physical Laboratory was established in 1900.[123] Under the directorship of Richard Glazebrook, it assumed responsibility for 'standardising and verifying instruments, for testing material and for the determination of physical constants'.[124] However, the laboratory had broader industrial aims than the provision of precise measurements. In 1909, it began setting up a department for aeronautical research, and an engineering division was created in 1911, and the Laboratory also conducted research into the standardisation of radium and X-rays.

There were thus two distinct visions of experimental physics in the early years of the twentieth century, headed by Glazebrook on the one side and Thomson on the other. Both men received their experimental training at the Cavendish, and indeed Glazebrook had also wanted to become Director, losing out to Thomson. The students of Thomson were now promoting their particular version of Cavendish physics outside of Cambridge, although they differed from Thomson in having less mathematical training and little interest in the ether.[125] Meanwhile, Glazebrook continued to promote precision measurement and standardisation. Within experimental physics, two competing visions of 'modern' had emerged.

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[120] Gay (2000), p.393.
[121] Gay (2007), p.152.
[122] Gay (2007). p.2.
[123] Moseley (1978); Pyatt and Dean (1983); Magnello (2000).
[124] Magnello (2000), p.47.
[125] Falconer (1989), p.109.

Physicists of the two traditions had very different ways of thinking about the progress of their discipline and situating shifts in thought and practice. This is exemplified by the rhetorics employed by Ernest Rutherford and Ernest Howard Griffiths in 1904 and 1906 respectively. Rutherford, as we have seen, was one of Thomson's most successful former research students, establishing a school of radioactivity research at Manchester. Griffiths, meanwhile, was educated at Cambridge, taking an ordinary degree in applied science. While staying on at Cambridge employed as a tutor, Griffiths carried out precision experimental research in the laboratory of Sidney Sussex College. Here he worked on the measurement of electrical and thermal units, collaborating with Glazebrook on standardization work at the Cavendish.[126] In 1895, Griffiths' experimental work had earned him sufficient esteem to be elected a Fellow of the Royal Society, and he also joined the Electrical Standards Committee of the British Association.

In 1906, by now Principal of the University College of South Wales and Monmouthshire, Griffiths served as President of Section A of the British Association. In his address, he made no mention of X-rays, radioactivity or electrical discharge, instead declaring that 'during the last twenty-five years the increase in our 'natural knowledge' has been greater than in any previous quarter of a century'.[127] Crucially, Griffiths was looking back not to 1895, the year of the discovery of X-rays, but to circa 1880. Furthermore, Griffiths characterised the progress of the previous 25 years by a devotion to the 'accurate determination of certain physical constants'.[128] For those engaged in the laboratory tradition of standardisation, the history of the last 25 years of physics could thus be depicted as a line of progress towards ever increasing precision. In this interpretation, by 1906 the National Physical Laboratory, not the Cavendish, was the centre of 'modern' physics in Britain.

Rutherford, meanwhile, set out his views in his 1904 book on *Radioactivity*. He opened with the declaration that the 'close of the old and beginning of the new century have been marked by a very rapid increase of our knowledge of that most important but comparatively little known subject - the connection between electricity and matter'.[129] In Rutherford's account, the impetus for this new knowledge came initially from Lenard's

[126] W. C. D. D (1932); Glazebrook (1932).
[127] *Report of the British Association for the Advancement of Science* (1906), p.479.
[128] *Report of the British Association for the Advancement of Science* (1906), p.476.
[129] Rutherford (1904a), p.1.

experiments on cathode rays (begun in 1888) and Rontgen's discovery of the X-rays. Rutherford made no mention of the development of precision measurement, while Griffiths ignored the theory-driven experimental work on the nature of matter. As a result, both men situated the roots of their discipline in different times and places.

While Rutherford's work was concerned with the fundamental structure of matter, Griffiths' 'modern' physics was closely linked to industry. Similarly, W. E. Ayrton, delivering the Presidential Address of Section A at the 1898 British Association meeting, had discussed the relationships between science and industry.[130] A year later, Oliver Lodge, delivering the Presidential Address to the Physical Society, declared that in 'Newton's time, pure science was altogether aloof from practice', but that now it was being used in wireless telegraphy, the Atlantic cable and the dynamo.[131] For many, new physics was industrial physics.

I have thus indicated the wide variety of physics research being undertaken in Britain in the early twentieth century, and the various possible categories of 'modern'. I close this section with a brief overview of the landscape of British 'classical' and 'modern' physics in the year 1910. At Manchester, 'modern' physics was underway in the radioactivity school developed by Rutherford, while Bragg was promoting 'modern' X-ray research at Leeds. C. G. Barkla was also advancing X-rays research, as Professor of Physics at King's College, London from 1909 to 1913. O. W. Richardson would join him with his thermionic research in 1914.[132] These men were all Cavendish physicists, trained under Thomson in ionic research, spreading their version of this 'modern' physics across the country. Similarly, G. W. C. Kaye, who had graduated from Cambridge in 1908 and then worked as J. J. Thomson's personal assistant at the Cavendish, began work on radium standards at the National Physical Laboratory in 1913.[133] This project, necessitated by the growing use of radium in industry and medicine, had in fact been initiated by Rutherford himself, a far cry from his dedicated investigations into the nature of matter.[134] Even among the Cavendish researchers, there was thus a wide variety of 'modern' physics. Notably, the Cavendish Laboratory itself was now becoming a 'victim of its own success', as these new research schools

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[130] *Report of the British Association for the Advancement of Science* (1898), pp. 767-777.
[131] Lodge (1897), pp.344-5.
[132] Hughes (2005), p.276.
[133] Griffiths (1941); Smith (1975), pp.3-10.
[134] This is briefly discussed in Smith (1975), pp.3-5. Boudia (1997) considers the 'radium industry' from the perspective of Marie Curie's involvement.

competed for the title of the most celebrated ‘modern’ physics laboratory in the country.[135]

Research into different kinds of ‘modern’ physics was underway in various institutions. The Bristol educated Arthur Tyndall became head of the physics department there in 1910, and also conducted ionic discharge research, on the mobility of ions in gases.[136] Low temperature research, then based predominantly in chemistry departments at the Royal Institution and University College London was also a type of ‘modern’ physics, having already successfully uncovered new elements. Finally, there were a number of institutions promoting more traditional research in physics and continuing to emphasise the value of teaching: Cardiff, Liverpool, Birmingham, East London College, Nottingham and Sheffield.[137] In 1907, Hugh Callendar was appointed Professor of Physics at the newly formed Imperial College, London. He was dismissive of the ‘modern’ physics arising from Cambridge studies of gas discharge and the new phenomena of radioactivity, referring to such work as the ‘playground of dilettante physicists’.[138] Indeed, J. S. G. Thomas, an industrially inclined physicist, later remarked that at the Royal College of Science, ‘classical physics was the only physics that mattered’.[139] British physics in 1910 was a diverse field where ‘classical’ work co-existed with various emerging types of ‘modern’ physics.

2.8 Problems in theoretical physics: continuity, discontinuity and descriptionism

The landscape detailed above is concerned with the work of experimental researchers. Theoretical physicists, including those continuing in the tradition of Maxwellian electrodynamics, were also responding to changes. For them, new developments were raising crucial theoretical questions concerning the continuous or discontinuous nature of matter. Their picture of the physical world had been dominated by the existence of the ether, which created continuity in nature by connecting all matter. However, both Larmor’s Electronic Theory of Matter and Thomson’s corpuscles suggested an underlying discontinuity. Oliver Lodge conceptually tied together these two theories,

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[135] Hughes (2005), p.276.
[136] Mott and Powell (1962).
[137] Hughes (2005), p.279.
[138] Thomas (1949), p.583.
[139] Thomas (1949), p.583; J. S. Thomas was trained in London and worked for the South Metropolitan Gas Company.

describing Thomson as the discoverer of Larmor's electron. In 1902, he delivered a series of lectures to the Institute of Electrical Engineers on the subject of electrons in which he depicted J. J. Thomson's 1897 experiments as a response to theoretical work on the electron.[140] Regardless of whether the problem was corpuscles or electrons, Lodge and other proponents of continuity were now faced with the problem of reconciling the new atomism with their physical worldview.

The saviour of continuity could be found in the ether. By linking together Thomson's corpuscles and Larmor's electron, Lodge was placing the phenomenon of discontinuity within the Electronic Theory of Matter. This, as we have seen, was concerned with the relationships between matter and the ether, with negative and positive particles being the only constituent elements of the ether. In this reading, all mass was electromagnetic in nature. In 1908, James Jeans, a Trinity College scholar and Second Wrangler, established the theory pedagogically with the publication of a textbook on the *Mathematical Theory of Electricity and Magnetism*.[141] This book laid out the fundamentals of Maxwellian electrodynamics, before incorporating in the concept of the electron. Jeans' textbook quickly became influential in Cambridge mathematical teaching, and would remain so until the late 1920s.[142] This did not mean that everybody in Cambridge became concerned with the relationship between ether and matter, or believed that all mass could be explained electromagnetically. As Warwick has noted, while Larmor was looking for a 'physical micro-structure of the ether', early twentieth century Cambridge students were more interested in applying the theory to particular problems.[143]

However, for those physicists who were preoccupied with notions of continuity and discontinuity, the Electronic Theory of Matter combined the newer experimental results with a continuous ether. Discontinuity could thus be found on the surface of the physical world, but the foundations of matter were continuous. This was the approach taken by J. H. Poynting, Professor of Physics at Birmingham and the only student of Maxwell's to become a 'Maxwellian'. In his 1899 address as President of Section A at the British Association, he discussed a dualistic conception of nature, with atoms and molecules floating in a distinct ether. Crucially, Poynting saw continuity as an 'ideal' sought by

[140] Falconer (2001); Lodge's lectures were subsequently published, as Lodge (1906).
[141] Jeans (1908).
[142] Warwick (2003), pp.376-382.
[143] Warwick (2003), p.380.

physicists, and discontinuity as part of a more descriptive method of practising physics.[144]

This notion of 'descriptionism' has been considered by John Heilbron, who regards it as a fundamental problem in fin de siècle physics. Heilbron suggests that physicists used this approach in order to deal with the challenges of accommodating new theoretical developments in their discipline. He argues that, faced with the notion that long held theories were no longer valid, physicists redefined their professional objectives. With descriptionism, physical laws stopped being reflections of the 'true' nature of the physical world, and became instead tools of convenience. Physicists, Heilbron argues, stopped explaining and started describing. This view was vocalised by continental physicists, and expounded by Henri Poincaré at the 1900 Universal Exposition in Paris, but it was based on the *practice* of British physicists. As we have seen, here theoretical ideas were rooted in the mental construction of mechanical models. This had the effect of emphasising how the physical objects of nature behaved, rather than an underlying causality. However, these models were not treated as purely descriptive, and Maxwell's followers conceived of a mechanical reality.[145]

The concept of descriptionism was related to the wider issue of discontinuity, arising from the apparently corpuscular nature of matter. The electron posed a considerable threat to the continuous worldview held by many physicists and dominated by the concept of an ether. In Britain, descriptionism played a role in discussions about continuity and discontinuity. This is evident in Poynting's address, and also in the address of the President of Section A the following year. Here, Joseph Larmor discussed a 'new domain' of physics, declaring that the science was trending towards a 'molecular order of ideas'. He warned that as a part of this trend continental physicists were considering rejecting the Newtonian dynamical principles which had formed 'the basis of physical explanation for nearly two centuries', and replacing them with 'a method of direct description of the mere course of phenomena, apart from any attempt to establish causal relations'.[146] Larmor noted that it was indeed often advisable to remove the 'scaffolding' of theory, preserving only the final formulae. However, the danger arose in deciding what was 'essential fact' and what was mere 'intellectual scaffolding'.[147]

[144] *Report of the British Association for the Advancement of Science* (1899), pp.615-623.
[145] Heilbron (1982); See also Hunt (1991).
[146] *Report of the British Association for the Advancement of Science* (1900), p.617.
[147] *Report of the British Association for the Advancement of Science* (1900), pp.626-7.

Larmor intended descriptionism to complement physics' ultimate aim of revealing how the natural world worked. He was warning against abandoning certain ideas, such as Newtonian mechanics and a continuous ether.

This argument was also made by Horace Lamb, Professor of Pure Mathematics at Owens College, Manchester, and a former Trinity College mathematician. Speaking as President of Section A in 1904, Lamb argued that many proponents of 'the more recent theories of Electricity' had replaced the laws of nature with 'rules by which we can tell more or less accurately what will be the consequences of a given state of things'.[148] He was suggesting that advocates of this new discontinuous worldview were also in conflict with supporters of material continuity in their conceptions of the purpose of physics. The electron physicists were apparently content with physical theories that did not contain a causal mechanism. Thus, physical atomism could be closely tied in with a descriptive view of nature. However, the three examples I have considered were all mathematical physicists. Indeed, the majority of Heilbron's descriptionists were seeking to describe everything through the use of fundamental mechanical principles. But descriptionism can also be viewed from the experimental point of view, with the abandonment of abstract explanatory theories in favour of concrete experimental observations. I now address this by considering the experimental descriptionism of the Cavendish physicist Norman Campbell, and how this affected his reception of relativity theory. In this case, the adoption by a British physicist of a 'modern' theory was closely related to his broader philosophical outlook.

As we have seen, the discoveries of various types of rays at the end of the nineteenth century were taken up by experimental physicists in the Cavendish Laboratory. However, there were also more theoretically based developments, relativity and quantum theory, emerging from the continent. Einstein proposed his principle of relativity in 1905, arguing that all motion was relative, with no standard frame of reference.[149] As Warwick has shown, early British responses to Einstein's paper 'On the Electrodynamics of Moving Bodies' were considerations not of a theory of relativity, but rather particular elements of the paper which were most relevant to their own research interests. While the mathematical Cambridge physicist Ebenezer Cunningham interpreted the paper as a contribution to electrodynamics, the Cavendish experimentalist

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[148] *Report of the British Association for the Advancement of Science* (1904), p.430.
[149] This would not be given the label 'special relativity' until after the development of the general theory in 1915, Warwick (2003), pp.405-6.

Norman Campbell read it as a tool for removing the ether from physics. Crucially, Campbell's reaction was part of a broader philosophical outlook: his desire to remove all unobservable entities from physics.[150]

Campbell graduated from the Natural Sciences Tripos in 1902 and took up research on atomic structure and radioactivity at the Cavendish. Here he developed an attitude towards physics which involved the rejection of ideas which could not be experimentally verified. In particular, he did not believe that mathematical equations corresponded to the physical world, and rejected both the ether and Larmor's Electronic Theory of Matter. In 1907 he wrote a textbook on *Modern Electrical Theory*, in which he characterised the Cambridge traditions of mathematical and experimental physics as entirely distinct, while also implicitly putting forward an early definition of modern physics. Here he referred to the 'old physics', represented by the work of Cambridge Wranglers, in contrast with the 'new physics' practiced by Cavendish experimentalists.[151] This new physics did not require a mathematical training, using instead visual models, and could boast the work of Faraday as its historical precedent. Furthermore, in Campbell's reading, the new physics proved to be 'much more suggestive than the old', leading to 'investigations which would never have been undertaken by the adherents of the older conceptions'.[152] Mathematical physicists clung to old ideas, hindering the prospect of progress. Experimentalists, however, held no such deep-set commitments, and were thus best placed to move forward. We can deduce from Campbell's text that he was developing definitions of 'classical' and 'modern' physics, based not on competing conceptual commitments, but rather on the latter's lack thereof. While these definitions did involve theoretical concepts, the main focus was on the *practice* of physics.

It was in this context that Campbell became interested in Einstein's ideas about relativity. He determined that relative motion could be defined within experimental physics, based on observations, while absolute motion relied on the propositions of theoretical mechanics. This was part of Campbell's ongoing project to describe the electrodynamics of moving bodies without recourse to the ether or mathematics. Of course in eliminating the ether, in favour of experimental studies of particles, Campbell was promoting a discontinuous picture of the physical world. Again, descriptionism was

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[150] Einstein (1905a); Warwick (1992, 1993a, 2003).
[151] Campbell (1907); See also Warwick (1993a).
[152] Campbell (1907), p.4.

connected to discontinuity, as Campbell was only interested in the particulate matter which could be directly examined and described, rather than the continuous and experimentally elusive ether. Furthermore, unlike the descriptionism discussed by Heilbron, Campbell used it to promote experimental over theoretical physics.

2.9 Conclusion: 'modern' physics in 1911

I end my introductory overview in 1911. This was the year that Rutherford published a paper detailing his nuclear model of the atom, consisting of a central charge surrounded by a cloud of electrons.[153] This work had come out of his experimental investigations into the nature of the alpha and beta rays emitted by radioactive elements. This was also the year of the first Solvay congress, an international meeting of physicists, designated by Staley as the birthplace of our definitions of 'classical' and 'modern'.[154] The congress was organized by the Belgian chemist and philanthropist Ernest Solvay and the low temperature physicist Walther Nernst, and was on the subject of 'Radiation and the Quanta'. As Staley has shown, Nernst believed the event to be one of significant importance, declaring to Solvay that 'we are currently in the midst of a revolutionary reformulation of the foundations of the hitherto accepted kinetic theory'.[155] The reformulation involved the work Nernst had been undertaking on the quantum theory of specific heats.

Nernst's quantum work involved research into the properties of matter at very low temperatures, and was fundamental in the transmission of this research topic into the realm of physics. The same year that Einstein completed his work on the electrodynamics of moving bodies, he also published three other important papers. One of these developed the new idea of the 'quantum', introduced by Max Planck in 1900. Planck had put forward a description of energy as emitted in discrete packets, or 'quanta', and Einstein utilised this concept in a new theory of light, suggesting that light was composed of discrete quanta.[156] In proposing that light is quantised, Einstein was ascribing discontinuous properties to energy. This corpuscular theory of light was part of a trend towards discontinuity that will be explored in the following chapter. However, here I consider a different aspect of quantum theory: Einstein's theory of specific heats.

[153] Rutherford (1911a).
[154] Staley (2005)
[155] Quoted in Staley (2005), p.554.
[156] Einstein (1905b); Kragh (1999), pp.66-8.

The specific heat (the amount of heat per unit mass required to raise the temperature of a substance by one degree Celsius) of solids was thought to be independent of temperature. However, a number of experiments carried out from the 1870s had suggested that specific heat increased at high temperatures. In 1905, James Dewar published the results of his experiments showing that at very low temperatures the specific heat began to approach zero. In 1907, Einstein used quantum concepts to construct a theory of specific heats which predicted these changes.[157]

This theory was considered in the field of low temperature research, then flourishing in Germany and the Netherlands. Here, low temperature research was underway in Onnes' low-temperature laboratory in Leiden and also Walther Nernst's Institute of Physical Chemistry in Berlin.[158] With Nernst, we find low temperature research located in the disciplinary home of physics rather than chemistry. While his official title from 1905 was Professor of Physical Chemistry, Nernst 'preferred to be recognized as a physicist engaged in chemistry rather than a physical chemist in the Ostwald sense'.[159] Furthermore, this physics was engaged with 'modern' ideas. Nernst was the first scientist to experimentally test Einstein's theory of specific heat.[160] He continued to work on the changes to specific heats at very low temperatures, concluding that as absolute zero was approached, the specific heat tended towards zero. This was in line with Einstein's theory.[161] From 1908, Nernst had been joined by a doctoral student, Frederick Alexander Lindemann (later Lord Cherwell).[162] Although British, Lindemann was born in Germany, and this is where he trained as a scientist. Under Nernst, he developed an interest in low temperature physics and its relations to the quantum theory. Together, they devised the 1911 Nernst-Lindemann formula for specific heats.[163] Lindemann would go on to establish his own low temperature research school, at Oxford from 1919, developing it throughout the 1930s with the successful recruitment and financing of Jewish émigré physicists.[164]

In Chapter Six, I explore how low temperature research was displayed at the Science Museum as a 'modern' physics of precision measurement, refined apparatus, and

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[157] Kragh (1999), pp.68-70.
[158] van Delft (2008).
[159] Hiebert (1982), p.111.
[160] van Delft (2008), p.343.
[161] van Delft (2008), pp.343-6.
[162] Berman (1987).
[163] Morrell (1992), p.267.
[164] Morrell (2005).

practical applications. Indeed, the exhibition was made possible by the financial backing of a number of industrial partners. In the 1930s, this ‘modern’ research into very low temperatures was far more reminiscent of the legacy of the National Physical Laboratory than Thomson’s Cavendish. This presents an alternative vision of modern physics, suggesting a rethinking of the Cavendish’s dominance in the first four decades of the twentieth century

This work would be strongly linked to industrial applications, but in 1911 low temperature research formed the basis of a discussion on dramatic theoretical changes in physics and the consequences this had for older, long held ideas. Organised by Nernst, the 1911 Solvay Congress included James Jeans, Rutherford, Lindemann, Einstein, Planck, Marie Curie and the Dutch physicist Hendrik Lorentz among its delegates. Staley has explored usages of the terms ‘classical’ and ‘modern’ here, arguing that Planck, in his paper on ‘The Laws of Heat Radiation and the Hypotheses of the Elementary Quantum of Action’, produced a definition of classical physics which differentiated modern physics from the older kinetic theory of gases. Modern physics was here defined as quantum physics. Furthermore, Staley proposes that this definition, put forward at an international discussion of the future of physics, was appropriated by the other delegates and ‘helped them reach a common understanding of what the past had involved’.[165] For my purposes, there is one notable corollary of Staley’s conclusion, of which he makes no mention. If a definition of modern physics was constructed in relation to new work on specific heats, then its origins lie in very low temperature research. As such, this research can be considered to fall under one definition of modern physics. However, very low temperature research subsequently became distanced from theoretical quantum discussions, and instead found a purpose in a number of industrial uses. Again, however, this can be considered ‘modern’ physics, but characterised very differently from low temperature’s 1911 incarnation. In the 1930s, very low temperature research was an expensive, industrially relevant type of ‘modern’ physics, focused on application rather than theory.

As I have considered in this chapter, and will continue to explore throughout my thesis, Staley’s argument is not applicable to the British case and here 1911 did not mark a moment of common understanding of the nature and purpose of ‘modern’ physics. The British experimentalist Norman Campbell was already conceiving of modern physics in

[165] Staley (2005), p.556.

very different terms, as a new style of research unhindered by obstructive theoretical commitments. And another experimentalist, Ernest Rutherford, appears to have ignored the categories of 'classical' and 'modern' entirely in 1911. Rutherford was in attendance at the Solvay Congress, but is notably absent from Staley's paper, having made no rhetorical contribution to the 'construction' of Staley's 'modern' physics, or indeed any 'modern' physics at this point. Furthermore, having been given the responsibility of writing up a report in *Nature* for British audiences, Rutherford was not without opportunity to do so.[166] In his report, Rutherford made no reference to 'classical' physics, to older theories or, indeed, to the past at all. Instead, he listed the titles of the papers, before praising Nernst's 'interesting account of the experiments upon the variation of specific heat with temperature down to low temperatures and of their explanation in terms of the "quantum" theory proposed by Prof. Einstein'.[167] Despite describing the question of specific heats as the most interesting part of the conference, Rutherford made no mention of the implications of this for past theories. He noted that 'many problems of modern physics' had been made clearer, but it is not evident in his article that he conceived of this as distinct from a corresponding 'classical' physics.[168]

Rutherford perceived the purpose of the conference in experimental terms. He was interested in the results of experiments, and the construction of theories which matched up to these results. The larger question of defining the discipline in relation to its past did not overly concern him. Furthermore, he was not involved in philosophically-charged debates over whether radiation should be conceived of as ultimately continuous or discontinuous, which I explore in Chapter Three. This is in spite of the fact that he himself was responsible for the development of such discontinuous ideas, in his nuclear atom. The research undertaken in Rutherford's laboratories in Manchester and Cambridge (as Director of the Cavendish from 1919) has come, to some extent, to be seen as 'modern' physics. Rutherford's tools were laboratory equipment, not mathematics; they were tangible objects, not abstract equations. As a result, where his theories departed from older ideas, there was less of a need to justify himself in this respect.

It is thus clear that Staley's account is not sufficient to explain the British case. Furthermore, the definition he proposes, based on distinguishing quantum theory from

[166] Rutherford (1911b).
[167] Rutherford (1911b), p.83.
[168] Rutherford (1911b), p.83.

'classical' concepts of energy, does not fit into the way 'modern' physics was described throughout the early twentieth century in Britain. In the following chapter, I shall consider a different definition. Instead of 1911 and the Solvay Congress, I examine 1913 and the annual meeting of the British Association for the Advancement of Science. My protagonist is not Max Planck, proponent of the new, but Oliver Lodge, defender of the old. And the definition arrived at is one which denotes 'classical' physics as continuous physics and 'modern' physics as discontinuous. As I shall show, the roots of these definitions came not from aspects of physical theories, but from the wider context of a 'spirit of revolution'.

Chapter Three: Continuity, discontinuity and 'revolution': approaches to authority in 'modern' physics

3.1 Introduction

In 1906, the French physicist Lucien Poincaré completed a book detailing the developments in physics over the preceding decade.[1] Translated into English the following year, *The New Physics and its Evolution* began with reference to a perception of physics as being in the midst of a revolution:

> 'The now numerous public which tries with some success to keep abreast of the movement in science, from seeing its mental habits every day upset, and from occasionally witnessing unexpected discoveries that produce a more lively sensation from their reaction on social life, is led to suppose that we live in a really exceptional epoch, scored by profound crises and illustrated by extraordinary discoveries, whose singularity surpasses everything known in the past. Thus we often hear it said that physics, in particular, has of late years undergone a veritable revolution; that all its principles have been made new, that all the edifices constructed by our fathers have been overthrown, and that on the field thus cleared has sprung up the most abundant harvest that has ever enriched the domain of science.'[2]

As indicated by the title of his book, Poincaré stressed that physics progressed through evolution rather than revolution, building on older ideas in order to advance. There was not discontinuity between the old and the new, as a revolution would imply, but instead the process was 'continuous'.

In this chapter I explore the notion of a 'revolution' in physics, concentrating on the years between 1911 and 1914, the advent of the First World War. I examine the rhetorics applied by scientists when discussing the nature of scientific progress. How did they negotiate the links between the 'new' physics and the old? And how did this contribute to emerging definitions of 'modern' physics? I answer this question by placing debates in physics firmly in the context of a broader shift in British culture, where 'modernist' writers and artists were also undergoing their own 'revolutions' and questioning the authority of long-held tenets and ideas. However, it was not only the intellectual elite who were tackling these questions: a combination of political upheaval and

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[1] For Poincaré, see Galison (2003).
[2] Poincaré (1907), p.1, italics my own.

technological advancement meant that ordinary people were dealing with a similar sense of revolution and rapid change. There was a common preoccupation among scientists, artists, writers and the wider 'public'. Debates about changes underway in physics thus had the potential to be of considerable relevance outside of scientific circles.

In physics, the debate about revolution centred on the existence of the ether and with it the notion of material continuity. Oliver Lodge, writing on the ether in 1909, argued that:

> 'Contact does not exist between the atoms of matter as we know them; it is doubtful if a piece of matter ever touches another piece, any more than a comet touches the sun when it appears to rebound from it; but the atoms are connected, as the comet and the sun are connected, by a continuous plenum without break or discontinuity of any kind. Matter acts on matter only through the ether.'[3]

Similarly, J. J. Thomson used his presidential address at the 1909 British Association to pronounce that discontinuous matter occupied 'but an insignificant fraction of the universe, it forms but minute islands in the great ocean of the ether, the substance with which the whole universe is filled.' Furthermore, the ether was 'not a fantastic creation of the speculative philosopher', but 'as essential to us as the air we breathe.' As a result he believed that the 'study of this all-pervading substance is perhaps the most fascinating and important duty of the physicist'.[4] The ether provided an underlying continuity to all matter, but it also dictated the purpose of the physicist: to study the true nature of reality.

This view was not taken by everyone. As we saw in the previous chapter, the Cavendish experimentalist Norman Campbell did not believe that physicists should study anything which could not be observed, and he was particularly opposed to the concept of the ether. Writing in 1909, he declared that '[t]he trend of modern theory is everywhere to replace by discontinuity the continuity which was the basis of the science of the last century'.[5] Discontinuity was thus tied into his definition of 'modern' physics. Furthermore, as we have seen, Campbell's rejection of the ether, and with it continuity, was a direct result of his philosophy of science, his notion of how the discipline of physics should be practiced. There was thus far more at stake here than simply a physical principle.

[3] Lodge (1909), p.110.
[4] J. J. Thomson, in *Report of the British Association for the Advancement of Science* (1909), p.15; Navarro (2005) has argued that Thomson had a deep metaphysical commitment to physical continuity.
[5] Campbell (1909), p.117.

In this chapter I will connect discussions about continuity and the ether, intensified following the development of quantum ideas, to related debates about materialism and vitalism. These discussions featured physicists, chemists, biologists and psychical researchers discussing the boundaries of disciplines of thought. They were arguing over what counted as science, and which phenomena scientific methods could explain. This, I argue, is what the parallel debate in physics was really about. Furthermore, this debate can also be found in the practice of 'modernist' writers and artists of the period, redefining the purpose of their work. By exploring this wider context, I reveal exactly what was at stake in the choice to retain or abandon the ether. Supporters of the ether were fighting for continuity of thought from the past to the present in order to maintain the existing definition of the discipline. The natural philosophers, such as Newton, had been searching to explain how the world worked; 'modern' physics appeared to be content with merely describing how it functioned. Physicists fighting for the ether, and continuity and tradition, were fighting to retain this philosophical approach to physics. They were not simply clinging to the ideas of the past, they were deeply committed to a particular notion of what physics was. In this sense, the ether came to almost define 'classical' physics in Britain.

3.2 Continuity and discontinuity in art and literature

In order to make these connections between the disparate worlds of art, literature and physics, and the everyday experiences of the British 'public', I take as my starting point the notion that science itself is a cultural activity.[6] Many studies have explored how scientific ideas might have influenced artists, but more recently historians have considered how the reverse might be the case as well. In particular, Morrisson has called for a symmetrical understanding of the relations between science and modernist art and literature.[7] Such an approach has been utilised to good effect by Miller, who suggests that both Picasso and Einstein had a common influence in the writings of Poincaré on time and simultaneity.[8] Peter Galison has explored in depth the wide-reaching influence of these new conceptions of time.[9] His study is one of both 'lofty abstraction and industrial concreteness', of 'a world where engineers, philosophers and physicists

[6] See the overview of SSK in my introduction.
[7] Morrisson (2002).
[8] Miller (2002).
[9] Galison (2003).

rubbed shoulders; where the mayor of New York City discoursed on the conventionality of time, where the Emperor of Brazil waited by the ocean's edge for the telegraphic arrival of European time; and where two of the century's leading scientists, Albert Einstein and Henri Poincaré, put simultaneity at the crossroads of physics, philosophy and technology'.[10] He thus connects the seemingly abstract interests of scientists, philosophers and artists to concrete industrial changes, linking the esoteric to the everyday. In this chapter, I take a similar approach, replacing Galison's simultaneity with the concepts of continuity and discontinuity.

These concepts were of fundamental importance in art, literature and physics during the early twentieth century. Everdell, focusing on the years surrounding 1913, argues that one can define modernism as a response to the collapse of 'ontological continuity', the idea that the world was essentially a continuous whole. We find this in the pointillism of Georges-Pierre Seurat's 1885 painting, *Sunday Afternoon on the Island of La Grande Jatte*, where a seemingly 'harmonious whole' is in fact separated into tiny dots of colour.[11] This concept of breaking down a continuous whole into its discrete parts was adopted by post-impressionists to produce the first cubist paintings. Picasso's 1907 *Les demoiselles d'Avignon* reduced the female form down to simplified shapes. From 1910 to 1912, he and Georges Braque took Cubism further towards abstraction, breaking objects into smaller parts until they were barely recognisable.[12] In 1910, Kandinsky created the first entirely non-figurative painting, later claiming to have been inspired by 'the further division of the atom'.[13] Here, we have explicit reference to the influence of atomic physics on 'atomic' painting. However, as I shall suggest throughout this chapter, there were also wider considerations of continuity and discontinuity that were coming to affect both artists and physicists.

Where painting explored ideas of discontinuous matter, the new medium of cinema allowed for new ways of thinking of time as discontinuous. As Kern has noted, this new technology 'portrayed a variety of temporal phenomena that played with the uniformity and the irreversibility of time'.[14] Many of the first film-makers exploited this novel opportunity to create visual representations of nonlinear narratives. Georges Melies, in 1896, discovered that by stopping and starting his camera, he could turn an omnibus into

[10] Galison (2003), p.14.
[11] Everdell (1997), pp.63-4.
[12] Macleod (1999) ; Henderson (2002).
[13] Everdell (1997), p.307.
[14] Kern (2003), p.29.

a hearse; Edwin S. Porter found that time could be chopped up even more dramatically by directly editing the film itself; and Louis Lumiére was the first to experiment with reversing time by running film backwards.[15] Similarly, novelists were exploring new ways of representing time textually, the most famous example being James Joyce's *Ulysses*, in which the twenty year journey of Odysseus was compressed into sixteen hours of Leopold Bloom's life, using interior monologues and comments by the author to expand the temporal range.[16] While *Ulysses* was first published (in serial form) in 1918, it was a continuation of earlier ideas; between 1909 and 1910, as Everdell notes in the title of his chapter on James Joyce in *Early Moderns*, the novel went 'to pieces'. Many writers were abandoning straightforward linear narratives in favour of more experimental approaches to the nature of time.[17]

Everdell's thesis of widespread discontinuity has been contested by Clarke and Henderson, who argue that the emphasis on discontinuity which he describes is actually far more characteristic of the later modern period, towards the end of the 1910s and the 1920s.[18] Henderson suggests that in the earlier modern period artists and writers were discussing not quantum theory or relativity, but the ether. She puts forward the example of the Italian painter and sculptor Umberto Boccioni, whose work was influenced by an interest in recent physical developments and a commitment to continuity.[19] Boccioni found continuity in the new discoveries of X-rays and the electron by conceiving of them as part of an ethereal physical world. However, as we have seen, others, such as Kandinsky turned to discontinuity. Furthermore, I am not concerned here with the role that scientific theory played in the works of particular artists, but rather the broader preoccupations which underlie the period. I argue for an approach that considers both continuity *and* discontinuity. Stephen Kern, taking his cue from Arthur Lovejoy, has proposed that one should not try and simplify the thinking of an age, but rather present it in terms of opposing sides and tensions.[20] Harrison, focusing his account of modernism on the year 1910, defines the period as being, at least in part, one of a battle between adversaries: order and chaos, vitality and death, ecstasy and despair, individuality and solidarity.[21] Perhaps we could add to this list: continuity and discontinuity.

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[15] Kern (2003), pp.29-30.
[16] Kern (2003), pp.30-31, 17-18
[17] Everdell (1997), p.283
[18] Clarke and Henderson (2002), p.97
[19] Henderson (2002).
[20] Kern (2003), pp.10-11; this is also an important tenet of the Sociology of Scientific Knowledge.
[21] Harrison (1996), p.13.

Thus early modernism does not need to be defined by either continuity or discontinuity; it can be identified instead in terms of a struggle between the two. I argue that this struggle is part of a broader concern about the relationship between the past and the present. Gay gave his study of modernism the subtitle 'The Lure of Heresy', arguing that one characteristic of modernist thought was the confrontation of convention, with artists and writers abandoning the practices of those who went before them.[22] However, this heresy is in fact more symptomatic of the avant-garde culture which emerged after the First World War.[23] Butler's study depicts the early modernists as innately conservative, breaking from their traditions only after serious exploration of the past.[24] This fragile relationship between the past and the present has been explored by a number of historians. In Miller's study of the common influences on Einstein and Picasso, he suggests that both cubism and relativity theory came out of a struggle to reconcile the old and the new.[25] Harris, although focusing on a slightly later period, has explored how a number of artists, in response to this widespread rejection of the past, tried to tie together modernism and English traditionalism.[26] Similarly, Gere, detailing the lengthy excavation of the ancient palace of Knossos in Crete, has discussed how the past was reinterpreted in light of the present, and how this went on to influence modernist writers and artists, including Picasso.[27] Artists and writers were thus facing the same questions as physicists, trying to determine the place that old authorities should hold in the future of their discipline. Here, continuity and discontinuity have wider meanings related to the nature of links with the past. Continuity represents a smooth transition between the old and the new, the past and the future; discontinuity suggests a revolution, a fragmentary break with the past and a dramatic shift in thought.

3.3 The broader context: revolution in politics and religion

A general feeling of revolution can be seen outside of the elite worlds of art and literature. An article in *The Times* in May 1912, titled 'Reviews and Magazines: Revolution or Reform?', declared that: 'Strikes, Socialism, Syndicalism, Federalism,

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[22] Gay (2008), pp.3-4.
[23] The effects of World War One on modernism are covered in Robert Wohl's essay review of histories of modernism, Wohl (2002).
[24] Butler (1994).
[25] Miller (2002).
[26] Harris (2010).
[27] Gere (2009).

Devolution, Disestablishment – pregnant signs of the times and their unrest – are the leading subjects of the reviews published in the merry month of May.'[28] In Britain, David Lloyd George's 'People's Budget' of 1909, which used taxes to redistribute wealth from the very rich to the very poor, had faced vehement opposition. The House of Lords vetoed it, and the Liberals used the subsequent election to fight for House of Lords reform. The result was a hung parliament, and the passing of the 1911 Parliament Act, removing the Lords' veto on financial bills.[29] For the British public, this comparatively minor political upset was accompanied by reports of revolution abroad. The Mexican Revolution began in 1910, Francisco I. Madero took power in 1911, and in early 1913 he was forced to resign before being assassinated. The Chinese revolution of 1911 saw the establishment of the Republic of China. Meanwhile, the Agadir crisis of 1911, in which Germany sent a gunboat to the Moroccan port, resulted in international tension and fear of war.[30]

For the British 'public', this political disruption and upheaval could be seen as part of a larger trend that characterised their culture and society in the years surrounding 1913: a move towards the 'modern'. As the rise of technology continued unabated, people's lives seemed to be changing dramatically at unprecedented speeds. This was accompanied by a preoccupation with the same issues of the place of the past which concerned writers and artists during this period. Rieger suggests that the word 'modern' captured a 'widespread conviction that the historical present was first and foremost an era of profound, irreversible, and man-made change'.[31] Many now viewed the present and future as disconnected from the past, and history became a 'lost domain'. Europe had entered, according to many commentators, a new historical era, known as 'modern times'.[32] Alongside this sense of a loss of history were numerous attempts to understand how these 'modern times' were related to the past: had there been a 'fundamental rupture between the present and the past', or was the present a result of 'continuous, incremental change'?[33] Technology was both progressive, as it made certain aspects of life easier or more efficient, and destructive, of tradition. If scientists wished to manage these 'public' experiences of scientific and technological change, they needed to ensure that 'modern' developments were seen to be compatible with earlier traditions.

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[28] 'Reviews and Magazines. Revolution Or Reform?', *The Times*, 1 May 1912, p.15.
[29] Dangerfield (1935).
[30] Gardner (1987).
[31] Rieger (2005), p.10.
[32] Rieger points to Harris (1994), p.36
[33] Rieger (2005), p.10.

The loss of tradition was particularly evident in the changing nature of people's relationship with religion. The early years of the twentieth century saw a considerable drop in church attendance in Britain. Partly in response to this, Anglicanism Modernism developed, with a more inclusive approach to Christianity. Here, reconciliation was attempted between science, philosophy and religion, in order to win back a diminishing congregation.[34] And while religious organisations were debating the place that scientific thought was to have in their new dogma, scientists were vehemently arguing for and against the intrusion of spiritualist ideas into their science. This debate spilled out into a more 'public' sphere, with the French philosopher Henri Bergson achieving fame in Britain. With his notions of continuity, Bergson was a part of the vitalism debates. As I shall shortly examine, questions about the role of past tradition continued to crop up in these discussions.

Furthermore, I explore just what was at stake in the abandonment of tradition, arguing that it was related to reconsiderations over the purpose, and limitations, of a discipline. If we return to the spheres of high culture, we find many modernist artists and writers redefining the function of their work. Fundamental to these considerations were the issues of the nature of reality and our access to truth. Peter Gay lists one characteristic of modernists as a 'commitment to a principled self-scrutiny'.[35] Artists and writers were no longer looking out into the world for their answers, but into themselves. In 1899, Arthur Symons published his essay on *The Symbolist Movement in Literature*, celebrating a 'revolt against exteriority, against rhetoric, against a materialistic tradition', in the search for the ultimate essence.[36] Similarly, Kandinsky believed abstract art was a route towards uncovering the reality behind surface appearances.[37] For some, the nature of truth had thus changed. For others, truth was no longer a consideration. An article on an exhibition of Futurist art in the *Cambridge Magazine* suggested that while Cubists tried not to represent things as they appeared, but rather as they were, the school of Futurists were abandoning '*vrai*' in favour of abstractness and subjectivity.[38] The art critic Roger Fry, who was responsible, in 1910, for staging the first post-impressionist art show in England, argued that subject matter was irrelevant, and focused only on aesthetic

[34] Bowler (2001).
[35] Gay (2008), pp.3-4.
[36] Quoted in Gay (2008), p.184.
[37] Macleod (1999), p.205.
[38] Severini (1913).

considerations.[39] In abandoning past traditions, these writers and artists were altering the scope of their work, constructing new definitions of art or literature. It is this redefining of a discipline which, I argue, was the underlying focus of the ether debates. Before turning to these, I explore concurrent discussions concerning materialism and vitalism, revealing parallels between these two debates. In both cases, negotiations were underway to characterise the nature of intellectual progress.

3.4 Materialism, vitalism and categories of knowledge

The topics of materialism and vitalism had been vigorously debated during the nineteenth century. This was against the backdrop of Darwinism and the promotion, by T. H. Huxley and his followers, of irreconcilable differences between science and religion. Huxley believed in scientific naturalism, a less extreme form of materialism in that it accepted that events in the mind were real. However, these mental events could exert no control over the natural world; the actions of nature and man were thus determined by material laws. This was, for many, a dangerous outlook: it disposed of free will and, crucially, the concept of a soul. A number of scientists responded by arguing for the compatibility of science and religion, and the nineteenth century ended with a push by many towards natural theology, a concept of evolution as divinely planned. In the twentieth century this position continued to be promoted by a small group of eminent scientists, no longer actively engaged in research but in possession of considerable 'public' influence.[40] In this respect there are parallels between the advocacy of natural theology and the defence of 'classical' physics. Indeed, Oliver Lodge was heavily involved in both of these ventures.

Debates surrounding vitalism were connected to disputes concerning spiritualism. As Oppenheim and others have shown, beliefs in psychical practices should not be dismissed as a fringe pursuit, but rather interpreted in the context of other Victorian intellectual beliefs.[41] Studies into the occult were not dismissed by all scientists. The Alchemical Society, founded in London in 1912, saw alchemists in conversation with mainstream chemists, discussing the spiritual implications of new developments in

[39] Macleod (1999), p.203.
[40] Bowler (2001), Chapter 4.
[41] Oppenheim (1988). A historiographical move towards this approach has been summarised by Noakes (2008b).

atomic science.[42] Similarly, the Society for Psychical Research, founded in 1882, boasted a number of prestigious physicists in senior positions, including J. J. Thomson, William Crookes, Lord Rayleigh and Oliver Lodge. Lodge was a liberal Christian, willing to reject aspects of Christianity in order to make it more compatible with science.[43] His own religious and scientific practices were brought together in his conception of the ether that, though not entirely immaterial, could be used in opposition to materialism.[44] For Lodge the ether connected the physical and psychical worlds.[45] Robert Bud has shown how vitalist convictions were closely related to perceptions of molecular biology between the First World War and the 1950s.[46] Similarly, here I explore how for physicists these ideas related to notions of continuity and discontinuity, themselves part of a discussion on the truth and the limitations of science, within the wider picture of the links between the past and the present, and the nature of 'progress'.

In the twentieth century, debates about vitalism and materialism were reignited as the ideas of the French philosopher Henri Bergson achieved wide recognition. Bergson had been known to philosophers for some time, and was a Professor at the College de France from 1900. In Britain, he found wider fame after the1911 translation into English of his book *Creative Evolution*.[47] Even before this time, he was already known, and between 1909 and 1911, more than 200 articles about Bergson appeared in English publications.[48] The aspect of Bergson's philosophy which found such broad appeal was his notion of time. Bergson argued that the time described by physicists was not real time, and instead there was such a thing as real duration, *durée réelle*, which could not be comprehended through the methods of science. While scientific time divided up the world, real time was continuous, a flowing process.[49]

In 1910, an American Professor of Philosophy, Joseph Leighton, declared that 'the whole logical crux of metaphysics centers in the problem of continuity and discreteness'.[50] Leighton was writing in *The Journal of Philosophy, Psychology and Scientific Methods*, and considering the views of Bergson. Here, Leighton argued that

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[42] Morrisson (2007).
[43] Bowler (2001), pp.98-99.
[44] Noakes (2005), pp.421-422.
[45] For Lodge's nineteenth century scientific research see Hunt (1991); For his psychical work see Wilson (1971), Root (1978) and Noakes (2005).
[46] Bud (2012).
[47] Bergson (1911).
[48] Gillies (1996), p.28
[49] Burwick and Douglass (1992); Gillies (1996).
[50] Leighton (1910), p.231.

the progress of science seemed 'to consist in the breaking up of the perceptual continua of immediate experience into discrete elements and events. Physics, chemistry, biology, and psychology pulverize the continua of space and motion, physical and vital processes, and consciousness, respectively.' However, if one looked for truth outside of mechanical laws of physics and chemistry, then one could again find continuity hidden amongst the discontinuity.[51] On this argument, the fundamentally discontinuous nature of physical ideas meant that physicists would never be able to uncover the true nature of reality, which was continuous. This interpretation of Bergson's work defined the limitations of physics, establishing it as merely a descriptive science, rather than a gateway to truth. By setting such limitations on physics, Bergson was also placing himself in the firing line of materialists, who thought all reality could be explained by mechanical principles.

These issues were discussed in the brief run of a new periodical, *Bedrock*. Founded in 1912, *Bedrock* promoted itself as the output of a rationalists' 'Facts Society' dedicated 'to bring together those who are desirous of testing in a thoroughly scientific manner the evidence, if any exists, for alleged facts which are accepted without verification by a large number of educated people'.[52] Its editors included the physician Sir Brian Donkin, the zoologist E. B. Poulton, and the astronomer H. H. Turner. Unsurprisingly, the magazine was sceptical about spiritualism, but it encouraged healthy debate, seeking articles from scientists and philosophers with opposing views. The journal ran for only eight issues (from April 1912 to April 1914) but during that time it featured heated discussions on the topics of vitalism and materialism, telepathy, and Henri Bergson's evolution. Described by Lodge as a 'hostile organ', *Bedrock* took an editorial stance very much in opposition to Bergson.[53] It featured a number of articles by the staunchly materialist Hugh S. R. Elliott, a scientific and philosophical writer, and author of *Modern Science and the Illusions of Professor Bergson*.[54] This highly critical book received a glowing review in *Bedrock*.[55]

We find in *Bedrock* arguments about whether 'modern' science was now materialist or vitalist. Elliott, in an early article on 'Modern Vitalism', declared it to be 'common knowledge that for some centuries past the sphere of mechanical interpretations has been

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[51] Leighton (1910), pp.232-3.
[52] Quoted in Brock (2008), p.451.
[53] Lodge (1913a), p.63.
[54] Hicks (1930); Elliott (1912a).
[55] 'Reviews' (1912).

increasing, while the sphere of spiritual interpretations has been decreasing.'[56] For Elliott, a move towards materialism was a fundamental aspect of the progress of modern science. However, from other perspectives, it would appear that science was progressing in an opposite direction. The notion of scientific progress was thus contested during this period, negotiated as a part of discussions concerning particular scientific viewpoints. An alternative to Elliott's view was conveyed by another sceptic of vitalism, Bryan Donkin, an editor of *Bedrock* and physician who would later criticise psycho-analysis for not being sufficiently 'scientific'.[57] In an article on 'Science and Spiritualism' he referred to an oft declared view 'in newspapers as well as from the pulpit and the platform that the "materialistic science" of the nineteenth century has receded before the "scientific philosophy" of such teachers as Professor Bergson in the twentieth'. However, Donkin countered that, for those 'who recognise no scientific revolution, nor any victory over the accepted methods of scientific research by any philosophies whatever', such attempts at reconciliation between science and spiritualism are regarded as 'mere logomachy'.[58] The myth of the death of materialism was also noted by William McDougall, a psychologist whose book *Body and Mind: A History and Defence of Animism*, in which he promoted a theory of evolution as driven by the mind, had been attacked by Elliott.[59] McDougall referred to a 'delusion widely prevalent at the present time; the delusion, namely, that science has now definitely emerged from and outgrown the materialistic phase which it had assumed in the second half of the nineteenth century'.[60] McDougall, an anti-materialist, suggested that this "phase" had not yet been defeated, but rather that the materialists had, for the most part, grown quiet. Framed in this way, the debate between vitalism and materialism was about the nature of modern science and how it was progressing. Had the development of science resulted in a materialistic or vitalistic point of view? The answer to this was significant as it determined what science was capable of. While materialism limited the scope of science to observations of the motions and properties of matter, vitalism looked for something more, an underlying explanation. The question of the purpose of science was thus being raised. And where physicists became involved in the debate they were questioning the purpose of physics specifically.

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[56] Elliott (1912b), p.312.
[57] East (1928).
[58] Donkin (1913): pp.501-2.
[59] Elliott (1912b).
[60] McDougall (1913), p.24.

As Bowler has noted, different areas of science came at the problems of materialism, vitalism and spiritualism 'with different expectations and prejudices'.[61] Consequently, as these various ideas converged into interdisciplinary debates, discussions arose as to the possibilities and limitations of the different disciplines, and accusations were directed at scientists seen to be intruding into areas where they had no expertise. Elliott argued that 'vital laws' were simply mechanical laws under a new name. He accused vitalist physicists of being unable to find such laws in their own discipline, and thus turning to biology to find them. [62] Here they were out of their expertise, and thus were afforded 'no greater importance to their remarks than to those of any educated individual with no special knowledge of the subject'.[63] A similar accusation was levelled by the Cambridge physiologist Ivor Ll. Tuckett, who contributed an article suggesting that those scientists who claimed to have found evidence for telepathy had been led astray by a 'will to believe'.[64] The main culprits, according to Tuckett, were Oliver Lodge and the recently knighted William F. Barrett, Professor of Experimental Physics at the Royal College of Science for Ireland, and a founder of the Society for Psychical Research. Tuckett quoted the zoologist Ray Lankester (who had written the introduction to Elliott's book on Bergson) as declaring, in a recent book, that no modern biologists believed in telepathy, it instead being the preserve of 'physicists who have strayed into biological fields'.[65]

Tuckett concluded by suggesting that Lodge and Barrett had accepted 'evidence obtained under conditions which they would recognise to be unsound if they had been trained in experimental psychology'.[66] And an identical accusation was directed at Tuckett, by a psychical researcher, J. Arthur Hill. Hill remarked how curious it was 'to find how apparently unscientific an educated man can be, even in our modern times, when he goes outside his own particular province'.[67]For while Tuckett may have read books on the subject of psychical research, he had no experimental experience, and thus was, according to Hill, ill equipped to comment. Hill declared that Tuckett's knowledge was subsequently not valid. As for his own work, Hill made sure to argue that psychical researchers had followed `the proper scientific method'.[68] By 'straying' into unknown

[61] Bowler (2001), p.161.
[62] Elliott (1912b), p.312.
[63] Elliott (1912b), p.318.
[64] Tuckett (1912).
[65] Tuckett (1912), p.181; Lankester (1912), pp.36-7.
[66] Tuckett (1912), p.204.
[67] Hill (1912).
[68] Hill (1912), p.350.

disciplinary territories, Barrett and Lodge had opened themselves up to accusations that they were ill-equipped and lacking expertise.

While Tuckett was accused of having no experimental experience, McDougall suggested that the materialists had too much. He accused Elliott, and other materialists, of sloppy reasoning, using statements such as ‘we are compelled to believe’, unaccompanied by ‘any train of reasoning from established premises’.[69] This inability to reason to a sufficient standard was a result, McDougall suggested, of the materialist’s disciplinary background. He argued that the `biological materialist is commonly a laboratory specialist', and subsequently had not developed a truly scientific or philosophical attitude.[70] Here, we find parallels with the new approach to experimental physics, promoted by Rutherford and exemplified by Campbell, where the results of the laboratory take precedent over theory. And, as we saw with Campbell, this kind of attitude could lead to the rejection of long-held theories, if they could not be experimentally verified.

Questions as to the place of old theories and tradition in science arose in the vitalism debates. Lodge accused Tuckett of being involved in a ‘crusade against truth’.[71] For Lodge, Elliott’s materialistic approach was too limiting in its approach to truth. Lodge ended his contribution to the debate with a dictum of Huxley, on which, he declared, the Society for Psychical Research had been founded:

> `The development of exact natural knowledge in all its vast range, from physics to history and criticism, is the consequence of the working out, in this province, of the resolution to “take nothing for truth without clear knowledge that it is such;” to consider all beliefs open to criticism; to regard the value of authority as neither greater nor less than as much as it can prove itself to be worth'.[72]

Lodge’s definition of scientific progress, as detailed here, required the willingness to reject old ideas, and question authority. As I shall show, when discussing the quantum and relativity theories his concept of progress was drastically different.

Lodge’s anti-authoritative approach to progress was reinforced in an attack on the materialism of Ray Lankester, who had been drawn into the debate after being quoted by

[69] McDougall (1913), pp.35-6.
[70] McDougall (1913), p.41.
[71] Lodge (1912b).
[72] Quoted in Lodge (1912b), p.341.

Tuckett. Lodge suggested that the likes of Lankester had surrounded themselves with a definite boundary, while Lodge and others 'after some exploration inside, have ventured outside the walls.'[73] Lankester's use of boundaries had the effect, Lodge argued, of obstructing progress:

> 'unless we strangely limit the possibilities of progress before the human race in the aeons of the future, surely the most advanced and modern man of science must admit, in a lucid interval, that posterity will regard him as one of the Ancients; as one too, perhaps, who is pathetically struggling amid a welter of ignorance to hold fast to his traditions, to secure himself in his fertile little oasis of materialistic knowledge, to defend it against the hosts of barbarism, and to resist the unwelcome incursion of even friendly messengers from alien and distant lands.'[74]

While the spiritualists were 'accused of lying, of megalomania, of folly, and of madness', they remained secure 'in the progress of the human race' and thus knew they would one day be proved correct. Lankester and his ilk were to be remembered as a 'curious obstruction which pioneers in this domain still have to encounter.'[75] Lodge's argument was thus that by adhering too strictly to past tenets, scientists on the side of materialism were obstructing the progress of science and the search for truth.

There were many tenets of science that Lodge would not discard, most crucially the existence of an ether, and the continuous nature of matter and energy. However, when discussing matters of spiritualism, he took the opposite approach. This was also the case in his exchanges about the interpretation of radioactivity. As we saw in the last chapter, Henry Armstrong had very strong beliefs about the employment of physical ideas and methods to study phenomena which he felt belonged to chemistry. In 1908, writing to Armstrong to discuss the chemist's disbelief in atomic charges, Lodge suggested that Armstrong was 'trying to be <u>too</u> conservative; though some conservativism on the part of a chemical leader is useful and desirable.'[76] He repeated this accusation in 1913, following the appearance of a review Armstrong had written of the third edition of Frederick Soddy's *Interpretation of Radium*.[77] This review was published in *Science Progress*, a specialist journal intended for practising scientists (seeking a general overview of developments outside of their discipline) and those in related professions

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[73] Lodge (1913a), p.63.
[74] Lodge (1913a), p.64.
[75] Lodge (1913a), pp.64.
[76] Lodge to Armstrong, 21 September 1908, ARM (underlined in original)
[77] Soddy (1912); H[enry] E[dward] A[rmstrong] (1913).

(such as schoolteachers) or with a serious interest in science.[78] Armstrong himself was an editor from 1909 to 1913. His review was extremely complimentary of both Soddy and the book, but he took some issue with the vague use by physicists of the term atom, arguing that if one was to use its original precise definition then radium, a material that breaks up into different materials, could not be defined as such. He reiterated an earlier argument of his that the inert gases, such as helium, were also not elements. He then moved on to the issue of conservatism, quoting Soddy's declaration that 'Natural conservatism and dislike of innovation appear in the ranks of science more strongly than most people are aware. Indeed science is no exception.'[79] Arguing that dislike of innovation was certainly not a problem in contemporary science (even if it may have seemed that way in 1908, when the lectures forming Soddy's book had been first delivered), Armstrong defended conservatism, suggesting that people are, by nature, conservative, as they must be in order to provide society with any sense of stability. Conservatism was, he argued, even *more* important for scientists, the first duty of whom was 'to be critical and to deny belief until satisfactory proof be given that he is justified in believing.'[80] In contrast to the views expressed by Lodge in response to Lankester, Armstrong argued that critical conservatism was the driving force of scientific progress.

With this in mind, it is unsurprising that Lodge found issue with Armstrong's review, writing a short notice in *Nature* to assert that Armstrong was wrong in refusing to accept the monatomic nature of the inert gases, and declaring his intent to 'criticise his attitude, in a friendly way, in the October number of the same journal'.[81] Lodge fulfilled his promise, with the rebuttal appearing in *Science Progress* in October, although Armstrong later wrote that he had difficulty in recognising anything 'friendly' about the response.[82] Here, Lodge confessed a 'good deal of sympathy' with conservatism, but maintained that such sympathy had a limit, and this limit was 'transgressed when facts are ignored and hypotheses wildly manufactured in order to retain some old and superseded exclusive and negative generalisation.'[83] However, as I shall show, when discussing the matter of continuity and discontinuity in physics Lodge characterised himself as a conservative. The difference was that while the conservatism of Lankester and Armstrong limited the scope of physics, Lodge's deep commitment to the ether

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[78] Bowler (2009), pp.167-7.
[79] Quoted in H[enry] E[dward] A[rmstrong] (1913), p.650.
[80] H[enry] E[dward] A[rmstrong] (1913), p.650.
[81] Lodge (1913b).
[82] Lodge (1913c); Armstrong (1914).
[83] Lodge (1913c), p.197.

instead extended it. He could thus be depicted as a conservative traditionalist about the ether, and a liberal progressive about spiritualism and the interpretation of radioactivity, but in both cases the motivation was the same: for physics to be used as a method to uncover truth, rather than simply describe what was on the surface. And this motivation reveals an important aspect about the continuity and discontinuity debates, that they were about the nature of the discipline itself. ‘Modern’ physics was not only discontinuous, it was severely limited in scope, and this was why a break from tradition was such a threat to many physicists. This was a break from the notion of physics as an ideal means to revealing the true nature of reality. ‘Modern’ physicists saw the future of their discipline as serving a very different purpose from that of its past. For the defendants of tradition, however, physics could only progress by maintaining its quest for truth.

3.5 Debating continuity and defining ‘modern’ physics

Concerns about continuity and discontinuity had originally stemmed from the discrete nature of matter. However, in the years leading up to 1913, the problem began to apply to energy as well. This had been proposed by Einstein’s 1905 work in which he suggested that light itself was quantum, and thus discontinuous. Studies on X-rays introduced a new problem into the question of the nature of light, as the rays were revealed to exhibit both corpuscular and undulatory properties. By 1912, William Bragg was also applying this concept of duality to visible light.[84] Discontinuous energy was incorporated into a theory of atomic structure by Niels Bohr, after spending some time with Rutherford in Manchester from 1912. Bohr proposed that a small positively charged nucleus was orbited by electrons that could jump from a higher energy orbit to a lower one, emitting a quantum of discrete energy as it did so. The idea of quantum radiation had now been applied to a theory of atomic structure, combining discontinuous matter and discontinuous energy.

Campbell praised Bohr’s theory in a 1914 *Nature* article on the structure of the atom. Crucially for Campbell, Bohr’s theory was in accordance with experimental results, explaining ‘more than any rival theory’, although it still contained assumptions which might have to be abandoned. Campbell would not make leaps of conceptual faith, but accepted those aspects of the theory which had been, in his mind, experimentally

[84] Wheaton (1983), p.208.

verified. He also made it clear that the theory rejected 'the principles of mechanics, which the most conservative are being slowly driven to abandon', replacing them with new 'fundamental propositions'. For Campbell, Bohr's theory represented a sharp divide between two groups of physicists:

> 'There are only two alternatives open to the modern theoretical physicist: he may either suppose that the principles of the older mechanics are true, and that all the brilliant results which have followed from the application of the conceptions of Planck and Einstein to the most diverse phenomena are illusory and devoid of evidential value; or he may suppose that they are not true. Bohr's theory offers him the choice in its most striking form.'[85]

Campbell had also recently released a second version of his *Modern Electrical Theory*, which again argued against the need for an ether in physical theories.[86] This was reviewed in *Nature* by Frederick Soddy, who noted:

> 'Physical theories at the present moment are so shaky at the foundations that the doubt arises sometimes whether the superstructure is not being built up too rapidly. The difficulties, now ten years old, in reconciling the undulatory and corpuscular types of radiation in one theory, the hopeless confusion that prevails as to the necessity for the existence of an ether, and the modern discrete or quantum theory of energy, seem to call for a more drastic reconsideration than we find here of many of the simplest physical conceptions and their experimental basis.'[87]

For Soddy, Campbell's rejection of many 'classical' physical principles was still too conservative. As we saw in the previous chapter, Soddy was an early researcher into radioactivity, collaborating with Rutherford at the turn of the century. As a chemist, he held no deep commitment to physical theories and was instead concerned with analysis of chemical properties and changes. He was also not one to bow down in the face of tradition more generally.[88] As Hughes has noted, in the 1920s and 1930s he campaigned for economic, social and institutional reform.[89] There was perhaps here a connection between his attitude towards rejection of long held theories in physics, and rejection of tradition more generally. For Soddy, where a system was no longer working, be it economical, social or physical, the answer often lay in dramatic reform. Progress could not necessarily be achieved through small measures but rather required profound conceptual shifts.

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[85] Campbell (1914).
[86] Campbell (1913)
[87] Soddy (1913), p.339.
[88] For Soddy in general, see Merricks (1996) and Kauffman (ed.) (1986)
[89] Hughes (2010).

Meanwhile, the problem of ‘reconciling the undulatory and corpuscular types of radiation’ was being explored by William Bragg, who discussed this issue in relation to the place of continuity and an older physical tradition. At the 1912 British Association meeting, he described the quantum theory of light as ‘one of the most remarkable developments in modern physics’.[90] However, he felt that this ‘modern’ point of view did not have to replace older ideas:

> ‘We ought not to think that in doing so we abandon the wave theory or its electro-magnetic development. Rather we might say that the radiation problem is too great to be seen all at once from any point to which we have hitherto attained, and that it is to our advantage to look at it from very side.’[91]

He argued that physicists should not be rejecting ‘the work of the past’ but rather ‘enriching’ it.[92] Here, continuity and discontinuity were directly related to the old and the ‘modern’, respectively. Furthermore, Bragg’s dualism was a call for an approach that supplemented tradition with new ideas, rather than completely overthrowing it. Physicists of the ‘Rutherford school’ were in agreement with Bragg, although they didn’t express their views in terms of tradition and revolution. Moseley and Darwin, who were using Bragg’s X-rays techniques in Manchester, agreed that the X-rays were displaying ‘contrary properties’.[93] Rutherford, who was thanked in Moseley and Darwin’s paper on the subject of X-rays, gave Bragg’s 1912 book *Studies in Radioactivity* a highly positive review in *Nature*.[94] Here, Rutherford considered the ‘apparently conflicting but fundamental properties of the X-ray’, noting that they ‘must be reconciled in any satisfactory theory of the X-rays’.[95] Wave-particle duality was here seen as a useful *temporary* theory, but would ultimately be replaced by something more self-consistent. Physicists would thus enable the discipline to progress by adopting new approaches where they were of experimental use, and worrying about an overarching satisfying theory later. This view was not exclusive to problems in quantum theory, and Eddington took the same approach in his work on astrophysics.[96] For such physicists as Eddington, Rutherford and Bragg, ‘modern’ physics advanced through the pragmatic adoption of useful theories.

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[90] Bragg (1913), p.558.
[91] Bragg (1913), p.558
[92] Bragg (1913), p.560.
[93] Moseley and Darwin (1913a, 1913b).
[94] Bragg (1912); E[rnest] R[utherford] (1912).
[95] E[rnest] R[utherford] (1912), p.695.
[96] Stanley (2007b).

The 1913 British Association meeting, held in Birmingham, featured two lengthy discussions on the topics of continuity and discontinuity. Oliver Lodge's presidential address was an hour and a half long criticism of the rise of discontinuous ideas. It was followed, two days later, by a three hour long discussion on the nature of radiation, in which the topics of tradition, and both physical and psychical continuity, were vigorously debated. Lodge was well known, and his speech, as I shall show, was widely anticipated and reported, primarily on the basis of his reputation as a public communicator of spiritualist ideas. The radiation discussion was also of interest to the 'public', as evident by the fact that apparently at least half of the audience was female. It was described in *The Times* as 'the most important feature' of that day.[97] Here, existing professional debates among scientists were given a wider platform. The Birmingham meeting was thus an excellent opportunity for definitions of 'modern' physics to be explored and established. As president of that year's meeting, a well known public figure, and a local of Birmingham, Oliver Lodge in particular had the advantage of setting the tone. Before the radiation discussion had even started, it was thus already framed in the context of continuity and discontinuity, the topics of Lodge's talk of two days before.

3.6 Continuity and discontinuity at the 1913 British Association meeting: Oliver Lodge and life after death

Lodge's address was an attack on what he considered to be 'modern tendencies' in science. He criticised a move away from continuity and towards discontinuity, the 'irresistible impulse to atomise everything'.[98] He saw this as not only a problem in physics, with subatomic particles and quantum radiation, but also in biology with the emergence of Mendelian heredity. Furthermore, the ether, which was able to provide an underlying continuity amidst this discontinuity, was being threatened by two other tendencies: 'negative generalisations' and 'vagueness'. Adopting the former, physicists were (according to Lodge) denying that certain kinds of knowledge could ever be attained. They were limiting what counted as physics, on the basis of their ability to

[97] The introduction to the discussion was written by *Times* reporter Ernest Brain. The discussion itself was reported on by E. E. Fournier D.'albe, whose interest in these topics I shall examine later in the chapter. 'British Association. Problems Of Radiation., Modern Universities And The State., Electric Heating And Cooking', *The Times*, 13 September 1913, p.10.
[98] Lodge in *Report of the British Association for the Advancement of Science* (1913), p.18. He was quoting Larmor's preface to Poincaré (1905).

experimentally observe or measure a phenomenon. The ether, for which there was no concrete experimental evidence, was thus coming to be dismissed as outside of the scope of physics. Related to this was Lodge's attack on what he termed 'vagueness', a concept closely related to Heilbron's category of descriptionism. Lodge argued that the 'modern sceptical attitude' looked not for explanation but merely a useful description of the natural world. He illustrated his point with arguments that had been put forward by Arthur Schuster (now retired) in a series of lectures delivered to the University of Calcutta.[99] Again, the 'modern' approach threatened the concept of the ether, which was not required for the newer atomic and quantum theories to function. Finally, as Lodge ended his address with a declaration of his belief in the abilities of science to observe the supernatural, he accused modern science of denying the existence of anything which could not be readily sensed or measured.[100] This accusation was in support of the ether as a physical principle, but also its use in his psychical work. Overall, Lodge urged for a 'conservative attitude'.[101] Where he had previously been opposed to conservatism, here Lodge saw it as a necessary approach, in order to save not just a physical principle but the nature of the discipline. A conservative attitude, with its commitment to continuity, was needed to retain physics' power to explain. 'Modern' physics was presented here as discontinuous, revolutionary, and limited in scope.

Much of the reception of Lodge's talk was focused on the religious aspects of his attack on discontinuity. He admitted his belief in the continuity of life after death, a continuity which, he believed, was made possible through the ether, which connected the physical and psychical worlds. This side of Lodge's address had been anticipated, and promoted, long before the meeting. In July, an article in *The Times* described Lodge as in opposition to 'conservative men of science', with many of his views being 'decidedly heterodox'.[102] A later article noted that although Lodge was 'widely known as an eminent physicist, his name is no less familiar as that of a convinced and avowed believer in the value of psychical research'.[103] The author of this piece suggested that the public's curiosity towards the address had been 'piqued' by Lodge's renown as a supporter of psychic matters, and the 'sceptical attitude', in this respect, of many of his

[99] Published as Schuster (1911).
[100] His main arguments are summarised in *Report of the British Association for the Advancement of Science* (1913), p.42.
[101] *Report of the British Association for the Advancement of Science* (1913), p.43.
[102] 'The British Association', *The Times*, 21 July 1913, p.4.
[103] 'British Association/ Sir Oliver Lodge's Address/ Continuity And Evolution', *The Times*, 11 September 1913, p.6.

peers. Following the announcement of the title of Lodge's talk, a number of journalists asked Lodge whether he was planning on speaking about the continuity of human existence, the immortality of the human soul.[104] And in August the *Daily Mail* declared that there would be a 'Mysterious Message from Sir Oliver Lodge' on the subject of 'Life after Death'; the 'world must wait on tenterhooks for a whole month to discover the meaning'.[105] Following the speech itself, the *Birmingham Daily Mail* described a rustle spreading through the audience as, after an hour of discussing physical matters, Lodge finally turned to the psychical; this was 'what they were expecting to hear about'.[106] The *Daily Graphic* reported this aspect of the speech as 'that in which the larger audience of the world outside must take a very much greater interest'.[107]

The public reception of Lodge's attack on discontinuity was framed in the context of his psychical beliefs and the debates surrounding vitalism and materialism. More broadly, it was seen as evidence of renewed relations between science and religion. This was unsurprisingly the approach taken in the religious press, with the *Church Times* declaring that materialism was dead, and Darwin had 'gone by the board'.[108] The atheist *Freethinker* was quick to attack such a strong statement, declaring that if Darwinism and materialism had been 'utterly discredited' it was only by 'the clergy and about half-a-dozen scientists who happen to be also Spiritualists.'[109] Much to the *Freethinker's* anger, it was not merely the explicitly religious press which was adopting Lodge in such a way. Chapman Cohen, the assistant editor and a prominent writer on secularism and atheism, complained that Lodge's address was receiving much more attention than a British Association president would usually expect, and that this was purely due to his role as an 'apologist for religion'.[110] With reference to the predictions being published before the talk, Cohen suggested that one paper had gone so far as to indicate that Lodge would produce 'proofs of a future life', and he noted the admission in *The Times* that the 'sensational disclosures which have been expected in some quarters were not forthcoming'.[111] Cohen was aware of the discrepancy between the talk itself and how it was reported, arguing that the majority of the address consisted of an excellent report on

[104] Keller (1983), p.101.
[105] Quoted in 'Current Cant', *New Age*, 14 August 1913, p.446.
[106] 'Continuity', *Birmingham Daily Mail*, 11 September 1913, p.4.
[107] 'The Mystery of After-Life', *Daily Graphic*, 11 September 1913, p.5.
[108] Quoted in 'Acid Drops', *Freethinker*, 5 October 1913, p.631.
[109] 'Acid Drops', *Freethinker*, 5 October 1913, p.631.
[110] C. Cohen, 'The Religion of Sir Oliver Lodge', *Freethinker*, 21 September 1913, pp.594-5.
[111] 'The New Agnosticism', *The Times*, 11 September 1913, p.7. This article was written by Peter Chalmers Mitchell.

science, with religion only entering at the very end. And yet, as he noted, the *Daily Mail* quoted one 'scientific listener' as describing Lodge's conclusion as a 'rhapsody on faith'.[112] In a later article, Cohen accused the *Church Times,* the *Manchester Guardian*, and the *Daily News* of forming a 'trinity' and declaring in their pages that science had dramatically changed its stance towards religion.[113] As Cohen was keen to point out, not only had there been no change in science, there had been no change in Oliver Lodge, who had been 'for years trying to harmonise science with religion'.

Such 'public' discussions of Lodge's talk, which was in parts a 'popular' exposition of 'modern' physics, were driven by discussions about the existence of ghosts and the compatibility of science and religion. Here, Lodge's 'conservative' physics was depicted as an alternative to materialist 'modern' physics, a new approach which reconciled science with religion. With the 'conservative' view being set up in opposition to 'modern tendencies', we can consider it as an attempt to define 'classical' physics. Here, this was not portrayed as old-fashioned or stagnant, but moving forward, simply in a different direction to 'modern' physics. 'Classical' and 'modern' physics thus presented two different approaches to progress, one which saw science becoming more inclusive and compatible with non-scientific pursuits, and one which severely limited science's engagement with religion and philosophy.

3.7 Debating 'modern' physics at the 1913 British Association

The radiation discussion, two days later, also fostered debates about the ether and the place of tradition in 'modern' physics, but here the focus was very different. Lodge was in attendance, and invited the committee of Section A to continue the debate at his house the following Sunday, but had little to contribute on the day.[114] The discussion was led by James Jeans, who had only recently become an advocate of the corpuscular nature of radiation.[115] Jeans was a Cambridge-trained mathematician, graduating as second wrangler in 1897 and educated in the importance of Maxwellian electrodynamics.[116] He was also, as Stanley has shown, committed to a scientific method that began with certain

[112] 'Sir O. Lodge's Religion. / "Personality persists beyond death" / "A New continent"', *Daily Mail,* 11 September 1913, p.3
[113] C. Cohen, 'The Glorification of Sir Oliver Lodge', *Freethinker*, 5 October 1913, p.625
[114] 'British Association. Problems Of Radiation., Modern Universities And The State., Electric Heating And Cooking', *The Times*, 13 September 1913, p.10.
[115] Kragh (2011), p.6; For Jeans, see Milne (1952).
[116] Warwick (2003), pp.464-5.

premises and from them deduced valid knowledge.[117] Unlike Eddington, Rutherford and Bragg, Jeans would not pragmatically adopt a theory simply on the basis of its current usefulness in explaining experimental results. His initial response to quantum theory had been an attempt to reconcile Planck's formula with electrodynamics. Jeans was in attendance at the 1911 Solvay Congress, where he expressed doubts about the quantum theory. However, the following year saw his 'conversion', possibly as a result of Henri Poincaré's adoption of the quantum theory.[118] Since Jeans' philosophy of science prevented him from accepting a theory unless he deemed it to be 'certain' and rigorously deduced, we can consider him as holding, after 1912, a significant commitment to the quantum theory. With this recent 'conversion' behind him, Jeans was well-equipped to lead the discussion on radiation at the 1913 British Association meeting.

Jeans began by declaring that Newtonian 'classical' mechanics could not explain all that was now known about radiation. As Campbell would do, he framed the debate as a battle between two conflicting ideas, in this case continuous and discrete radiation. Jeans was firmly on the side of discontinuity. Furthermore, he related this 'modern' discontinuity to the philosophy of descriptivism:

> 'The boldest and simplest attempt at reconciliation between the conflicting theories lies in abandoning the ether altogether, and relying on some purely descriptive principle, such as that of relativity. There is probably no adequate reason why the ultimate interpretation of the universe should be expected to be dynamical rather than kinetic and descriptive.'[119]

Jeans, in accord with his Cambridge training, still desired a dynamical explanation, one which explained rather than described, and made a suggestion as to what the meaning of Planck's constant might be. His approach was thus not as drastic as some supporters of tradition might have feared.

Other British physicists in the room took an opposing view. Augustus Love, another wrangler and former colleague of Larmor's, refused to accept that 'existing theories of dynamics and electrodynamics need to be supplemented by the theory of the quanta', instead proposing that recent results could be interpreted within 'ordinary theories'.[120]

[117] Stanley (2007b).
[118] Jeans' 'conversion' to quantum theory has been explored by Gorham (1991); See also Hudson (1989); Poincaré's own conversion was established in Poincaré (1912).
[119] *Report of the British Association for the Advancement of Science* (1913), p.380.
[120] *Report of the British Association for the Advancement of Science* (1913), pp.383-4; For Love, see his obituary, Milne (1941).

Joseph Larmor, commenting on the new work relating to specific heats at very low temperatures, suggested that 'there is nothing in it that is destructive to the principles of physics which have led to so rich a harvest of discovery and synthesis in the past.'[121] He looked for reconciliation between the old and new ideas, continuing with a search for interactions between the ether and electrons. Furthermore, he used the rhetoric of destruction, clearly placing the new ideas in the context of a dramatic revolution. The issue at stake was thus the abandonment of old ideas, of traditional tenets of physics.

Issues raised at the radiation discussion were propagated in a 'professional' medium, in Jeans' *Report on Radiation and the Quantum-Theory*, which he wrote for the Physical Society in 1914.[122] For a more public audience, the debate was reported in *The Times* and *Nature* by the electrical engineer, Edmund Edward Fournier d'Albe. Jeans' *Report* had a considerable impact on British physics, playing a significant role in the acceptance of quantum theory by his peers.[123] Here he unequivocally described the quantum theory as a 'complete departure from the old Newtonian system of mechanics'.[124] He discussed the British Association debate in a section on 'Attempts to Reconcile Radiation Phenomena with the Classical Mechanics', detailing Love's and Larmor's arguments and noting that the discussion at Birmingham had made it 'abundantly clear that the quantum-theory is far from being regarded as inevitable yet by many of the English school of physicists'.[125] Of course, this very book would subsequently persuade many of this 'English school' to alter their views on the theory. Jeans presented Love's and Larmor's arguments as valid attempts at reconciliation, but they did not play a role in his own exposition of the theory. He also ended his report with a brief consideration of the properties of an ether compatible with quantum theory.[126] Ultimately, he presented the shift in thought, from Newtonian to quantum, as being one of continuity and discontinuity: 'The keynote of the old mechanics was continuity . . . The keynote of the new mechanics is discontinuity'.[127] The idea of a relationship between modern physics and discontinuity, so very much at the forefront of quantum discussions at the 1913 British Association, had now been described in a report which would go on to influence the adoption of quantum theory by many British physicists. The concepts of continuity

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[121] *Report of the British Association for the Advancement of Science* (1913), p.386.
[122] Jeans (1914).
[123] Gorham (1991), pp.473-4.
[124] Jeans (1914), p.1.
[125] Jeans (1914), p.23.
[126] Jeans (1914), pp.88-9.
[127] Jeans (1914), p.89.

and discontinuity, as well as the abandonment of Newtonian mechanics, were being embedded in professional discourse on quantum theory and 'modern' physics.

The rejection of Newton's tradition was also a primary theme in the 'public' account of the meeting, as reported in two articles in *The Times* written by d'Albe, then an assistant lecturer in physics at Birmingham.[128] D'Albe was a member of the Society for Psychical Research and had fairly radical views on what the 'new' physics could teach about the nature of the soul. He believed that the discrete nature of matter, as revealed by the electron, provided evidence for continuity of life after death.[129] D'Albe was also deeply committed to the ether, and had opposed relativity theory on the basis of a perceived threat to it. He thus had much in common, intellectually and institutionally, with Oliver Lodge. He did not, however, share Lodge's opposition to quantum theory, hoping instead that 'the investigation of this fascinating problem will teach us a great deal about the interstellar aether which conveys the messages'.[130]

While D'Albe's first account of the report was, in the style of a conventional *Times* British Association report, a straightforward account of contributions, he also wrote a more sensationalist editorial. Here he described the debate as a 'pitched battle between the adherents of the doctrines of Young and Fresnel, Maxwell and Hertz on the one hand, and the revolutionary followers of Planck, Einstein, and Nernst on the other.'[131] Ultimately, it was an 'old controversy' between continuity and discontinuity. While the battle was still 'raging', d'Albe noted that opinion seemed to be in favour of the quantum theory. Detailing the views of the opposition, he described Love as fighting 'with conviction for the older and more conservative view', while Larmor was 'somewhat pathetically seeking the way of salvation through the falling *debris* of cherished views'.[132] D'Albe was portraying the entire debate as one of conservatism versus revolution, over whether physicists should retain the views of the past or adopt the dramatic new ones. D'Albe did not appear too threatened by the choice being made,

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[128] 'Dr. Fournier D'albe. Reading By Sound', *The Times*, 8 July 1933, p.14; The articles in *The Times* are 'British Association. Improvement Of British Canals., Incubation Of Eggs In Egypt', *The Times*, 13 September 1913, p.6; 'British Association. Problems Of Radiation., Modern Universities And The State., Electric Heating And Cooking', *The Times*, 13 September 1913, p.10. In both cases, d'Albe wrote only the paragraphs relating to the radiation discussion (information from News International archivist)
[129] Noakes (2008a), p.328.
[130] D'Albe (1914).
[131] 'British Association. Improvement Of British Canals., Incubation Of Eggs In Egypt', *The Times*, 13 September 1913, p.6.
[132] 'British Association. Improvement Of British Canals., Incubation Of Eggs In Egypt', *The Times*, 13 September 1913, p.6.

and I suggest that the reason for this is that, fundamentally, he did not see a real change occurring. For D'Albe, quantum theory was a means to uncovering new information about the ether. In neither of his *Times* articles did he report Jeans' comment about abandoning a dynamical approach. As such, for him the discipline was not actually under threat of true revolution.

D'Albe's understanding of quantum theory as a tool for further research into the nature of the ether explains why he differed from Lodge here in his views on conservatism. D'Albe was an electrical engineer, and Lodge had spent much of his career considering this subject from a physical point of view. They were both members of the Society for Psychical Research, and they both held a strong commitment to the ether. However, their approach to the concept of discontinuity differed dramatically. This was because D'Albe did not think discontinuity threatened the stability of the discipline of physics. Lodge, on the other hand, saw discontinuity as representative of a shift in the way physics was practiced, towards descriptionism. Both Lodge and D'Albe's decision over whether to adopt this 'modern' physical principle was thus contingent on how 'revolutionary' they perceived it to be. I end this chapter by considering the views of a young physicist who *did* hold the same views as Lodge regarding discontinuity, but differed in many other respects. This case indicates the importance of the underlying issue of disciplinary revolution in interpretations of quantum physics.

Samuel Bruce McLaren had studied at Trinity College, Cambridge and graduated as third wrangler in 1899. From 1906 to 1913 he was a lecturer in mathematics at Birmingham University, the same institution that housed both Lodge and D'Albe. McLaren, however, was a very different kind of physicist. He was young, had developed a friendship with Bohr during his visit to England, and fully grasped the mathematics of the new physics.[133] And yet, as Keller notes, McLaren's 'emotional response' to quantum energy was similar to Lodge's. Writing in the *Philosophical Magazine* (of which Lodge was an editor), McLaren accused 'Einstein's idea of the Quantum' of being 'destructive of the continuous medium and all that was built upon it in the nineteenth century'.[134] McLaren's desire to retain the continuous medium of the ether was more than simply a commitment to a physical principle. He began his piece by declaring that 'the unrest of our time has invaded even the world of Physics, where scarcely one of the

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[133] Keller (1983), pp.102-3.
[134] McLaren (1913); McLaren is discussed in Keller (1983), pp.100-102.

principles long accepted as fundamental passes unchallenged by all'. The problem was not simply the discontinuity of energy, but rather the discontinuity of progress, of physics proceeding not by gradually building upon the work of those who had gone before, but by tossing old theories aside and replacing them with wildly different ones. And this predicament was not exclusive to physics. Indeed, quite the opposite: McLaren believed that a more general 'unrest of our time' had infected physics, and referred to a 'spirit of revolution'.[135] He was relating the developments in physics to a broader cultural and social shift, tying together 'modern' physics with modernity in general.

Throughout the discussions on quantum theory at the 1913 British Association meeting, the focus was on revolution. For many British physicists, the extent of this revolution was fundamental in their reception of the theory, as were their own particular views on the nature of scientific change. Jeans, despite his Cambridge training in 'classical' electrodynamics, had no qualms about shifting his allegiances, once it became clear to him that quantum theory was both rigorously deduced from first principles, and in accordance with experimental observations which the older mechanics could not explain. While Jeans was content to discard Newton's laws, Lodge, Larmor and Love were firmly against such a revolution, strongly believing that the new should be reconciled with the old. D'Albe, meanwhile, described the situation as 'revolutionary', but did not perceive a threat to the ether, and thus this 'revolution' was not sufficiently destructive to older traditions to dissuade him from praising the quantum theory. As revealed by McLaren, here revolution was not simply about Newton, but also about a larger question of the place of old theories, and idols, in a changing discipline. In this way, physical continuity and the continuity of physics were closely linked. The ether was crucial for the former, connecting together the discrete atoms which made up the material world, but it also saved the latter by creating a continuous line between Newton and 'modern' physics.

3.8 Conclusion

McLaren's 'spirit of revolution' reveals the broader context in which the debates among physicists about the discontinuous nature of matter and energy played out in Britain. The emergence of modernism in art and literature was accompanied by concerns about the

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[135] MClaren (1913), p.43.

place of old tenets in new modes of thought. Meanwhile, in discussions concerning vitalism and materialism, scientists from a variety of different disciplines pontificated on the nature of modern science. At the root of these debates lay questions about the limits of scientific knowledge and the boundaries between disciplines. With such notions came reconsiderations of the purposes of these disciplines.

We can interpret arguments by physicists about the ether within this wider framework. The choice to either keep or discard the concept of continuity was a choice to maintain or reject traditions in the discipline. And this preoccupation with tradition was not exclusive to physics but rather an all-pervading characteristic of the 'modern' age in which these conversations took place. As we saw in the fields of art and literature, the abandonment of tradition was often related to a reconsideration of the purposes, and limitations, of the discipline. This partly explains why the notions of continuity and discontinuity were so vigorously debated. With quantum energy, the larger problems seen to be facing physics were condensed down into a single physical theory. It was a theory of discontinuity, and thus representative of the larger discontinuity the discipline was facing. The question of whether or not matter and energy were ultimately continuous or discontinuous came to represent a bigger question about the nature of intellectual progress. Was physics to move forward through building on the theories of the past, or through rejecting them entirely? And in rejecting these old tenets, how was the discipline of physics to be redefined?

The move towards discontinuity was accompanied by a descriptionist conception of physics' limits, similar to the 'vagueness' referred to by Lodge. The role of physicists was thus altered, distanced from their earlier philosophical function to one of mere utility, being able to describe and predict the behaviour of nature. In debates about materialism, vitalism and spiritualism, there was a similar preoccupation, with the question also arising as to what kinds of knowledge scientists are able to obtain. It was not a matter of mere 'conservatism', automatic rejection of new ideas. As I have shown with Lodge, this notion was relative to the context in which it was used. Lodge was thus at some times a 'conservative' and at others a 'revolutionary', but the purpose was always the same: to increase the scope of science. A limited approach was his definition of 'modern' physics, and he used his position as president of the British Association to warn the 'public' of this change, this discontinuity of the past, and defend the old stance. This address was highly anticipated and received considerable press attention. The definition of 'modern' physics therein was quite different from that of Max Planck two years previously. It allied 'classical' physics not with a particular theory or approach, but more generally as an opponent of disruptive revolution, a concept to which Lodge's audience would certainly have related.

Furthermore, I have explored the interplay between the scientific and various 'public' receptions of the changes underway in physics in 1913. The radiation discussion at the British Association was a 'professional' debate, but with a 'public' audience, which was expanded by the reports in *The Times*. Furthermore, it was framed in the context of the immediate aftermath of Lodge's more 'popular' speech. The issue of discontinuity was heavily promoted by Lodge and under consideration in the radiation debate. It also entered the strictly 'professional' domain, incorporated into Jeans' *Report*, written after the meeting, which would go on to influence British physicists. The same preoccupations can be found in both 'professional' and 'public' discourses of quantum theory. With the problem of discontinuity related to the larger question of the nature of intellectual and cultural change, 'professional' discourse appears to have been influenced by the much wider cultural context.

This chapter has considered the notions of continuity and discontinuity, with their multiple meanings, as a significant preoccupation in both the public and professional reception of quantum theory in Britain. Continuity was representative of the ether, which connected discrete matter together into one, but also the idea of continuous change, of building on past theories rather than discarding them. Discontinuity represented the 'modern' physics of the quantum, but it also signified a drastic break from the past, a revolution. In the following chapter I consider this same issue of revolution, but in the case of relativity theory. Here, the categories of continuity and discontinuity were not as relevant to the principle, which did not deal in the physics of the very small, but the problem of revolution remained the same. Indeed, the particular way in which relativity theory was initially popularised established a rhetoric of revolution, around which physicists subsequently needed to negotiate.

Chapter Four: Rhetorics of revolution in relativity theory: the popularisation of 'modern' physics in the 1920s by Oliver Lodge and Arthur Stanley Eddington

4.1 Introduction

On 28 July 1914, war broke out in Europe. For many British physicists this resulted in their scientific work being directed towards practical wartime needs, with several engaged with X-ray or wireless work.[1] However, the war had other consequences as well. Oliver Lodge's son, Raymond, was killed in battle, and in 1916 Lodge published a book describing their subsequent communications via a psychic medium.[2] This personal tragedy could only have deepened Lodge's desire to believe in a connection between the physical and spiritual realms. With the ether serving as the conduit through which messages between the living and the dead travelled, it was now more than ever fundamental to Lodge's scientific worldview. In July 1919 he retired from his position as Principal of Birmingham University, declaring his intention to now devote the remainder of his years to studies of the ether.[3] Merely months after his retirement it would seem that the ether did indeed need his full attention, as Einstein's theory of general relativity achieved some fame as a potential 'destroyer' of the mysterious substance.

General relativity was an extension of Einstein's earlier theory (which was now coming to be labelled special relativity) to now encompass gravitation. The law appeared to explain a long-standing discrepancy between theory and observation with regards to the orbit of Mercury, which was more accurately predicted by Einstein's General Theory than by Newton's laws. The theory attracted considerable media attention in November 1919, following an expedition that May to test a second relativistic prediction, that the sun's gravitational field should deflect the light from stars. Organised primarily by Eddington and Astronomer Royal Frank Dyson, the expedition saw two teams of scientists travel to Sobral in Brazil and Principe in Africa to observe the deflection of

[1] Hughes (2005), p.280; Hull (1999); For an example, Rutherford's war time work on submarine detection is detailed in Wilson (1983), pp.339-385.
[2] Lodge (1916).
[3] 'Retirement of Sir Oliver Lodge. Study of the ether of space', *The Times*, 28 February 1919, p.7.

starlight during an eclipse.[4] Following the announcement of the results, in favour of Einstein, at a joint meeting of the Royal Society and the Royal Astronomical Society, *The Times* printed a report under the dramatic headline 'Revolution In Science. New Theory Of The Universe. Newtonian Ideas Overthrown'.[5] Both the expedition and its subsequent popularisation have been explored at length by historians, with Earman and Glymour contesting Eddington's depiction of the expedition as a crucial experiment.[6] There were three possible outcomes suggested: the full amount of deflection derived from Einstein's theory; a half-deflection claimed to be in accordance with Newtonian gravitation; or no deflection at all. Building on Earman and Glymour's account, Sponsel has explored how this 'trichotomy' of results was promoted, considering the role played by the Joint Permanent Eclipse Committee (JPEC) which organised the expedition. The JPEC, with Dyson, Eddington and the astronomer A. C. D. Crommelin at its helm, used connections at *The Times* to conduct a publicity campaign, creating interest in the expedition as a crucial test of Newton and Einstein's theories of gravitation, and then depicting the results as an uncontroversial confirmation of relativity theory.[7]

While Sponsel portrays this campaign as a resounding success, I argue that the trichotomy had unintended consequences. In May 1919, with the JPEC's publicity work well underway, an article in *The Manchester Guardian* discussed the expedition, ending with the note: 'It is a useful reminder in this age of enlightenment that however tall and wonderful be the structures that science builds she is all but childishly ignorant still of the bases on which they are reared'.[8] The following year, in a report of the 1920 meeting of the British Association, the same newspaper noted the 'malicious pleasure' with which biologists had greeted a perceived damage to 'the claim to exactness of the physical sciences, which was held to give them a higher rank than their own'.[9] Such reports interpreted relativity theory as revealing the fallibility of physics, through the destruction of long-held tenets. As such, a considerable amount of 'damage control' was required on the part of the JPEC to frame the results as perhaps 'revolutionary', but not

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[4] The expedition is detailed in Earman and Glymour (1980), Stanley (2003) and Sponsel (2002).
[5] 'Revolution In Science. New Theory Of The Universe. Newtonian Ideas Overthrown.', *The Times*, 7 November 1919, p.12.
[6] Earman and Glymour (1980).
[7] Sponsel (2002).
[8] 'The Eclipse of the Sun', *The Manchester Guardian*, 26 May 1919, p.6.
[9] 'The British Association', *Manchester Guardian*, 27 Aug 1920, p.6; This article was written by Charles Reginald Green, a journalist who had originally trained as an engineer: *Manchester Guardian* reporter diaries list 'CRG' as being in Cardiff during this period: Diaries (1-68), GUARD; The staff address lists for 1920 give a C R Green working as sub-editor: Staff Address Lists (223/16/1-49), GUARD; A *Times* obituary for C R Green provides bibliographic detail: 'Mr C. R. Green', *The Times*, 24 January 1976, p.14.

destructively so. Eddington and others were not helped in this matter by Oliver Lodge, who was on his own mission to 'save' the ether. Lodge, portraying himself as in opposition to the relativists, publicly attacked the theory as a threat to Newton and the ether, and warned physicists not to interpret the results too dramatically. I re-evaluate the historiography of the eclipse publicity and the popularisation of relativity, arguing that Lodge's role in discussions of both the expedition and the theory was as important as Eddington's. While Eddington may indeed have been 'Einstein's bulldog', his was not the only voice heard.[10]

Furthermore, my analysis of Lodge's publicity work extends beyond relativity theory and into the popularisation of 'modern' physics in general. The literature on popular physics during the 1920s has tended to focus on the activities of those physicists we would now consider to be 'modern'. Michael Whitworth has examined in depth the publication and reception of Eddington's 1928 *The Nature of the Physical World* and Jeans' 1930 *The Mysterious Universe*, both of which sold in unprecedented numbers for physics books of the period.[11] Bowler, in his survey of early twentieth century popular science, describes Lodge as having 'reinvented himself as a public figure in the early twentieth century' and suggests that this received some criticism.[12] However, despite acknowledging Lodge's status as a well-known public figure, Bowler is fairly dismissive of Lodge's contributions to popular 'modern' physics, referring to his books on the subject only to point out their factual errors and inconsistency with contemporary scientific consensus.[13] In this chapter I consider Lodge's efforts at popularising 'modern' physics in the 1920s, and argue that his work was not necessarily judged as out-of-date, and was praised by both non-scientific reviewers and his scientific peers. As such, it would seem that a significant amount of the popularisation of 'modern' physics was carried out by a physicist we would now regard as decidedly 'classical'.

This has implications for notions of scientific consensus, which have been noted by Bowler, albeit not in his study of popular science. Elsewhere, Bowler considered the significance of the popularisation work undertaken by scientists such as Lodge, long retired from research but still in positions of influence. Discussing the group of scientists who campaigned against materialism, Bowler notes that 'many of these scientists were

[10] Stanley (2003), p.71.
[11] Whitworth (1996).
[12] Bowler (2009), pp.219-20; Bowler notes that the rationalist Joseph McCabe accused Lodge, and J. J. Thomson, of using his position as a popular writer to present out-of-date scientific theories to the public.
[13] Bowler (2009), p.36; p.137.

by now senior figures and no longer active in research' but 'were able to use their influence to gain access to editors and publishers in order to mount a public campaign of considerable effectiveness' and successfully convince church leaders 'that they represented the opinion of the scientific community as a whole'.[14] Lodge was, as I discussed in the previous chapter, one such campaigner against materialism. Similarly, his 'public' communications of 'modern' physics were also widely seen and heard. I propose that in a study of his activities and their reception we find the lines between 'classical' and 'modern' blurred. Furthermore, Lodge's opposition to many aspects of the new physics created a perception of 'modern' physical theories as impermanent and unstable. Contrary to Bowler, I suggest that, with multiple voices heard, a picture of scientific consensus was not achieved, and the early twentieth century was a period of uncertainty, in both 'professional' and 'public' depictions of physics.

In this chapter I explore responses, in both 'public' and scientific spheres, to relativity theory following the eclipse expedition. I begin by considering the initial media response, and particularly the role of *The Times*' science correspondent Peter Chalmers Mitchell, who promoted the notion of a revolution destructive to the theories and legacy of Newton. I then examine how Eddington and Crommelin attempted to reframe the revolution as less harmful to old ideas, and how Lodge undid this work through discussing the destruction in order to attack the theory. I then explore discussions in a more scientific arena, considering Lodge's role as editor of the *Philosophical Magazine* and the variety of divergent views presented in *Nature's* 1921 special issue on relativity. I then consider the popularisation work of James Rice, a Liverpool physicist, who, as a result of his particular background, tackled the problem of revolution with an alternative view of progress to that espoused by Eddington. I go on to consider, more broadly, Lodge's status during the 1920s as a populariser of 'modern' physics. Finally, I compare the different notions of progress put forward by Lodge and Eddington, the two key figures in the popularisation of both relativity theory and 'modern' physics during this period.

In doing so, I explore notions of 'public' trust in science. Ideas of 'revolution' and 'destruction' are worrying in a discipline perceived as a stable foundation of knowledge. In this chapter, I examine how physicists negotiated these terms, attempting to emphasise the exciting nature of scientific change without threatening physics'

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[14] Bowler (2001), p.20.

reputation. Many of these ‘popular’ discussions and expositions were not simply for the benefit of a non-scientific ‘public’, but were written and published while debates *within* the physics community were underway. As such, they had an impact on the shaping of professional consensus, and the rhetorical reactions to a perceived need to defend relativity theory against accusations of instability made their way into professional discourse. The ‘public’ communication of relativity theory can thus be interpreted as representative of a ‘dialogue’, rather than ‘deficit’ model of science communication, with the information travelling in both directions.[15]

4.2 Navigating ‘revolution’: British newspapers, the JPEC and Oliver Lodge

I begin by considering the nature of the initial reports of the expedition, and the publicity work subsequently undertaken by Eddington, Crommelin and Lodge. These three men had different objectives when discussing relativity theory in ‘public’, and different conceptions of the nature of scientific progress. Eddington was a Trinity College graduate, Senior Wrangler of 1904, and Director of the Cambridge Observatory from 1914.[16] He was also an early supporter of relativity, publishing an exposition of the general theory in *Nature* in 1916, and in 1918 produced an official *Report on the Relativity Theory of Gravitation* for the Physical Society.[17] Eddington was extremely active in the organisation of the expedition, which Stanley has argued was closely related to Eddington’s Quaker religion and consequent desire for international cooperation.[18] He was certainly eager to promote both the theory and the success of the expedition. Furthermore, he viewed the practice of science as a process of trial and error, with theories constantly being moulded by new observable evidence. Theoretical foundations did not need to be permanent; they were merely scaffolding required for physical thought to progress.[19]Eddington’s philosophy was thus compatible with an ‘overthrow’ of Newton, as this was simply part of the normal process of scientific progress. While Eddington led the expedition to Principe, the astronomer Andrew Claude de la Cherois Crommelin was part of the team that went to Sobral. Crommelin

[15] For an overview of the dialogue and deficit models of science communication, see Trench (2008), p.119.
[16] For Eddington, see Stanley (2007a).
[17] Eddington (1916); Eddington (1918); Eddington’s interest in relativity theory is detailed in Warwick (2003), pp.462-8.
[18] Stanley (2003) relates Eddington’s role in the expedition to Quaker “adventurers” who travelled afar to promote international peace.
[19] Stanley (2007b).

was also a Trinity College graduate of the Mathematical Tripos, and was appointed an assistant at the Royal Observatory, Greenwich in 1891.[20] Here, he was briefly joined by Eddington, who was Chief Assistant to the Astronomer Royal from 1906 until being awarded a Fellowship of Trinity College in 1907. Crommelin became a Fellow of the Royal Astronomical Society in 1888, and served as a secretary there from 1917 until 1923. He was also president of the British Astronomical Association from 1904 to 1906. It seems likely that Crommelin was more concerned with the astronomical implications of relativity theory than the physical, or indeed philosophical.

Eddington and Crommelin were both important members of the JPEC, but this committee was not entirely responsible for eclipse discussions, either before or after the event. Oliver Lodge had been aware of relativity for some time, and the special theory had featured in his 1913 British Association polemic against discontinuity. Here he interpreted it as suggesting that time was discontinuous and threatening to relegate the ether of space ‘to the museum of historical curiosities’.[21] In 1917, with the general theory now well known among a number of British physicists, Lodge attempted to defend the Electronic Theory of Matter, suggesting ways in which it could explain the Mercury anomaly.[22] As I showed in the previous chapter, Lodge was intent on rescuing the notion of continuity from the past to the present, and the purpose of scientific theories as explaining rather than simply describing. Both of these commitments were fundamental in his reception and promotion of general relativity theory.

However, neither Eddington and Crommelin nor Lodge were wholly responsible for the immediate media response to the expedition. Following *The Times*’ ‘Revolution in Science’ article, the expedition was reported in other national newspapers. The *Daily Express* discussed ‘Upsetting the Universe’, the *Observer* explored ‘The Baseless Fabric of the Universe’, the *Daily Herald* declared a ‘Bloodless Revolution’, while the *Daily Mail* simply observed that ‘Light [had been] Caught Bending’.[23] It is not surprising that journalists and newspaper editors were interested in the story. Having been framed by *The Times* as a revolution against Newton, the eclipse expedition fitted into at least two

[20] Anonymous (1939).
[21] *Report of the British Association for the Advancement of Science* (1913), pp.18-19.
[22] Warwick (2003), pp.470-5.
[23] ‘The revolution in Science’, *The Times*, 7 Nov 1919, p.12; ‘Upsetting the Universe’, *Daily Express*, 8 Nov 1919; ‘The Baseless Fabric of the Universe’, *Observer,* 9 Nov 1919; ‘Bloodless Revolution’, *Daily Herald*, 8 Nov 1919; ‘Light caught bending’, *Daily Mail*, 7 Nov 1919.

of the 'news values' described by Gregory and Miller: *unexpectedness* and *elitism*.[24] Revolution, by its very nature, is unexpected and dramatic, while the references to Newton's 'downfall' meant that the story was about perhaps the most famous scientist known to British people. By declaring revolution in its report, *The Times* had created a story with clear interest to newspapers. Furthermore, in the context of the recent world war, and the Russian revolution of 1917, revolution was a topical subject. Indeed, the socialist *Daily Herald's* report noted that it was 'nice to know that there is somewhere the "Times" doesn't object to revolution'.[25]

The Times' first article thus had a lot to answer for. This was written by Peter Chalmers Mitchell, a zoologist of some eminence as Secretary of the Zoological Society of London (a post he held from 1903 to 1935) and a Fellow of the Royal Society (elected in 1906).[26] From 1911, he had been supplying *The Times* with zoological articles, and during the First World War he worked in Lord Northcliffe's Department of Enemy Propaganda. In 1919, apparently on his own suggestion, Mitchell was given a permanent role at *The Times*, as a special writer on science.[27] Northcliffe was impressed by Mitchell and committed to good science reporting, declaring that this new appointment would help make *The Times* 'the organ of research that it should be.'[28] Mitchell, meanwhile, was apparently spurred on by a belief in the importance of science in government policy; accordingly, he felt it was important that the public 'take an interest in scientific policy', and become aware of what research was being undertaken, or ought to be undertaken, and which agencies and people were involved.[29] With this in mind, he hoped for a regular weekly column, which he was finally given in March 1921. 'Progress of science' was intended to be both 'intelligible to the educated public' and 'not distasteful even to the specialist in his own subject'.[30] Here, he discussed a variety of topics, covering biology, physics, astronomy, chemistry and technology, as well as broader considerations of the funding and management of science.

[24] Gregory and Miller (1998).
[25] 'Bloodless Revolution', *Daily Herald*, 8 November 1919.
[26] For Mitchell, see Crook (1989), Duncan (1980) and Hindle (1947). He also has an autobiography, Mitchell (1937)
[27] Sir Peters Chalmers Mitchell Managerial File (MAN/1/ 1911-1935), NEWS; Mitchell (1937).
[28] Northcliffe to Corbett, 25 January 1919, NORTH.
[29] Chalmers Mitchell to Dawson, 28 Feb 1919, NEWS.
[30] Mitchell (1937), pp.274-5.

Mitchell claimed in his autobiography that the article about the joint meeting had been entirely his decision.[31] Given *The Times*' existing interest in the expedition, carefully managed by the JPEC, this seems unlikely. However, as both a Fellow of the Royal Society and recently appointed 'science correspondent', Mitchell was an ideal choice. Furthermore, he was already familiar with and interested in the philosophical implications of relativity theory, having attended a 1916 meeting of the Aristotelian Society, an arena for the discussion of philosophy, featuring papers on the subject by Joseph Larmor and A. N. Whitehead.[32] Mitchell's 'Revolution in Science' article was not entirely in accord with the dramatic headline, which was almost certainly written by a sub-editor eager to provide the story with the 'news value' of unexpected drama.[33] Instead, it reads as a diplomatic presentation of a number of conflicting opinions, with the most definite statement being that it was 'generally accepted' that Einstein's prediction had been confirmed. While Earman and Glymour would certainly argue that the eclipse photographs did not conclusively show this, Mitchell was no doubt responding to the views of the majority of the scientists at the meeting and at the dinner afterwards (which he also attended).[34] Furthermore, Mitchell did not present the meeting as one of complete consensus, noting that 'there was difference of opinion as to whether science had to face merely a new and unexplained fact, or to reckon with a theory that would completely revolutionize the accepted fundamentals of physics'. He noted that 'the scientific discussion centred more in the theoretical bearings of the results than in the results themselves', and ended by stating that 'the question remains open as to whether the verifications prove the theory from which the predictions were deduced'.[35] In this article, contrary to the headline, Mitchell did not write that relativity theory had been proven. The more cautious views of scientists had been reinterpreted by an editor in need of drama to make a more exciting story.

However, Mitchell also wrote an editorial in *The Times* that same day, which was less balanced and more revealing of his own philosophical interest in relativity theory. In his 1915 book, *Evolution and the War*, Mitchell had written of the separation between mind and reality, declaring that scientific 'laws' were of the human mind 'rather than of the

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[31] Mitchell (1937), p.268.
[32] 'Abstract of the Minutes of the Proceedings of the Aristotelian Society for the Thirty-Seventh Session' (1915-1916), p.364.
[33] Correspondence with Nick Mays, Archivist at News International Archive; For 'news values', see Gregory and Miller (1998).
[34] Mitchell (1937), p.268.
[35] 'The revolution in Science', *The Times*, 7 Nov 1919, p.12

extended world'.[36] Crook has suggested that Mitchell was attracted by the philosophical notion of scientific uncertainty arising out of the 'new physics'.[37] It certainly seems that the concept of Newton's possible 'overthrow' was of interest to Mitchell, as it was suggestive of the impermanence of even long held scientific theories and thus reinforced the idea that they were not objective reflections of the true nature of the world. Mitchell's editorial opened with the declaration that '[f]rom EUCLID to KEPLER, from KEPLER to SIR ISAAC NEWTON, we have been led to believe in the fixity of certain fundamental laws of the universe'. However, with the announcement of the eclipse expedition, 'the scientific conception of the fabric of the universe must be changed'. While a third prediction of Einstein's remained in doubt, Mitchell suggested that 'it is confidently believed by the greatest experts that enough has been done to overthrow the certainty of ages and to require a new philosophy of the universe, a philosophy that will sweep away nearly all that has hitherto been accepted as the axiomatic basis of physical thought'. [38] While his report of the meeting had depicted a cautious acceptance of experimental results, here Mitchell was explicitly proclaiming the certainty of an 'overthrow' of old ideas. Having only recently been made a permanent member of staff, this focus on overthrow may have been partly influenced by a desire to produce an exciting article for his new employers. However, it is clear that Mitchell's pre-existing philosophical commitments certainly had a significant effect on his discussion of relativity theory. Mitchell's philosophy was thus an additional factor, with the work of the JPEC and the demands for drama from newspaper editors, contributing to the early reporting of the eclipse expedition.

With this groundwork now laid down, the JPEC worked to respond to the hyperbolic headlines and reframe the results as less damaging to Newton and the concept of the permanence of scientific truth. Science, and particularly physics, based much of its reputation on its ability to obtain a more objective truth than that achieved in other intellectual pursuits. If its former 'truths' were now found to be false, then this raised the question of why science should be trusted at all. With physics staking a claim as the 'foundational' discipline upon which all other sciences were built, this issue was of considerable importance. 'Revolution' was thus a dangerous word. I now consider the articles and lectures delivered by Eddington and Crommelin in the months following the

[36] Mitchell (1915), pp.6-7.
[37] Crook (1989), p.342.
[38] 'The Fabric of the Universe', *The Times*, 7 November 1919, p.13; The third test was a prediction of the gravitational redshift of light.

November announcement, and explore how they carefully managed the 'revolutionary' aspects of relativity theory. Both Lodge and Eddington responded very quickly to the various headlines that had appeared in newspaper reports of the eclipse results, writing in periodicals aimed at an educated but non-specialist audience. Eddington, writing in the *Contemporary Review* in late November, referred to 'REVOLUTION in Science – Newton and Euclid dethroned – Bending of Light – the Fourth Dimension – Warping of Space!'[39] He accused such judgements of being perhaps 'too hasty', but admitted that the 'fundamental nature of the change has not been exaggerated'.[40] Indeed, he also attempted to lay to rest any claims of Newton's overthrow, arguing that Newton had in fact predicted, in his *Opticks*, that light could bend. Furthermore, he ended by declaring that it was 'not necessary to picture scientists as prostrated by the new revelations, feeling that they have got to go back to the beginning and start again. The general course of experimental physics will not be deflected, and only here and there will theory be touched.'[41] Eddington was continuing to assert that the results of the expedition *had* been significant, in line with the JPEC pre-eclipse publicity. However, he was also playing down the references to revolution, insisting that neither Newton, nor the practice of physics, should be threatened by the results.

Lodge, unsurprisingly, had a rather different perspective. Writing in the *Nineteenth Century*, also in late November, he too accused the press, particularly *The Times*, of arousing 'widespread interest' through hyperbolic headlines.[42] He warned the physics world to not take the implications of the results too far, to not 'be revolutionary to a rash and hasty in extent'. He argued for the interpretation of the new results in terms of a 'generalisation' of the old theories, noting that this is what had been achieved, 'joyfully', by the electronic theory of matter. Lodge did not deny all aspects of the theory; he admitted that he had no choice but to accept the empirical evidence. However, he interpreted it more conservatively; where, so he claimed, Einstein denied the necessity of an ether in physical theories, Lodge felt this was too rash. Where Eddington tried to rhetorically soften the potential blow to 'classical' physics, Lodge emphasised this. Eddington was minimising the consequences of relativity theory in order to defend it, Lodge was highlighting the potential for these consequences in order to warn against the wholehearted adoption of all aspects of the theory.

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[39] Eddington (1919), p. 639; The *Contemporary Review* was intended to provide reviews and discussions of art and literature from a liberal Christian perspective, Glasgow (1998).
[40] Eddington (1919), p.639.
[41] Eddington (1919), p.643.
[42] Lodge (1919a), p.1189.

Eddington was not alone in this respect. Crommelin wrote a piece for *The Observer* a mere nine days after the official announcement of the eclipse results. Discussing Einstein's theory, and its accordance with the observed perihelion of mercury and the results of the eclipse expedition, he also made sure to note that 'the practical consequences of Einstein's Law on astronomical calculations would be very slight'. Furthermore, it did 'not seem to be necessary that all followers of Einstein should abandon their belief in the ether'. However, they would have to alter their concept of the ether, accepting that it had no effect on the motion of bodies through it, and that we were unable to detect any information about such motion.[43] In January 1920, he spoke to the Science Masters' Association and informed his audience that the majority of astronomers now believed that the gravitational aspect of Einstein's theory had been confirmed. However, he insisted that 'some newspapers went too far in speaking of the Einstein theory as overthrowing the Newtonian theory' and again remarked that 'it would not be necessary to make new planetary tables at all'.[44] Crommelin was carefully interpreting and promoting the results of the expedition as significant, but not revolutionary, and certainly not overtly challenging to theoretical commitments to the ether or practical work in astronomy.

Crommelin was thus depicting continuity between past and present physics, through accepting the possibility of an ether compatible with relativity theory, and insisting that the practice of astronomy would barely be altered. Eddington took this approach even further, arguing that relativity theory represented a consistency in physical approach, whilst the deniers of relativity were in fact themselves changing the rules. In 1920, he published a non-technical exposition of relativity theory, intended for both general readers and those with existing knowledge of the subject.[45] Here, he began with the declaration that relativity theory had 'provoked a revolution of thought in physical science'.[46] However, he later qualified this bold statement with a discussion of the nature of the change. He noted that the theory was one of geometry, rather than the models of the Victorian Maxwellian tradition. However, he argued that the route to a geometrical theory had been a conventional one: 'As the geometry became more complex, the

[43] Dr. A. C. D.Crommelin, 'Einstein's Theory. What it means and what it involves. The curvature of space. The spectrum test', *The Observer*, 16 November 1919, p.9.
[44] 'The Einstein Theory: New Planetary Tables Not Necessary', *Manchester Guardian*, 7 Jan 1920, p.7.
[45] Eddington (1920), p.vi; Whitworth (1996) has described this book as 'non-technical' rather than 'popular' in the sense of a wide audience.
[46] Eddington (1920), p.v.

physics became simpler; until finally it almost appears that the physics has been absorbed into the geometry. We did not consciously set out to construct a geometrical theory of the world; we were seeking physical reality by *approved methods*, and this is what has happened'.[47] Thus, the highly abstract and mathematical nature of relativity theory did not represent a departure in the methods of physics. It had instead been arrived at through the existing methods of physics, the process which had led to theory was depicted here as one of evolution, not revolution.

However, the work of Crommelin and Eddington to minimise the revolutionary consequences of Einstein's theory was threatened by Oliver Lodge, who continued to pursue his own agenda of conceptually deepening the chasm between relativity theory and the legacy of Newton. In a January article in the *Fortnightly Review*, another general arts periodical, a tone of conflict was immediately established with the title 'The Ether versus Relativity'.[48] Here, Lodge argued that the relativists followed a 'doctrine' which saw physical theories as 'founded upon convenience rather than upon an impossible striving towards absolute truth.'[49] Lodge, however, would rather consider theories as 'partially erroneous' but still aimed towards truth. The relativists, in this interpretation, were not only using different physical theories and concepts, they were changing the very purpose of the discipline. This was in direct opposition to Eddington's depiction of the development of the theory. The problem of this deeper change, which had so troubled Lodge in 1913, was intensified by relativity theory. Lodge was continuing to use 'public' forums to defend physics as he knew it, while Eddington and Crommelin used these same forums to defend a newer approach.

Furthermore, Lodge disagreed with Crommelin's conception of the ether as now fundamentally undetectable. Lodge thought that physicists 'need not admit that never by any means whatever shall we be able to observe motion through the ether', although the chances of such an observation were getting slimmer.[50] The well-known experiments by Michelson and Morley in the 1880s had failed to detect the movement of the earth through the ether, and FitzGerald and Lorentz had proposed a contraction hypothesis, that the contraction of matter was concealing any effect of the ether.[51] However, Lodge believed that the ether could still be detected, and suggested a possible method of

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[47] Eddington (1920), p.183; italics my own.
[48] Lodge (1920a).
[49] Lodge (1920a), p.54.
[50] Lodge (1920a), p.57.
[51] Warwick (2003), pp.360-1.

determining the existence of the ether, by making an 'ether-stream', putting the ether in motion. He said that it was quite possible 'and as some think likely' that some ether was in motion, flowing along lines of magnetic force. In a strong magnetic field, this flow might be detected optically. As he pointed out in his article, he had already attempted such an experiment, the results of which were published in the *Philosophical Magazine* in 1907, but it had been unsuccessful.[52] Lodge insisted that this experiment ought to be repeated, and that 'extraordinary and expensive means' were required to detect the extremely slow speed of the ether-stream. Lodge saw 'no reason why a National Laboratory should not undertake such an experiment'.[53] In an article in the *Philosophical Magazine* of May 1919, Lodge had described such an experiment, referring to his nineteenth century work detailed in 1907. Here, he had ended by suggesting that there was a vast amount of energy 'locked up in the aether', and thus detecting it could 'have a bearing on really practical problems'.[54] This was a very different conception of the ether from that proposed by Crommelin. Lodge was promoting the ether as detectable and, crucially, arguing that it was in the public interest to undertake such a detection. He was calling for an expensive, nationally funded, test of the ether. While Crommelin suggested a theoretical compatibility between the ether and relativity theory, Lodge's commitment to the ether required more than conceptual negotiations.

I have shown that Crommelin and Eddington, prominent members of the JPEC, were promoting the results of the eclipse expedition as confirming Einstein's general theory of relativity, whilst also presenting this theory as simultaneously significant and not significant. They referred to a dramatic shift in thought, but one which did not require an 'overthrow' of long held theoretical commitments (for example to the ether) or a change in much of the practice of physics. Lodge, by contrast, was doing almost the opposite, arguing that the results did not confirm all of relativity theory, and that this theory did represent a dramatic (and unwelcome) shift in thought and practice. As he had done in 1913, he was equating the development of 'modern' physics with the notion of revolution. This was done most notably at the 1920 British Association meeting. Here, there was a surprising lack of papers on the topic of relativity theory, considering it was the first British Association meeting since the announcement of the results and Eddington was President of Section A. Eddington, however, chose to give his

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[52] Lodge (1907).
[53] Lodge (1920a), p.58.
[54] Lodge (1919b), p.471.

presidential address on the topic of the internal constitution of stars.[55] This perhaps can be explained by the suggestion subsequently made in the astronomical journal, the *Observatory*, that there had been an absence of relativity theory at the meeting 'because those chiefly concerned had become a little jaded with the strenuous conflict'.[56] Such conflict, continuing along the lines of that first *Times* headline, served to emphasise the overtly revolutionary aspects of the theory which Eddington was trying to obscure.

Lodge, of course, had an entirely different agenda, and so it is not surprising that he *did* discuss relativity theory at the meeting, and even labelled his talk a 'Controversial note on popular relativity', thus setting himself outside of the scientific mainstream.[57] Here, he again admitted that Einstein's equations were supported by experimental observations, but argued that some interpretations of these equations were 'threatening to land physicists in regions to which they had no right of entry', into metaphysical reasoning 'beyond their ken'.[58] He disagreed with any attempts to 'build up on an equation an elaborate metaphysical structure', arguing that such equations were open to numerous interpretations.[59] Lodge's mode of physical reasoning, as noted in Chapter One, was the Maxwellian method of model building. At the British Association meeting, Lodge ended his talk by suggesting that relativists who were using the success of the equations to create a metaphysical structure which would 'complicate the rest of the universe unduly', should perhaps 'be regarded as Bolsheviks and pulled up'.[60] This last comment was particularly damning for those physicists who were trying so hard to depict Einstein's theory in terms other than revolutionary. Lodge's comparison was a very quotable sound-bite, reported in *The Guardian*, the *Daily Telegraph* and the *Daily News*.[61] As with his 1913 emphasis on continuity and discontinuity, Lodge was here placing developments in physics in a broader social and political context, ascribing a deeper significance to 'revolution' in physics.

Throughout the early 'public' reception of general relativity theory, Lodge was working to unravel the careful work of Crommelin and Eddington, promoting a rhetoric of

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[55] *Report of the British Association for the Advancement of Science* (1920); for Eddington's astrophysical work, see Stanley (2007b).
[56] 'Notes' (1921), p.321.
[57] *Report of the British Association for the Advancement of Science* (1920), p.352.
[58] Lodge (1920b), p.325.
[59] Lodge (1920b), p.326.
[60] Lodge (1920b), p.326.
[61] 'British Association: Go-As-You-Please Schools. Sir Oliver Lodge And Einstein's Theory', *The Manchester Guardian,* 27 August 1920, p.6; 'Sir Oliver Lodge on Einstein's Theories', *Daily Telegraph*, 27 Aug 1920; *Daily News* quoted in 'Notes' (1920).

revolution in order to reveal what he saw as the inherent dangers of immediately accepting all of the consequences of relativity theory. The early 'public' response to relativity theory was thus influenced by journalists, who emphasised a dramatic revolution, but also by Lodge's reinforcement of this chasm between the old and the new. Members of the JPEC had succeeded in framing the expedition as a crucial test of Einstein's and Newton's theories, but they were not responsible for the continued emphasis on revolution, and indeed this had not been their intention. Lodge's interpretation, in line with the 'news value' of a dramatic event, was as influential as the arguments used by Eddington and Crommelin.

4.3 Debating ether and progress for the scientific 'public'

While Lodge, Eddington and Crommelin tackled the problems of relativity and revolution in newspapers and non-scientific magazines, similar debates were underway in more scientific arenas. This section considers Lodge's role as editor of the *Philosophical Magazine* in providing an outlet for ether theories in the wake of the eclipse expedition. Here, relativity was not presented as a certainty. I also examine the contributions to a 1921 special issue of *Nature*, which served as a platform for both technical discussions and broader philosophical debates. Here we find a lack of consensus that needed to be glossed over in more popular expositions.

The articles that appeared in the *Philosophical Magazine* around this time are indicative of Lodge's approach to the consequences of the eclipse expedition, as he was one of the editors of this publication. The role of the *Philosophical Magazine*, as a home for research that was contrary to many 'modern' developments, is studied in Chapter Five. There I include the example of a paper by Joseph Larmor that interpreted the eclipse expeditions as in support of Newtonian mechanics. This paper was, as I will show, ultimately rejected by the Royal Society's *Proceedings*, but immediately accepted, by Lodge, into the *Philosophical Magazine*. I shall argue that the publishing policies of the Royal Society's *Proceedings* and the *Philosophical Magazine* promoted different definitions of 'classical' and 'modern' physics, and the place of the former in current physics research. For the purposes of the current chapter, I briefly consider how Lodge used the *Philosophical Magazine* as a home for alternative views on relativity, promoting further study of the ether.

I have already referred to Lodge's May 1919 article in the *Philosophical Magazine* which suggested a possible method of experimenting on the ether. Here, he referred to Larmor's *Aether and Matter*, published in 1900, and his own 1907 description of his ether experiments, which had been undertaken even earlier, in 1892 and 1893.[62] As an editor of this journal, Lodge had considerable control over its contents. He was thus able to use this publication to present opinions contrary to the 'relativists'. In February 1920, he communicated a paper by Ludwig Silberstein, a lecturer in mathematical physics at the University of Rome, which argued that the eclipse results could be interpreted as providing evidence for the existence of an ether.[63] Lodge followed this article with his own 'Note on a possible structure for the ether', declaring that 'Dr. Silberstein's communication gives me an opportunity for calling attention to a paper of mine on many points in connexion with the ether which must surely be of interest even to those who are contemplating the abandonment of that medium'.[64] Again, Lodge was promoting his 1907 paper which, written only two years after Einstein had published his paper on the electrodynamics of moving bodies, and describing experiments conducted long before then, did not take into consideration any aspect of relativity theory. The following June, the *Philosophical Magazine* published a paper by Dr. G. Green, a lecturer in Natural Philosophy at the University of Glasgow, which suggested an analogy between characteristics of the ether and certain fluids.[65] Here, Green noted that '[r]ecent experimental observations have compelled us to modify certain ideas regarding the physical characteristics to be associated with the aether, in proving that the aether is capable of acting as a very slightly refracting medium in strong gravitational fields'.[66] This was of course a reference to the eclipse observations, and Green ended his paper by suggesting that the analogy he had presented could be viewed as 'an avenue of escape from the principle of relativity', as it could suggest the possibility of detecting velocity relative to the aether.[67] Lodge published on the ether again in 1921, this time responding to a 1913 paper by McLaren (who had died in the First World War).[68] Throughout the 1920s, more papers appeared in the *Philosophical Magazine*, explaining apparently relativistic effects within the framework of ether physics, and contributing to a research

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[62] Lodge (1919b), p.471.
[63] Silberstein (1920).
[64] Lodge (1920c).
[65] Green (1920).
[66] Green (1920), p.651.
[67] Green (1920), p.659.
[68] Lodge (1921c).

programme to establish an 'etherial' worldview.[69] Through the publication of his own and others' papers in the *Philosophical Magazine*, Lodge was promoting experimental study of the ether. He was attempting to save not simply the concept of the ether, but also direct physical research towards ether studies. An undetectable ether, as proposed by Crommelin, was of no interest to Lodge as such an ether would essentially be irrelevant to further research, having no effect on physical phenomena.

Outside of the *Philosophical Magazine*, an opportunity for a variety of physicists to present their views on relativity to a non-specialist (but scientific) audience arose in February 1921, with the publication of a special issue of *Nature* dedicated to the topic. Contributions were received from a number of British physicists, including Dyson, Crommelin, Eddington, Jeans, Cunningham, Campbell and Lodge. The mathematician G. B. Mathews discussed non-euclidian geometries, while the philosopher Wildon Carr contributed an article on the metaphysical implications of the theory. There were also articles written by Hermann Weyl and Hendrik Lorentz, and an outline of the theory by Einstein himself. Some contributors focused on technical details: Dyson's account of the eclipse observations paid little attention to relativity theory itself, and was instead concerned with the verification work of astronomers.[70] However, those contributors who did discuss relativity theory and its implications, presented opposing views on the existence of the ether, the correct approach to the practice of physics, and the nature of truth. This special issue of *Nature* displayed wildly diverging approaches to the theory of relativity, as held by national and international 'experts' on the subject, who had been invited by the editor.[71]

The contributions by Campbell and Jeans both described relativity as in opposition to the ether. As we saw in the previous chapter, Jeans was willing to discard the ether if a robust theory required him to do so. Furthermore, his scientific method meant that he was not committed to visual models, like Lodge, but also eager to consider rigorously deduced mathematical models. Indeed, in 1930, Jeans declared in his popular book, *The Mysterious Universe*, that the 'Great Architect of the Universe now begins to appear as a pure mathematician.[72] In his *Nature* contribution, he argued that the construction of mechanical models was not 'the only known means of guidance to the discovery of new

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[69] Synge (1922); Hartree (1923); Press (1925); Meksyn (1927).
[70] Dyson (1921).
[71] D. D. (1921); The editor of *Nature* was then the astronomer Richard Gregory. I have been unable to ascertain the identity of "D. D.", who wrote the introduction to this edition.
[72] Jeans (1930), p.134; See Whitworth (1996) for a discussion of this book.

laws of Nature', suggesting an 'even more fruitful means of progress' through the generalisation of known laws. A generalised law (such as relativity) would then suggest other specialised laws, which themselves could be tested in order to confirm the generalisation.[73]

When Jeans referred to 'generalisation', he was not necessarily describing what is now known as the general theory of relativity. In his paper, Jeans described the earlier relativity theory as the 'main trunk' of a tree and the 'gravitational theory' as 'only one branch, although a vigorous and striking branch'.[74] As such, the title of this article was 'The General Physical Theory of Relativity', but the content was devoted to special relativity, which for Jeans *was* the general theory. Jeans was, by 1921, fully committed to special relativity, but unsure of its application to gravity. However, the special theory alone had considerable consequences, and Jeans declared that the electromagnetic ether now needed to be 'either amended or abandoned, and the indications are strong that the less drastic course will not suffice.'[75] For Jeans, relativity represented two fairly significant departures from the tradition of Maxwellian electrodynamics: the replacement of mechanical models with mathematics, and the desertion of the ether.

Campbell, as we saw in Chapter Two, was on the opposite side of the spectrum from Jeans, believing instead in a highly experimental approach, building theories on concepts which could be empirically detected. In this issue of *Nature*, he discussed the roles of 'Theory and Experiment in Relativity'.[76] He noted that many experimental physicists found relativity theory 'disturbing' because the ideas could not be conceived of as analogous to concepts in experimental laws. However, Campbell used this observation to promote his particular descriptionist approach to experimental physics. He argued that the mechanical models conceived of by experimental physics could not be confirmed as actually 'true', only able to predict results. Relativity was no different. As such, physical theories were simply tools of prediction, not true representations of the natural world. This was the kind of thinking that Lodge attacked, but Campbell had not adopted it as a consequence of relativity theory, but rather was an early supporter of relativity theory because of his existing philosophy of descriptionism. Campbell saw relativity theory as a valuable method, because for him all of physics was simply the utilisation of methods.

[73] Jeans (1921), p.792.
[74] Jeans (1921), p.791.
[75] Jeans (1921), p.792.
[76] Campbell (1921a).

Notably neither Crommelin nor Eddington made reference to the ether in their articles. Crommelin's paper on the perihelion of Mercury set forward the failures of other attempts to explain the anomaly, before conceding that astronomers had been 'driven by exhaustion to Einstein's law as the only satisfactory explanation'. He did not discuss any implications of the law, and indeed noted that the theory had 'no effect on orbital planes', in accord with his earlier remark about the work of astronomers not being disrupted by relativity theory.[77] Eddington discussed philosophical and relativistic approaches to the concept of time, in a paper perhaps too abstract and metaphysical to attract controversy.[78] Both Crommelin and Eddington appear to have been avoiding drawing attention to revolutionary aspects of relativity theory.

However this issue of *Nature* sparked debate about the properties of the ether, and if it could still be said to exist, with which Eddington quickly became involved. A British meteorologist, Leo Bonacina, wrote in to *Nature* in response to the various depictions of the nature of reality.[79] He suggested that the relativists seemed to perceive space as not conditioned by matter, but rather the foundation of matter and forces, themselves merely the outcome of the geometry of the universe. Eddington responded to this by noting that while relativists did indeed not consider space as matter, they did ascribe to it other 'dynamical attributes', thus making space a 'physical medium'. He insisted that, contrary to some depictions of the theory, it did not reject the ether, but rather 'added to the importance of the ether by enlarging its functions.' What some relativists labelled 'space', or even the 'world', could be considered as the ether, thus placed at the centre: it was the 'fundamental substratum of everything'.[80] The apparent revolutionary change in thought was thus simply a matter of semantics. Eddington was depicting continuity between 'classical' and 'modern' physics by suggesting that relativity theory was expanding the role of the ether. This rhetorical device allowed him to defend relativity theory against any accusations of 'revolution'. He was thus employing similar methods in both his 'popular' and 'professional' discussions of the theory. Campbell, unsurprisingly, disagreed. He argued that this new 'ether' was as different from the old ether as to be unrecognisable. Eddington's ether did not have 'density, elasticity, or even velocity', whereas 'the aether of pre-relativity days, which relativity *has* done away with

[77] Crommelin (1921), p.788.
[78] Eddington (1921a).
[79] Bonacina (1921); Biographical details from Pike (2000).
[80] Eddington (1921b).

– has all those properties. In particular, it has the last.'[81] The change in concepts of the ether, which Eddington argued was simply a matter of terminology, Campbell described as far more fundamental.

The ether was also, unsurprisingly, mentioned in Lodge's contribution to the special *Nature* issue. Here he discussed 'The Geometrisation of Physics, and its Supposed Basis on the Michelson-Morley Experiment'.[82] Here, he argued that the results of the Michelson-Morley experiment could be explained using the methods of ether physics, and did not have to lead to such drastic changes as proposed by relativists. Lodge described relativity theory as a method, and argued that while this method was a 'remarkable achievement', it could not replace efforts to uncover actual truth. He insisted that 'a physicist is bound in the long run to return to his right mind; he must cease to be influenced unduly by superficial appearances, impracticable measurements, geometrical devices, and weirdly ingenious modes of expression; and must remember that his real aim and object is absolute truth, however difficult of attainment that may be, that his function is to discover rather than to create, and that beneath and above and around all Appearances there exists a universe of full-bodied, concrete, absolute, Reality'.[83] Campbell depicted this move away from theories of truth and towards predictive methods as part of the progress of physics. Lodge, however, portrayed it as merely a transitional period, thus representing relativity theory as still being in the early tentative stages of development. As we shall see, this depiction of an uncertain transition is evident throughout Lodge's popularisations of 'modern' physics.

It is notable that Lodge was in this issue in the first place, given the staunch opposition to relativity expressed in his article, and indeed allowed to submit an article twice the size of any of the other contributors. This reveals that professionally his views were seen as valid. Furthermore, I suggest that his 'public' attitude to relativity theory was also changing at this time. Where previously he had set himself firmly in opposition of the relativists, with his 'controversial note' and warnings of revolution, now Lodge was beginning to present himself as holding views to some extent compatible with theirs. On 31 October 1921, Lodge gave a lecture to the Liverpool Philosophical and Literary Society, later published as *Relativity: A Very Elementary Exposition* in 1925.[84] Here he

[81] Campbell (1921b).
[82] Lodge (1921b).
[83] Lodge (1921b), p.800.
[84] Lodge (1925).

detailed exactly where he 'differ[ed] from relativists'. While they considered discussions of absolute motion to be meaningless, Lodge believed that 'talk of absolute motion with reference to nothing at all, is meaningless, but I think that we have a standard, and that standard is the ether of space'.[85] For all 'practical purposes', the ether could serve as the standard of rest. While there was difference of opinion among relativists as to the existence of the ether, Lodge here argued that they had not 'abolished' it, but rather ignored it, 'because it is not necessary to their system'.[86] While Lodge's views were not in agreement with those of the relativists, his work was not inconsistent with theirs, but rather entirely separate. This stance, not in direct opposition to relativity theory, allowed Lodge to present himself as a suitable populariser of the subject.

Indeed, it meant that, in public, Lodge's views could be conflated with those of more 'modern' physicists. In 1923, speaking as President of the British Association, Rutherford felt compelled to comment on the popular narrative of revolution ascribed to relativity theory. He declared that 'it was an error far too prevalent to-day that science progresses by the demolition of former well-established theories' and that the oft-stated assertion that Einstein had overthrown Newton's gravitational work was simply not true. Einstein had generalised and broadened the basis of Newton's work, proceeding in a 'typical case of mathematical and physical development', whereby 'a great principle is not discarded but so modified that it rests on a broader and more stable base'.[87] This speech was broadcast on the wireless, and reported in the daily press.[88] There was concurrence from Oliver Lodge, labelling as 'nonsense' the view 'stated in the press that Einstein has exploded Newton'. He argued that what 'our scientists are doing is supplementing Newton, which is a legitimate thing to do.'[89] Rutherford and Lodge's views were here depicted as identical, concealing a deeper separation. Rutherford had little interest in the consequences of relativity for his own work; it had no bearing on his experimental research into subatomic matter. Professionally, Rutherford was dismissive of the theory, referring to research on the subject as 'excursions into Topsy-Turvy land', and warning his students 'Don't let me catch anyone talking about the Universe in my

[85] Lodge (1925), p.19.
[86] Lodge (1925), pp.19-20.
[87] *Report of the British Association for the Advancement of Science* (1923).
[88] Wireless broadcast referenced in Hennessy and Hennessy (2005), p.282; Reported at the time in *The Manchester Guardian* (13 September 1923), *Daily Mail* (13 September 1923), *Liverpool Echo* (13 September 1923).
[89] 'Galileos and Keplers in our midst', *The Manchester Guardian*, 20 September 1923, p.10.

department'.[90] However, he was not fundamentally opposed to the consequences of the theory, as Lodge was, but rather simply not interested, viewing the theory as irrelevant to his own work. Where Rutherford was repeating the arguments promoted by British relativists, Lodge's approach, on the basis of his views presented elsewhere, can be interpreted slightly differently. He may have been suggesting that relativity was compatible with the Newtonian approach he was taking; rather than actually building on Newton's work, relativists were 'supplementing' it by exploring very different avenues. This is not necessarily the same as Rutherford's modification. In their public pronouncements during the British Association, however, any difference between Lodge and Rutherford's views was not evident. The lines between 'classical' and 'modern' physicists (represented here by Lodge and Rutherford) were thus blurred. Indeed, the case of Rutherford reveals the variety of definitions of 'modern' during this period. He dismissed relativity theory and engaged with quantum theory only when it was relevant to his reductionist physics of atomic structure, as with his collaborations with Bohr in 1912 and 1913.[91] He would however come to be seen as a 'modern' physicist, and Lodge as 'classical'. But in their 'public' responses to relativity theory, there was not always a clear distinction between the two.

The relativity issue of *Nature* reveals a lack of consensus among physicists as to the significance of relativity theory, its permanence in physical theory, and the existence and nature of the ether. Furthermore, with Lodge's management of the *Philosophical Magazine*, here attempts to reconcile the results of the eclipse expedition with the ether were promoted. However, public statements, such as those made by Lodge and Rutherford, concealed the differences and presented instead a unified view. Indeed, while physicists were still debating fundamental aspects of the theory, they were also writing popular expositions. These could be used to promote a particular approach to both relativity and physics in general, suggesting consensus where there was none. As I have already noted, Eddington's *Space, Time and Gravitation*, an early popular book on the subject, carefully depicted the theory as both revolutionary and the natural continuation of conventional approaches in physics. The contested state of general relativity theory in British physics gave popular expositions a significant role in the formation and presentation of scientific consensus.

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[90] Rutherford to Joly, 12 January 1920 (2312/362) JOLY; De Bruyne (1984), p.87. de Bruyne graduated from the Natural Sciences Tripos in 1927, and worked at the Cavendish until 1931.
[91] Bohr worked on his quantum model of the atom during a stay in Rutherford's Manchester laboratory.

4.4 James Rice as an alternative relativity enthusiast

The supporters of relativity featured in this chapter so far had all received much of their physics training at Cambridge University.[92] In this section I consider the popularisation work of another relativity enthusiast with a different institutional background. James Rice, senior lecturer in physics at Liverpool, actually had more in common institutionally with Lodge, who had been professor of physics and mathematics there from 1881 to 1900. Rice was a fervent writer on relativity, producing both a textbook and a popular exposition, but not a researcher, publishing no academic papers on the subject. I consider how Rice's background and approach to physics affected his work on relativity. Rice also felt the need to defend the theory against accusations of revolution, but he did this in a different way from Eddington. Again, the notion of scientific progress was used, but Rice's definition of progress differed considerably from Eddington's.

James Rice was educated at Queens University, Belfast and the Royal University of Ireland before being appointed Senior Physics master at the Liverpool Institute, a prestigious grammar school, in 1902.[93] In 1914, he became a senior lecturer in physics at the University of Liverpool, and was made associate professor in 1924. Upon arrival at the university, Rice quickly became involved with the student's Physical Society, taking the position of honorary Vice-President that same year. Here, he introduced students to quantum and relativity theory, and in 1917 two sessions of the society were devoted to Rice's exposition of relativity. He informed the audience that 'many physicists' now regarded 'Newton's Laws of Motion as only approximately true'.[94] In January 1920, amid increased interest in relativity following the eclipse expedition, Rice gave a series of nine public lectures on the subject.[95]

Rice was not content with delivering lectures to interested members of the public and the more specialist audience of the Physical Society. He also wanted to make relativity theory part of undergraduate education in physics. With this in mind, he prepared a textbook for physics students, published in 1923.[96] Here, he introduced the subject with reference to a 'revolution' which had taken place 'in the mental attitude which the

[92] Larmor, Campbell, Eddington, Crommelin.
[93] Obituary of Rice in Donnan (1936); For the Liverpool Institute Schools see Tiffen (1935).
[94] 'Minute Books of the Liverpool Physical Society', LIV.
[95] Rowlands (1990), p.268.
[96] Rice (1923).

physicist maintains towards the concepts which have been invented in the past in order to reach those broad generalisations which are the proud possession of his science’. He noted with regret that it was ‘only too easy to acquire the notion that the new knowledge is dealing out death and destruction to the principles won so laboriously since the time of Galileo and Newton’. He believed that this ‘disastrous misapprehension’ was the result of students being introduced to the theory only after completing their undergraduate education, and thus having to suddenly readjust their understanding of basic physical concepts. Rice thought that instead the students’ minds should be ‘gradually adapted to the new idea as the usual University courses are pursued’.[97] Rice was thus also intent on minimising the destructive elements of the ‘revolution’ caused by relativity theory. The nature of physics education constructed a divide between ‘classical’ and ‘modern’ physics, presenting the latter as fundamentally different from the former, and Rice was eager to bridge this. He believed that by absorbing the theory into existing education, such a divide would be revealed as artificial.[98]

In 1927, a more intentionally popular exposition of Rice’s was published, in the Benn’s Sixpenny Library series. This series produced short, cheap books on a variety of subjects, each written by a specialist ‘expert’ author.[99] Rice here gave an overview of the theory, whilst also discussing the nature of scientific progress. He agreed with the ‘man-in-the-street’ that relativity did deny ‘common sense’, because common sense was based on ‘common experience’, but that this did not mean the theory denied truth.[100] He suggested that over the past fifty years, experimenters had become more skilled, and apparatus more precise, and as such the knowledge obtained by physicists was no longer in accord with the common experience of mankind. The ‘public’ thus now needed to trust the ‘experts’, who were not plucking their theories out of thin air, but rather basing them on more accurate empirical evidence than ever before. Physics was very much grounded in reality, even more so than before because physicists now had greater access to this reality. Rice was presenting a narrative of progress, one based on the improvement of scientific apparatus and precision measurement. In this respect relativity theory was not a fundamentally new kind of physics, it was simply the old physics improved.

[97] Rice (1923), p.v.
[98] Warwick (2003) has revealed the crucial role played by pedagogy in the development of a scientist’s approach to new ideas. See also Kaiser (ed.) (2005) for essays on the relations between pedagogy and the practice of science.
[99] For Benn’s Sixpenny Library, see Bowler (2009), pp.75-95.
[100] Rice (1927), pp.5-6.

Rice also played down the notion of 'revolution' by pointing out that only three hundred years ago, Galileo and Kepler had been 'defying reason', and Newton had helped mankind 'break away from the last traces of medievalism in science and accept as "reasonable" a revolution in ideas about the universe far more catastrophic than that change in outlook to which men are being urged at present'.[101] Thus, when compared with Newton's work, 'modern' physics was not really a revolution at all. Furthermore, the Newtonian scheme had already contained 'a limited kind of relativity, known as "mechanical relativity"', and all Einstein had done was expand this notion, thus building on the work of Newton. Rice made his point explicit: 'This should serve to forewarn the reader against the belief, fostered in quarters where sensationalism pays, that Einstein's work in some mysterious way has destroyed Newton's. The absurdity of such a suggestion will only be too apparent as we proceed. Two centuries of experiment and mathematical analysis lie between the two men, and Einstein stands on the shoulders of the greatest scientific man who has ever lived.'[102] Just as his textbook was designed to counter notions of revolution, so too did Rice's popular exposition make clear that any threat to Newton's legacy was merely illusory.

Rice also used his book to convey the purpose of science as a morally worthwhile pursuit of truth. He noted that the 'average man' might be astonished by physicists' concern with the minute difference between theoretical prediction and empirical evidence which had led to the formation of relativity theory. Perhaps such a man might accuse such a concern as being indicative of 'precision measurement gone mad'.[103] However, Rice argued, this was not merely a technical discrepancy, but evidence of an error in the theory. Physicists were not happy to simply accept something which mostly worked: they were seeking truth, and any errors revealed that truth had not been found. Rice was reinforcing his narrative of progress, of physics moving forward through the 'satisfactory removal of those minute discrepancies between theory and experiment, which worried the physicists for a generation'.[104] Theory had progressed from Newtonian laws to relativity theory through the straightforward process of obtaining ever more precise experimental evidence and working to construct theories which matched this evidence as accurately as possible. This was how truth was achieved.

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[101] Rice (1927), pp.6-7.
[102] Rice (1927), p.8.
[103] Rice (1927), p.14.
[104] Rice (1927), p.76.

Notably, this was exactly the same model of progress as depicted in Rice's other contribution to the Sixpenny Series, *An Introduction to Physical Science*.[105] Here he detailed nineteenth century developments in physics, stopping at the 'threshold of the revolutionary changes' of the twentieth century.[106] Again, he emphasised the fundamental role of 'exact definition and measurement'.[107] In the practice of science, there was thus a clear continuity between the 'classical' and the 'modern'. While the 'revolution' produced dramatically different new results, the process remained the same.

Rice was perhaps an unexpected promoter of 'modern' physics. He was educated in Ireland and spent his early career affiliated with a school rather than a university. His background in teaching was reinforced on his move to Liverpool. From 1900, the physics department here was under the management of Lionel Wilberforce, who promoted the importance of both research and teaching.[108] It seems likely that Rice, a schoolteacher, was hired on the basis of his strength with regards to the latter. In accord with his background, Rice's interest in relativity theory appears to have been almost entirely pedagogical, and he published very few research papers on any area of physics during his career. I can find only four, published in the *Philosophical Magazine* between 1914 and 1925.[109] He also made two contributions to the *Transactions of the Faraday Society*, but these are more summaries and expositions of existing literature than unique research.[110] The remainder of his output was pedagogical, and as well as the books already mentioned, he wrote two appendices on quantum theory for a textbook on physical chemistry, and an introduction to statistical mechanics.[111] Rice was, in this respect, not so much a practising relativity physicist, but rather an educator on the subject. Rice's aim was to integrate relativity theory into university education.

Furthermore, his emphasis on the importance of precision measurement in the development of the theory can be interpreted as partly a result of his existing exposure to physics. As noted in Chapter Two, much of university physics in the early twentieth century was directed towards teaching and precision measurement. Rice was thus incorporating relativity theory into this existing framework, depicting it as part of the

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[105] Rice (1928).
[106] Rice (1928), p.76.
[107] Rice (1928), p.63.
[108] Hughes (2005), pp.271-3.
[109] Rice (1914, 1915a, 1923b, 1925).
[110] Rice (1915b, 1926).
[111] Lewis and Rice (1918); Rice (1930).

successful tradition of precision measurement, through which scientific progress was achieved. As such, there was no need to separate the 'classical' from the 'modern'; it was all simply physics. Furthermore, we find in Rice's writing a very different explanation of how physics progressed to that of Eddington. Both physicists were responding to, and rejecting, claims of a destructive revolution, and their solution was to emphasise a continuity of practice, arguing that the nature of physics as a discipline had not changed. The same methods were continuing to produce results. However, Rice and Eddington's characterisations of these methods differed considerably, with Rice discussing precise experimental work and Eddington, a Cambridge wrangler, describing mathematics. This difference was a result of their diverging backgrounds and approaches to physics in general.

4.5 Oliver Lodge as an expounder of 'modern' physics

I have shown that the discussions of relativity theory found in *Nature*, popular books and non-specialist magazines contained repeated allusions to a separation between an old and new kind of physics. Lodge emphasised the extent of this separation, in order to warn physicists against abandoning their past, and promoted a version of relativity theory which had less revolutionary consequences. Eddington and Rice conceptually minimised the difference, presenting the new physics as a natural progression of the past in order to deflect accusations of the destruction of past theories. The problem of revolution was not distinct to relativity theory, as seen in the previous chapter. Moving beyond relativity, I consider the popularisation of 'modern' physics more generally. In this section I consider Lodge's role in such ventures, proposing him as an alternative to other, more apparently 'modern', popularisers.

Eddington was one such 'modern' populariser, most notably in 'public' discussions of relativity theory. Looking beyond this area of physics, however, there are other examples, promoting a different 'modern' physics. In his capacity as Fullerian Professor of Chemistry (and Director of the Davy-Faraday Laboratory) at the Royal Institution, William Bragg worked to promote the value of scientific research and teaching. His 'public' communications included broadcasts on the BBC, talks to various industrial audiences, and speeches at luncheons and dinners. Throughout, he carefully linked science to industry, celebrating the moral virtues of pursuits for knowledge, but also emphasising how the products of such a pursuit were utilised by industry. He campaigned for a closer relationship between science and industry.[112] Rutherford, meanwhile, was continuing to promote the products of his reductionist school of physics. In his 1923 British Association presidential address, he praised the 'power of the scientific method' in 'extending our knowledge of Nature'.[113] Here he noted that one could 'confidently predict an accelerated rate of progress of scientific discovery, beneficial to mankind certainly in a material but possibly even more so in an intellectual sense'.[114] For Rutherford, knowledge was important for its own sake and the utility of science lay in its ability to reveal new information about nature, not in its practical applications. This approach was even more pronounced in the 1930s, when Rutherford fought against prevalent public notions of the potential for utilising nuclear disintegration to create a source of unlimited power.[115] Among the 'modernists', there was thus considerable difference of opinion as to what 'modern' physics was, and how it should be promoted.

Lodge differed from Bragg, Rutherford and Eddington in that he was not an active researcher in a scientific institution. Indeed, as I have discussed in this chapter and the previous one, Lodge's views were at odds with many of his peers. However, they continued to be widely heard. In 1913, as President of the British Association, he was provided with the opportunity to deliver an address in which he attacked many aspects of 'modern' physics. When relativity became a topic of major discussion, Lodge was invited to contribute to a special issue of *Nature* on the subject. As I shall explore in the remainder of this chapter, his name was not immediately associated with 'classical' physics. Indeed, he instead built up a reputation as one of the country's leading expounders of 'modern' physics, and this image was supported by many of his scientific peers. Lodge thus had a considerable amount of influence on how these categories were defined, and on the 'public' perception of the stability of 'modern' physics and the nature of scientific progress.

Lodge's popular expositions were not necessarily dismissed outright by his peers. In 1924, his *Atoms and Rays* was published by Ernest Benn.[116] Here, Lodge detailed 'current' knowledge about matter, discussing the structure of the atom, quantum theory

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[112] Bragg's science communication work at the Royal Institution has been explored by Hughes (2002b).
[113] *Report of the British Association for the Advancement of Science* 1923, p.23.
[114] *Report of the British Association for the Advancement of Science* 1923, p.24.
[115] This has been discussed most recently by Jenkin (2011).
[116] Lodge (1924).

and the nature of energy. Throughout, he described the ether as the fundamental 'cementing substance' that held everything together and was responsible for the transmission of energy.[117]The book was subsequently reviewed in *The Observer* by Edward Andrade, then Professor of Physics at the Artillery College in Woolwich. Andrade had worked with Rutherford in Manchester in 1914, conducting research on the gamma-rays emitted from radium. Andrade's studies were interrupted by the war, during which he served as an artillery officer, and on return he was appointed, on the advice of Rutherford, Professor at Woolwich.[118] However, while there were no opportunities for atomic research at the Artillery College, Andrade remained inspired by his work with Rutherford, and published a comprehensive textbook on *The Structure of the Atom* in 1923.[119] This book opened with a lengthy dedication to his former teacher, and made no reference to the ether.[120] Where Lodge used *Atoms and Rays* to promote an underlying continuity, Andrade portrayed matter and energy as discontinuous particles and quanta. There was much in Lodge's book for him to object to.

Nonetheless, Andrade's review of Lodge's *Atoms and Rays* was in parts extremely positive, praising Lodge's 'freshness, charm and polished simplicity of style'. He described the 'great skill and enthusiasm' with which Lodge discussed the quantum theory. However, he also warned the reader that Lodge was 'rather unorthodox . . . in his constant reference of everything back to the ether'. He remarked that physicists had barely any knowledge at all about the ether, and knew simply that, as Einstein had showed, it 'has not got any mechanical properties, which rather spoils its usefulness'. Furthermore, Andrade suggested that Lodge had fundamentally misunderstood some aspects of the development of modern theories, attributing Rutherford's experiments on the scattering of alpha particles to C. G. Barkla. Indeed, Andrade believed that there was 'a certain amount of confusion in the description of the scattering experiments themselves'.[121] This confusion was confirmed in subsequent correspondence with Lodge, who thanked Andrade for his kind review, and insisted that he remembered Barkla 'working at Scattering long ago' and had 'regarded him rather as the pioneer in that branch'.[122] Andrade rightly pointed out that Lodge was confusing X-rays with alpha

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[117] Lodge (1924), p.12.
[118] Obituary in Cottrell (1972).
[119] Andrade (1923).
[120] 'A Dedication to Sir Ernest Rutherford' in Andrade (1923).
[121] E. N. da C. Andrade, 'Books of the Day: The New Physics', *The Observer*, 10 August 1924, p.5.
[122] Lodge to Andrade, 13 August 1924, LODGE.

rays, and that Barkla had only ever worked with the former.[123] This was rather a significant error to have made in an exposition about 'modern' physics, and yet Andrade had not attacked the book in the *Observer*, simply suggesting that the reader needed to be careful to differentiate 'the certain' from 'the less certain'.[124] It would seem that he believed enthusiasm to be of more importance, in the popularisation of science, than accuracy. Indeed he ended his review by praising Lodge's 'power of communicating the fascination of scientific research which no other author, perhaps, possesses to the same extent'.[125] Lodge's role was thus not to communicate the details of 'modern' physics, but rather its value. Andrade was happy for Lodge to introduce the 'public' to these topics, as long as they were aware that some of the details were a little controversial. Indeed, in A*toms and Rays*, Lodge himself asked students to 'regard the present volume as introductory to more advanced treatises', including Andrade's.[126]

Atoms and Rays received a more positive review in *Discovery*, a magazine for a general reader interested in the latest research, both scientific and non-scientific.[127] The editor until1921 was A. S. Russell, a reader in chemistry at Christ Church, Oxford, who had worked with Soddy in Glasgow, and Rutherford in Manchester on the chemistry of radioactive elements.[128] Even after resigning from his post as editor in 1921, Russell continued at the magazine as a scientific adviser.[129] It is thus likely that he was responsible for the review of Lodge's book in 1924, or at least approved of the judgement. Here, it was declared that to 'the student of Physics, as well as to everyone who is interested in Physical Science, the appearance of a new publication by Sir Oliver Lodge is always a memorable event'.[130] As with Andrade, here we have another former colleague of Rutherford's promoting Oliver Lodge's ether-centric 'modern' physics as a valuable exposition. Lodge's ability as an accessible writer was again seen as more important than the particular view of physics he held, or technical mistakes he made.

Throughout the 1920s, Lodge continued to adopt the role of author of introductory texts on 'modern' physics. In 1924, the Royal Society organised a display of 'Pure Science' for the British Empire Exhibition. I explore this exhibition in depth in Chapter Six,

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[123] Andrade to Lodge, 23 August 1924, LODGE.
[124] E. N. da C. Andrade, 'Books of the Day: The New Physics', *The Observer*, 10 August 1924, p.5.
[125] E. N. da C. Andrade, 'Books of the Day: The New Physics', *The Observer*, 10 August 1924, p.5.
[126] Lodge (1924), p.vii.
[127] For *Discovery*, see Bowler (2009).
[128] See Russell's obituary, Anonymous (1972).
[129] 'Editorial Notes' (1921).
[130] 'Reviews of Books: Atoms and Rays' (1924).

considering how it influenced collecting policy of 'modern' physics at the Science Museum. However, there is one aspect of the organisation of this event that is of particular relevance to this chapter. The exhibition ran for six months in 1924, and then was reopened from May to October 1925. For the second showing of the exhibition, Lodge was appointed Vice-Chair of the organising committee, not to help with the exhibition itself, but rather to write an introductory article for the publication *Phases of Modern Science*, which was to be produced concurrently with the exhibition.[131] Indeed, Lodge made it clear on appointment that he would be too busy for any meetings.[132] The exhibition itself was organised by practising physicists, who constructed models or loaned apparatus they had themselves used. Lodge, who had not conducted physical research for 25 years, was not asked to help organise the exhibition, but his reputation as a skilled writer on accessible 'modern' physics meant that he was much in demand for the handbook.

Similarly, Lodge's contribution to Benn's Sixpenny Library series was also an introductory overview of the field. His *Modern Scientific Ideas: Especially the Idea of Discontinuity* was the published version of a series of radio talks on 'Atoms and Worlds', broadcast in 1926.[133] Here, as the title indicates, Lodge continued with his regime of promoting continuity. While he acknowledged that 'modern science has recently brought to light a great many examples of discontinuity', he insisted to his readers that '[c]ontinuity remains the fundamental idea to which scientific philosophy will in the last resort return'.[134] Thus, his discussion of quantum energy was centred on an exploration of how this new phenomena could explain interactions between matter and the ether. And more generally, Lodge depicted modern physics as being in a state of transition, awaiting its 'Newton' to provide a full (and continuous) explanation. In his exposition, both the relativity and quantum theories were useful tools for the accumulation of data, but they were not the end-point of physics. Such attention to the ether was, as Andrade had already noted, 'unorthodox'. Andrade himself provided an

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[131] Lodge's appointment (and the article he is to write) is referred to in a letter from Thomas Martin (Secretary of the British Empire Exhibition) to F. E. Smith, 25 February 1925, File: F. E. Smith, British Empire Exhibition 1924 Correspondence, RS; The decision to appoint him came out of the Publications Subcommittee – File: Publications Subcommittee, British Empire Exhibition 1924 Correspondence, RS; Lodge is listed as Chair in Royal Society (1925).
[132] Thomas Martin (Secretary of the British Empire Exhibition) to Lodge, 21 October 1925, British Empire Exhibition 1924 Box 2, RS.
[133] Lodge (1927).
[134] Lodge (1927), p.13.

alternative account with his *The Atom*, also published in the Sixpenny library.[135] Here, he stated that the 'atomic, or granular, nature of things seems to prevail, as against the non-atomic, or continuous, nature, wherever we turn in our quest for fundamental facts'.[136]

An anonymous review in *Nature* noted that the two books covered similar ground, and that it would thus be 'of considerable interest to note the varying manner of treatment of the same material by two decidedly individualistic writers'.[137] Notably, Andrade's book was not depicted as being more accurate, or even 'better', than Lodge's, but simply different. In a review in *Discovery*, the two books were presented as complementary.[138] The review was written by V. E. Pullin, Director of Radiological Research at the Royal Arsenal in Woolwich (an institutional neighbour of Andrade's Artillery College). Here, Pullin reviewed Lodge and Andrade's books, as well as Rice's contribution on relativity, which together comprised the physics output of the Sixpenny series. Pullin spent only a sentence discussing Lodge's book, devoting far more space to the other two. However, he described *Modern Scientific Ideas* as 'an excellent preamble' to these more specialised books, providing an overview of modern physics. Again, Lodge, a very public critic of many aspects of both quantum and relativity theory, was considered suitably qualified to write such a preamble. Indeed, Pullin declared that to 'acclaim Sir Oliver Lodge as an expounder of modern science would be to gild the lily'.[139]

Lodge was assisted in his efforts at public communication of science by Peter Chalmers Mitchell. Throughout the 1920s, Mitchell corresponded frequently with Lodge, predominantly on the subject of spiritualism. He was in opposition to Lodge's views on the subject, describing himself as a 'materialist', but engaged in friendly debate.[140] And Lodge's name cropped up again and again in Mitchell's column. In an article on the transmutation of metals, Mitchell recalled 'a famous dictum of Sir Oliver Lodge' on the amount of energy contained within a piece of chalk.[141] Discussing low temperature research, Mitchell referred to a recent suggestion by Lodge that the changes in properties

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[135] Andrade (1927).
[136] Andrade (1927), p.65.
[137] 'News and Views' (1927), p.827.
[138] Pullin (1928).
[139] Pullin (1928), p.165.
[140] Mitchell to Lodge, 9 August 1927, LODGE.
[141] 'The Progress of Science. Transmutation of Metals. "Synthetic Gold"', *The Times*, 20 December 1921, p.8.

of matter at very high temperatures might also occur at the opposite end of the scale.[142] When writing on discussions concerning hydrogen as the 'primitive element', Mitchell used Lodge's explanation on the matter, which 'brings in the most difficult theories in modern physics'.[143] Despite having no research experience in these areas, Lodge was an authority on 'modern physics'. Indeed, in a 1925 article on the radioactivity work underway in Cambridge on the disintegration of matter, Mitchell quoted Lodge's discussions of Rutherford's work, rather than going to the source itself.[144] Lodge was repeatedly treated as a general expert on 'modern' physics. He obtained this authority not through his proximity to the research itself, but through the reputation and status he had built up over many years. The importance of prestige and relationships in the perception of scientific authority is discussed in the next chapter.[145]

Mitchell was a Fellow of the Royal Society, but with regards to physics he was an amateur. In a column on quantum theory, he wrote that, in an attempt to become acquainted with 'the conceptions of modern physicists', he had been 'reading once more two books written by two very distinguished physicists, Sir Oliver Lodge's "Atoms and Rays" and Mr. Bertrand Russell's "The ABC of Atoms"'.[146] Neither Lodge nor Russell were researchers into quantum theory, and indeed Russell was not really a physicist, but rather a logician and mathematician. As a non-physicist, Mitchell no doubt found Lodge's expositions more accessible than more in depth offerings, such as Andrade's 1923 textbook. Furthermore, his friendship with Lodge meant that he was also more inclined to include Lodge's work in *The Times*. In1923, he informed Lodge that he had 'succeeded in persuading the *Times* to put in a small adaptation of part of your lecture [to the Rontgen Society]'.[147] One of Mitchell's columns in 1927 was devoted entirely to Lodge's address on 'A Century's Progress in Physics', which had been delivered as part of University College, London's centenary celebrations.[148] Here, Lodge repeated the complaint he had been making for some time, at least since 1913, that there was a tendency in 'modern science' to consider physical laws in terms of utility rather than

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[142] 'The Progess of Science. Low-Temperature Research. Cryogenic Laboratories', *The Times*, 7 August 1923, p.8.
[143] 'The Progress of Science. Atoms and their Nuclei. Hydrogen as Primitive Matter', *The Times*, 30 March 1925, p.9.
[144] 'The Progress of Science. Atomic Systems. Disintegration of Matter', *The Times*, 27 April 1925, p.7.
[145] This issue has been explored by Shapin (1994) in a study of the trust relationships of seventeenth century gentlemen-philosophers.
[146] 'The Progress of Science. Ultimate Facts of the Universe. The Quantum Theory', *The Times*, 27 October 1924, p.19.
[147] Mitchell to Lodge, 9 November 1923, LODGE.
[148] 'Progress of Physical Science. Sir Oliver Lodge on Television', *The Times*, 15 March 1927, p.14.

ultimate truth. Lodge's relationship with Mitchell, combined with his skill as an accessible writer and speaker, allowed him to establish a reputation as an authority on 'modern' physics, and thus to promote his particular view of the subject, and of the nature of progress. Just as Eddington and Crommelin used contacts at *The Times* to promote their eclipse expedition, Lodge was able to utilise his friendship with Mitchell to gain exposure of his views.

4.6 Oliver Lodge and Arthur Stanley Eddington: defining 'modern' physics

I thus propose that Lodge's 'public' writings can be considered a 'classical' alternative to those produced by the likes of Eddington, Rutherford and Bragg. This suggests a more complex picture of the period, one which does not see the 'modern' physicists taking centre stage. As I have shown, Lodge himself was not necessarily regarded as a 'classical' physicist, and was not promoted as such by editors, journalists and even other physicists. Furthermore, his vision of modern physics differed considerably from those who practiced it; he saw quantum and relativity theory as providing interesting and useful methods and techniques, but by no means a true picture of reality. I end this chapter by comparing two expositions of 'modern' physics from the late 1920s, one written by Eddington and one by Lodge. Here we find two very different approaches to the notion of scientific progress, both of which found a 'public' audience.

The Eddington example is particularly revealing because it contains an attempt to actually define 'classical' physics, and thus explain what made this 'classical' physics different from the modern. This was done in his 1927 Gifford Lecture, in which he summarised 'the great changes of scientific thought which have recently come about'.[149] The lectures were published in a popular book, *The Nature of the Physical World*. As Michael Whitworth has shown, while *Space, Time and Gravitation* was considered by its publishers to be merely a 'non-technical' book, *Nature of the Physical World* was instead decidedly 'popular' and hugely successful.[150] Here, Eddington devoted his first chapter to a study of 'The Downfall of Classical Physics', a response to the kind of revolutionary rhetoric being promoted by Lodge. Indeed, a reference to accusations of 'Bolshevism' is presumably an allusion to Lodge's 1920 British Association discussion.

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[149] Eddington (1928), p.v.
[150] Whitworth (1996).

Eddington described the ‘downfall’ as beginning not with relativity or quantum theory, but with atomism. Indeed, he declared that ‘when I hear to-day protests against the Bolshevism of modern science and regrets for the old-established order, I am inclined to think that Rutherford, not Einstein, is the real villain of the piece’:

> ‘When we compare the universe as it is now supposed to be with the universe as we had ordinarily preconceived it, the most arresting change is not the rearrangement of space and time by Einstein but the dissolution of all that we regard as most solid into tiny specks floating in void. That gives an abrupt jar to those who think that things are more or less what they seem. The revelation by modern physics of the void within the atom is more disturbing than the revelation by astronomy of the immense void of interstellar space.’[151]

By emphasising the revolutionary nature of this earlier, widely accepted, theory, Eddington was again minimising the revolutionary aspects of relativity here. His suggestion of Rutherford as an *alternative* ‘villain’ is also revealing of how Rutherford was being portrayed in other narratives of ‘modern’ physics; his work was not seen as destructive, whereas Einstein’s was.

The reason that the nuclear model of the atom had not been conceived of in the same way as relativity theory was because, according to Eddington, it was compatible with ‘classical’ physics, a category that he noted had not ever been ‘closely defined’. He proposed that classical physics included all theories and concepts which fitted into ‘the scheme of natural law developed by Newton in the Principia’.[152] This scheme now ‘broke down’ because relativity and quantum theory were incompatible with it. However, Eddington insisted that it was ‘absurd’ to think that Newton’s scientific reputation had been ‘shattered’ by Einstein, and that to imagine that ‘Newton's great scientific reputation is tossing up and down in these latter-day revolutions is to confuse science with omniscience’.[153] Ultimately, Eddington argued that the nature of progress demanded the acceptance of great changes. Scientists were continually altering their outlook, exploring old phenomena from new perspectives:

> ‘Scientific discovery is like the fitting together of the pieces of a great jig-saw puzzle; a revolution of science does not mean that the pieces already arranged and interlocked have to be dispersed; it means that in fitting on fresh pieces we have had to revise our impression of what the puzzle-picture is going to be like. One day you ask the scientist how he is getting on; he replies, "Finely. I have very nearly finished

[151] Eddington (1928), p.1.
[152] Eddington (1928), p.4.
[153] Eddington (1928), pp.201-2.

> this piece of blue sky." Another day you ask how the sky is progressing and are told, "I have added a lot more, but it was sea, not sky; there's a boat floating on the top of it". Perhaps next time it will have turned out to be a parasol upside down; but our friend is still enthusiastically delighted with the progress he is making.'[154]

Eddington was here denying that the revolution in physics had been destructive to the older theories, suggesting instead a process of modification. Nothing was completely rejected, but rather repositioned with relation to newer ideas. This was in accord with Eddington's general scientific outlook, as analysed by Stanley.[155] Eddington could promote 'modern' physics without destroying 'Newton's scientific reputation'. In Eddington's interpretation, Newton remained a fundamental part of the progress of physics.

The Nature of the Physical World sold more than two million copies in 1928, and more than seven and a half million the following year.[156] As Whitworth has declared, it 'lifted popular-science publishing to new heights'.[157] However, despite this success, Eddington was not the only voice of 'modern' physics. A year after Eddington's book was published, *Discovery* ran a series on the 'future of the sciences', as part of their 'endeavour to devote most of our space to new facts and ideas'.[158] Lodge was chosen to contribute on the subject of 'pure physics'.[159] Yet again, he was being presented as anything but a 'classical' physicist. In 'The New Outlook in Physics', he declared that the immediate problem was to 'weld together the newer and the older discoveries into an all-embracing system which shall include them all'.[160] Lodge described the present state of physics as 'bounded on the north by mathematics, on the south by experiment, on the west by accumulated experience of the past, and on the east by intuition and speculation'. While the east brought 'great promise for the future', there were still 'hopes of a clearer sky when the clouds have cleared away'. Physics was thus in a 'transitional period' covering 'what seemed the satisfying illumination of the nineteenth century and the vague uncertain brilliance of the twentieth'.[161] He ended with the declaration that: 'The work may have to go on for a century before the sun rises, but through the haze and mists of the twilight we catch a glimpse of a rosy and hopeful

[154] Eddington (1928), p.352.
[155] Stanley (2007b).
[156] Figures from Whitworth (1996), p.67.
[157] Whitworth (1996), p.65.
[158] 'Editorial Notes' (1929a).
[159] 'Editorial Notes' (1929b).
[160] Lodge (1929), p.109.
[161] Lodge(1929), p.109.

dawn'.[162] Lodge's interpretation of scientific change was very different to Eddington's. He promoted a notion of 'modern' physics as fundamentally incomplete, awaiting its proper reconciliation with 'classical' physics. For Lodge, there would ultimately be no divide as the newer ideas would combine with older frameworks. He thus had no need, as Eddington did, to emphasise the role played by 'classical' physics in the development of 'modern' physics because this had not yet been realised.

4.7 Conclusion

In this chapter I have tried to contribute to the existing literature on both the 'public' aftermath of the 1919 eclipse expedition and the popularisation of 'modern' physics more generally. While such studies have focused on the work of 'modern' physics, particularly Eddington, here I have considered the situation from an alternative perspective, proposing Oliver Lodge as a viable contender to Eddington's position as the principal expounder of relativity theory in the 1920s. Furthermore, I have argued that Lodge's influence carried beyond relativity theory and into expositions of 'modern' physics more generally. The status of Oliver Lodge as the voice of 'modern' physics has considerable implications as to how the discipline was promoted in 'public' spheres. Lodge promoted a very different idea of 'modern' physics, based on an idea of incompleteness, from that espoused by Eddington, Rutherford and Bragg.

Lodge himself knew that he was not entirely 'in touch' with the new physics, and this is certainly confirmed by his confusion over X-rays and alpha rays. He privately admitted to Andrade that the discipline had 'gone on so rapidly this century that it is difficult to keep pace with it, especially as for most years I have been occupied with University management, -- perhaps unduly occupied therewith.'[163] In 1923, the publisher Joseph Dent asked Lodge to write an accessible encyclopaedia of physics and chemistry. As documented by Haimes, Lodge spent the last fifteen years of his life working on this manuscript but never completed it. Haimes suggests that one of the reasons for incompletion was Lodge's lack of confidence: 'even in areas where he, himself, felt supremely confident, his views were not shared by the majority of younger, twentieth century physicists'.[164] Despite such reservations, Lodge continued to write for a general

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[162] Lodge (1929), p.112.
[163] Lodge to Andrade, 13 August 1924, LODGE.
[164] Haimes (1992), p.18.

readership, and his word continued to be taken as that of an expert. His ability as a science writer was promoted by the editors and writers of both science magazines and *The Times*, by the publishers who continued to support him, and by his scientific peers.

As such, Lodge's views were widely heard, despite them being, as noted by Andrade, hardly orthodox. This had the effect of emphasising the destructively revolutionary nature of relativity theory, in the wake of the eclipse expeditions, while others worked hard to defend themselves against such accusations. Notions of revolution, continuity and discontinuity subsequently featured in both 'public' and 'professional' discourse of 'modern' physics. The 'professional' rhetorics were influenced by the 'public', indicative of a dialogue model of science communication, rather than a deficit. Furthermore, Lodge described 'modern' physics as being in the very early stages of development, informing his readers that the discipline was currently somewhat unstable and unsure of itself. Regarding those who did promote relativity, quantum energy and discontinuity as the definitive future of physics, Lodge firmly placed himself in opposition to them. A general reader tackling both Andrade's *The Atom* and Lodge's *Atoms and Rays* or *Modern Scientific Ideas* would find himself faced with an image of physics in a state of controversy. Lodge's work, revealing starkly different views to those held by many of his peers, emphasised the lack of a consensus among physicists. His writing was not portrayed as the work of an out of touch outsider, but the product of a prestigious and capable communicator of the latest research. The result was a general picture of 'modern' physics as speculative, indefinite, and possibly only temporary.

With Lodge's editorship of the *Philosophical Magazine*, his authority extended beyond the 'public' sphere. He was able to not merely promote the value of research into the nature of the ether, but also provide a professional home for such work. The *Philosophical Magazine* thus fostered what we would now consider to be 'classical' physics, throughout the 1920s. In the following chapter, I consider the 'modern' alternative, the *Proceedings of the Royal Society of London*. In this journal, under the management of James Jeans, research into 'modern' physics was promoted and published. However, there was no consensus among physicists as to just what constituted 'modern' physics, and how to determine the credibility and value of a research paper. While 'modern' physics was being defined and promoted to the 'public', similar debates were underway in decidedly 'professional' circles, even as late as the 1920s. In both spheres, the broader question remains the same. I continue to explore how definitions of ‘modern’ physics were constructed, and again we find debates and negotiations underway between those physicists who would later come to be seen as ‘classical’ and those who would be ‘modern’.

Chapter Five: Networks, Negotiations and Prestige: Managing 'modern physics' in the Royal Society's Proceedings

5.1 Introduction

Steven Shapin's now classic study of social epistemology, *A Social History of Truth*, explored the role of relationships of trust in knowledge-making. Shapin argued that in the seventeenth century these relationships were built on genteel codes of conduct. He ended his study with a consideration of how these values had continued into modern times, suggesting that the trusted 'gentleman' had been replaced by an authoritative 'expert'. He argued that in the practice of science, authority still stems from relationships, and here 'patterns of institutional training and theoretical or practical affiliation do the work done for gentlemen by family and kin'.[1] In this chapter I examine these relationships at play in the management of the Royal Society's *Proceedings*.[2] In exploring how 'trust' was both gained and lost, I consider two principal modes of procuring authority. Many younger scientists acquired trust through the patterns of institutional training described by Shapin, forging relationships through a common pedagogical background and scientific outlook. Older, established scientists may have initially gained trust in this way, but some now maintained it through the status they had built up over a lengthy career. I have already shown this happening with Lodge, in 'public', but in this chapter I argue that the situation was similar in 'professional' circles. Crucially, as the discipline developed, scientists were often divided in their theoretical commitments and views of what physics should be. In the negotiations between older 'classicists' and younger 'modernists' the limits of trust are revealed. I will show that, once earned, trust could again be lost.

In the 1920s, the *Proceedings* was gaining a reputation as the top physics journal in Britain.[3] I propose that this was partly on the basis of its refereeing system, which gave the impression of a fair and objective assessment of paper submitted. However, as I will

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[1] Shapin (1994), p.415.
[2] From 1905, the *Proceedings* was split into two journals: Section A for mathematics and physical science, and Section B for biological science. Throughout this chapter, where I refer to the *Proceedings* I am indicating Section A, not Section B.
[3] Brock and Meadows (1998), p.163.

show throughout this chapter, the process of accepting or rejecting a paper into the *Proceedings* was strongly influenced by relationships of trust. In this way decisions were made in a similar fashion to the journal's closest competitor, the *Philosophical Magazine*, which did not have a formal refereeing system but instead a small group of editors, responsible for making decisions. However the Royal Society did not face the same accusations of mismanagement. Furthermore, while the *Philosophical Magazine* struggled to carve out a niche for itself in the landscape of physics journals, it began to be seen as a more inclusive journal, while the Royal Society was the home of a new orthodoxy. This chapter will show that the Royal Society's definition of orthodox physics was closely related to the definition of modern physics held by the network of physicists who managed the *Proceedings*. In this way, a more concrete definition was being established.

I begin with an outline of the landscape of physics publishing in the 1920s, considering where *Proceedings* was situated alongside the *Proceedings of the Physical Society* and the *Philosophical Magazine*. I show that the choice of where to publish a paper was not based solely on the perceived "quality" of a journal, but rather how its particular style and purpose fit in with the aims of the author. I then examine the organisational structure of the Royal Society's Physical Committee, responsible for the management of the *Proceedings*, suggesting that many decisions were made on the basis of trust. In the remainder of the chapter, I use a number of case studies to explore the consequences that these relationships of trust had on such decisions, and the *types* of physics which were promoted by this Royal Society "inner circle". Just as Lodge fostered research into the ether, James Jeans attempted to make the Royal Society's *Proceedings* the home of 'modern' physics, and he and his referees objected to papers which did not take into account quantum or relativity theory. In this chapter, I reveal the contested nature of modern, valuable, and credible physics. While attempts were being made to achieve consensus in 'public', the same negotiations were underway in professional publications.

5.2 Alternatives to the Proceedings*: the* Philosophical Magazine *and the* Proceedings of the Physical Society of London

In the 1920s there were four British journals dedicated to the publication of new research in physics: the *Philosophical Magazine*, the *Proceedings of the Physical Society of London*, the Royal Society's *Proceedings*, and its *Philosophical Transactions of the*

Royal Society A. While *Transactions* had originally been the Society's main journal, with *Proceedings* a vehicle for abstracts and society news, by the 1920s their roles had changed. In 1913, the Sectional Committee for Physics revoked Standing Order 42, which had allowed only papers appearing to 'mark a distinct step in the advancement of natural knowledge' to be recommended for the *Transactions*. It was noted that this created an 'erroneous impression as to the merits of papers published in the "Proceedings".' Instead the journals were differentiated on the basis of length, with *Proceedings* reserved for short papers, and the *Transactions* for 'communications in the nature of monographs and such as have numerous or elaborate illustrations'.[4] *Transactions* was more expensive and slower to produce, and as such was often not an author's first choice. James Jeans noted that a paper in the *Proceedings* would 'secure more rapid publication and much better publicity' than one in the *Transactions*.[5] Indeed, in 1925, William Lawrence Bragg explicitly requested that a paper he had co-written, 'The Crystalline Structure of Chrysoberyl', appeared in the *Proceedings* and not the *Transactions*.[6]

An alternative to both of these journals was the *Proceedings of the Physical Society*. This Society was, as I noted in Chapter One, consciously formed as an alternative to the Royal Society, with the intention of providing a space for the discussion of new experimental techniques and instrumentation. Furthermore, the meetings included physical demonstrations of apparatus and techniques, thus providing valuable insight that could not be gained from simply reading a paper. Initially, papers read at Physical Society meetings were published in the *Philosophical Magazine*, and then reproduced in the Physical Society's own *Proceedings*. In 1910, this practice stopped, and furthermore the Society introduced a rule whereby only papers read before the Society could be published in their own *Proceedings*.[7] This strong link between papers and meetings, which served to emphasise the value of physical demonstrations and the sense of community, continued up until the 1940s.[8]

The Proceedings of the Physical Society had a clearly defined role, distinct from that of both *Proceedings* and *Transactions*, and this can be seen in discussions over where particular papers should be published. In 1930, Ezer Griffiths, Secretary of the Physical

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[4] 'Physics and Chemistry Sectional Committee Minutes', 22 May 1913, RS.
[5] JH Jeans to OW Richardson, 27 October 1922, OWR.
[6] JH Jeans to OW Richardson, 12 November 1925, OWR.
[7] W. Francis to John Joly, 24 October 1910, JOLY; Lewis (1999), p.31.
[8] Lewis (1999), pp.138-9.

Society from 1929 to 1937, was asked to review a paper submitted to the Royal Society. He suggested it would be more suitable for the Physical Society, because it followed on from an earlier work published there, the results contained within were not final, and there did not 'appear to be any particularly novel feature to single it out for publication in the Proceedings of the Royal Society'.[9] While Griffiths was subsequently accused (by the communicator of the paper) of institutional bias, a second referee agreed with him, and the paper was published in the *Proceedings of the Physical Society*.[10] Similarly, when William Bragg read through a paper of X-ray work by Harold Pealing, he noted that he would rather take it to the Physical Society than the Royal Society, as they could get a 'good discussion there and I could get a paper of my own in at the same time', allowing for a more interesting and useful meeting. This was no reflection on quality, as Bragg noted that if Pealing preferred, he could certainly get it in to the *Philosophical Magazine* or the Royal Society's *Proceedings* with ease.[11] The opportunity afforded by the Physical Society for discussion was not always welcome, and O. W. Richardson admitted that many of his students, while capable experimenters, struggled to express themselves verbally, and so were 'not attracted by the prospect of having to make a long lecture about the paper such as the Physical Society loves'.[12] By the 1920s the Physical Society had carved out a niche for itself as a place for open discussion, demonstrations of experimental developments, and tentative results.

This idea of the various journals as cooperating rather than competing is reinforced by the fact that the Royal Society provided a great deal of financial support to the Physical Society.[13] Furthermore, many physicists were involved with the management and selection process of multiple journals. George Carey Foster, editor of the *Philosophical Magazine* from 1911 to 1919, was one of the founders of the Physical Society. O. W. Richardson served as president of the Physical Society from 1926 to 1928 (and thereafter as honorary foreign secretary), but also as Chair of the Physical Committee of the Royal Society from 1925 to 1926, and again from 1929 to 1930. While in his Royal Society position, the Physical Committee rejected a paper that he himself had communicated, and he subsequently sent it on to the *Philosophical Magazine* (where it

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[9] FE Smith to OW Richardson, 2 October 1930, OWR.
[10] FE Smith to OW Richardson, 2 October 1930, OWR; 'CH Lees Referee Report 1930' (No. 152), RS; Details about referees are listed in the 'Register of Papers', RS.
[11] WH Bragg to H Pealing, 11 March 1921, BRAGG.
[12] OW Richardson to A Ferguson, 31 December 1929, OWR.
[13] Copy of draft letter 'Secretary of the Physical Society to Treasurer of the Royal Society', 1926, RS.

was approved).[14] The Richardson example suggests that the *Philosophical Magazine* might have been seen as a place to send papers not good enough to get into the Royal Society's journals. This view was reinforced by judgements against the *Philosophical Magazine*'s lack of a refereeing system. However, as I will show in this chapter, quality was not the only feature considered by the Royal Society's referees and committee. But the absence of formal referees may have affected the reputation of the *Philosophical Magazine*.

This reputation was under threat throughout the early twentieth century. The *Philosophical Magazine* was struggling to distinguish itself as an alternative to *Proceedings* (except as a last resort), particularly after the Physical Society had adopted the role of publishing experimental techniques. Throughout the early twentieth century, a number of accusations were made against the journal, and much of this was on the basis of its organisational structure. In the 1920s, the editorial committee consisted of two members of the Taylor & Francis publishing family, W. Francis Jr. and Richard T. Francis. They were accompanied by John Joly, Oliver Lodge, J. J. Thomson and George Carey Foster.[15] There was no official refereeing system, although this was carried out informally, with the editors passing on papers they felt ill-equipped to judge. As I shall show, the process at the Royal Society was in fact similar, but was cloaked under layers of rules and formalities.

Furthermore, not everybody thought that formal refereeing was the best approach. When, in 1910, John Joly invited Joseph Larmor to join him as an editor, the response was not positive. Larmor suggested that while the *Philosophical Magazine* now had some good content, it also had 'much more that is poor'. Crucially, Larmor felt that '[m]uch will depend on the success of the new volume of the Phys. Soc'. He thus saw the Physical Society as the journal's competition, not the Royal Society. Indeed, he suggested that following the Royal Society's lead of having formal referees would 'close the door too tight'. Instead, Larmor proposed the creation of an editorial board which would represent a number of British societies. Lord Rayleigh also felt this was the best option, but it was not adopted by Taylor & Francis.[16]

[14] The paper was Curtis (1926); I discuss this later in the Chapter.
[15] Brock and Meadows (1998), p.213.
[16] Larmor to Joly, 6 November 1910, JOLY.

Criticisms of the *Philosophical Magazine's* editorial system continued to surface. In 1921, the National Union of Scientific Workers drafted a widely circulated letter expressing doubts about the involvement of the editorial board in the management of *Philosophical Magazine* content. Lodge responded to this letter, which was 'virtually attacking the management of the *Philosophical Magazine*', in the pages of *Nature*. He insisted that 'the referees mentioned on the title-page of that journal are frequently consulted, and that their services are not so nominal as the writers of the circular suppose'. He continued by declaring that he personally believed the *Philosophical Magazine* was 'well managed; that a conservative attitude towards old-established organs is wise; and that it is possible to over-organise things into lifelessness.'[17] As he had done in 1913, Lodge was again adopting the rhetoric of conservatism to suit his particular purpose.

By the middle of the 1930s, the *Philosophical Magazine*'s reputation was still in trouble, and its editorial process lay at the root of the problem. When, in 1937, Patrick Maynard Stuart Blackett, a former Cavendish experimentalist, was offered a role as one of four editors, he turned it down, revealing that he personally thought the journal had 'lost its great position that it used to hold and should still hold', and that this was putting a great deal of pressure on the Royal Society's *Proceedings*. He put much of the *Philosophical Magazine's* recent failures (although he did not specify exactly what the journal was doing wrong in terms of actual content) down to a lack of a formal refereeing system and the fact that it was owned by a commercial publishing company and not a scientific society (this latter flaw he felt contributed to the former, as scientists were less willing to take the time to review papers for a non-scientific company).[18] J. J. Thomson, one of the *Philosophical Magazine's* own editors, was also dissatisfied with the journal, writing in a private letter that unless it improved, he no longer wanted to be associated with it. However, like Larmor, he did not see refereeing as the way forward, instead proposing a paid editor who was energetic, active and business like, with a 'wide knowledge of physics including modern physics'.[19]

Thomson appears to have perceived the problem as being that the *Philosophical Magazine* was not 'modern' enough to provide a suitable alternative to the *Proceedings*. However, the fact that neither he, Larmor or Rayleigh supported the adoption of a formal

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[17] Lodge (1921a).
[18] PMS Blackett to WH Bragg, 8 November 1937, HILL.
[19] J. J. Thomson to anon, c. 1935, THOM (the recipient is presumably Taylor & Francis).

refereeing system suggests that this was a positive way for the *Philosophical Magazine* to distinguish itself. A less critical approach to the acceptance of papers did not necessarily result in a journal of poorer quality, but instead created the opportunity for a wider variety of content. And the publication process could be sped up considerably with the absence of Royal Society bureaucracy. As Lodge's letter in *Nature* suggested, excessive organisation could be to the detriment of a publication.

In this overview of the main three British physics journals available in the 1920s, I have suggested that they were intended not to work in competition with each other, but rather to each serve a specific purpose. In the remainder of this chapter I shall consider in depth the case of the Royal Society's *Proceedings*, suggesting that some of those responsible for its organisation were working to provide it with an identity as a 'modern' physics journal. This occurred under the helm of James Jeans, who served as the society's Physical Secretary from 1919 to 1929, and led a 'vigorous campaign' to attract a higher standard of papers.[20] He was subsequently praised by Rutherford, then president of the Royal Society, for increasing both the quantity and quality of papers published.[21] I argue that rather than representing the epitome of an objectively refereed quality journal, the content of the *Proceedings* was directed by relationships of trust. Where differences of opinion arose (generally between a referee and a communicator) as to whether a paper should be published, the outcome was the result of a choice about who should be trusted and who should not. In this way, certain trusted physicists were able to use the journal to try to promote their idea of 'modern' physics.

5.3 The organisational structure of the Royal Society: the influence of the 'modern'

I begin by considering the organisational structure which fostered these relationships of trust, and the types of physicists represented. At the Royal Society, it was required that all papers be communicated by a Fellow before being considered by a Sectional Committee (of mathematics, physics or chemistry, with the latter two combined until 1919).[22] The Sectional Committee for Physics consisted of nine members, with three retiring each year; a retired committee member was then ineligible for election the

[20] Brock and Meadows (1998), p.144.
[21] Rutherford (1927), p.305.
[22] 'Minute books of the Physics and Chemistry/Physics Sectional Committee', RS.

following year.[23] There were five chairs heading this Committee in the 1920s: from 1919 this was Richard Glazebrook, followed by Robert John Strutt (the 4th Lord Rayleigh) from 1922, O. W. Richardson from 1925, Alfred Fowler from 1927, and Richardson again from 1929 to 1930.[24] Exploring the institutional backgrounds of these Chairs, we find many similarities. Glazebrook, Rayleigh and Richardson all studied at Trinity College, Cambridge, and spent time in the Cavendish. Indeed, of the twenty two men who were present at committee meetings from 1921 to 1930, fourteen of them had some connection with Cambridge (and eleven had researched at the Cavendish).[25] However, Cambridge was not the only influence, and also not a homogenous institution. As I noted in Chapter One, there were three main traditions here in the early twentieth century, which can be broadly characterised as mathematical physics, reductionist experimental physics, and precision measurement.

The committee also featured thirteen members who were, or had been, affiliated with one of the London universities, which, as we saw in Chapter One, were more industrially minded than Cambridge. Furthermore, there were nine committee members who were based, either currently or previously, in nonacademic institutions, including Greenwich Observatory, a brewery, the Department of Scientific and Industrial Research, the military and the National Physical Laboratory.[26] There were, of course considerable overlaps between all of these categories (see Table 5.1). Indeed Glazebrook was a seasoned Cavendish researcher who had then been appointed the first Director of the National Physical Laboratory. Fowler, an astronomer and the only chair without a Cambridge background, spent his education and career at London's Royal College of Science, a constituent college of Imperial College from 1909. In 1908, he was joined by Rayleigh, working on the 'radioactivity of minerals in relation to the earth's internal heat'.[24] Glazebrook also spent time here, as Director of the Department of Aeronautics from 1920 to 1923. And Richardson too was based in London by the 1920s, at King's College. It is thus clear that we must be careful in characterising physicists as characteristic of a Cambridge tradition; there were multiple traditions within Cambridge, including a style appropriate to the more practically oriented institutions.

[23] Royal Society (1921).
[24] Information about the members of the committee is taken from the 'Minute books of the Physics Sectional Committee', RS.
[25] See Table 5.1.
[26] Many worked for the military during the First World War, but I have only placed them in the non-academic category if they were also employed by the military in peacetime, such as Lyons and F. E. Smith.

One final position completed the managerial structure of the *Proceedings,* and this was the Physical Secretary, who, from 1919 to 1929, was the Cambridge mathematician James Jeans. It was under his guidance that decisions were made regarding whether a paper should be sent to a referee, and if so who was most suitable. Although the entire Committee was officially responsible for all decisions, in practice the Chair and Secretary often worked alone. This was certainly true of the 1920s, and when Richardson first took his post Jeans told him that he had 'always tried to avoid meetings of the Committee as far as possible and have generally been successful'.[27] This was achieved using a stipulation known as Standing Order 43: a paper was distributed to all members of the committee and, if nobody objected within a week, it was passed for publication.[28] Only those papers opposed during this process, or earlier flagged up by Jeans or the Chair, were properly discussed at a meeting. For most papers, the first destination was an independent referee, chosen by Jeans and the Chair, but for some this stage was bypassed altogether. Of the 61 of his own papers that Robert Strutt (Lord Rayleigh from 1919) communicated between 1906 and 1926, all but one was passed for publication without review.[29] Such treatment was not reserved only for Lords: Sydney Chapman, a Trinity College graduate employed at Imperial, also had his work cleared for publication, trusted by Jeans to be 'always very careful and reliable in all his work'.[30]

Trust in a communicator could extend further, to include work they themselves had not undertaken. Younger or less prestigious researchers were able to have their work published without referee if it was communicated by a 'trusted' physicist. It was thus extremely beneficial to a physicist to be part of a network of trust. In 1925, Jeans noted that future chair Fowler was 'always careful in what he communicates', and so didn't send his communications on for review.[31] Ernest Rutherford, now Director of the Cavendish Laboratory, received similar treatment. By the 1920s, research at the Cavendish had become more divided, as Thomson stayed on in his own room, while Rutherford directed a less mathematically informed style of research, no longer containing a commitment to the ether.[32] In 1925, he sent in three papers produced by researchers in his laboratory, accompanied by a covering letter noting that he had spent

[27] Jeans to Richardson, 2 January 1925, OWR.
[28] Royal Society (1921).
[29] 'Register of Papers', RS.
[30] Jeans to Richardson, 25 February 1926, OWR.
[31] Jeans to Richardson, 9 July 1925, OWR.
[32] Falconer (1989), p.109; Cockcroft (1954), pp.22-3.

'a good deal of time getting these papers into shape'; Jeans sent them to the printers before he had even been given confirmation from Richardson.[33] By building up a relationship and reputation with the Secretary, physicists were able to ensure the continued acceptance of their work, or, as in the case of Rutherford, the work of an entire research school, into the Royal Society. As such, Rutherford's particular style of 'modern' physics was ensured publication in a prestigious journal. Furthermore, while in 1911 Rutherford had published much of his work in the *Philosophical Magazine*, now he was choosing the Royal Society. This was contributing to an idea of the *Proceedings* as more 'modern' than the *Philosophical Magazine*. It would seem that Rutherford, perceiving the *Proceedings* as a more credible journal, was perpetuating such a notion.

Clearly the relationship of trust held between the communicator and committee members was directing the choice to accept a paper. Trust could be earned over time by continually sending in papers of a high quality. Certain communicators developed reputations for being more careful than others. However, the mere name of a communicator was also important, as it conveyed a message of expertise not just to committee members, but to readers of the journal. Indeed the status of work could be affected by the institution it came from. In one instance, Jeans, after receiving a paper written by Sorbonne physicist H. Weiss, and sent in by the Cambridge zoologist Cresswell Shearer, noted that he knew 'nothing of [Shearer's] reliability as a communicator'. However, discovering that the paper came from research undertaken in the Royal Institution Laboratory, Jeans suggested asking for the opinion of its Director, William Bragg, before deciding whether to 'pass under Standing Order 43'.[34] The paper subsequently appeared in the *Proceedings* as communicated not by Shearer but Bragg, suggesting that the communicator was responsible for convincing not just the committee, but also the reader, of a paper's value.[35]

For many of the papers that *were* sent onto a referee, Jeans and the Chair had choice of reviewer. These referees were given the power to decide not only if a paper was of a sufficiently high standard, but also if the kind of physics within it was appropriate for the pages of the *Proceedings*. Navarro has suggested that in the mid 1920s there were only two Fellows of the Royal Society sufficiently qualified to judge papers relating to quantum theory. As a result, Charles Galton Darwin and Ralph Howard Fowler, both

[33] Jeans to Richardson, 9 July 1925, OWR.
[34] Jeans to Richardson, 21 April 1925, OWR.
[35] Weiss (1925).

Trinity College graduates, became the 'arbiters of quantum physics'.[36] Here, Rutherford's influence can be seen: Darwin studied under him at Manchester, while Fowler married his daughter and became a 'theorist-in-residence' at Rutherford's Cavendish.[37] In a similar position to Fowler and Darwin was Arthur Stanley Eddington, As I have shown, Eddington was an early supporter of relativity, and, as a Fellow of the Royal Society from 1914, was perfectly suited to become something of an 'arbiter' himself for this area of physics.[38] As I shall explore, having committed relativists and quantum theorists as trusted referees influenced the types of physics accepted into the *Proceedings*. If a paper did not take into account these views, and the referees believed it should have done, then it was rejected.

Relationships of trust were at play in the management of the *Proceedings*. As I shall explore, certain referees and communicators were trusted to make judgements over what was valuable physics and what was not. While the *Proceedings* was not managed exclusively by 'modern' Cambridge men, the influence of this institution, in the form of referees such as Darwin, Fowler and Eddington, cannot be overlooked. In the following section I explore the consequences of this influence for a physicist whose work did not match up to the vision of 'modern' physics held by the circle of Fellows trusted to judge his work. This is found in the case of a paper written by Richard Hargreaves and communicated to the Royal Society by Joseph Larmor in 1921.

5.4 Judging value: 'classical mechanics lead nowhere at all'

Hargreaves had studied at St. John's College, Cambridge (as had Larmor) and placed in the top ten in the 1876 Mathematical Tripos.[39] From here he had chosen the pure mathematics route, writing a 1901 book on arithmetic, but he also produced occasional papers on mathematical physics, which he sent either to the *Philosophical Magazine* or the Cambridge Philosophical Society for publication.[40] In the late nineteenth century he had been influenced by J. J. Thomson's vortex theory of ether and matter.[41] By 1920, he was still considering the structure of matter, and, now employed as a lecturer at

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[36] Navarro (2009), p.321.
[37] Navarro (2009), p.320.
[38] Stanley (2007a); Douglas (1956).
[39] Warwick (2003), pp.515-516.
[40] Hargreaves (1901); Hargreaves noted in a letter to Larmor that he usually sent his mathematical physics to these two destinations, Hargreaves to Larmor, 30 September 1921, LAR.
[41] Kragh (2002), p.43.

Liverpool University, began working on a new paper, a densely mathematical approach to the structure of atoms. He wrote to Larmor to enlist his help in relating the work to current atomic research.[42] This area of physics was undergoing rapid advances in the 1920s. Rutherford had, in 1919, discovered artificial disintegration, and was now working on a model of the nucleus, while also delivering lectures on the subject, to the Royal Institution and Physical Society.[43] Meanwhile, Bohr's quantum theory was acquiring increasing credibility in Britain, particular with the help of Jeans' continued promotional work. From 1919, Jeans was proposing the quantum work produced by Bohr and his school as the only viable option for understanding radiation.[44] Larmor, however, ignored such developments, conceiving of Hargreaves' paper as an alternative to the nuclear model of the atom proposed by Rutherford in 1911. Gradually he and Hargreaves turned the paper into one Larmor believed would appeal to physicists, explaining in general terms what his atomic scheme was and how it differed from the nuclear model; confident in the paper, Larmor sent it on to the Royal Society.[45] However, Hargreaves' finished paper, 'Atomic Systems based on Free Electrons, both positive and negative, and their stability', did not take into consideration quantum theory, and as such was not well received at the Royal Society. Neither Larmor nor Hargreaves held views on atomic structure which matched up to the referees' definition of credible 'modern' physics.

Hargreaves was now subject to the networks of trust at the Royal Society, which determined the fate of his paper. The first referee was John William Nicholson, a Trinity College graduate and former Cavendish researcher, now working at King's College, London. Nicholson was also a mathematician, but unlike Hargreaves had a history of applying mathematics to physical problems. He had published a number of papers on coronal and nebular spectra, and was an early adopter of quantum ideas.[46] He was a friend of Eddington's, the two men having studied together at Manchester, and, more notably, his name features frequently in Niels Bohr's 1913 series of articles on the quantum theory of atoms and molecules.[47] While Nicholson criticised Bohr's quantum model of the atom, he did so in a way which displayed substantial knowledge of the new

[42] Hargreaves to Larmor, 5 October 1920, LAR.
[43] Stuewer (1986).
[44] Kragh (2011), p.29; Jeans (1919, 1924).
[45] Hargreaves to Larmor, 4 November 1920, LAR.
[46] Wilson (1956).
[47] Wilson (1956), p.209; McCormmach (1966).

kinds of physics involved.[48] Kragh suggests that, despite this, Nicholson remained a 'classical physicist', and McCormmach describes his work as 'starting with an attempt to understand the constitution of matter by wholly classical laws'.[49] I suggest, however that the case of Nicholson demonstrates one of the difficulties of characterising physicists of this period in such a way. Indeed, when judging Hargreaves' work, Nicholson did not recommend publication, declaring that the author was 'certainly not in touch to any sufficient extent with modern developments'.[50] In this context, Nicholson appears as a modernist, not a classicist.

Nicholson's short assessment of the paper is very revealing of his view of the purpose of the *Proceedings*:

> 'I am not able to recommend this paper. There are many respects in which it is quite ingenious, but we cannot allow that it is in any way, in its fundamentals, in accord with really definite experimental knowledge – it can never in fact be a vital contribution to atomic theory. Though well worthy, in many parts, of publication, I cannot consider that the Royal Society should accept the responsibility of publishing it. The author is certainly not in touch to any sufficient extent with modern developments, and only displays an ingenuity which is very much wasted. The best course is that the author should be asked to withdraw it.'[51]

Notably, Nicholson did not say that the paper itself was bad, or even unpublishable, but rather judged that it was not suitable for the *Proceedings*. He was making a judgement on the *value* rather than the *quality* of the work. As Nicholson was committed to quantum theory, he did not see how an atomic model which did not take this into account could help further knowledge on the subject. Nicholson was thus recommending rejection on the basis that the paper was not in accord with his vision of 'modern' physics.

Jeans agreed completely with Nicholson's report, informing Larmor that 'in these problems the law of inverse square and classical mechanics lead nowhere at all' and that there was no reason to publish any more of these types of papers.[52] Jeans himself was a staunch 'modernist' in the topic of atomic structure and quantum theory, but he also placed 'trust' in his chosen referee. However, Jeans could also not dismiss Larmor, who

[48] Kragh (2011).
[49] Kragh (2011), p.28; McCormmach (1966).
[50] 'R Hargreaves Referee Report 1921' (No.141), RS.
[51] 'R Hargreaves Referee Report 1921' (No.141), RS.
[52] Jeans to Larmor, 17 November 1921, LAR.

had acquired a certain amount of regard during his lengthy career. Larmor was Lucasian Professor of Mathematics from 1903 to 1932, a prestigious Cambridge position once held by Isaac Newton.[53] His 1900 *Aether and Matter* had formed a significant part of Cambridge electrodynamics pedagogy at the beginning of the twentieth century.[54] Furthermore, he had been a Fellow of the Royal Society since 1892. While Larmor was perhaps now 'out of touch' with many developments in physics, he maintained much of his former status. And so, when Larmor insisted that the paper be sent on to a second referee, Jeans complied. The second referee was Darwin, who suggested publication only with 'considerable modifications'.[55] Crucially, he advised that the paper needed to take into consideration Bohr's theory, essential, he insisted, for defining the size of the atom. Again, a committed quantum theorist was criticising the paper because it did not fit in with his own views. The paper was officially rejected at a meeting of the Sectional Committee for Mathematics. The Chair of this meeting was Nicholson himself, and Eddington was also present.[56] With this in mind, it is perhaps not so surprising that publication was not recommended, and Jeans informed Larmor of the decision the following day.[57] For Hargreaves 'the blow fell heavily' and he suspected, correctly, that the paper had been read by a referee far too committed to other views to give an unbiased opinion.[58]

He drastically shortened the paper and sent it on to the *Philosophical Magazine*. Larmor followed this with a letter to Lodge, detailing his support for the paper.[59] He informed Lodge that the Royal Society had accused the author of knowing nothing about modern spectral theory, a theory Larmor felt was 'mostly nonsense'. Lodge agreed with Larmor, noting that modern spectrum results concerned the planetary electrons and not the constitutional structure of the proton or nucleus.[60] He put complete faith in Larmor's opinion, declaring that 'I know nothing at present about Hargreave's paper: but on your recommendation I feel sure that the Phil. Mag will find room for it.'[61] Larmor and Lodge did not hold Darwin and Nicholson's commitment to quantum theory, and were willing to explore atomic theories which could function without this notion. The paper was

[53] On the Lucasian Professorship, and its holders, see Knox and Noakes (2003).
[54] Larmor (1900); Discussed in Warwick (2003), pp.376-81.
[55] 'R Hargreaves Referee Report 1921' (No.141), RS.
[56] 'Sectional Committee for Mathematics', Minute Books (CMB/46/4), RS.
[57] Jeans to Larmor, 27 January 1922, LAR.
[58] Hargreaves to Larmor, 9 August 1922, LAR.
[59] Larmor to Lodge, 12 August 1922, LODGE.
[60] Lodge to Larmor, 15 August 1922, RS-LAR.
[61] Lodge to Larmor, 15 August 1922, RS-LAR.

finally published in the December issue of the *Philosophical Magazine*, and featured a note of thanks to Joseph Larmor.[62] In this new version Hargreaves also admitted his ignorance of recent experimental results in physics, appealing to an interdisciplinary approach, whereby his mathematical work could be interpreted by somebody more knowledgeable in this arena.[63] Meanwhile, Larmor completed his role of advisor and confidant with a letter to *Nature*. Here, he enthusiastically pointed readers towards Hargreaves' 'long and interesting mathematical paper'. He noted that while the author had 'modestly disclaim[ed] authority to judge whether the properties he discovers have any substantial analogy with the radio-active and spectroscopic phenomena of actual atoms' this type of rigorous mathematical analysis could only help to expand the range of ideas in the field.[64] Where Nicholson and Darwin believed that Hargreaves' paper could not lead to progress in physics, Larmor fundamentally disagreed. For him, this kind of mathematical physics *was* extremely valuable. The credibility of classical physics was under negotiation.

While the Royal Society networks had been detrimental to Hargreaves' publication success, those of the *Philosophical Magazine* had worked to his favour. Here we have a small circle of Royal Society Fellows rejecting a certain kind of physics, on the grounds of it not being 'modern' enough; they were treating the *Proceedings* as the natural home of quantum theory. Meanwhile the *Philosophical Magazine*, on the advice of Larmor, quickly accepted it. For both journals, the decisions were based on the advice of what were deemed to be trusted experts. These experts were influenced by their own notions of the nature of modern physics, and commitments to particular theories. But the divide between modern and classical was not entirely clear cut. Just as Lodge was trusted in public spheres to disseminate 'modern' physics, so too did Larmor have some influence at the Royal Society. In the following section, I consider the status of physicists such as Larmor, established Fellows of the Royal Society, who continued to command respect even as they became further distanced from actual scientific research. They were communicating and promoting the types of physics which Jeans and his trusted circle now saw as classical and out of date, but their prestigious reputations could not be completely ignored. Jeans, the Physical Committee and the referees thus struggled to find a balance between these competing issues of high status and classical physics.

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[62] Hargreaves (1922).
[63] Hargreaves (1922), p.1091.
[64] J[oseph] L[armor] (1922).

5.5 The careful treatment of established fellows

In the case of Hargreaves, we found that Larmor, despite his prestigious position at Cambridge University and influence in early twentieth century Cambridge pedagogy, did not hold sufficient influence at the Royal Society to ensure publication of a paper. Notably, Larmor had previously served as Physical Secretary there, from 1901 to 1912 (at which point Arthur Schuster had taken over, before Jeans was appointed in 1919). Furthermore, between 1903 and 1919, all nine papers he submitted to the Royal Society were published without review.[65] And yet now a paper he wholeheartedly recommended had been rejected. In the beginning of the 1920s there was a shift in Larmor's status at the Royal Society. Trust in him was diminishing. Shortly after the Hargreaves debacle came to a close, Larmor submitted his own paper to the Royal Society and, as we shall see, it was sent on to referees, who then produced negative reports. Larmor was accustomed to having his papers accepted without question, and now they were being criticised. Committee members needed to carefully negotiate this somewhat awkward situation. Crucially, Larmor's status at the Royal Society was in a period of transition, as he continued to hold a degree of influence.

Before detailing the processes involved, and decisions made, in reviewing Larmor's paper, I briefly consider two cases of poorly received papers sent in by established physicists of some prestige. These papers were not published, but the Physical Committee took considerable care over their decisions not to publish, and communications with the authors. In 1926, the Royal Society received a paper by Charles Herbert Lees, Vice Principal of Queen Mary College, London, on the specific heats of gases.[66] The paper received an unfavourable report from Hugh Callendar, the precision experimentalist, who felt the paper was too 'highly theoretical and somewhat speculative'.[67] He suggested that the paper contained a small correction of use in experimental work of which experimentalists themselves were already aware. The accusation here was similar to that directed at Hargreaves: both physicists were attempting to enter into domains where they did belong. Where Hargreaves tried to apply his 'classical' mathematics to 'modern' physics, Lees was contributing an unwelcome theoretical approach to an experimental subject. Again, the problem was not

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[65] Royal Society Register of Papers, RS
[66] For Lees, see Wilson (1953).
[67] 'Report on Paper by C. H. Lees, on "The Determination of the Specific Heats of Gases at Constant Pressure &c.', OWR.

one of *quality* but of *value*. Despite this negative report, Jeans did not want to reject the paper unless he absolutely had to as Lees was a 'Fellow of some standing' and he felt they 'ought to be rather generous in giving him the benefit of any doubt there may be'.[68] The paper was sent to Lindemann, who also failed to find anything novel and useful in the paper, and a report was also produced by Richardson himself, who was ultimately in favour of publication, but expressed little enthusiasm for the paper. Jeans felt that it was best to advise withdrawal, and the paper was eventually not published, but instead read at a meeting.[69]

The problems arising from Lees' paper were not about 'classical' or 'modern' physics, but rather differing approaches to the place of theory in experiment. However, in a different case, we find the notion of an older physicist being 'out of touch' with 'modern' ideas. William Mitchinson Hicks had been a Fellow of the Royal Society since 1885 and retired since 1917. Towards the end of his life, Hicks had become engaged in work exploring the structure of spectra, for which he received the Adam's Prize in 1921.[70] However, when he sent a paper on 'The Quantum Derivation of the Zeeman Effect' to the Royal Society in 1925, it was rejected. The first referee, R. H. Fowler, wrote a lengthy criticism, declaring that the paper served only to 'obscure an issue otherwise clear'.[71] Fowler believed that by misunderstanding 'modern' research in quantum physics, Hicks had failed to make a valid contribution to the field. The paper was, however, sent to a second referee, presumably on the basis of Hicks' status. This second referee was, predictably, the Royal Society's other resident quantum theory expert, C. G. Darwin. And it comes as little surprise that Darwin stated he 'should definitely side with the referee against Prof. Hicks'. He criticised the author for 'having adopted an out of date view of the rules of quantising'.[72] Hicks' unfamiliarity with what Darwin and Fowler deemed to be 'credible' physics meant that neither referee judged the work to be suitable for publication. Notably, Darwin admitted that he had 'not read all the detail of the paper', and had thus made his judgement rather quickly. With the paper already rejected by a 'trusted' referee, and apparently revealing the author to be out of touch, Darwin had no desire to examine the whole paper carefully. Furthermore, his report references a letter Hicks had written to *Nature* on the same topic, which

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[68] Jeans to Richardson, 14 May 1926, OWR.
[69] Jeans to Richardson, 22 June 1926, OWR; 'Societies and Academies' (1926a, p.842.
[70] Milner (1935).
[71] 'W. M. Hicks Referee Report 1925' (No. 186), RS.
[72] 'W. M. Hicks Referee Report 1925' (No. 186), RS.

Darwin used as further evidence against the paper itself. He was thus not merely judging the quality of the paper, but rather the author's general approach to the topic.

Despite this rejection, Hicks sent another paper to the Royal Society in 1931, on the spectrum of gold, and again faced accusations of being unfamiliar with 'modern' developments. However, here there is evidence of the Physical Committee discussing the correct way to treat a Fellow such as Hicks, who was 'out of touch' but had acquired some status during his scientific career. A. Fowler's referee report criticised the author's 'utter disregard for the methods of analysis which have been so successfully employed by other workers in recent years.'[73] Despite this damning report, Frank Edward Smith, the then secretary, was unsure as how to proceed. Hicks was a senior Fellow of the Society and Smith did not want to do 'anything wrong'. However, he also didn't want to 'publish anything which is not useful'.[74] The paper was thus sent to a second referee, John Cunningham McLennan, a physicist who had trained in the Cavendish under Thomson at the very end of the nineteenth century.[75] McLennan reproached Hicks' unorthodox choice of abbreviations and notation, which he claimed made the paper very difficult and time-consuming to read. But even more objectionable was the fact that Hicks had either 'failed to note' or 'ignored' recent work on the subject by McLennan himself.[76] Hicks' failure to be 'modern' was a failure to consider the 'modern' work undertaken by McLennnan. The contingency of the definition of this concept is thus particularly evident here, as we find it dependent on the referee's own work.

Both Hicks and Lees had their papers very carefully considered, and subjected to more than one review in the hope of finding something of value. The committee would only reject these papers if they thought it was absolutely necessary, as they did not want to adversely affect the relationship between the Royal Society and these established Fellows. The treatment of their papers was influenced greatly by social judgements about their status and standing in the field, and thus their work could not be immediately dismissed. However, in both cases the referees, with the support of the Physical Secretary made the judgement that the work was not valuable because it failed to sufficiently consider 'modern' developments in the field. The 'classical' nature of the papers was too strong a problem to be ignored in order to appease a respected figure. In

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[73] 'W M Hicks Referee Report 1931' (No. 250), RS.
[74] F. E. Smith to Richardson, 30 January 1932, OWR.
[75] Eve (1935).
[76] 'W M Hicks Referee Report 1931' (No. 250), RS.

the following case of Larmor, I show how again the Physical Committee treated his work very carefully, hoping to publish it if possible. Unlike Hicks and Lees, Larmor's paper was seriously considered for publication. It was ultimately withdrawn however, as Larmor, in consultation with Oliver Lodge, believed he should be afforded more trust and respect from the Committee than he was given.

The paper sent by Larmor to the Royal Society was intended to antagonise those who had rejected Hargreaves' paper, and test their boundaries. Enraged by his experience with Hargreaves, Larmor confided in Lodge, accusing Jeans and the Royal Society of refusing to publish any more Newtonian atomic theory, and declaring his intent to take on 'Jeans and the other dogmatic exponents'.[77] Larmor was constructing a picture of the Royal Society as run by strict 'modernists', in competition with physicists like himself, and working to influence the direction of scientific research according to their own interests and theoretical commitments. Despite his complaint about the Society's attitude towards atomic theory, Larmor's attack was not against quantum theory, but was rather a paper 'On the nature and amount of the gravitational deflection of light'. Here, Larmor was responding to recent developments in relativity theory, concerning the deflection of light, as observed at the 1919 eclipse expedition. Larmor's 'attack' was on relativity theory, not quantum theory. It would seem that he placed these two concepts into the same category, and it was one that divided him from the 'dogmatics' of the Royal Society. For Larmor, relativity and quantum theory appear to have both been part of the same problem: the commitment of a 'dogmatic' faction within the Royal Society to a certain type of 'modern' physics.

It is not entirely clear what Larmor was arguing in his paper. Indeed, Eddington himself, who was chosen as first referee, struggled to understand what had been written. Like much of Larmor's work, it suffered from bad handwriting and poor clarity of exposition.[78] Larmor would later announce, in a letter to the *Times*, that following Einstein's own methods he had arrived at half the amount of deflection, and thus

[77] Larmor to Lodge, 4 October 1922, LODGE.
[78] Larmor's handwriting was 'notoriously illegible', Hall (1969), p.74. In a letter to Lodge, Larmor himself remarked, 'Alas my handwriting!' following Lodge's struggle to decipher an earlier communication, Larmor to Lodge, 24 March 1922, LODGE; Lodge tried to bring to Larmor's attention the difficulty of reading his works, and noted, 'You are of course entitled to your own style, but it certainly is a difficult style.' Lodge to Larmor, 10 June 1925, Lodge to Larmor, 1 May 1929, RS-LAR; Eddington himself noted that in Larmor's later years, 'his style, never lucid, grew more and more involved', Eddington (1942), p.205.

Einstein had not been victorious in the eclipse expedition.[79] This is indeed what Eddington seems to have gathered from his first reading of the paper, leading him to immediately recommend that it be withdrawn.[80] A second reading, however, led Eddington to retract his first report, as he now believed that Larmor was not approaching the problem from an Einsteinian stance, but rather putting forward 'a new theory altogether rejecting the relativity postulates'. As Cunningham had done with special relativity theory in 1907, Larmor was interpreting aspects of the theory within his own views.[81] Eddington noted that he would have liked to ask Larmor to remove the 'innuendo against Einstein's deductions', he acknowledged that it was 'of course, impossible to ask him to present it in the way I should like', and instead suggested that the paper simply be accepted. In Eddington's acceptance of this paper, we find his method of proceeding with a paper presenting a view he himself did not agree with. He told Jeans that they could not refuse to publish the paper, as he could not find a definite error in Larmor's argument. Furthermore, the publication of only an abstract would present a 'summary judgement of a distinguished professional', and one not open to criticism. Thus the only remaining option was to 'admit that it is a tolerable presentation of an anti-Einsteinian view' and publish in full. In this case, 'those who understand the relativity theory can either satisfy themselves or publicly reply to the paper'.[82]

This was a remarkably different approach to that taken by Nicholson and Darwin when reviewing Hargreaves' paper. This can be accounted for by the different attitudes taken when considering experimental and theoretical developments, and the status which Larmor held professionally. Darwin and Nicholson disregarded Hargreaves' paper because it did not consider the quantum theory which they believed had been confirmed by experimental results. While Nicholson was a mathematician, he had adopted quantum methods on this basis. Eddington, however, was more interested in theoretical reasoning: he noted that, in this regard, there were no errors in Larmor's paper. Indeed, as I have discussed in the previous chapter, Eddington's philosophy of science was an inclusive one: he believed in using whatever techniques produced results, and worrying about an overarching theory later.[83] Furthermore, Larmor could not be dismissed as easily as

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[79] Joseph Larmor, 'Einstein and Gravitation', *The Times*, 17 April 1923, p.15. Larmor declared he had made this clear in a recent publication, presumably this paper, which was published in the *Philosophical Magazine* in 1923, Larmor (1923).
[80] 'J Larmor Referee Report 1922' (No. 147), RS; Eddington refers to an earlier report, presumably removed from the records.
[81] Cunningham is discussed in Warwick (1992, 2003).
[82] 'J Larmor Referee Report 1922' (No. 147), RS.
[83] Stanley (2007b).

Hargreaves, who was a scientist of far less distinction. Larmor did not command sufficient 'trust' to ensure that a paper he recommended, such as Hargreaves', would be published, but Eddington was somewhat obliged to consider Larmor's own work, even if he did not agree with the views contained therein.

However, the paper ultimately was not published. After carefully reading it for a third time, Eddington came to the conclusion that the first part, suggesting an incompatibility between the principle of least action and relativity, was very good. Notably, this could be interpreted in two ways. Larmor, committed to the principle of least action, saw it as a rebuttal against relativity theory. Eddington interpreted this in light of his own work: he believed a physical principle of Least Action to be 'nonsensical', and thus the incompatibility only supported his belief in relativity theory.[84] Meanwhile, the second part of the paper, an 'illegible morass', needed to be typed up before it could be properly considered. Eddington, however, suggested taking the second part 'on trust', as it appeared to deal with electrodynamics, a field in which he believed Larmor was expert.[85] At the next meeting of the Sectional Committee for Mathematics, the paper was discussed by Eddington and Jeans, joined also by Nicholson, pure mathematician J. E. Littlewood, and Trinity College physicist, G. I. Taylor.[86] Eddington's suggestion of publishing the entire paper on trust was not taken (partly no doubt because Jeans was critical of the paper), and the committee decided that the paper should be split into two parts, with only the first recommended for publication.[87]

Larmor was unhappy with this suggestion and decided it was time to move on to a different publication. He told Lodge that he could not afford to 'quarrel' with the Society as long as he still had 'students to help along into the world'.[88] Larmor was thus aware of the significant role the Royal Society could play in a scientist's career; indeed, his own Fellowship contributed to his current position of authority. Lodge meanwhile was shocked by the Society's actions, declaring that it 'seems awful cheek to question a paper of yours if you stand to your guns, and I am not surprised that you think of withdrawing it.' He declared that 'any paper by you the Phil Mag will be proud to print'

[84] Eddington (1942), p.204; 'J Larmor Referee Report 1922' (No. 147), RS; For Larmor's commitment to 'Least Action', see Warwick (1993b).
[85] 'J Larmor Referee Report 1922' (No. 147), RS.
[86] 'Sectional Committee for Mathematics', Minute Books (CMB/46/4), RS.
[87] Two days after this meeting, Lodge referred to Jeans' criticisms in a letter to Larmor: Lodge to Larmor, 9 December 1922, RS-LAR.
[88] Larmor to Lodge, 8 December 1922, LODGE.

and that he was writing at once to the publishers to see if there was space in the January issue.[89] Larmor did withdraw the paper, and it was quickly published in the *Philosophical Magazine*.[90]

In this episode, we find a variety of trust relationships playing out. Eddington suggested taking the entire paper of Larmor, whose work had played such a major role in his own education, on trust, despite finding parts of it incomprehensible. Furthermore, he devoted considerable time and energy to reviewing this paper, reading it at least three times. The remainder of the committee, including James Jeans, did not share the same level of trust. And after their final decision, Lodge's take on the matter was that the Royal Society should have had more trust in Larmor. Again we see Lodge putting complete faith in Larmor, rushing to publish the paper in the *Philosophical Magazine*. Furthermore, the importance of maintaining amicable working relationships arises. While Larmor was keen not to fight with the Royal Society, for the good of his students, the Society were equally careful in their correspondence with Larmor, aware that there were many things they could not ask of him. Relationships with longstanding Fellows such as Larmor were treated with caution. Beyond the initial question of quality, there was thus more to consider in the publication of a paper than whether it was 'classical' or 'modern'. The position of the author also had to be taken into account. While Lodge developed authority in public, Larmor maintained some of his influence in this professional sphere. Both physicists were responding to the changes underway in the discipline, situating themselves within a new framework of physics where their contributions were no longer seen as valid.

5.6 The limits of trust

While relationships were important, they were not always sufficient to securing publication of a paper, as in the cases of Larmor, Hicks and Lees. Even the Chair of the Sectional Committee for Physics did not hold enough influence, on his own, to ensure publication. In 1925, Richardson communicated a paper by William Edward Curtis, a physicist also at King's College, London, on 'New Series in the Secondary Hydrogen Spectrum'.[91] The referees did not recommend publication, with Alfred Fowler declaring

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[89] Lodge to Larmor, 9 December 1922, RS-LAR.
[90] Larmor (1923).
[91] Register of Papers (MS 588), RS.

that he felt the time had come 'when the publication of scraps about this spectrum should be discouraged'.[92] Jeans suggested that the paper be withdrawn, but Richardson pushed ahead, sending the reports back to Curtis. Curtis' replies were, in Jeans' mind, not satisfactory, failing to address Fowler's general criticism.[93] The Committee met in January and, going against their Chair, decided not to publish it. Instead they suggested, as Fowler had recommended, that it be repackaged into a more comprehensive paper.[94] Richardson, still confident in the work as it stood, instead communicated it to the *Philosophical Magazine*, where it was published.[95] He proudly stuck by his decision many years later. Reporting on a paper also concerning the hydrogen spectrum, he noted that the strongest lines of the system had been arranged in series by Curtis 'in a paper which I communicated to the Royal Society about 7 years ago. This paper was rejected by the Physics Committee but I felt sure there was something in it and I subsequently got it published in the Philosophical Magazine'.[96] The *Philosophical Magazine* here takes the role of an alternative to the *Proceedings*, a home for rejected papers. Crucially, however, this rejection was not always on the basis of quality, but rather judgements about what a valuable contribution to physics was. Fowler reviewed the paper negatively, not because Curtis' study was incorrect, but because he felt that further studies of the topic in question did not add to the progress of physics.

Richardson's position was not powerful enough to singlehandedly ensure the publication of a paper. However, there were other ways of negotiating publication of an undesirable article. Henry Thomas Flint worked in the physics department at King's College, London, first as lecturer and then as reader, from 1920 to 1944.[97] In 1927, Richardson communicated a paper by Flint and his colleague J. W. Fisher on 'The Fundamental Equation of Wave Mechanics and the Metrics of Space'. It was sent first to Eddington for review, and he did not recommend publication, on the basis of technical flaws.[98] After being advised to withdraw the paper, however, Richardson vehemently disagreed, arguing that the paper was an important contribution to quantum theory, and that it needed to be 'considered by a referee who is familiar with the difficulties both of the quantum and relativity theories'.[99]

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[92] Jeans to Richardson, 30 November 1925, OWR.
[93] Jeans to Richardson, 16 December 1925, OWR.
[94] Jeans to Richardson, 29 January 1926, OWR.
[95] Curtis (1926).
[96] Richardson to FE Smith, 14 July 1932, OWR.
[97] Anonymous (1971).
[98] 'H Flint and JW Fisher Referee Report 1927' (No. 73), RS.
[99] Richardson to Jeans, 17 June 1927, OWR.

Although Richardson strongly objected to the idea of rewriting the paper to meet the referee's criticisms, Flint and Fisher agreed to withdraw and resubmit.[100] However, after a week of reworking they came to the conclusion that 'the referee is wrong'. Richardson informed Jeans that they would no longer withdraw the paper, and instead were requesting to have it dealt with in its original form.[101] Dutifully, Jeans sent the paper on to a second referee, George Barker Jeffery, Professor of Pure Mathematics at University College, London. Despite his position, Jeffery was an applied mathematician at heart, and had a particular interest in relativity theory, publishing a number of papers and a book on the subject.[102] It is not evident, however, that he lived up to Richardson's criteria for a suitable referee, having, it would seem, no background in quantum theory. This was rectified after Jeffery finally returned the paper, with an unenthusiastic recommendation for partial publication, and it was passed on to one of the Royal Society's resident quantum theory experts, in this case R. H. Fowler.[103] Fowler believed the paper to be of a relativistic nature, rather than a quantum one, and thus felt he was unqualified to judge. He instead consulted with Eddington and, finding that a negative review had already been given, was 'not prepared to distance from him in this matter'.[104] Fowler obtained his knowledge of the value of a relativity paper from the very referee to whom he was supposed to be providing an additional or alternative opinion. Here, we find the process of refereeing being prone to circularity, with a limited number of referees inside the trusted 'circle'.

At this point the paper would have been destined for certain rejection, had it not been for the appearance of an additional paper, also written by Flint. Richardson submitted Flint's 'Relativity and Quantum Theory' with a note he himself had written, and told Jeans that if the paper was too uncertain and speculative then he would send it to the *Philosophical Magazine* instead.[105] This paper was sent to Fowler for review, and he returned his report, alongside his assessment of the earlier Flint and Fisher paper, on 25 November. Jeans informed Richardson that Fowler 'could not recommend publication of the [Flint and Fisher] paper on its own merits, but that he would sooner see it published than prevent or delay the publication of the paper by Flint and yourself'. He noted that 'the

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[100] Richardson to Jeans, 26 June 1927, OWR.
[101] Richardson to Jeans, 4 July 1927, OWR.
[102] Titchmarsh (1958).
[103] Jeans to Richardson, 1 December 1927, OWR.
[104] 'H Flint and JW Fisher Referee Report 1927' (No. 73), RS.
[105] Richardson to Jeans, 3 November 1927, OWR.

Royal Society might reasonably insist on your either amending or very much shortening the original Flint and Fisher paper, but we are prepared to leave the matter largely to your own judgement'. Jeans' own personal opinion was that the manuscripts should be returned to Richardson, allowing Flint and Fisher to 'condense their paper as much as possible and meet the criticism of the first referee', a criticism which to Jeans seemed very serious. He noted that nobody consulted felt that 'the reputation of Flint and Fisher will be improved by the publication of the paper in its present form'. Ultimately, however, Jeans was 'very anxious' that the newer papers by Flint and Richardson be 'got through without further delay'.[106]

In the February 1928 issue of *Proceedings*, all three papers appeared: the original by Flint and Fisher, the quantum and relativity paper by Fisher, and Richardson's note (presented as a separate paper co-authored by Flint and Richardson).[107] As a trusted 'expert' in relativity theory, Eddington had almost complete control over the destiny of the first paper, with Fowler bowing to his greater knowledge. While Richardson, as Chair, had some influence, Eddington's expertise in this situation commanded greater respect. However, with the emergence of a second paper, deemed by Fowler to be of great importance, the situation changed somewhat. In order to ensure the speedy publication of this new paper, Fowler altered his stance on the original paper, agreeing to publish it as well. The new paper, which dealt with both quantum and relativity theory, was representative of the 'modern' physics which Fowler felt belonged in the Royal Society. Crucially, here he was ensuring that this paper appeared in *Proceedings* rather than in the *Philosophical Magazine*. This case suggests that Fowler and Jeans were working to establish the Royal Society as the home of 'modern' physics.

If we compare the positions held by Richardson and Larmor at the Royal Society in the 1920s, it would seem that the former, as a member of the Committee, was better placed to get work into the *Proceedings*. However, the more that this work diverged from the referees' conceptions of valuable physics, the harder this became. Some years later, in 1936, Richardson raised the issue of Royal Society orthodoxy when responding to another negative review of a paper of Flint's. Eddington had recommended publication, on the grounds that while he had criticisms of the paper, on the 'Ultimate measurements of space and time', he would need to write a whole paper in itself in order to express

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[106] Jeans to Richardson, 1 December 1927, OWR.
[107] Flint (1928); Flint and Fisher (1928); Flint and Richardson (1928).

them clearly.[108] The paper was sent to a second referee, P. A. M. Dirac, who had studied quantum physics under R. H. Fowler at Cambridge.[109] He was strongly opposed to publishing the paper, arguing that Flint was using entirely different concepts from those of the usual quantum mechanics. Dirac declared: 'I cannot understand this paper at all'.[110] Richardson fought back against these comments, admitting that while he was unable to tackle the mathematics in the paper himself, he had faith in Flint, and had never known him to make a mathematical mistake. He noted that Flint was somewhat outside of the community of practising quantum physicists, using 'different mathematical techniques from those customary in the quantum theory', which was perhaps why the referee found the paper incomprehensible. He warned that he personally 'would not like to take the responsibility for refusing to publish this paper'.[111]

Richardson's arguments failed to sway the opinion of either referee, but the paper was still ultimately published. Here, Richardson apparently had more authority than in his earlier attempts to convince the Society of Flint's value. A contributing factor to this may have been the presence of a new Physical Secretary, Frank Edward Smith, who took over from Jeans in 1929. Smith's education was not from Cambridge, but the Royal College of Science, and he worked under Glazebrook at the National Physical Laboratory from 1900 to 1920, before moving on to the Admiralty and then the Department of Scientific and Industrial Research.[112] Unlike Jeans, Smith was not himself an 'expert' in quantum theory, and thus willing to place more trust in Richardson than Jeans had been. Smith suggested that while Flint might be advised to reconsider his submission, 'the Society will, however, publish the paper if you still wish it'.[113] Richardson, confident that Flint was able to answer every one of the referee's complaints, which he believed were down to a fundamental misunderstanding of the paper itself, used the power bestowed upon him, and the paper was published.

In the negotiations between Richardson, Flint and the Physical Committee, we find different views of what constituted *valuable* contributions to the discipline. Throughout, Richardson and Flint accused the referees of failing to understand the work, and its

[108] 'H Flint Referee Report 1936' (No.135), RS.
[109] Kragh (1990).
[110] 'H Flint Referee Report 1936' (No.135), RS.
[111] Richardson to Smith, 21 October 1936, OWR.
[112] Goodeve (1972).
[113] F. E. Smith to Richardson, 17 November 1936, OWR.

importance. This was a considerable problem for physics during a period of such rapid change as the early twentieth century. As the case of Flint's 1936 paper reveals, the use of new mathematical techniques had the equivalent effect of a paper being written in an entirely different language. As I have already shown, a similar problem was found with Hicks' 1931 paper on the spectrum of gold, which McLennan struggled to decipher. In this regard, consensus was vital. But even for mathematical techniques and notation which had achieved some degree of consensus, among certain 'modernists', there were still difficulties for those on the outside of this specialism. In 1928, Oliver Lodge responded to continental developments in quantum mechanics in a letter to his friend J. Arthur Hill. He argued that the mathematics required to understand much of the new physics was developing too quickly for physicists to keep up with, and many (himself included) were being 'left behind'. Lodge had spent 'ten years' learning the mathematics required for the physics of his day, and yet this was of little use in comprehending the new work. He complained that even Joseph Larmor, 'a Pure Mathematician', was being 'spoken of as if he were now on the shelf'.[114] Reviewers approached a paper from their own scientific perspective, formed by their pedagogical and institutional background. They were looking not simply for quality, but for a valuable and credible contribution to the field. This was a highly contested category, and the problem of determining value increased in complexity as 'modern' papers became ever more incomprehensible to those trained in 'classical' techniques.

5.7 Conclusion

In this chapter, I have explored the negotiations at play in the publication of physics in the Royal Society's *Proceedings*. I have shown that, while the *Philosophical Magazine* came under criticism for not having a formal refereeing process and instead placing the control of the magazine in the hands of a small minority of physics, *Proceedings* was managed in a very similar way. In both journals, relationships of trust were often significant in deciding whether or not to publish a paper. At the Royal Society, a small network of committee members and trusted referees and communicators played a role in directing the content of the journal. The *Philosophical Magazine* was also managed by a network of editors and the circles of physicists whom they trusted. Somebody such as Larmor, whose influence was diminishing at the Royal Society, found that the *Philosophical Magazine* network still considered him an authority.

[114] O. Lodge to J. A. Hill, 5 March 1928, in Hill (1932), pp.222-225.

At the *Proceedings*, unlike at the *Philosophical Magazine* and *Proceedings of the Physical Society*, 'tentative' papers were not encouraged, and in this respect it was perhaps seen as the more orthodox journal. However, 'orthodox' was a contested category, as I have shown in the debates about whether to publish 'Newtonian atomic theory' or papers appearing to challenge Einstein's results. Committed quantum theorists, such as Fowler and Darwin, ensured that their own particular strand of 'modern' physics found a welcome home at the Royal Society. By taking sides in these debates, members of the Royal Society network of trust were promoting and establishing their own version of 'modern' physics.

There were, however, limits to this trust, and where two members of the Royal Society network differed in their opinion of a paper, choices were made as to who was the greater authority in that particular situation. There were a variety of different influences directing the content of the *Proceedings* and ensuring whether 'modern' or 'classical' physics was published. This included longstanding Fellows, who had, over the duration of a lengthy career, built up power that could not easily be broken. One can appear in many ways to be situated outside of an emerging orthodoxy, whilst still exercising some control. As a result, the Royal Society's *Proceedings* could not become an entirely 'modern' physics journal. Instead, competing versions of 'classical' and 'modern' physics were taken into consideration.

The editorial management of the Royal Society's *Proceedings* presents a case study in the transition from classical to modern physics. Here, old and new theories and approaches were assessed in terms of their 'value'. A physics paper was seen as valuable if it contributed to the progress of the discipline. As I have shown, for many physicists responsible for 'gate-keeping' at the Royal Society, progress could be achieved only through what they deemed to be proper consideration of 'modern' theories, quantum and relativistic. While 'classical' papers might be technically proficient, they were not useful in this respect. At the *Philosophical Magazine*, physicists such as Larmor and Lodge saw 'modern' theories as simply one potential route to progress, not the only option. Here, 'classical' papers were gladly accepted, and interpreted as valuable contributions to the progress of physics. There was a divide between conceptions of 'classical' and 'modern' physics, and those who promoted research of both, or of just the latter. For some, the transition from classical to modern was complete, for others it was still underway and the role of the former in the future of physics was not yet confirmed.

In this chapter and the previous one, I have explored attempts to establish both scientific and 'public' consensus in a rapidly changing discipline. I have found that throughout the 1920s, there were continued discussions about the value and place of 'modern' ideas with regards to the 'classical'. In the following chapter, I consider the effect of this lack of consensus on the displays of physics in the Science Museum during the 1920s and 1930s. I show that the Museum was under many different influences, each with their own idea of just what constituted 'modern' physics and how it should be presented to a variety of 'publics'. The lack of consensus found in 'public' and professional dialogues was further cemented in these museum displays.

Chapter Six: Competing visions of 'modern' physics at the Science Museum in the 1930s

6.1 Introduction

Where I have so far in this thesis considered dialogues between scientists and the 'public' in the form of lectures and published material, in this chapter I consider an alternative, object-based, medium. The Science Museum in South Kensington, London was attracting a million visitors a year by the 1930s, and provides an interesting case study in the presentation of science, and 'modern' physics, to the 'public'.[1] I explore the depiction of physics at the Science Museum from the 1920s through to the end of the 1930s, presenting the different influences, and competing visions of 'modern' physics that resulted.

Throughout this period, the Museum was devoting efforts to forging and maintaining connections with industry, and attention was given to exhibitions conveying how scientific research was being used for practical purposes. In this context, there was interest in current physical research with clear applications to industrial demands. As a result, 'modern' physics was defined by its ability to solve practical problems, achieved through increasing experimental precision and the development of more advanced apparatus. The sub-disciplines of geophysics and low temperature research fit into this characterisation and were represented in temporary exhibitions. However, at the same time a very different kind of 'modern' physics was also being promoted at the Science Museum. Henry Lyons, Director of the Museum from 1920 to 1933, was inspired by the success of the Royal Society's contribution to the 1924 British Empire Exhibition and sought to create a similar gallery at the Science Museum. Here the focus was on the type of research underway at the Cavendish, the 'microphysics' of atomic structure. In 1931, a former Cavendish researcher, Francis Alan Burnett Ward, was hired and subsequently given the responsibility of developing this gallery. He also organised a temporary exhibition, 'Atom Tracks', celebrating the twenty-fifth anniversary of the construction of C. T. R. Wilson's cloud chamber, an apparatus which contributed to knowledge about

[1] Morris (ed.) (2010), Appendix 3, lists the visiting figures from 1909 to 2008. For the 1930s, these can be found in the Science Museum Annual Reports, SM.

subatomic particles. In the 1930s, this type of physics had no connection to industry and its practical implications had not yet been explored.

In this chapter I ask why particular physicists chose to work with the Science Museum. I consider Cavendish experimental physicists promoting their particular reductionist style of research; geophysicists with practical concerns; and low temperature physicists hoping to display relations between their work and wider industrial needs. I suggest that such contributors were representing and promoting particular institutions and corresponding definitions of modern physics. The Science Museum itself was subject to the interests of its staff as well as governmental pressure to represent industry and a broader mandate to inform the public about scientific developments and history. I begin by considering the aims of the Science Museum in the 1920s, and how physics was presented, before exploring Lyons' efforts to rearrange the physics collections in a way which would portray a certain kind of modern physics. Before looking at further work towards this gallery in the 1930s I consider the Museum's increasing need to represent the concerns of industry, and how an exhibition on geophysics was suited to this aim. I end by examining two temporary exhibitions, both of which involved the input of physicists but presented very distinct notions of what current physical research looked like and why it was important. I suggest that in the 1936 exhibition 'Very Low Temperatures' we find an alternative 'modern' physics to that found in the Cavendish-focused 'Atom Tracks' of 1937. Furthermore, the Science Museum's depiction of these two competing visions of current science shows that even at the end of the 1930s an established, and uncontested, definition of modern physics had not been achieved. Different scientists worked to depict different notions of modern physics and the nature of scientific progress, managing the public reception of the changes underway in the discipline. The various publics which visited the Museum were thus being presented with a variety of characterisations of current research in physics.

6.2 The aims and obstacles of the Science Museum in the 1920s

From 1920 to 1933, the Science Museum was under the management of Henry Lyons, a geophysicist with a military background who had been working at the Museum since 1912.[2] Lyons' first duties concerned the problem of increasingly limited space. The Imperial War Museum had been founded in 1917 and temporarily housed at Crystal

[2] Follett (1978), pp.35-39; Dale (1944).

Palace. When this lease ran out in 1924, the government gave the War Museum the Science Museum's Western Galleries. The Science Museum had already planned to construct three new spaces (the Eastern, Central and Western blocks) but was now pressed to speed up this process. However, these would not be opened until 1928, 1961, and 2000, respectively. As a result, the Museum in the 1920s and 1930s had to be careful in its collecting policies, and fully aware of the purposes of the objects it was acquiring.[3]

With many of its collections in storage, and the construction of the Eastern block underway, the staff of the Museum under Lyons began to think about how to structure the collections. By the time of Lyons' appointment as Director, the collections were organised into two divisions: 'Machinery and Inventions' and 'Science'[4]. The science collections had originally been intended for artisans (and the teachers of artisans), who would learn how to apply science to industry, and then use this knowledge to obtain wealth for the country.[5] Lyons redefined the target audience of the Museum, from artisans and professionals to non-specialist visitors, including children.[6] In 1922, he listed four different types of visitors, in order of importance: ordinary visitors, technical visitors, students, and specialists.[7] The new structure of the collections needed to reflect a consideration of these different 'publics'.

In 1923, the collections were reorganised into four divisions: Industrial Machinery and Manufacture; Mechanical Engineering, Land Transport and Construction; Water Transport and Air Transport; and Science and Scientific Instruments. Within these four divisions were about 50 or 60 groups, representing a particular industry or branch of science. It was planned to divide each group into two collections of objects, depicting historical development and current practice. The Museum thus hoped to depict the progress of science and industry, with every object presented as performing a specific function more efficiently than had been previously done. The historical collections were to remain mostly stable and permanent. Those objects depicting current practice, however, were to be part of temporary collections, loaned to the Science Museum until they had been superseded or were no longer of interest. As the 1923 Annual Report noted, 30 to 40 per cent of its annual acquisitions were gifts, 50 to 60 per cent were

[3] Scheinfeldt (2010), pp.41-6.
[4] 'Science Museum Annual Reports for 1920', SM.
[5] For the changing conceptions of the museum's audience, see Mazda (1996).
[6] Mazda (1996), p.5.
[7] Mazda (1996), p.31.

loans, and only 5 to 10 per cent were purchased by the Museum or specially made in its workshops.[8] The various different types of visitors were presented a particular vision of science and industry, as progressive and useful.

The science collection was under the care of David Baxandall, who had worked at the Museum since 1898. Before then he had studied mechanics at the Royal College of Science and worked with the astronomer Norman Lockyer. Baxandall's particular interest was early scientific instruments, especially astronomical, optical and mathematical instruments.[9] The physics collections were grouped into different phenomena: optics, sound, heat and electricity and magnetism.[10] However, with many of the objects in storage, it was the perfect time to consider a new arrangement and, crucially, how to represent 'modern' physics at the Museum. In 1923, about a fifth of the science collections were put on display in the unfinished Eastern block: groups of objects from astronomy, surveying, meteorology, optics, chemistry, botany and sound.[11] The choice of which objects to redisplay first was later described as being not down to a 'predetermined and logical scheme', but rather 'opportunism', slotting in whatever would fit as space became available.[12] At the time, the collections still remaining in storage were noted as being 'in course of considerable development and rearrangement'.[13] Notably, the aspects of science that Baxandall was most familiar with, including astronomy and optics, were the first to go, in part, back on display. This suggests that these groups of objects were more complete, having had the attention of the keeper of science for some time, and thus that the choice to keep certain items in storage was made on the basis of how much additional restructuring was required. For many of the physics objects, including geophysics, heat and electricity, modern developments in the field were beginning to be taken into consideration.

In 1923, Lyons began preparations for an ambitious new scheme to reorganise the physics objects. At the centre of this was a new section, tentatively titled 'Properties of Matter and Physical Phenomena', which would present the late nineteenth and early twentieth century research into atomic structure. Lyons' own background had exposed him to a very different kind of physics from this. He had joined the military in 1881, and

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[8] 'Science Museum Annual Reports for 1923', SM, pp.4-6.
[9] See Baxandall's obituary in Anonymous (1928)
[10] Lyons to Bragg, 24 January 1925, SM-PHYS.
[11] 'Science Museum Annual Reports for 1923', SM, p.15.
[12] 'Science Museum Annual Reports for 1935', SM, p. 34.
[13] 'Science Museum Annual Reports for 1924', SM, p.6.

studied military engineering, but cultivated his interest in geology alongside this. In 1896, he began organising a Geological Survey of Egypt to determine the structure and mineral content of the country's deserts, and his duties here led to him retiring from the army. In his new role, he became involved with a wide range of scientific disciplines, including geology, geography, geodesy, geophysics, hydrology, meteorology and astronomy.[14] Before joining the Museum in 1911, Lyons' exposure to science, and physics, was thus mostly focused on its utility to practical concerns.[15] Furthermore, the aims of the Museum, which collected and displayed objects of science *and* industry, were also directed towards this end. It is thus necessary to ask why Lyons began to focus his attentions on an area of physics of apparently little practical value, and how he intended to present such research. I suggest that Lyons was influenced by his involvement in the 1924 British Empire Exhibition.

6.3 A Cavendish approach to 'modern' physics: 'Pure Science' at the 1924 British Empire Exhibition and 'Physical Phenomena' at the Science Museum

This exhibition was an ambitious, state funded celebration of the craftsmanship, agriculture and trading and transport organisations of all of the territories of the British Empire.[16] It took place in Wembley from April to November 1924, and then for a further six months in 1925. Here, the fruits of science were presented within the Palace of Industry, with sections organised either by an appointed committee or a representative association.[17] Each industrial section featured a science display, showing how scientific research had been applied to that industry, and for this the organising committee of the British Empire Exhibition enlisted the help of the governmental Department of Scientific and Industrial Research and various state Research Associations. However, it was also decided to have a central exhibit of 'pure science', organised by a committee chosen by the Royal Society.[18]

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[14] Dale (1944); Scheinfeldt (2010), pp.48-50.
[15] Although in the early 1890s he had also undertaken work of purely historical interest, researching with Norman Lockyer into the astronomical significance of the alignment of Egyptian temples. The work was originally detailed in Lockyer (1894), and has been recently discussed in Bud (2010).
[16] The aims were summarised by the Prince of Wales at the opening ceremony, Knight and Sabey (1984), p.12.
[17] For an overview of the British Empire Exhibition see Knight and Sabey (1984).
[18] 'Department of Scientific and Industrial Research. Advisory Council. Memorandum on the British Empire Exhibition 1924', 28 June 1922, British Empire Exhibition 1924 (CD/43), RS.

Richard Glazebrook was chosen to chair this committee, but was at first hesitant to take the post, due to time constraints.[19] Initially, the role was filled by F. E. Smith and Herbert Jackson, a former King's College, London chemist who was now Director of Research at the British Scientific Instrument Research Association.[20] However, Glazebrook remained on the committee throughout Jackson and Smith's reign, and took over as chair at some point in early 1924.[21] Thomas Martin, a scientific administrator with a background in metallurgical research, was appointed secretary of the exhibition and was responsible for much of the correspondence with exhibitors.[22] The remainder of the committee consisted of a combination of meteorologists, biologists, engineers, chemists and physicists, representing both academia and industry. This mixture was reflected in the exhibition itself, with sections on various types of physics, including precision measurement, geophysics and atmosphere physics, as well as biology. (Chemistry was featured in a separate exhibit, alongside its industrial uses, by the Joint Chemical Committee in cooperation with the Royal Society committee.) The central focus of the 'Pure Science' exhibit, however, was a display of research into the structure and behaviour of the tiny particles that made up matter.[23] The exhibit intended to display the principles of science, beginning with atomic and nuclear theory, rather than its instruments or technologies. Objects were arranged according to phenomena: the electron, thermionics, photo-electricity, positive rays, radioactivity, the origin of spectra, X-rays, wave measurement, metrology and metallurgy.[24] When the editor of the *Journal of Scientific Instruments* sent the committee a letter his journal had received criticising the exhibition for lacking any 'methodological' arrangement regarding the instruments, Martin replied that this had been 'an exhibition of Pure Science and only such instruments were shown as were necessary to illustrate fully the scientific work which

[19] Discussions concerning the appointment of the chair can be found in the Royal Society Archives: Royal Society to Herbert Jackson, 19 February 1923, Royal Society to Richard Glazebrook, 23 March 1923, British Empire Exhibition 1924 (CD/43); Richard Glazebrook to Hardy, 13 June 1923, British Empire Exhibition 1924 (CD/44), RS.
[20] Royal Society to Glazebrook, 6 July 1923, British Empire Exhibition 1924 (CD44), RS; For Jackson, see Moore (1938).
[21] T. Martin (Secretary of British Empire Exhibition) to John S. Anderson (editor of Journal of Scientific Instruments), 14 January 1925 , British Empire Exhibition 1924 Box 1, RS; Martin notes that Anderson has incorrectly referred to F. E. Smith as the Chairman, when in fact Glazebrook had been 'for the past year or more'.
[22] Obituary in Gale (1972). Martin served as general secretary of the Royal Institution from 1929 to 1950.
[23] The exhibits and committee are listed in the exhibition's official handbook, Royal Society (1924). This is different from *Phases of Modern Science* (Royal Society, 1925), which describes the committee and exhibits of the exhibition's second run in 1925. In Morton's (2000) study of the Pure Science Exhibition, he does not distinguish between the two, which causes some confusion: for example, he names Oliver Lodge as a member of the committee, as listed in Royal Society (1925), when in fact Lodge did not join the committee until 1925, for the sole purpose of contributing to the handbook. Here, my focus is on the 1924 exhibit, as it is the organization of this which influenced Lyons.
[24] Royal Society (1924).

was exhibited'.[25] The handbook explained that the purpose of the exhibition was to 'trace the history of each modern discovery or invention from its early stages to its later applications, and to show the manner in which those applications are based on the work and discoveries of men seeking, in the first place, to improve Natural Knowledge, to discover "how things go," without any thought of their ulterior applications'.[26] The aim was thus to depict a narrative of science and industry as arising from 'pure' investigations, unencumbered by thoughts of practical utility.

Through its central display, the exhibition emphasised reductionist experimental physics, investigations into the discontinuous nature of matter and the moral purpose of the pursuit of knowledge. This was a 'Cavendish' definition of 'modern' physics as promoted by Rutherford. It is thus unsurprising that the Cavendish was considerably involved in the organisation of this central display. Their involvement was decided early on and noted by a Joint Sub-Committee, chaired by Glazebrook and including William Bragg and Herbert Jackson. This was formed in October 1923 to deal with the problem of subjects on the boundaries of physics and chemistry, and early meetings listed these topics as atomic structure, spectroscopy and metallography.[27] The sub-committee drew up a list of proposed exhibits on this borderline, beginning with a number requiring input from the Cavendish: the 'Electron and its story', thermionics, photo electricity, positive rays, radioactivity and electron tracks. Outside of the Cavendish, the committee also suggested a display of X-ray analysis by William Bragg, spectroscopy work (and its connection with atomic theory) and metallography.[28] Glazebrook was to act on behalf of both the Royal Society and the Joint Chemical Committee to approach potential exhibitors and try to obtain duplicates where possible for both the chemistry and pure science section. Discussions quickly began with the Cavendish Laboratory, represented by Patrick Blackett, the point of contact between the Committee and the laboratory.[29] With Blackett's cooperation, the committee acquired details of exhibits to be supplied by Rutherford, C. T. R. Wilson and F. W. Aston. Wilson contributed photographs of atomic

[25] John S. Anderson (editor of Journal of Scientific Instruments) to Thomas Martin (Secretary of the British Empire Exhibition), 13 January 1925; Thomas Martin (Secretary of the British Empire Exhibition to John S. Anderson (editor of Journal of Scientific Instruments), British Empire Exhibition 1924 Box 1, RS; The letter was published as Macalpine (1925).
[26] Royal Society (1924), p.143.
[27] 'Notes of a meeting of the Joint Sub-Committee appointed by Minute 7 October 22nd', File:Carpenter – County Chemical Co., British Empire Exhibition 1924, RS.
[28] 'Notes for Joint Exhibits', File:Carpenter – County Chemical Co., British Empire Exhibition 1924, RS.
[29] Thomas Martin (Secretary of the British Empire Exhibition) to PMS Blackett, 4 December 1923, File: Cambridge, British Empire Exhibition Box 3, RS.

structure taken using his cloud chamber apparatus, developed in 1912.[30] As I shall later discuss, this equipment formed the basis of an exhibition at the Science Museum in 1937. Aston was involved for his spectrographic work on isotopes, which had been vigorously supported by Rutherford as contributing to work on his nuclear model of the atom, and earned Aston the Nobel Prize for Chemistry in 1922.[31] Rutherford, meanwhile, provided a lengthy proposal for a section on radioactivity and its bearing on atomic structure, to include both his own work and that of others.[32] These contributions were to form the basis of the 'Pure Science' exhibit.

The very centre of the 'Cavendish Laboratory exhibit', however, was the cathode ray tube used by J. J. Thomson in what had now come to be viewed as the discovery of the electron. Negotiations with Thomson were separate to those with Blackett, as he was based in a separate part of the laboratory, since retiring from his Directorship in 1919. Furthermore, it transpired that this apparatus was in fact currently held at the Science Museum.[33] As Morton has noted, the tube had originally been displayed at the Museum as a device used for the measurement of the velocity of Cathode rays and the ratio of charge to mass. At the British Empire Exhibition, however, the tube was depicted as apparatus used to detect the existence of electrons.[34] Furthermore, the exhibit's handbook published in 1924 featured a short essay by Thomson on 'The Electron' in which he noted that it had been 'discovered' in 1897, thus equating his experiments on corpuscles with the discovery of the electron.[35] The exhibition was promoting the 'discovery' story. It also served as an example of how an object already on loan to the Science Museum could be displayed as part of an exhibit of 'modern' physics, in a celebration of the physics of the very small. As I shall shortly show, it appears as though Lyons was paying close attention and considering how he too could apply 'modern' interpretations to the Museum's collections.

Furthermore, the Museum's involvement did not end with the contribution of the cathode ray tube. Lyons was involved in early conversations concerning how science

[30] 'Particulars of Exhibition Proposed by C. T. R. Wilson', File: Cambridge, British Empire Exhibition Box 3, RS; For a discussion of the cloud chamber's development and use, see Galison and Assmus (1989).
[31] 'Particulars of Exhibition Proposed by F. W. Aston', File: Cambridge, British Empire Exhibition Box 3, RS; For Aston's spectroscopy work, and its appropriation by Rutherford, see Hughes (2009b).
[32] 'Exhibits by Sir Ernest Rutherford', File: Cambridge, British Empire Exhibition Box 3, RS.
[33] Thomas Martin (Secretary of the British Empire Exhibition) to PMS Blackett, 22 December 1923, File: Cambridge, British Empire Exhibition Box 3, RS.
[34] Morton (2000), pp.34-6.
[35] Royal Society (1924), p.13.

would be represented in the British Empire Exhibition as a whole, and was added to the Royal Society's Committee in July 1923.[36] Although his involvement was officially as part of a Geophysics Subcommittee, Lyons also acted as a representative of the Science Museum.[37] In November 1923, Glazebrook, Lyons and Thomas Martin toured the Museum to draw up a list of objects there that could be used for the display at Wembley.[38] This included a number of X-ray tubes, including ones used by Oliver Lodge, and a selection of 'Fleming valves', designed by the electrical engineer J. A. Fleming for use in wireless telegraphy and telephony.[39] The tubes were placed in the X-ray section of the exhibit, while Fleming's thermionic valve formed part of the thermionics exhibit, illustrating the industrial applications of this 'pure science'.[40] Here, as with Thomson's cathode ray tube, was evidence of the successful appropriation of objects in the Science Museum as part of a history of 'modern' physics. The British Empire Exhibition was thus serving to illustrate how some of the Museum's existing collections could form part of such a display. As I shall show, Lyons was already beginning to think about how he could reproduce the 'Pure Science' exhibit in his Museum.

The exhibit was not merely inspiring Lyons, but also supplying the Museum with several objects. This included an exhibit of thermionics apparatus supplied by O. W. Richardson, who had been approached by the Science Museum before being asked to contribute to the British Empire Exhibition. Such work clearly fit well in the narrative being constructed by the Royal Society committee, building as it did on Thomson's "discovery" of the electron. Shortly after the Science Museum had approached Richardson with the hope of acquiring items for a display of this work on thermionics, the Royal Society committee had the same idea. Richardson, replying to Glazebrook, noted that such a display was difficult to achieve, because much of the apparatus was very fragile and hadn't lasted. However, he suggested that, under his supervision, models could be made in place of the objects that hadn't survived. These could be displayed at the British Empire Exhibition, before finding a permanent home at the

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[36] 'Letters concerning the British Empire Exhibition Committee 1925', MS777, RS.
[37] 'Geophysical Sub-Committee Members', File: Geophysical Sub-Committee (A), British Empire Exhibition Box 4, RS.
[38] Thomas Martin (Secretary of the British Empire Exhibition) to Henry Lyons, 26 November 1923, File: Science Museum, Ryons, Rontgen Society, Dr. Owen, British Empire Exhibition Box 7, RS.
[39] 'Note of visit to Science Museum with Sir R. Glazebrook and Col Lyons', 22 November [1923], File: Science Museum, Ryons, Rontgen Society, Dr. Owen, British Empire Exhibition Box 7, RS.
[40] Royal Society (1924), pp. 156, 146-8.

Science Museum.[41] The construction of the models was jointly funded by the British Empire Exhibition and the Science Museum.[42] While the Museum was here acquiring objects it had already been seeking, at a discounted price, there were many other new items also offered up. As the exhibition came to a close, Thomas Martin wrote to the exhibitors who had specially constructed models, asking if they would be happy for the models to be housed at the Science Museum.[43] The Museum was thus provided not only with the inspiration for a 'modern' physics gallery, but also some of the content. The Royal Society's 'Pure Science' exhibit did not consist solely of reductionist Cavendish style physics. There was also a section on metrology, featuring a number of contributions from the National Physical Laboratory, of which Glazebrook had been Director from 1899 to 1919. The laboratory was now under the management of Joseph Petavel, who was also on the committee, and J. E. Sears, Superintendent of the Metrology Department, contributed an essay for the handbook on 'The Principles of Fine Measurement'.[44] The National Physical Laboratory was also responsible for a number of items in the metallurgy section. A small section on astronomy and astrophysics featured photographs of meteors (supplied by F. A Lindemann and G. M. B. Dobson) as well as photographs of the 'Verification of Einstein's Theory of Relativity', supplied by the Astronomer Royal Frank Dyson. There was also the geophysics section, in which Lyons had been involved, including geodesy, seismology, terrestrial magnetism, atmospheric electricity, meteorology, hydrology and atmospheric pollution.[45] With limited space at Wembley, a group of objects intended for this exhibit were instead displayed at the Science Museum while the British Empire Exhibition was ongoing.[46] However, it was the displays of reductionist 'modern' physics which appear to have affected Lyons' approach to the Science Museum most significantly. In December 1923, as plans for the British Empire Exhibition were underway, Lyons sent a

[41] Richardson to Glazebrook, 1 November 1923, OWR.
[42] Thomas Martin (Secretary of the British Empire Exhibition) to Henry Lyons, 22 January 1924, File: Science Museum, Ryons, Rontgen Society, Dr. Owen, British Empire Exhibition Box 7, RS; 'Funds. Estimates required by the Department of Overseas Trade', File: Richard Glazebrook, British Empire Exhibition Box 7, RS.
[43] For example, Thomas Martin (Secretary of the British Empire Exhibition) to W L Bragg, 22 October 1925, WL Bragg to Thomas Martin (Secretary of the British Empire Exhibition), 27 October 1925, British Empire Exhibition Box 1, RS.
[44] Royal Society (1924), pp.69-76.
[45] The content of the exhibition is detailed in Royal Society (1924).
[46] This is discussed in Thomas Martin (Secretary of the British Empire Exhibition) to the National Physical Laboratory, 7 March 1924, File: Geophysical Sub-Committee (A), British Empire Exhibition Box 4, RS.

memo to Baxandall detailing his plan to create a new section displaying ‘Properties of Matter and Physical Phenomena’.[47]

Lyons’ correspondence with Science Museum staff and external physicists reveals the influence that the ‘Pure Science’ exhibit had on his thoughts about how physics should be displayed at the Science Museum. In later letters to both William Bragg, with whom he’d worked on the Wembley exhibit, and Andrade, who Bragg suggested as an ideal advisor for ‘Physical Phenomena’, Lyons explained his wish to create something similar to the Royal Society’s exhibit.[48] Lyons’ plan was to replace the categories of physical phenomena (heat, light, sound, etc.) with sections of optical, thermal, acoustic and electrical instruments, detailing their development. Alongside this, ‘Physical Phenomena’ would display ‘some modern conceptions of fundamental phenomena’.[49] Lyons saw the current Science Museum arrangement as a problem because the discovery and development of many new physical theories did not fit into only one phenomenon.[50] He could see no way of slotting elements of the ‘Pure Science’ exhibition into existing Science Museum sections.

Concurrently with the development of ‘Physical Phenomena’, Lyons was attempting to arrange the acquisition of such elements. A week after sending Baxandall his thoughts about a new gallery, Lyons wrote to Rutherford to ask if the Science Museum could have the models of atomic structure created at the Cavendish for the Wembley exhibition or if, failing that, a duplicate set could be produced at the Cavendish, and paid for by the Museum. He declared that it was ‘not creditable that there is no reference to atomic structure and what is known of it in the Museum at the present time.’[51] While Lyons was beginning negotiations with Rutherford, the Museum and the British Empire Exhibition committee were already communicating with Richardson about the thermionics display. It would seem that Lyons was using Richardson’s idea of creating models for temporary display at the British Empire Exhibition and permanent display at the Science Museum, and extending it. He was hoping to capitalize on the opportunity of the British Empire Exhibition to acquire relevant objects for ‘Physical Phenomena’.

[47] Lyons to Baxandall, 12 December 1923, SM-PHYS.
[48] Lyons to WH Bragg, 24 January 1925, SM-PHYS; Lyons to Andrade, 27 January 1925, SM-PHYS.
[49] Lyons to WH Bragg, 24 January 1925, SM-PHYS.
[50] Lyons to Baxandall, 12 December 1923, SM-PHYS.
[51] Lyons to Rutherford, 19 December 1923, Nominal File: Rt. Hon. Lord Rutherford (1494), SM.

Before seeking outside help in the planning of the new section, Lyons placed 'Physical Phenomena' under the charge of Herman Shaw, assistant to Baxandall.[52] Shaw, as a geophysicist, had similar scientific interests to Lyons and had been hired in 1920 shortly after Lyons' appointment as Director.[53] However, his scheme failed to impress Lyons. Shaw proposed arranging the physics collection into seven groups: Constitution of Matter (a suitable home for many of the 'Pure Science' exhibits); Properties of Matter (which would contain relativity); General Physical Phenomena; Thermal Phenomena (to house Richardson's thermionics display); Optical Phenomena; Acoustical Phenomena; and Magnetic and Electrical Phenomena.[54] While Lyons' original memo had, somewhat ambiguously, suggested a rethink of the existing schemes for electricity, heat, light and sound, his later correspondence with Andrade and Bragg made it clear that he was looking for a complete shift in emphasis, replacing phenomena with instruments. Such a scheme would differentiate 'Physical Phenomena' from the other sections, affording it a central role, as 'Pure Science' had taken amid the industrial sections at Wembley. The influence which the 1924 exhibit had on Lyons continued to be evident.

Indeed, Lyons' principal problem with Shaw's scheme was that it followed the physics collection's existing organisational structure too closely. Lyons, impressed by an exhibit he had seen *outside* of the Museum, was looking for something completely different. Indeed, he suggested that Shaw take notes of the content of the Wembley exhibit, with regards to what should be represented at the Science Museum.[55] However, in Lyons' opinion, Shaw persisted in structuring his scheme too strictly around existing Science Museum acquisitions, and in early 1925 Lyons began seeking outside advice.[56] Presumably on Lyons' request, Shaw drew up a list of three physicists who might be of assistance: Aston, Andrade and the Imperial College based Herbert Dingle.[57] Baxandall advised Lyons that, while of these three men he would choose Dingle, he did not think anybody was better suited to the task than Shaw himself.[58] It was at this point that Lyons stopped listening to his staff entirely on this matter, seeking advice from William Bragg and admitting that he was 'not at all sure that those here are fully competent to advise'.[59]

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[52] SM-PHYS.
[53] Ward (1950).
[54] 'Properties of Matter and Physical Phenomena', SM-PHYS.
[55] 'Rutherford, Sir E.' Lyons to Shaw, 4 March 1924, Nominal File: Rt. Hon. Lord Rutherford (1494), SM.
[56] In a letter to Bragg, Lyons noted that he preferred to enlist outside men for such a job because they wouldn't be overly influenced by existing collections, Lyons to WH Bragg, 24 January 1925, SM-PHYS.
[57] Shaw to Baxandall, 21 January 1925, SM-PHYS.
[58] Baxandall to Lyons, 23 January 1925, SM-PHYS.
[59] Lyons to WH Bragg, 24 January 1925, SM-PHYS.

Crucially, Bragg himself had been involved with the Pure Science exhibition which Lyons was hoping to emulate.

Bragg recommended Andrade, on the basis of his logical mind, wide knowledge of physics and interest in the history of the development of scientific instruments. Lyons took this advice and wrote to Andrade, again emphasising his desire to have ‘new ideas which would probably never occur to a museum officer handicapped by a too intimate knowledge of the collections themselves’.[60] Progress was initially hindered by Andrade’s work preparing lectures for the Royal Institution and Lyons’ subsequent trip to Egypt.[61] However, after this, work on Physical Phenomena stopped entirely, and would not be resurrected until the 1930s. In the autumn of 1925, much of the physics collections were moved out of storage and put on display in the Eastern block (which was now partially open). This included electrical, acoustical and thermal instruments, suggesting that Lyons’ desire to group the collection by instruments rather than phenomena had been taken up. However, his central ‘Physical Phenomena’ section appears to have disappeared. It was noted that ‘much still has to be done before the arrangement of the collections there will be complete’ but that the objects were ‘now in order and available to the public’.[62] Priority was given to taking the collections out of storage, rather than restructuring them.

With the physics collections now on display, there was a less pressing need to reorganise them. Furthermore, I suggest that changing priorities of the Science Museum resulted in attention being directed away from ‘Physical Phenomena’, which intended to promote a Cavendish definition of ‘modern’ physics. As the 1920s progressed, the Museum began to pay closer attention to its relationship to industry, and the version of ‘modern’ physics that Lyons had been planning to present was no longer relevant to the Museum’s needs. The following section will explore how demand to represent industrial, and governmental, needs required from the Museum a very different representation of physics from that proposed in ‘Physical Phenomena’.

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[60] Lyons to Andrade, 27 January 1925, SM-PHYS.
[61] Lyons to Andrade, 27 January 1925; Lyons to Andrade, 11 May 1925, SM-PHYS.
[62] ‘Science Museum Annual Reports for 1925’, SM, p.7.

6.4 An increasing interest in industry at the Science Museum, 1926-1936

Throughout the second half of the 1920s, we find increasing attention being paid by the Museum to more fully representing the industrial results of recent scientific research. The 1926 Annual Report detailed the Museum's 'present policy' in terms of illustrating the development and current practice of 'industry' and 'industrial groups'.[63] That year, a gallery was set aside exclusively for temporary exhibitions 'in order that the public might have an opportunity of seeing what is being done in some directions to develop industry by the application of science and scientific methods.'[64] The Department of Scientific and Industrial Research (DSIR) was informed, and distributed information to the various Research Associations, explaining that either they or their related industries now had the opportunity of collaborating with the Science Museum.[65] The new gallery solved many of the Museum's problems. As Morris has noted, such exhibitions allowed the Museum to deal with the pressures of industry, without letting them exert too much influence on the permanent collections.[66] Furthermore, it appears that the problems of space and money were also important factors. In 1923, as I have shown, Lyons split the collections into categories of historical and current practice, with the latter intended to be temporary on account of space limitations. With temporary 'Research Exhibitions', as they began to be called, the Museum could put on temporary displays of current practice at very little cost or effort, with the Research Association responsible for planning and organising the display, and providing objects.[67] With one dedicated room set aside for this purpose, the Museum had solved the problem of finding space in the collections to slot in loaned objects displaying current practice. Furthermore, the Science Museum approached the DSIR, a government run organisation. As I shall show, in the following years there was increasing pressure for the Museum to display nationally funded practical research.

The first two organisations to take advantage of the new opportunity were the DSIR's Research Committee on Adhesives and the National Physical Laboratory (NPL).[68] The

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[63] 'Science Museum Annual Reports for 1926', SM, p.9.
[64] 'Science Museum Annual Reports for 1926',SM, p.6.
[65] A. L. Hetherington (Department of Scientific and Industrial Research) to Anon (Research Association), 10 June 1926, SM-SPE.
[66] Morris (2010b), p.212.
[67] Lyons to Secretary, Board of Education, 1 November 1926; Lyons to Secretary, Board of Education, 15 February 1927, SM-SPE.
[68] 'Science Museum Annual Reports for 1926', SM, pp.6-7.

NPL exhibition featured the apparatus and materials relating to research in its departments of metallurgy, engineering and physics.[69] As I discussed in Chapter One, the NPL represented a very different type of 'modern' physics to the Cavendish, and this exhibition was thus a considerable departure from the 'Physical Phenomena' scheme. The physics section of the NPL exhibition featured apparatus that determined moisture amounts and thermal conductivity in cold storage, equipment used to estimate the efficacy of different materials in protecting against X-rays, vacuum pumps, and apparatus used to study acoustical problems.[70] It was an exhibition of how current physical research could be applied to industry. In taking up this opportunity, the NPL could advertise its work, and forge new links with industrial partners, while the Science Museum was able to present current research in science, at little cost to itself. As a result, a very different kind of physics from that envisaged by Lyons in his 'Physical Phenomena' gallery was presented.

The need to depict industrially relevant physics became of increasing importance in the economic and political context of the early 1930s. In the aftermath of the 1929 Wall Street crash, Britain was faced with a severe economic depression. With this came a growing feeling among scientists that they needed to justify their research to society and portray their work as serving a useful and necessary function.[71] This was particularly the case in the Cavendish, where research was directed towards the 'pure' subject of the structure of matter. Rutherford, who chaired the Advisory Board for the Department of Scientific and Industrial Research from 1931 to 1937, found himself repeatedly attacked by his colleagues there for 'misusing gifted young men in the Cavendish to transform them into scientists chasing useless knowledge'.[72] There was a growing need for physicists to promote their research as 'useful' and in the 'public' and national interest. One effective way of doing this was to emphasise the links between science and industry.

In the 1930s, the Science Museum itself also came under pressure to represent the needs of industry. In 1930, during a Royal Commission on Museums, the Federation of British Industries advised that the Science Museum become 'of more practical use to those

[69] 'An Exhibition of Material and Apparatus used in Research', 1926, SM-SPE.
[70] 'An Exhibition of Material and Apparatus used in Research', 1926, SM-SPE.
[71] McGucken (1984).
[72] Rutherford quoted in Oliphant (1972), p.146.

actually engaged in industry as distinct from students'.[73] In accordance with this, the Museum's Advisory Council was expanded, the number of members increasing from ten in 1925 to thirty two in 1931. Nearly all of these new members represented industrial concerns.[74] The 1932 Annual Report, produced by the Advisory Council, noted that while the Museum had always attracted large numbers of 'those engaged in technical industry', it was less well known to those occupying leading positions.[75] The value of appealing to such industrialists had been evident in the 1926 Adhesives exhibition, after which both the DSIR and Science Museum had been contacted by 'traders and others interested in utilizing the results in their business'.[76] A need to attract this new audience, and thus help create links between industry and national scientific research, influenced the representation of science at the Museum in the 1930s. As I shall show, an industrially relevant version of 'modern' physics was promoted.

This was intensified in 1933, when E. E. B. Mackintosh took over as Director. His enthusiasm for special, industrially-led, exhibitions led to a marked increase in frequency.[77] In 1939, he summarised the purposes of these exhibitions:

> 'to explain to the community the how and why of what they regard as the material and everyday things of life, and to stimulate their interest in the research and up-to-date progress of a science or industry in this country. In particular the object is to be educative and helpful to the serious student and research worker, and to provide a valuable and visual instrument of instruction to the rising generation.
>
> 'In the case of an industrial exhibition, it exposes and advertises the latest advances in the industry concerned, and at the same time gives interest and incentive to other industries.'[78]

The aim was thus to promote the importance of British industry to the general 'public', and to provide an educational service to scientific and industrial students and research workers.

As we move from the 1920s to the 1930s, we find the Museum's aims focused towards its relationship with industry, partly in response to a changing relationship between the 'public' and science, as evident in the Social Relations of Science movement. As I shall

[73] Quoted in Scheinfeldt (2010), p.52.
[74] Scheinfeldt (2010), p.54.
[75] 'Science Museum Annual Reports for 1932', SM, p.5.
[76] Lyons to Secretary, Board of Education, 15 February 1927, SM-SPE.
[77] Morris (2010b), pp.213-4.
[78] E. E. B. Mackintosh, 'Special Exhibitions at the Science Museum', March 1939, Special Exhibitions Guide (Z62), p.1, SM.

explore in this chapter, scientists could use the Science Museum's temporary exhibitions to present their research as worthwhile. As a result, a number of exhibitions portrayed the practical results of physical research, a far cry from the Royal Society's 'Pure Science' exhibition that had influenced Lyons. However, the Museum was not exclusively devoted to this new aim. In the remainder of this chapter, I consider the display of two competing visions of 'modern' physics at the Museum in the 1930s. Cavendish-style research into atomic structure influenced attempts to again develop 'Physical Phenomena' and formed the basis of a 1937 'Atom Tracks' exhibition. Meanwhile, exhibitions on applied geophysics and low temperature research conveyed the work of physicists as industrially relevant and centred on the improvement of technique and apparatus, rather than the development of new esoteric theories. These temporary exhibitions conveyed the results of physical research that we would now consider as 'classical', but at the Museum this work was portrayed as 'cutting edge' research, which indeed much of it was. I shall show that even in the 1930s, the lines between 'classical' and 'modern' physics were not sharply distinct, and debates were still underway as to just what definitions these terms should be given. This was reflected in the displays at the Science Museum.

6.5 'Physical Phenomena' in the 1930s: Cavendish 'modern' physics at the Science Museum

The year 1930 saw a restructuring of the science collections into two divisions. Division IV was then known as 'Optical Instruments, Chemistry', but would quickly lose its title altogether on account of the lack of cohesion in this group (which also contained astronomy, mathematics and photography). The fifth division contained 'Physical and Geophysical Instruments', with the physics objects sorted into electrical and magnetic instruments, thermal instruments and acoustical instruments.[79] (See Table 6.1) Geophysics was displayed separately to physics, as had also been the case at the British Empire Exhibition.[80] The division of science into two mirrored the physical structure of the science collections, which had been split into two galleries on their move out of storage. Baxandall, whose interests lay in astronomy, optics and mathematics, was made Keeper of Division IV, while Shaw, a geophysicist, was promoted to keeper of Division V. He was accompanied by two assistant keepers: W. G. Plummer had worked on crystal

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[79] 'Science Museum Annual Reports for 1930', pp.16-17, SM.
[80] Royal Society (1924).

structure under William Bragg at the Royal Institution, and was hired by the Science Museum in 1928.[81] He was joined in 1931 by F. A. B. Ward, fresh from the Cavendish where he had been conducting research on alpha-rays with Rutherford.[82] As I detailed in Chapter One, Bragg and Rutherford were perhaps the most successful of the early twentieth century 'Cavendish diaspora', trained under Thomson in experimental research of subatomic particles before establishing their own research schools. Plummer and Ward were thus both continuations of this tradition. Both of these appointments were made while Lyons was still Director, suggesting a deliberate hiring policy on his part. He was continuing to approach the physics collection with the Pure Science exhibition in mind.

Shaw, who had failed to impress Lyons with his scheme of the physics collections in the 1920s, was best placed to oversee the geophysical collections. Meanwhile, Ward was given responsibility of resurrecting 'Physical Phenomena and the Properties of Matter', beginning work on this in 1934. Lyons was no longer Director, having retired in 1933, but he would become Chairman of the Advisory Council in 1935, thus exerting a continuing influence on the management of the Museum.[83] Furthermore, with 'Physical Phenomena' his original idea, it seems plausible that he proposed the resurrection in one of his last acts as Director. Ward suggested splitting the subject into 'The Structure of Matter' and 'Physical Phenomena'. The first of these would illustrate 'the present state of our knowledge relating to the structure of the atom', as well as the assembly of atoms into molecules, and molecules into gases, liquids and various forms of solid. Ward hoped to exhibit, where possible, the experimental evidence on which existing knowledge was based, as well as a depiction of the historical development towards our understanding of the structure. His scheme for this section laid out the sub-section of radioactivity, with its bearing on the structure of the atomic nucleus, in particular detail. As Ward noted, he was 'relatively familiar' with these ideas.[84]

'Physical Phenomena', meanwhile, was envisioned as a 'unifying section', 'illustrating firstly the way in which some important principle appears in widely different branches of Physics, and secondly illustrating how each physical principle has important consequences or applications in everyday life'. There would be no historical background

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[81] Plummer (1925); Follett (1978), p.61.
[82] Ward (1987); Rutherford, Ward and Wynn-Williams (1930); Rutherford, Ward and Lewis (1931).
[83] Dale (1944), p.803.
[84] Ward to Shaw, 10 July 1934, SM-PHYS.

in this section, as this would be covered wherever the phenomena appeared elsewhere in the Museum. He proposed dividing Physical Phenomena into four subsections: Vibrations and Wave-Motion; Thermal Phenomena; Electrical Phenomena; and Optical Phenomena. This section would, as later detailed in the Annual Report, tackle the problem of 'the growing fluidity and interpenetration of the sciences', the blurring of lines between different disciplines and subdisciplines.[85] It was to serve as an introduction to a number of phenomena which would then be referred to, in a variety of contexts, throughout the remainder of the physics collection.

For Ward, the 'Structure of Matter' section would be the Museum's representation of 'modern' physics. Ward's particular definition of this had been formed during his training with Rutherford at the Cavendish. During the early 1930s, Cavendish physics was reaching a wider 'public' platform as researchers at the Laboratory collaborated with scientific journalist J. G. Crowther to disseminate the results of their work. When in February 1932, James Chadwick, Rutherford's Deputy at the Cavendish, found experimental evidence suggesting the existence of the neutron, Crowther was responsible for conveying this work to the 'public', emphasising the 'intellectual' value of this reductionist physics, and downplaying any industrial or commercial applications. This was in response to the wishes of the Cavendish researchers, including Rutherford. Even further publicity was achieved, following the 'splitting of the atom' by John Cockcroft and Ernest Walton, also in 1932. The nascent field of nuclear physics, under development at the Cavendish, was promoted in newspapers and non-specialist magazines throughout the 1930s.[86] From this, a particular understanding of 'modern' physics emerged, characterised by research into the minute particles which made up matter, and an understanding of the purpose of science as of intellectual rather than industrial value.

This was the definition of 'modern' physics promoted by Ward in the development of the new gallery. The 1935 Annual Report featured a description (presumably heavily influenced, if not written, by Ward) of the 'Structure of Matter' section as illustrating the development of 'what is somewhat loosely termed "Modern Physics" or "The New Physics" as distinct from "Classical Physics"'.[87] This would seem to be the first explicit

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[85] 'Science Museum Annual Reports for 1934', pp.26-7, SM.
[86] Crowther's relationship with the Cavendish Laboratory, and the neutron and 'atom splitting' publicity, has been explored in depth by Hughes (2009a).
[87] 'Science Museum Annual Reports for 1935', p.35, SM.

use of the term 'modern physics' (to describe a new kind of physics, rather than simply new work) by the Science Museum.

> 'Broadly speaking, whereas classical physics was concerned with the behaviour of matter in bulk, the new physics deals with the properties of individual molecules and atoms and with the structure of the atom itself. It arose originally from a study of the discharge of electricity through gases at low pressures and of the phenomena of radio-activity, but it has extended its scope enormously and its ideas now permeate the whole of Physics – naturally enough, since it is concerned with the ultimate structure of matter'.[88]

This modern physics was an exploration into atomic structure, seen to be underpinning the entire discipline. The 'Structure of Matter' section was to be historical, with four sections dealing 'mainly with classical physics', and a final two with modern ideas. In his scheme, Ward was thus given the power to not only define modern physics but also write its history. The foundations mentioned in Ward's definition of modern physics are clearly references to J. J. Thomson and Rutherford, the two prominent twentieth Century directors of the Cavendish laboratory.

Notably, Ward's definition leaves no room for relativity, which was to remain in the astronomy section of the Museum.[89] Ward certainly wouldn't have been directly exposed to relativity theory by Rutherford, who was dismissive of it with relation to his own work.[90] There was the opportunity to attend Eddington's lectures on the subject, but Ward made no mention of these in his reminiscences (which detail the content of his physics training).[91] Furthermore, the collections which the Science Museum held pertaining to relativity theory were photographs taken during the 1919 expedition, and thus already connected to astronomy.[92] Relativity had also been grouped under astronomy at the British Empire Exhibition, again with photographs of the eclipse expedition.[93] Relativity had a pre-existing disciplinary space at the Museum, and Ward had no reason to change this. The 'Physical Phenomena' gallery was presenting only one possible version of modern physics, equated with nuclear physics. Relativity theory,

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[88] 'Science Museum Annual Reports for 1935', p.35, SM.
[89] H. Spencer Jones to Lyons, 11 December 1924, Schemes for Development: Astronomy, 1925-1957 (ED79/118), SM.
[90] See Chapter Four.
[91] de Bruyne (1984), p.87; Ward (1987) recalled studying thermionics and X-rays, quantum theory, and attending Rutherford's course on the structure of matter.
[92] These had been acquired in 1920: F. G. Ogilvie (Director) to Astronomer Royal, 27 January 1920, Nominal File: Royal Observatory, SM.
[93] Royal Society (1924), pp.196-7.

another form of ‘modern’ physics, was not situated professionally alongside Ward’s ‘modern’ physics, nor was it conveyed to the ‘public’ in the same terms.

The year after Ward’s detailed account there was nothing to report, with it being noted that little progress had been possible ‘owing to pressure of other work’.[94] Some of this ‘other work’ underway in Division V was the development of the geophysics collection. Shaw, who had a geophysical background, maintained this interest throughout his career at the Museum. In 1920, the same year that he was hired, the Museum purchased the Eötvös Gravity Torsion balance, a device used to measure the gravitational acceleration at the Earth’s surface.[95] Originally used in geodesy, the balance later found a second use in determining the existence and nature of underground mineral deposits. On Lyons’ instruction, Shaw and his then assistant Ernest Lancaster-Jones spent many years working with this object. The work earned Shaw his doctorate and attracted attention from the Australian Government and the Geophysical Survey Research Committee of the DSIR.[96] With Ward now responsible for ‘Structure of Matter’ and ‘Physical Phenomena’, Shaw could continue to devote his efforts towards geophysics. The split of Division V into ‘physics’ and ‘geophysics’ provides an interesting framework for thinking about ‘modern’ or ‘new’ physics during the 1930s. In geophysics, we find a very different definition to that being promoted by Ward.

6.6 Applied geophysics as an alternative ‘modern’ physics at the Science Museum

In 1932, Alexander Oliver Rankine, Professor of Physics at Imperial College, delivered the Presidential Address of Section A at that year’s meeting of the British Association in York. Rankine was known for his work on the viscosity of gases, but from about 1928 he had become interested in applied geophysics, and the title of his address was ‘Some Aspects of Applied Geophysics’.[97] Here, Rankine noted that this subject was ‘as different as it could very well be from those flights of theoretical physics – relativity, quantum theory, wave mechanics, and the like – which those of us with slower minds and more pressing other occupations try so desperately to follow. In our admiration and,

[94] ‘Science Museum Annual Reports for 1936’, p.25, SM.
[95] ‘Science Museum Annual Reports for 1920’ SM; The work is detailed in Shaw and Lancaster-Jones (1922a, 1922b, 1923); For Lancaster-Jones see Shaw (1945).
[96] Follett (1978), pp.152-3; ‘Science Museum Annual Reports for 1935’, pp.48-9, SM.
[97] Thomson (1956); Rankine (1932).

perhaps, envy of the apparent ease with which the pioneers in these new fields make progress, we are inclined, wrongly, I think, to allow it to be assumed *that modern physics and atomic physics are one and the same thing*.'[98]

Rankine pointed out that atomic physics was, of course, not the only kind of new physics around. Where atomic physicists attacked new problems, other kinds of physicists were making great advances in 'the precision of measurement, in the choice of methods, and in the invention and design of physical tools' in order to tackle 'old problems hitherto unsolved'. The 'fundamental basis' of applied geophysics was not new, resting as it did not on 'let us say, wave mechanics', but rather 'the old gravitational theory of Newton and the electromagnetic theory of Maxwell'. However, Rankine argued that 'its successful application continues to demand the highest experimental skill that training in physics can provide, and initiative ability equal to that more frequently directed in less practical channels'.[99] This style of practical physics was not only characteristic of geophysics but also the wide variety of work underway at the National Physical Laboratory, and, as I shall show, research into very low temperatures.

Furthermore, practical physics was represented at the Science Museum. It seems likely that Shaw had some contact with Rankine, who was also based in South Kensington. Shaw, Rankine and Lancaster-Jones (who was relocated from the science collections to the Science Museum Library in 1928) were also all involved with the Imperial Geophysical Experimental Survey, a study undertaken to ascertain the utility of geophysical methods for prospecting in Australia. All three men advised on different sections of the published report.[100] In 1931, the Museum began devoting a section of the collections to 'Applied Geophysics', and held a temporary exhibition on the subject, displaying the methods and equipments used in geophysical prospecting. An accompanying handbook, Shaw's responsibility, was printed in three editions.[101] Here, it was noted that the application of geophysics to the 'investigation of subterranean conditions and anomalies' was a 'comparatively recent advance, which has occurred mainly during the last ten years, and continues to develop rapidly.'[102] Furthermore, as

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98 Rankine (1932), p.421, italics my own.
99 Rankine (1932), p.421.
100 Broughton Edge and Labey (1931).
101 'Science Museum Annual Reports for 1931', p.14, SM; 'Science Museum Annual Reports for 1935, p.49, SM; The handbooks were published in 1931, 1932 and 1936; A report of the exhibition and handbook can be found in Broughton Edge (1931).
102 Shaw (1931). p.3.

'might be expected in such a new science as Applied Geophysics, development continues to be very rapid, and the instruments and methods are continually undergoing changes and modification which tend to improve their sensitivity'.[103] This statement, published a year after Rankine's British Association address, is in accord with his reference to precision measurement and instrument design, whilst also emphasising the novelty of the subdiscipline. The content of the handbook was divided into Magnetic Methods, Gravity Methods, Seismic Methods and Electrical Methods. The catalogue of exhibits followed this division, but ended each section with the results of surveys conducted using the methods. This presented two sides to applied geophysics: the development of precise techniques and tools, and the practical results which it achieved.

The exhibition was organised internally, with Shaw presumably responsible. A wide variety of organisations and individuals donated or loaned objects for exhibit, including Lancaster-Jones and Rankine. Many of the items of historical interest were supplied by the Royal Society, the Royal Observatory, Greenwich, and the Meteorological Office. A number of governmental national and international organisations contributed: the Geological Survey, the Société de Prospection Géophysique, the Canadian Geological Survey, the Institute of Practical Geophysics of the U.S.S.R. and the Geological Committee of the U.S.S.R. There was interest from oil companies, with Phillips Petroleum donating a magnetic survey they had undertaken of an area in Texas Panhandle, and the Anglo-Persian Oil Company (later British Petroleum) contributing a number of items displaying the results of their explorations in what is now Iran.[104] The resulting exhibition thus promoted international cooperation between science and industry. Each contributing party had something to gain from their involvement: oil companies could provide their profit-driven practice with a scientific and 'public' legitimacy, while governmental organisations could also promote the results of publicly funded ventures as being for the good of the nation. As for the Museum itself, this exhibition was a suitable response to the recent pressure it had come under to represent the needs of British industry.

The exhibition was held from March to October, coinciding with the British Association Centenary meeting, taking place that year in London.[105] As part of this meeting, Lyons held an evening reception at the Museum, ensuring the exhibition would come to the

[103] Shaw (1931). p.10.
[104] Contributors named in Shaw (1931), pp.51-96.
[105] 'Science Museum Annual Reports for 1931', p.14, SM.

attention of interested parties, who might perhaps include any attendees of the British Association session on Geophysical Methods of Prospecting.[106] Shaw and Lancaster-Jones were themselves both contributors to this session. Finally, beyond the daily tours already available, the Museum staff also offered to provide closer examination of the objects to anybody who desired this.[107] While the Museum was forging close links with the scientists and related professionals (such as teachers) at this meeting, there seems to have been less of an effort made with the wider public, and there was no mention of the exhibit in the press. This was thus an example of the types of exhibitions Mackintosh was keen to hold, which would unite industry and science, and serve as educational tools. Under Mackintosh's direction, in the context of a wider need by scientists to present their work as industrially relevant, a certain kind of 'modern' physics was being presented at the Museum.

It was intended that the collections from this exhibition would be displayed permanently in their own section within Division V, but space (as always) resulted in the objects being scattered about wherever they could fit. In the 1935 Annual Report it was declared that, given the interest held in this section by the 'geologist, the mining engineer and the scientific world in general', a 'special effort should be made to find a place for it in the permanent collections at the earliest opportunity'.[108] Notably that same Annual Report also detailed the resurrection of the Physical Phenomena section; in stark contrast to applied geophysics, it was 'proposed to proceed gradually'.[109] Within Division V, two competing visions of 'modern' physics had emerged. In 'Applied Geophysics', the novelty lay in the development of new methods, more precise techniques and advances in instrumentation, and practical relevance outside of academic physics. 'The Structure of Matter', however, represented a physics that was new in its aims, delving into the structure of the tiny particles that made up matter. Both of these 'modern' physics were promoted by the Science Museum, the former fitting into a general mandate to display the relationship between science and industry, and the latter of personal interest to Lyons, who had been inspired at Wembley before hiring physicists with a Cavendish-style background to continue his work. I end this chapter by considering two temporary exhibitions pertaining to physical research and held at the Science Museum in 1936 and 1937. One represented Rankine's view of modern physics and the other promoted

[106] *Report of the British Association for the Advancement of Science* (1931), pp.xxvii-xxviii.
[107] Report of the British Association of the Advancement of Science (1931), p.xxix.
[108] 'Science Museum Annual Reports for 1935', p.49, SM
[109] 'Science Museum Annual Reports for 1935', p.35, SM.

Ward's Cavendish inspired subatomic research. These exhibitions reveal the multiplicity of available definitions of 'modern' physics, and the motives behind their employment. Even as late as the 1930s, 'modern' physics was not a definitive category, and this is particularly evident in the displays of the Science Museum.

6.7 'Very Low Temperatures': practical modern physics

'Very Low Temperatures' was held in 1936, but its origins lay in a 1934 exhibit on 'Refrigeration', which came shortly after the Advisory Council's adoption of a policy to hold special exhibitions illustrating the advance of science in industry. 'Refrigeration', along with an exhibit on 'Rubber' also held that year, was the Museum's first application of this policy. Both 'Rubber' and 'Refrigeration' were staged 'on a rather more comprehensive scale than [had] hitherto been possible', made possible by 'many months of unstinted work' on the part of research bodies and firms, and attracted a large number of visitors.[110] Alongside the exhibits provided, and often specially constructed, by manufacturing firms, 'Refrigeration' also featured exhibits from the DSIR-run Low Temperature Research Station (where food refrigeration was researched) in Cambridge and the National Physical Laboratory.[111]

'Refrigeration' was originally intended to include a small section concerning the attainment and uses of very low temperatures but, at the suggestion of the Russian physicist Peter Kapitza, it was decided to afford this subject its own independent exhibition.[112] Kapitza was the founding Director of the Mond Laboratory in Cambridge, built the previous year to house cryogenic and magnetic facilities for his researches into the conductivity of matter at very low temperatures. The laboratory had been opened the previous year and made possible by considerable funds supplied by the Royal Society and the DSIR, on Rutherford's request.[113] As Hughes has noted, this was not a universally popular use of Royal Society funds.[114] In 1934, Kapitza was detained in Russia indefinitely (it would transpire to be permanent), and the following year, the situation was widely reported in the press.[115] In response to Rutherford's campaign to

[110] 'Science Museum Annual Reports for 1934', p.A2, SM.
[111] 'Science Museum Annual Reports for 1934', p.7, SM.
[112] This is noted in the exhibition handbook and an article in *Nature*: Crawhall (1937); 'Very Low Temperatures: Exhibition at the Science Museum' (1936).
[113] Badash (1985).
[114] Hughes (2010), p.S104.
[115] See Badash (1985).

bring him back to England, Henry Armstrong wrote a letter to *The Times* expressing his relief at this 'loss' to British physics. He argued that he was not alone in thinking that public money should not have been spent on an 'academic professorship'. This complaint was in the context of an attack on Rutherford's 'atom-smashing brigade' who, Armstrong argued, did not produce knowledge relevant to 'national needs'. The funding and employment of non-British citizens, such as Kapitza, only exacerbated this problem for Armstrong, as he believed it was to the detriment of the development of British talent.[116]

Long before this letter, Kapitza appears to have been aware of such criticisms, carefully promoting the Mond Laboratory as relevant to industrial research. At its opening in 1933, Stanley Baldwin (speaking in his role as Chancellor of Cambridge University) delivered a speech, written by Rutherford, in which he discussed the importance of very pure science for future industrial advancement.[117] *The Times* praised the laboratory as having 'the ideal of knowledge for its own sake' whilst also showing that 'the scientific investigator is one of the most valuable public servants, and that pure science is one of the most productive investments of the State'.[118] Discussing the design, the *Manchester Guardian* declared that it 'serves as a model of what a twentieth century laboratory should be'.[119] With this in mind, Kapitza's suggestion to devote an entire Science Museum exhibition to low temperature research can be interpreted as a deliberate move to defend his laboratory as being in the public interest. The exhibition would promote the fruits of his research as relevant to industrial needs. Furthermore, this served to emphasise the wider value of 'pure' research more generally, promoting a favourable reception of work undertaken not just at the Mond, but also at the Cavendish. The exhibition was beneficial to both Kapitza and Rutherford.

By the time that planning began on 'Very Low Temperatures', Kapitza was in Russia. His place on the committee was filled by John D. Cockcroft, the replacement Director of the Mond Laboratory. Before assuming this responsibility, Cockcroft had been a researcher at the Cavendish, but had previously worked at Metropolitan-Vickers, and the company supported him financially throughout his time in Cambridge. Equipment and

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[116] H. E. Armstrong, 'Foreign scientists in Britain. Professor Kapitza's recall to Russia', ***The Times***, 7 May 1935, p.8.
[117] 'Mond laboratory opened', *The Manchester Guardian*, 4 February 1933, p.8; Rutherford's involvement is noted in Jones (1954).
[118] 'A Great Laboratory', *The Times*, 4 February 1933, p.11.
[119] 'Our london correspondence', *The Manchester Guardian*. 8 February 1933, p.8.

funding supplied by Metropolitan-Vickers contributed to Cockcroft and Walton's 1932 work 'splitting' the lithium nuclei to produce alpha-particles.[120] Cockcroft was thus fully aware of the utility of links between science and industry. Furthermore, he was also sensitive to the pitfalls of 'public' science communication, having been subjected to a barrage of press enquiries following the unauthorised publication of a sensationalised report of his 'atom splitting' research in a London newspaper.[121] Both he and Kapitza worked closely with J. G. Crowther, ensuring that the Cavendish Laboratory, and the results of the research therein, was presented in a favourable light.[122] Indeed, much of the coverage of Kapitza's detainment was managed by Crowther. Both Kapitza and Cockcroft had reason to be involved in an exhibition which allowed them a degree of control over how their research was presented to a public audience.

The exhibition was organised by T. C. Crawhall, of Division I, who had been responsible for 'Refrigeration'. He was assisted by a committee, chaired by Henry T. Tizard, who had worked on chemical thermodynamics at Oxford (with a substantial amount of funding from the Shell company) and as assistant Secretary to the DSIR, before becoming Rector of Imperial College, London.[123] Tizard had also been on the Museum's Advisory Council since 1930.[124] The remainder of the committee consisted of a mixture of academic scientists, researchers in industry, and industrialists. There were two representatives of the Clarendon Laboratory in Oxford. As noted in Chapter One, F. A. Lindemann researched low temperatures in Berlin before becoming Professor of Experimental Philosophy at Oxford in 1919. In the 1930s, his aim to promote the laboratory as a leading home for low temperature research began to be realised with the influx of Jewish émigré physicists, supported by grants from Imperial Chemical Industries (ICI).[125] Lindemann achieved this through his influential position on the ICI Research Council; he also obtained funding from the Rockefeller Foundation and the British Oxygen Company. [126] However, while Lord Nuffield injected nearly four million pounds into the University, Lindemann failed to secure any money from him for his lab, which Nuffield saw as overly abstract and distant from practical applications.[127] There

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[120] Allibone (1984); Cathcart (2004).
[121] 'Science's Greatest Discovery', *Reynold's News*, 1 May 1932, p.1; This event has been examined by Hughes (2009a).
[122] Hughes (2009a). See Hughes (2007) for a study of Crowther's negotiations with his editors.
[123] Jones and Farren (1961).
[124] Members of Advisory Council listed in 'Science Museum Annual Reports', SM.
[125] Morrell (2005).
[126] Hughes (2005), p.293.
[127] Morrell (2005), p.263.

was a clear incentive for Lindemann to emphasise the practical aspects of his low temperature research school. He was joined on the committee by his colleague Franz Eugen Simon, a reader in thermodynamic at the Clarendon Laboratory following the invitation of Lindemann as part of an ICI grant for refugee scientists.[128] Like Lindemann, Simon had also researched low temperatures in Germany and taken his PhD under Nernst, the physicist responsible for the 1911 Solvay Congress.

Ezer Griffiths, another committee member, also had something of a connection to Lindemann, having tested his and Nernst's theories of specific heats while at University College, Cardiff. From 1915, and for the remainder of his working life, Griffiths was based in the Heat Division of the National Physical Laboratory, working on both high temperatures and low temperatures. The latter he explored in relation to their uses in refrigeration. At this time he was also Secretary of the Physical Society.[129] Morris Travers, whom we met in Chapter One as Ramsay's assistant and developer of apparatus for the liquidation of hydrogen, was also on the committee. He had since co-founded Travers & Clark, Ltd., a company which built high temperature furnaces for melting glass. He was also involved, in 1925, with the founding of the Institute of Fuel. In 1927 he returned to Bristol as Honorary Professor, Research Fellow and Reader in Applied Chemistry.[130] J. C. McLennan, who had established a cryogenics laboratory at the University of Toronto, was also originally on the committee, but died in 1935.

The committee also featured members representing industrial companies. J.D. Pollock, Chairman of the British Oxygen Company and the Metal Industries Company, was on the committee, and also personally donated £500 for the exhibition.[131] Furthermore, Pollock was presumably also responsible for British Oxygen's contribution of £1800, which paid for the bulk of the exhibition.[132] Elsewhere on the committee, C. C. Paterson represented the General Electric Company, as Director of its Research Laboratories. Henry Mond, Lord Melchett, was Director of Imperial Chemical Industries, which his father had formed in 1926 through the merging of four separate companies. Imperial Chemical Industries donated £450 to the exhibition.[133] Melchett was also the grandson

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[128] Morrell (1992); Kurti (1958)
[129] Darwin (1962).
[130] Bawn (1963); Kostecka (2011).
[131] 'Exhibition of Very Low Temperatures: Their Attainment and Uses – Minutes of the Executive Committee Meeting held in the Science Museum on Wednesday, 27th November, 1935', pp.2-3, SM.
[132] 'Very Low Temperatures Exhibition. Expense (mainly costed)', SM-SPE.
[133] 'Very Low Temperatures Exhibition. Expense (mainly costed)', SM-SPE.

of Ludwig Mond, who had donated the money to the Royal Society which was then used on the Mond laboratory (although Armstrong's letter in *The Times* had included a suggestion that the benefactor would not have been happy with the final destination of his funds). Notably, Melchett had expressed an interest in the Museum's special exhibitions in December 1934.[134] Finally, the committee was completed by Robert Stewart Whipple, then a joint managing Director of the Cambridge Scientific Instrument Company, who had worked on the development and construction of instruments for measuring temperature.[135] We thus find in the committee a variety of interests, industrial and academic. What were their reasons for being involved in this exhibition?

No attention was drawn to these companies and individuals in the exhibition itself. In the earlier 'Refrigeration' exhibition, the items had not been accompanied by the names of their manufacturers, 'the object of which was mainly to ensure that the exhibits were representative of general principles and applications of refrigeration, and that they should follow a carefully preconceived plan'.[136] The same approach appears to have been taken in the organisation of 'Very Low Temperatures', with no mention made of the source of the objects. The individuals, institutions and organisations involved were thus only indirectly promoting themselves, and this was achieved through the direct promotion of the study of low temperatures and their applications.

This is evident in Simon's suggestion, at a committee meeting, to rearrange the introductory exhibits, displaying the production of low temperatures before exhibits displaying liquefaction. He suggested the inclusion of exhibits demonstrating the meaning of temperature reduction and, in particular, the concept of absolute zero. Furthermore, he proposed the inclusion, in the section on temperature measurement, of magnetic thermometers.[137] The magnetic method of thermometry was used at very low temperatures when conventional methods were no longer suitable.[138] This method was not used in industrial contexts, but instead in academic research into the properties of matter. Furthermore, it was a fundamental part of Simon's own research.[139]By introducing this concept into the exhibition, Simon was relating industrial low

[134] Mackintosh to Melchett, 1 December 1934, SM-SPE.
[135] Thomas (1954).
[136] 'Science Museum Annual Reports for 1934', p.7, SM.
[137] 'Exhibition of Very Low Temperatures: Their Attainment and Uses – Minutes of the Executive Committee Meeting held in the Science Museum on Wednesday, 27th November, 1935', SM.
[138] T. C. Crawhall and O. Kantorowicz (1937), p.23.
[139] Morrell (1992), p.295.

temperature research to his own work, which could otherwise be portrayed as simply the abstract theorising of university scientists. Rather than directly promoting the Clarendon as industrially relevant, he was promoting his own 'pure' physics as fundamentally connected to these practical aims. As we shall see, this idea was also discussed in the opening ceremony.

The exhibition was split into six main sections: Temperature Reduction; Temperature and Pressure Measurement; Liquefaction and Solidification; Storage and Transport; Applications; and Properties. These were accompanied by an additional collection of historical apparatus, depicting the advance of knowledge on the subject.[140] Many of the objects on display were purpose built for the exhibition, and a large number were interactive, inviting the Museum visitor to press a button or pull a lever. This kind of exhibition was a new development, first introduced to the Museum in 1932, and was intended to both entertain and educate.[141] It had been proposed by Richard Glazebrook, after observing a similar practice in the Deutsches Museum in Munich.[142]These workable exhibits recreated, to some extent, the laboratory experience, with many emphasising the role of measurement and observation in the progress of physics. Glazebrook, as noted in Chapter One, represented the precision measurement school of experimental physics, maintaining this tradition at Cambridge before moving to the newly created National Physical Laboratory. His recommendation guided the presentation of experimental physics at the Science Museum.

Visitors to Very Low Temperatures, held in the Museum's main exhibition room, were treated to apparatus they could personally control. (Fig. 6.1) At the push of a button, one could direct hot or cold bursts of air at a glass tube containing pressurised carbon dioxide, and observe the corresponding changes in physical state. (Fig. 6.2) Another exhibit allowed the visitor to observe the relationship between temperature and electrical conductivity, using their own touch to increase the temperature and then view the results on a galvanometer. Other interactive exhibits illustrated how a number of temperature and pressure measurement devices worked, allowed visitors to control a hand-operated compressor, and (again with bursts of air) explored the operations of high vacuum

[140] Crawhall (1936).
[141] The first such exhibit was 'Electrical Measurement' in 1932, described in 'Science Museum Annual Reports for 1932', pp.9-11, SM.
[142] 'Royal Commission on National Museums and Galleries: Report by Sir Richard Glazebrook on the Deutsches Museum, Munich', in 'Papers for Meeting on 13 June 1930', Meeting Papers 1930-1938 (Z193/1), SM.

Dewar vessels.[143] Here, the Rankine model of modern physics was being presented: old questions, and phenomena, being explored through the development of new techniques and apparatus. This was accompanied by objects demonstrating the industrial applications of these techniques: commercial liquefiers, containers for the transport of liquid oxygen, a machine which used an oxy-coal gas flame to cut metal, a model of a manufacturing plant for the production of solid carbon dioxide. (Fig. 6.3) There was a full size liquid oxygen container, about 4 to 5 feet tall, decorated in labels detailing each part's function.[144] (Fig. 6.4) A large model of the chemical constitution of air was used as a basis for a number of exhibits exploring the practical uses of the rare gases. (Fig. 6.5)Theory and application were thus displayed side by side.

The exhibition attracted the support of eminent scientists, with William Bragg, in his capacity as President of the Royal Society, opening the exhibition, and Lord Rayleigh chairing this ceremony.[145] As I noted in Chapter Four, Bragg was keen to promote his own particular views on the purpose of physical research. He was also interested in the role that museums could play in the 'public' reception of scientific work. Four years previously, in an address at the opening of a new physics laboratory at the University of Leeds, Bragg had defended the utility of 'pure' physics, declaring that 'there are few things more fascinating in scientific work than the unexpected application of some discovery, made in the progress of a research which seems to be out of touch with everyday things'.[146] Even the most abstruse subject was thus worthy of attention, and funding, because it might very well have a practical function. Bragg also praised the recent work of science museums in demonstrating to 'John Citizen' how a laboratory, and scientific research, really functioned. He suggested that a physics laboratory, in an idealised world, could also fulfil this function, educating the public on not only the results, but also the practice of science.

Bragg's address at the opening of 'Very Low Temperatures' emphasised the 'pure' and 'applied' aspects of the exhibit. He introduced the theory of absolute zero as a curiosity, before discussing how it had led to unexpected applications, a case study which perfectly matched up to the ideal presented in his speech at Leeds. He noted that it was very 'odd

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[143] Detailed in T. C. Crawhall and O. Kantorowicz (1937).
[144] Height estimated from photograph, see fig 6.4.
[145] It was Crawhall's suggestion to ask Bragg: 'Exhibition of Very Low Temperatures: Their Attainment and Uses – Minutes of the Executive Committee Meeting held in the Science Museum on Wednesday, 27th November, 1935', SM, p.6.
[146] Bragg (1932), p.496.

to see air liquefied and running like water', 'rubber lose all its spring and become as fragile as a piece of china', 'fruit powdered under a hammer and quicksilver made into a mallet'. And from these 'more fundamental effects which the scientist observes closely' came important industrial applications, mostly from the use of liquid oxygen: it produced hot flames for cutting metal, and was used in welding, medicine, mine rescue work, underwater work and high altitude flying, as well as being mixed with cotton to create explosives. From liquefied air, neon and argon could be extracted for electrical lamps and advertisement signs.[147] The 'pure' research into the properties of matter at very low temperatures was performed by scientists satisfying their own curiosities, but had been appropriated by resourceful workers in industry. The two pursuits were thus separate but connected, and both were important. Bragg also discussed the function of the Science Museum as not just to display past, but also present, science, which was necessary for the security and welfare of the nation. The role of special exhibitions in particular was to encourage and demonstrate the importance of current scientific research.[148]

This split between theory and application was reinforced by an accompanying series of seven lectures. A sub-committee had been formed to organise these, consisting of Cockcroft, Lindemann and Simon. Together they delivered lectures on the 'Approach to the Absolute Zero', recruiting others to give three talks on the industrial uses of low temperatures. Travers was responsible for an introductory lecture describing the history of the development of the technique of low temperature research, a history of which he was part, and reference was made to the development of apparatus at University College, London.[149] Throughout the promotion of the exhibition, there was a continuing theme of the industrial applications of seemingly esoteric scientific research. The exhibition was thus of considerable value to the physicists involved: Simon, Lindemann and Cockcroft. It presented their work as necessary for the industrial future of the country, at a time when they were struggling to gain funding and public recognition.

Such recognition was certainly attained, as 'Very Low Temperatures' received a considerable amount of press attention. The *Observer* focused on the 'romantic appeal of the Absolute Zero', noting that at one of the lectures (delivered by a 'university expert')

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[147] Typescript of pages 2 and 3 of speech, 33D/7, BRAGG.
[148] Draft of page 1 of speech, 33D/8, BRAGG.
[149] 'Exhibition of Very Low Temperatures: Their Attainment and Uses – Minutes of the Executive Committee Meeting held in the Science Museum on Wednesday, 27th November, 1935, SM, p.3; T. C. Crawhall and O. Kantorowicz (1937).

'it is expected that a temperature will be produced which will be lower by a substantial margin than any which exists in nature, even in space itself'.[150] Similarly, the *Manchester Guardian* described the exhibition as 'a much more romantic affair than it sounds', depicting the industrial applications as an unintended benefit of a 'scientific adventure'.[151] The ideals of Bragg's speech were thus being propagated in the national press. *The Times* detailed the nature of the exhibit itself, advertising the apparatus 'which the visitor may work for himself by pressing a button'.[152] Whether describing Lindemann's lecture or the details of the exhibit, there was an emphasis on novelty, on the first public display of this new research.

'Very Low Temperatures' was an opportune platform for presenting a certain image, and implicit definition, of 'modern' physics. Here, the combined interests of a number of physicists resulted in a display that merged apparently abstruse scientific research with clear industrial applications. This was the image that Bragg, Cockcroft and Lindemann wanted associated with their laboratories. Such a representation was crucial to obtaining funds and both public and professional support. Furthermore, it was a considerable departure from the definition of 'modern' physics proposed by Ward. However, the following year Ward organised his own temporary exhibit, and the outcome was very different. Here, a different 'modern' physics was on display, and it aimed to create a different 'public' reception of the purpose and nature of new scientific research

6.8 'Atom Tracks': 'modern' physics as microphysics

The exhibition was called 'Atom Tracks' and Ward had two principal aims in mind. It was a means to obtaining a sought after object for his Structure of Matter section, and also a way to promote the physics, and physicists, that he was most familiar with from his time at the Cavendish Laboratory. The exhibition was centred on the cloud chamber, an apparatus developed by C. T. R. Wilson, whilst at Cambridge in 1912. Wilson originally constructed the chamber to study cloud formation, but he found that if he passed a beam of X-rays through it, small wisps of cloud would surround the tracks of electrons ejected from the air by the X-rays.[153] The introduction of radium into the chamber revealed the straight tracks of the alpha rays emitted from it. This method

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[150] 'Low temperature exhibition', *The Observer*, 8 March 1936, p.11
[151] 'The search for absolute cold', *Manchester Guardian*, 5 March 1936, p.20.
[152] 'The Absolute Zero: An Exhibition of Low Temperatures', *The Times*, 5 March 1936, p.11.
[153] See Galison and Assmus (1989) for a discussion of the cloud chamber.

proved to be very successful in studying the structure of the atom, and was used by subatomic researchers both within and outside of the Cavendish. In 1932, the American physicist Carl Anderson used it to find the first experimental evidence of the positron. In Cambridge, Patrick Blackett had conducted cloud chamber experiments on the disintegration of the nucleus of the nitrogen atom. This was experimental 'microphysics', directed towards revealing information about the structure of the atom.

Ward hoped to display this original apparatus, as well as a selection of photographs obtained using the method. As it was now 25 years since Wilson first constructed the chamber, Atom Tracks was designated as an exhibition to mark this anniversary. The Science Museum was accustomed to holding anniversary exhibitions and so this fitted in to an existing model of celebrating a particular institution, scientific object or (more commonly) person. In this case, the celebration was ostensibly of a piece of scientific apparatus, but there was also an underlying celebration of the Cavendish Laboratory. Anniversaries and commemorations can be used to promote the agendas of specific groups of people.[154] In the case of the 1931 Faraday centenary, academic physicists used it to align their work with the electrical industry, promoting a vision of industrial progress led by basic scientific research, not invention. Faraday became the 'icon for their campaign'.[155] That same year the Cavendish had also organised a similar event, celebrating the centenary of the birth of James Clerk Maxwell, the Laboratory's first Director. John Cockcroft, aware of the importance of careful promotion of Cavendish research, was responsible for organising and publicising this event.[156] Physicists used the occasion to appropriate Maxwell's legacy for their own purposes: James Jeans discussed how Maxwell ascribed physical truth to mathematic equations, a philosophy in which Jeans also believed; Planck discussed Maxwell's influence on theoretical physics in Germany; and Oliver Lodge positioned Maxwell's work within the history of wireless telegraphy.[157] The centenary also served as a celebration of the Cavendish Laboratory, situating it in relation to Maxwell's legacy. I shall show that Ward's use of cloud chamber photographs were also used to promote the Cavendish, as the centre of atomic physics, displaying the widespread results of apparatus developed there and also, more broadly, a particular Cavendish style of experimental physics.

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[154] The role of commemoration in the history of science has been explored in a dedicated volume of Osiris: Abir-Am and Elliott (eds.) (1999); Galison (1983) has also considered the ways in which physicists have reinterpreted the past to fit the aims of the present.
[155] Macleod and Tann (2007), p.411.
[156] John D. Cockcroft to John Joly, 13 July 1931, JOLY.
[157] Thomson et. al. (1931).

Ward began the plans for this exhibition in February 1937, writing to Shaw that he thought it would be good to commemorate the anniversary, and also that this would be an opportunity to obtain Wilson's original apparatus for the Science Museum.[158] In 1932, Wilson had promised to donate his cloud chamber to the Museum after he had finished using it. But since his retirement he had left it in Cambridge, and the Museum was left to negotiate with Rutherford at the Cavendish. As a former student of Rutherford's, Ward presumably felt best placed to try and get this apparatus once more. The exhibition would have at its centre this apparatus, accompanied by photographs taken in the last 25 years, illustrating 'the wide field of utility, and the great variety of results which have been obtained by its aid'. The exhibition would thus present this strand of 'modern' physics as very much a Cavendish product, made possible only by work originally carried out at Cambridge. From the initial planning stages, Ward had co-operation with the Cavendish on his mind. He suggested to Shaw that they first hold the exhibition in Cambridge, for about a week, before transferring it to the Science Museum. Following this, the 'Museum would of course insist that in return for its work in organising the Cambridge exhibition the original Wilson apparatus should come permanently to the Museum at the close of the Cambridge show'.[159] A Cambridge exhibition would also be diplomatically advantageous to efforts to obtain the photographs for display. Ward planned for the Cambridge exhibition to feature about 60 photographs, with this slimmed down by about half for the Science Museum, removing the images that were only of interest to physicists engaged with research into atomic structure. Ward planned a further selection of photographs for permanent exhibition in Structure of Matter. He noted that such photographs would 'be more readily supplied if the exhibition is first held in Cambridge, since among workers in atomic physics the name of the Cavendish Laboratory is probably better known than that of the Science Museum'.[160] Ward was thus carefully negotiating with the Cavendish to ensure the Museum could obtain the items he desired for both the temporary exhibition and his permanent gallery.

When 'selling' this exhibit to Mackintosh, Shaw detailed the difficulties the Museum had found in trying to obtain the apparatus, depicting the exhibit as an ideal solution, as it would also result in the permanent display of many photographs. He noted that '[m]ost

[158] Ward to Shaw, 3 February 1937, SM-ATOM.
[159] Ward to Shaw, 3 February 1937, SM-ATOM.
[160] Ward to Shaw, 3 February 1937, SM-ATOM.

of the people who have worked with the Wilson Chamber are known to Dr Ward personally, and he would be most likely to secure their co-operation'. Ultimately, the plan was an excellent way of 'killing several birds with one stone'.[161] Of particular value to the Museum at that point was the fact that the exhibition wouldn't take up much space: with only 30 photographs and one piece of apparatus, Ward thought that he could set it up in the demonstration room, or even the corridor. The plans were approved by Mackintosh, with the small size of the exhibition, and the attainment of the cloud chamber apparatus, no doubt the main incentives.

Ward, meanwhile, was now at work utilising the various contacts he had at the Cavendish, most crucially of all, his former supervisor Rutherford. He wrote to Rutherford as soon as Mackintosh had approved the plans, detailing the exhibit and his wishes to permanently display Wilson's apparatus.[162] Rutherford liked the idea of a Cambridge exhibition, but decided it would be best to hold it concurrently with the British Association meeting in Cambridge, which was to take place in 1938. With this in mind, it would make more sense for the exhibition to take place first at the Science Museum.[163] The initial exhibition would however still have Rutherford's 'blessing', and applications to obtain the photographs for exhibit could be made from the Cavendish Laboratory (although Ward decided that this was unnecessary).[164] The final destination of the Wilson apparatus, however, was still undecided. Weeks before the exhibition, Rutherford died, without a decision being made, and the cloud chamber remains at the Cavendish today.

In the end, Ward obtained far more photographs than he had been expecting, and there were more than 80 in the exhibit. This number was not whittled down for the Science Museum exhibit, presumably on account of this now taking place first. The accompanying handbook, written by Ward, began with a declaration that '[t]he advance of science depends to an ever-increasing extent upon improvements in experimental technique'.[165] Here, as with very low temperatures and geophysics, the emphasis was on the advance of experimental technique. However, the purpose of these experimental developments was very different from the 'modern' physics defined by Rankine. Ward's handbook detailed the invention of new experimental methods as leading to further

[161] Shaw to Director, 16 February 1936, SM-ATOM.
[162] Ward to Rutherford, 23 February 1937, SM-ATOM.
[163] Dee to Ward, 21 April 1937, SM-ATOM.
[164] Ward to Shaw, 26 April 1937, SM-ATOM.
[165] Ward (1937), p.3.

'knowledge of the nature and structure of atoms'. However, he noted in his introduction that the main function of Wilson's cloud chamber had actually been the confirmation of results found by other methods. Its principal purpose was thus to confirm theory; as Ward noted, 'the testimony of a Wilson track photograph often carries conviction where other evidence might fail to satisfy'.[166] This was an experimental physics that provided 'proof' of theories into the structure of matter. It was a very different idea of progress to that found in 'Very Low Temperatures'. In the former exhibit, pure research was presented as an initial step to application. In 'Atom Tracks', confirmation of theory was the goal. In the Science Museum in the 1930s, we thus find diverging public presentations of the nature of scientific change. Scientists were using the Museum to promote their views on this issue and present their work as contributing to progress.

There was no 'practical' purpose to this physics, and certainly no industrial application. It also lacked the precision of low temperature research, much of which concerned the exact measurement of pressure and temperature. While Ward did discuss the use of the cloud chamber in the deduction of the 'actual position of the tracks in space from geometry', he also referred to 'qualitative work' that required only a single photograph of the chamber.[167] Furthermore, where both 'Applied Geophysics' and 'Very Low Temperatures' had been structured around different types of techniques, 'Atom Tracks' was split into categories of physical properties: alpha rays, beta rays, x-rays, gamma rays, protons, deuterons, neutrons, cosmic rays and radioactivity. This was a very different kind of experimental physics, driven by theory not application. It was a Cavendish definition, reinforced by Ward's quotation of Rutherford describing the cloud chamber as 'the most original and wonderful instrument in scientific history'.[168]

The exhibition itself was a far more modest affair than 'Very Low Temperatures'. (Figs. 6.6 and 6.7) It featured as a centrepiece the Wilson chamber, surrounded by the 80 or so photographs, grouped according to the categories used in the handbook. While Ward had originally suggested holding this exhibition in a corridor, it was ultimately too large and was placed in the demonstration room. This room was still, however, less than half the size of the exhibition room housing 'Very Low Temperatures'.[169] Furthermore, 'Atom

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[166] Ward (1937), p.5.
[167] Ward (1937), p.4.
[168] Ward (1937), p.3.
[169] These rooms remain in the Science Museum today, and measurement details were given to me by Robert Bud, Keeper of Science and Medicine, 23 July 2012.The demonstration room was about 65m².

Tracks' did not have the grand opening of 'Very Low Temperatures', nor the keen press attention. Crucially, it did not have the financial backing from industry, from which 'Very Low Temperatures' had considerably benefited. The two different definitions of 'modern' physics were thus elaborated in very different styles of exhibition. There was the 'public' laboratory envisaged by Bragg, made possible by the workable exhibits first suggested by Glazebrook. The room was full of activity, buttons pressed, levers pulled, and an interaction with the science. 'Atom Tracks', by contrast, was a static display. However, the cloud chamber was not dissimilar from the scientific apparatus on show in 'Very Low Temperatures'. The accompanying labels referenced pumps and pistons, and components used for adjusting volume and cooling with water. (Fig. 6.8) Through this display, the photographs of subatomic particles were given a tangible connection to laboratory practice that was easier to comprehend than the rather abstract photographs.

Where 'Very Low Temperatures' was the outcome of a large committee, 'Atom Tracks' was the work of one man, and the motives behind it differed considerably. Ward hoped to use the exhibition as a means to obtaining the cloud chamber and photographs for his 'Structure of Matter' gallery in development. While he failed to secure Wilson's apparatus, he was able to collect a considerable number of the photographs. Furthermore, the exhibition itself served as an advertisement for the definition of 'modern' physics Ward was hoping to achieve with his permanent gallery. Here, he promoted the style of physics underway at the Cavendish, his alma mater, and indeed the type of physics he himself had been researching until relatively recently. The focus was on the structure of matter and the nature of subatomic particles. The research was valuable in and of itself, not because of any broader applicable purpose. It was Ward's conception of 'modern' physics, one passed on to him during his training under Rutherford. The depiction of 'modern' physics on display at 'Very Low Temperatures' was also compatible with how Rutherford wanted his research disseminated, but it was very different from that seen at Atom Tracks. The opening address by Bragg had much in common with the speech Rutherford had written for Baldwin at the opening of the Mond Laboratory, conveying how 'pure' research could be applied in industry. In these two temporary exhibitions at the Science Museum we find two different methods of promoting the utility of 'modern' physics.

Gallery One (which housed low temperatures) was probably about 150m², although this is only a rough estimate as the boundaries have changed since the 1930s.

6.9 Conclusion

In this chapter I have considered the competing visions of ‘modern’ physics on display at the Science Museum in the 1930s. I have discussed two explicit definitions, one describing microphysics as the foundations of theoretical understanding, the other emphasising the industrial applications of refined apparatus and techniques. The former was clearly visible in ‘Atom Tracks’, the latter in ‘Applied Geophysics’. The model of ‘modern’ physics in ‘Atom Tracks’ was a result of the personal commitments of F. A. B. Ward, but also a desire by Lyons, who hired Ward, to display Cavendish-style modern physics at the Science Museum. ‘Applied Geophysics’ came out of a need to more fully represent the industrial purpose of science. In ‘Very Low Temperatures’, however, we find something of a merging of the two. This exhibition promoted Bragg’s conception of the purpose of physics, where ‘pure’ research should be encouraged on the basis that it produces practical results unexpectedly.

Amidst these competing definitions, there was one coherent message, and it concerned scientific progress. As we saw in Chapter Four, Oliver Lodge had a considerable influence on much of the ‘public’ portrayal of ‘modern’ physics. His expositions of the subject were driven by his own commitments to the ether and a belief that the future of physics lay with a continuous, not discontinuous, model of nature. As a result, he depicted ‘modern’ physics as being in a state of transition, awaiting reconciliation with the older theories and concepts. Progress involved looking to the past as well as the future. This was not the case at the Science Museum. The focus was on highlighting the achievements of physics, and this was reinforced by the involvement of active researchers who wanted to promote their work and institutions. These physicists were using the Museum to disseminate their work to new audiences, but also convey a broader notion of the nature of scientific change and progress. In doing so, they promoted the importance of ‘modern’ physics, as well as its stability. By representing, through images, models and actual scientific apparatus, the concrete results of physics, they were rejecting any notion of the discipline as being unstable. Such an approach could serve as a response to the accusations of ‘modern’ physics being destructively revolutionary, a notion brought to the forefront in both ‘popular’ and ‘professional’ responses to relativity theory’s apparent overthrow of Newton. If such a revolution did indeed cast aspersions on the ‘foundational’ science’s ability to uncover objective truth, the status of physics was defended at the Science Museum. Here, the successes of 'modern' physics were on display, and it was extolled as a producer of knowledge, whether this be 'intellectual' or 'practical'.

Chapter Seven: Conclusion

7.1 Introduction

The year 1942 marked the three hundredth anniversary of Isaac Newton's birth. In the midst of a world war that was to emphasise the future applications of 'modern' physics, celebrations were held in honour of the discipline's past. This was an occasion, however, not only to celebrate the great past achievements of Newton, but also consider the place they held in the physics of the 1940s and beyond. It was an opportunity to reframe his work in the context of 'modern' physics, and explore its current value. If reconciliation could not be made between 'classical' and 'modern' physics, then there was the possibility that physicists might lose claim to their 300-year old idol.

Fortunately, for modern physicists and Newton alike, James Jeans managed to save the legacy of Newton. He did so at the Royal Society's celebrations of Newton's Tercentenary, which took place that November, during the society's Anniversary Meeting.[1] Henry Dale, as President of the Society, delivered the introductory address, proclaiming the fundamental role of Newton in the progress of Western science and philosophy. He then detailed the purpose of the subsequent three talks: Andrade was to speak about the revolutionary aspects of Newton's theories in his own time; Rayleigh would discuss Newton's prowess as an experimental physicist; and finally James Jeans was taking on the task of providing 'some reassessment of the validity and permanence of Newton's system, in relation to the immense advances of knowledge in our own times'.

> 'There are many who have not the mathematical equipment to follow them in detail, who are nevertheless aware that revolutionary changes have been taking place in conceptions of the mechanics of the universe and of its ultimate material units. How is the Newtonian system affected by the quantum mechanics at opposite ends of the stupendous scale? Is it being supplemented, modified or superseded after its centuries of dominance?'[2]

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[1] The Royal Society had hoped to put on a lavish international celebration, but this wasn't possible during wartime. Dale Papers –Royal Society – Correspondence – Newton Tercentenary Celebrations (HD/6/8/7/12), RS; A second event was held in 1946 and included an excursion to Trinity College, lectures at the Royal Institution, a visit to Covent Garden opera house and a garden party at Buckingham Palace. 'Provisional programme of Newton tercentenary celebrations, 15-19 July 1946', Dale Papers – Royal Society – Correspondence – Newton Tercentenary Celebrations (HD/6/8/7/12/130), RS.
[2] Dale (1943), 225.

The role that Newtonian physics was to play in the future of the discipline was thus still of considerable importance to Dale, who, as a biological and medical scientist, was an impartial observer of the recent physical 'revolution'. Newton's place in 'modern' physics was also crucial to the tone of this, and future, celebrations of physics' most famous ancestor. Was Newton from thereon to become a figure of merely historical curiosity, or continue to be depicted as relevant to the work of contemporary physicists?

Jeans addressed these questions head on, noting that physicists of course had 'no doubts as to [Newton's] greatness, but we probably feel less confident in our powers to assess his ultimate position in science than we should have done fifty years ago'. Indeed, where the immediate successors of Newton had claimed 'a quality of finality and uniqueness' in Newton's work, this was something 'which we know better than claim for him to-day'.[3] Jeans considered the work of Planck, Rutherford and Einstein, each representative of a different 'modern' physics: the quantum, the nuclear, and the relativistic, respectively. He noted that they had uncovered new 'ante-chambers' in Newton's 'temple' of knowledge, and considered the implications this had on how we were to remember Newton:

> 'There are some – although mostly laymen in science – who see science primarily as something that is for ever changing. For them the science of any period is like the sand-castles that the children build on the sea-shore; the rising tide will soon wash them away, and leave the sands clear for the new array of castles which will be built the next day. Those who hold such views are led, somewhat naturally, to make such statements as that Newton is out-of-date and superseded.'[4]

However, this was not how science worked, for 'Science is knowledge, and the primary characteristic of knowledge is not that it is for ever changing, but that it is for ever growing'. Jeans proposed that a more suitable metaphor than the sandcastle, which is washed away and replaced, would be a 'vast building' on which new floors are added and new wings constructed. This building was 'the embodiment of scientific truth, and the truths of science are the same, no matter who discovers them'.[5] Such a metaphor proposes an image of Newton not as wrong, but instead limited, uncovering some of the truth, but not all of it; and Jeans explored this in his talk.

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[3] Jeans (1943), p.251.
[4] Jeans (1943), p.232.
[5] Jeans (1943), p.232.

He proposed that there were ‘three worlds’, and in each different scientific laws applied. The ‘small-scale world of electrons and of atomic physics in general’ was subject to the laws of quantum mechanics, ‘the man-sized world’ by Newtonian mechanics, and the ‘world of the great nebulae’ by the laws of relativity. While all of these worlds were ultimately governed by the same laws, ‘factors which are all-important in one become mere insignificant corrections in the others’.[6] Thus, Newtonian mechanics was not completely incorrect, it was rather only correct in his designated world, the only world to which had access in the seventeenth century. His laws were ‘inadequate only with reference to the ultra-refinements of modern science’. As such, these laws were still of use in 1942. They had considerable practical utility, for the astronomer and the engineer, and ‘in the science of everyday life’.[7]

Jeans’ lecture was, first and foremost, a defence of Newton in the face of ‘modern’ physics, a call for reconciliation between the old and the new. He was proposing a model of science as progressing through building on the work of predecessors, standing on the shoulders of giants. Nothing once perceived as valuable was to be overthrown or superseded. He was able to do so by situating ‘classical’ physics in a different world to ‘modern’ physics. In Jeans’ narrative of the progress of the discipline, Newtonian physics had not only been of benefit in the construction of modern theories, it was still in use today. Classical physics was the physics of the everyday and if one wanted to garner information about this particular world, then Newton’s path was the route to be taken.

In making this division between worlds, Jeans was also defining ‘modern’ physics in a certain way. By describing Newtonian physics as the science of the ordinary, he was implying that non-Newtonian physics was the science of the extraordinary. He was distancing current physical research from the experiences of ordinary people. ‘Modern’ physics did not describe their experiences; it described new, incomprehensible lands. Indeed, Jeans’ talk appeared to be separating ‘modern’ physics from any practical application, depicting it as an endless search for knowledge, but not to any end in particular. In contrast to this, ‘classical’ physics remained Newtonian, firmly situated in the real, observable, world.

[6] Jeans (1943), p.258.
[7] Jeans (1943), p.259.

This implicit judgement of 'modern' physics was, however, secondary to the main thrust of Jeans' talk, that of the old and the new co-existing. Henry Dale appears to have been particularly taken by these conclusions. Two weeks later, he delivered another tercentenary lecture, this time to the Rotary Club of Grantham, the town in which Newton was born. Much of his speech related back to the context of the war that had prevented a larger celebration from taking place, and he had intended to end a section of his talk on this note:

> 'It is fitting, I think, that even in the midst of the greatest and most terrible of all wars, we should allow ourselves to pause and stand aside, here where the bud of his genius began to open, and to ask ourselves what manner of man this Newton was, and how the child of the Woolsthorpe manor farm, and the little scholar of the King's School in Grantham, came to win and to retain the wonder and admiration of a world'.[8]

This was the conclusion Dale had originally typed out in preparation for his speech. However, he subsequently crossed this section out and added a series of handwritten notes about Einstein. He wrote that 'Some people, whose knowledge is of a superficial kind and based on hearsay talk as though Newton's conclusions have been superseded'. He noted that Einstein himself did not believe this, but was rather 'Newton's most enthusiastic admirer'. These topics were covered in Jeans' talk, in which he had also referred repeatedly to the acclaim that Einstein had expressed towards Newton's contributions, and thus it seems likely that Dale edited his speech after hearing Jeans. He altered the tone of this section, changing it from a celebration of Newton's origins to a defence of his future. The fact that such a defence needed to be made was another reminder of the apparent threat imposed by 'modern' physics.

The rhetorical work done by Jeans, and utilised by Dale, in defending Newton reveals that the place of 'classical' physics in relation to the 'modern' was not yet concretely established. Furthermore, we find a broad definition of 'modern' physics here, encompassing nuclear and quantum physics and relativity theory. I conclude my thesis by exploring how I have tried to answer two principal questions: How and why did British physicists define the 'modern' and the 'classical' in the first four decades of the twentieth century? How did these discussions impact public trust in physics, and more generally science? My thesis thus presents a case study in the larger question of how

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8 'Sir Isaac Newton. Lecture delivered at the King's School, Grantham, on Monday December 14th, 1842. By Sir Henry Dale, President of the Royal Society', Newton Tercentenary Lecture 1942 (HD/4/3/7), RS.

scientists manage 'public' expectations of science during a period of 'revolution', when science's reputation of stability and permanence comes under question.

7.2 Changing definitions of 'modern' physics

Throughout my thesis I have shown that the categories of 'classical' and 'modern' were not well defined for much of this period. Even as definitions began to be established, they were still debated, and what we now deem to be 'classical' was often presented as modern physics, even if this term was not always explicitly employed. Richard Staley's definition, taken from Max Planck's usage at the 1911 Solvay Congress, differentiated classical from modern on the basis of its adherence to developments in quantum theory.[9] However, exploring the British case, and particularly the 'public' realm, such a definition is not sufficient. Here I consider the variety of ways in which British physicists defined these terms.

Instead of definitions based on technical aspects of physical theories, I have found broader concepts in use. The notions of continuity and discontinuity, explored in Chapter Three, had multiple meanings, even at the purely physical level. They could refer to a notion of energy as discontinuous, but also matter. As such, quantum mechanics was not a problem in and of itself, but part of larger considerations. And this was not a new development, but rather one that had been growing ever since physicists first began to conceive of atoms as real. Rather than setting up 'modern' physics in opposition to older ideas, attacks on discontinuity conflated the two to some extent. Here, 'modern' physics was the intensification of an old problem.

By considering this notion of continuity, I have placed commitments to the ether in a wider context. British physicists did not necessarily fight to maintain the ether for its own sake, but rather as a means to regaining a picture of the physical world as ultimately continuous. Furthermore, discontinuity was related to a new understanding of the limits of physics, replacing explanation with descriptionism. The responses of British physicists to changes in their discipline were influenced by pre-existing philosophical commitments and beliefs about the purpose of their work. In this respect my study draws on Paul Forman's account of the German reception of quantum mechanics, a type of 'modern' physics that also raised broader philosophical issues, concerning acausality

[9] Staley (2005).

and indeterminism.[10] The introduction of these concepts into 'modern' physics stemmed from the earlier problem of the duality of light, as both a wave and particle. For Bragg, in 1912, this had been a matter of experimental pragmatism, but with the emergence of the Copenhagen interpretation, developed from 1924 to 1927, it was suggested that this duality was an ontological fact, part of a broader theory of complementarity. Here, it was proposed that it was theoretically impossible to ever acquire complete understanding of all physical phenomena. Physicists could not reduce their studies down to a set of mechanical laws, and determinism was threatened. Forman argued that German physicists immediately recognised the acausal implications of quantum mechanics and responded positively, as a result of the particularities of Weimar culture, where criticisms of deterministic and fully causal science were prevalent.[11] But in the British intellectual environment the issue of indeterminism was not as predominant, and as such was mostly ignored in the early reception of quantum mechanics.[12] As a result, these concepts did not initially come to define 'modern' physics in the way that discontinuity had.

However, by the 1930s, the notion of acausality was entering British definitions of 'modern' physics and being used to characterise a new approach, distinct from 'classical' methods. In 1934, James Jeans delivered the presidential address of the British Association. Here, he described Newtonian mechanics as containing a doctrine of 'mechanistic determinism'.[13] The entire physical universe was reduced down to logical processes, and every action could be determined by the laws of physics. Now, however, physics was no longer 'concerned to study the Newtonian universe which it once believed to exist in its own right in space, and time'. Instead, it set itself a more 'modest task of reducing to law and order the impressions that the universe makes on our senses'. It was 'concerned with appearances rather than reality'. In Jeans' reading, physics had now completely cut its ties with philosophy, and descriptionism reigned supreme.

Jeans had been, as I have shown, a supporter of 'modern' physics for some time, and played a significant role in ensuring that the Royal Society's *Proceedings* was a

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[10] Forman (1971).
[11] Forman (1971).
[12] Savage (1979) and Forman (1979) point to Eddington, Jeans and Norman Campbell as the exceptions in the British case. This is unsurprising considering what I have so far shown about these three men: Eddington and Jeans were both interested in the philosophical implications of physics, while Campbell, a 'militant' experimentalist, believed that physicists placed too much weight on theories and concepts that could not be subject to experimental confirmation.
[13] *Report of the British Association for the Advancement of Science* (1934), p.5.

'modern' journal. Furthermore, he was also one of a small minority of British physicists who did show an early interest in the concept of indeterminacy.[14] However, if we consider the thoughts of a decidedly 'classical' physicist on the subject of determinism, we find them to be rather similar. Allan Ferguson studied and worked at the University College of North Wales, Bangor until 1919, before lecturing at Manchester College of Technology, and then becoming assistant professor at the East London College. He was closely involved with the Physical Society, serving as president from 1938 to 1941, and his major research interest was the subject of surface tension.[15] And yet he too saw the fate of physics as towards descriptionism. Two years after Jeans' address at the British Association, Ferguson served as President of Section A and used this opportunity to discuss 'Trends in Modern Physics'.[16] As Jeans had done, Ferguson described a move from a 'naively realistic' Newtonian determinism to a physics that dealt only with the '*conceptual* world'.

> 'Whatever the form of the picture, the hard-pressed physicist of to-day remains on firm ground if he refuses to confuse the concept – the world-picture – with the percept; if, making this distinction, he studies the question of the reality underlying phenomena as philosopher rather than as physicist; if he is as ready to discard outworn models as ever Maxwell was'.[17]

If the 'modern' physicist wanted to continue attempting to explain reality, he thus had to accept that this was not physics but rather philosophy. Physics could only describe.

However, as I have shown in Chapter Six, the indeterminist, descriptionist world of quantum mechanics did not affect all depictions of 'modern' physics even in the late 1930s. In the exhibitions at the Science Museum we find a very different picture. In 'Structure of Matter', the Cavendish reductionist approach was presented as the route to knowledge about the internal structure of the atom, the true nature of the foundations of the physical world. The exhibit displayed photographs of this reality, and the apparatus through which these photographs were obtained. Here, there were no larger philosophical questions about the nature of physics, and the purpose of the discipline was presented as being to answer questions about the nature of physical reality, the same questions that Newton had been asking some 300 years before. This was to some extent also the case in 'Very Low Temperatures', with its section on the approach to absolute

[14] Savage (1979); Forman (1979).
[15] Rankine (1952).
[16] Ferguson (1936).
[17] Ferguson (1936), p.788.

zero, and the unusual properties displayed by matter as this theoretical limit was approached. The remainder of this exhibit, however, depicted a more practical purpose of physics, as responding to the demands of industry, developing necessary industrial techniques. In a time of economic instability, such work was very much in the national interest, and thus physical research had a clear purpose that could not be shaken by new theoretical speculations.

Modern physics was variously displayed as discontinuous, as reductionist, as indeterminist, and as technological. It could be a different ‘world’, the ‘real’ world, or a purely conceptual world. The particular definition was dependent on context, on whether it was being published in a scientific journal, presented to the audience at the British Association, or put on display for visitors of the Science Museum. There were different definitions for different ‘publics’. There were also different definitions coming from different physicists, and applied for particular purposes. Lindemann, Simon, Kapitza and Cockcroft promoted their particular brand of ‘modern’ physics as industrially relevant, in order to depict their laboratories as ‘useful’ and in the national interest. Oliver Lodge presented ‘modern’ physics as a threat, emphasising its discontinuity in order to align it with other revolutionary ideas, suggesting that it was important for others to attempt, as he did, to ‘save’ the ether. Jeans carefully defined modern as a broadening of classical, in order to save the legacy of Newton. Ward displayed Cavendish physics as the route to deeper knowledge, promoting the purpose of physics he had been trained to believe in. There was not one distinct message, one underlying concept that defined what separated the present from the past.

Such definitions of ‘modern’ physics were to be shaped by future practitioners. This is particularly notable in the case of Ward’s ‘Atom Tracks’ photographs. As Alan Morton has noted, ‘Atom Tracks’ ‘portrayed nuclear physics as abstruse academic research without practical applications. But within a year of the exhibition opening, nuclear physicists’ own perceptions of their subject changed with the discovery of nuclear fission’.[18] This was in 1938, one year before the start of the Second World War, which of course ended with a shocking display of the potentials of nuclear power. The Science Museum was closed during the war and reopened in February 1946 with a huge exhibition of German Aeronautical Developments. Alongside this was a smaller Special Science Exhibition, on the topics of Atomic Energy, Uranium, X-rays and their

[18] Morton (2011), p.14.

application, and the Quartz Crystal Clock.[19] The first of these, 'intended to illustrate the structure of the atom and the nature of its energy store', featured a number of items originally destined for the 'Structure of Matter' section: a set of Bohr atom models, F. W. Aston's mass spectrograph, Cockcroft and Walton's 'atom splitting' apparatus, as well as 'several specimens of original apparatus used by Rutherford and J. J. Thomson, lent by the Cavendish Laboratory, Cambridge'. The exhibition was also 'extensively illustrated' by several of the atom tracks photographs. These pictures, which less than a decade before had constituted an exhibition of esoteric pure science, were now part of a much bigger narrative. So too was the Cavendish. 'Atom Tracks' and the 'Structure of Matter' had become 'Atomic Energy', and the history of the Laboratory and its research were in the process of being rewritten.

In the 1946 Science Museum, 'modern' physics was redefined in relation to the Second World War. Here was an early example of what Hughes has termed 'bomb historiography', in which the physics of the early twentieth century was reinterpreted and emphasised in relation to its use in the war effort.[20] Radioactive disintegration, research on the nucleus and isotopes, and the 'splitting' of the atom, were situated within a linear history leading to the development of the atomic bomb. As Cambridge physicists had done with Maxwell in 1931, scientists were harnessing history for their own particular purposes. A new 'modern' physics, nuclear physics, was being constructed, with the defining characteristic of its enormous practical applications. Rutherford's carefully promoted rhetoric of research as a means to intellectual, not practical, advancement had been replaced.[21] And he was no longer around to provide an alternative viewpoint, having unexpectedly succumbed to a fatal hernia in 1937. Rutherford's demise was shortly followed by the deaths of many of the most prominent members of the generation of physicists that preceded him: Oliver Lodge and J. J. Thomson in 1940, and William Bragg and Joseph Larmor in 1942. Eddington and Jeans, perhaps the most widely known physicists of the younger generation on account of their popular expositions of 'modern' physics, followed suit in 1944 and 1946 respectively. A new generation of physicists were now free to rewrite history, with no opposition from the vocal commentators of the first half of the twentieth century.

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[19] 'Science Museum Annual Reports for 1946', p.3, SM.
[20] Hughes (2002a), p.351.
[21] Jenkin (2011) has recently suggested that Rutherford was well aware of the potentials of nuclear power, while he publicly dismissed this as 'moonshine'.

While Staley places considerable weight on Max Planck's 1911 discussion of quantum physics, I suggest that a more telling clue as to the construction of modern day definitions of 'classical' and 'modern' physics can be found in Planck's words of several decades later: 'A new scientific truth does not triumph by convincing opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it.'[22] This statement can apply not only to the content of 'modern' physics, with the new breed of physicists adopting previously contested ideas, but is also revealing of how the discipline came to be defined. The careful work of our early twentieth century physicists was replaced by new rhetorics, closely informed by the demands of the Second World War, and applied by younger physicists familiar with the terms 'classical' and 'modern'. The circumstances of the early twentieth century were retrospectively reinterpreted in relation to this imposing event, and the history of 'modern' physics was rewritten.

7.3 Negotiating progress: maintaining 'public' trust in science

With the Second World War, physics had a clear purpose, and nobody could question its wider importance. Indeed, where the discipline came under criticism this was on the basis of being *too* powerful, of physicists having been *too* successful in their creation of potent technologies. In the first half of the twentieth century, however, there were different issues at play in the 'public' reception of physics. Throughout my thesis I have explored how physicists struggled to present their work as necessary and relevant, responding to changes in 'public' understanding of physics. Fundamental to these presentations was the question of how 'modern' science' stood in relation to past theories. As new ideas appeared to 'overthrow' the old, there was a sense among many 'publics' that physics had lost its claim to ultimate objective knowledge. As I noted in Chapter Four, a *Manchester Guardian* writer interpreted relativity theory as revealing an underlying weakness in physicists' abilities, while a number of biologists were apparently pleased to observe physics lose its place as the 'foundational' science with access to the highest order of knowledge. Throughout my thesis, I have explored attempts by physicists to counter such arguments and maintain 'public' trust in science. Here I have focused on the 'production' of 'public' interpretations of science, rather than

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[22] Planck (1949) wrote this in his autobiography, in reference to the 'defeat' of Ostwald and the energeticists, pp.33-4. Kuhn (1970) later utilised these words in his discussion of the nature of scientific change, p.151

the ‘reception’. The discussions of scientific progress in widely reported British Association addresses, popular books and articles and non-specialist lectures are revealing of how physicists were working to present their discipline to more general audiences. Evidence of how such efforts were received is of course not easy to find, but if appropriate sources were uncovered then this would certainly be a fruitful path to explore. I end my thesis by considering how I have contributed to knowledge of the ‘production’ of such ideas. In the face of a scientific ‘revolution’, what rhetorical devices did physicists employ to maintain a sense of stability and permanence in their field?

I begin to answer this question by reflecting on the efforts by one physicist to not obscure but rather emphasise the potentially destructive consequences of this revolution. In Oliver Lodge’s 1913 attack on discontinuity, this ‘modern’ trend in physics was situated within a wider context, the ‘spirit of revolution’ referred to by McLaren. Discontinuity of matter and energy was related to a more fundamental discontinuity in the progress of physics, an abandonment of past theories. Outside of physics, in literature, religion and art, we find similar challenges to past authorities, and considerations of the place of old ideas in a changing discipline. A series of international political revolutions provide these deliberations with a sense of urgency and realism. Discontinuity thus referred to a revolutionary break with tradition. In his 1913 address, Lodge was arguing that a ‘modern’ approach altered physics’ former purpose as the means to uncovering the true nature of reality. ‘Modern’ physicists were treating the discipline as containing the tools to merely describe the world not explain it. In this respect, Lodge was emphasising the weaknesses of ‘modern’ physics. Here, he was not trying to acquire ‘public’ trust in ‘modern’ physics, but rather to reveal the reasons why the new physics could not be trusted, in order to promote a return to older ‘conservative’ approaches. For Lodge, much of ‘modern’ physics *was* unstable and ultimately untrustworthy, and its reputation could only be rescued with renewed attention to ‘classical’ physics.

However, Lodge was not entirely dismissing new developments, but rather situating them within what he saw as the broader progress of the discipline. Throughout Lodge’s popular expositions of ‘modern’ physics, he informed the ‘public’ that physics was in a state of transition. He depicted ‘modern’ physics as incomplete, awaiting its twentieth century ‘Newton’ to tie together the seemingly incompatible strands of new knowledge and connect them to past theories. Lodge depicted the nature of scientific progress as being contingent on *consolidation* between the old and new. The 'public' thus had reason to trust 'modern' physics because it would eventually be reinterpreted as compatible with Newtonian physics. Nothing would have been lost, and so there was no ultimate threat to the stability of the discipline.

Lodge's position as a 'public' figurehead of science meant that his views were widely heard, complicating the efforts of other physicists to promote the utility of 'modern' physics. There was thus an increased need by these 'modernists' to provide a degree of 'damage control', presenting their own interpretations of how physics was progressing. In some respects, Eddington used a similar tactic to Lodge, but promoted consolidation not as a future goal but rather a present reality. He argued that the ether had not been rejected, merely redefined, and thus the most important element of the discipline's 'classical' history remained in 'modern' physics. However, more broadly, Eddington solved the problem of progress by emphasising a consistency in the practice of physicists. In Eddington's framing of the development of 'modern' physics, the 'revolution' was not a threat, as the rules of the discipline had remained the same. 'Modern' techniques were a natural progression from the past to the present, and physics had ultimately not changed, it had simply improved. There was no need for the 'public' to alter their perceptions of the subject, as it was continuing to perform the same function it had always done.

Eddington was not the only physicist to maintain consistency between the 'classical' and the 'modern' through promoting refinement rather than revolution. James Rice also used this method, but approached the issue from an experimental perspective, presenting the history of physics as a linear progression of refined experimental techniques and apparatus. This had the added benefit of tackling the perception of 'modern' physics having become ever more inaccessible to the 'public' on account of its incomprehensibility outside of specialist circles. Rice depicted the change as being not intellectual but practical. Physicists had access to aspects of the natural world which ordinary people did not have, but this was because of the instruments they used not the theoretical arguments, and complicated mathematics, they employed. Again physics had not changed, it had simply developed enhanced technologies, opening up previously unseen territories, which had always existed but had until now been unreachable. The 'public' thus had more reason than ever to trust scientists, who were now better equipped to produce objective knowledge.

The idea of 'modern' physics as a refinement of techniques and technology was promoted by A. O. Rankine, who argued that many physicists were answering old questions with new methods. Such a narrative of progress was also on display at the Science Museum throughout the 1930s, with its exhibitions of the apparatus being used by scientists to effective purpose. As well as depicting the utility of 'modern' physics, in terms of both intellectual and practical achievements, the Museum also laid out a narrative of progress through technology. These displays were guided by physicists, actively involved in the material interpretation of their work to 'public' audiences. At the Science Museum there was no hint of controversy or a lack of consensus among scientists, and visitors were provided with an image of unfettered progress.

Not all interpretations of the development of 'modern' physics involved a notion of continuous progress connecting the past to the present. James Jeans' 1942 vision was a disjointed one, in which different types of physics existed in different worlds. However, they were all ultimately connected, with these worlds governed by specialised versions of the same general laws. Notably, the case of Jeans reveals a sharp distinction between his 'public' and professional depictions of 'modern' physics and scientific progress. This is in stark contrast to Lodge, who promoted his consolidation approach to the 'public', but also encouraged the active research of ether physics in the *Philosophical Magazine*. Jeans, in his management of the Royal Society's *Proceedings*, instead discouraged the publication of research that did not follow a 'modern' approach, deeming it irrelevant to the progress of the discipline. In his addresses to a 'professional' audience, the same approach can be found. In 1926, as president of the Royal Astronomical Society, Jeans was responsible for presenting the Gold Medal to Einstein for his researches on relativity and the theory of gravitation. Here, he declared that Einstein had, in 1905, started a 'revolution in scientific thought to which as yet we can see no end'.[23] Although admitting that, in terms of practical results, there wasn't really much the matter with Newton's laws at all, Jeans did not use this as a reason to deny any massive overhaul. As he pointed out, although there was nothing wrong in this sense, the fact that Newton's laws couldn't fit into the new, four dimensional, reality meant that 'there was as much wrong as the difference between truth and error, which the true man of science regards

[23] 'The President's Address' (1926), p.264.

as the biggest magnitude with which he ever has to deal'.[24] The message here was quite clear: Newton was wrong and Einstein was right.

However, this was not the case at the Newton tercentenary, or when writing in his bestselling 1930 book, *The Universe Around Us*, which he described as popular, written in simple language and intended to be comprehensible to readers with no specialist scientific knowledge.[25] Here, describing how one can use the speed in orbit and distance from the sun of any planet in order to determine the sun's gravitational pull, Jeans noted that this 'provides striking confirmation of the truth of Newton's law of gravitation'. And while Einstein had 'recently shewn that the law is not absolutely exact', this amount of inexactness was only revealed in Mercury's orbit, 'and even here it is so exceedingly small that we need not trouble about it for our present purpose'.[26] Indeed, in the following few pages, Newton's law was 'confirmed' twice more, and Jeans found himself again levying 'toll on the mathematical work of Newton'.[27] When he moved away from celestial space on to notions of time, Jeans yet again found that 'it is a matter of complete indifference for our present purpose whether we use the law [of gravity] in Newton's or in Einstein's form; for stellar problems the two are practically indistinguishable, and there is abundant evidence, particularly from the observed orbits of binary stars, in favour of either'.[28] For practical purposes, Jeans noted that he was happy to use either theory, or even any other 'not entirely dissimilar law'. When celebrating Einstein, the value of Newton's work was diminished; when celebrating Newton, of course it was increased considerably; and when addressing an audience he deemed as 'popular', Jeans made sure to emphasise that Newton's laws were still correct. Jeans' concepts of progress were changeable, according to context. For the progress of science to be framed in a way acceptable to the 'public', Jeans believed it was important to retain the legacy of Newton.

Indeed, throughout these diverging efforts to maintain 'public' trust in physics, Isaac Newton is the one consistency. Through him, scientific consensus and stability was achieved. There was an overarching desire among physicists to find a place for Newton in both the history and the future of the discipline. His name was a reminder of physics' past glories, its unequalled achievements, and its foremost position in the hierarchy of

[24] Anonymous (1926b), pp.264-5.
[25] Jeans (1929), Preface; The book is discussed in Whitworth (1996).
[26] Jeans (1929), p.44.
[27] Jeans (1929), pp.45-6.
[28] Jeans (1929), p.158.

knowledge. ‘Modern’ and ‘classical’ physicists alike worked to maintain an unbroken link to this symbol of their past accomplishments, as a beacon of the unlimited potential that lay ahead. Physicists could reject past theories, but they needed to save their idols.

Tables

Table 5.1: Institutional affiliations of members of the Royal Society's Physical Committee, 1921-1930

Committee Member	Cambridge	Cavendish	London	Non-academic	Other University
F. W. Aston 1877-1945	1910-1945	1910-1945		**Brewery**[1] 1900-1903	**Birmingham** 1893-1900 1903-1910
C. V. Boys 1855-1944			1873-1897	**Metropolitan Gas Referee** 1897-1939	
W. H. Bragg 1862-1942	1881-1885		1918-1942		**Adelaide** 1885-1908 **Leeds** 1909-1915
W. L. Bragg 1890-1971	1909-1914 1945-1953	c.1910-1914 1945-1953		**Royal Institution** 1953-1966	**Adelaide** 1904-1908 **Manchester** 1919-1937
F. W. Dyson 1868-1939	1888-1894			**Greenwich Observatory** 1894-1905 1910-1933 **Royal Observatory, Scotland** 1905-1910	
A. Fowler 1868-1940			1882-1940		
R. T. Glazebrook 1854-1935	1872-1898	1876-1898	1920-1923	**National Physical Laboratory** 1899-1919	**Liverpool** 1898-1899
F. Horton 1878-1957	1901-1914	c.1901-1914	1914-1946		**Birmingham** c.1896-1901
J. H. Jeans 1877-1946	1896-1904 1910-1912	1899-1900		**Royal Institution** 1935-1946	**Princeton** 1905-1909
F. A. Lindemann 1886-1957				**Royal Aircraft Factory** 1914-1919	**Berlin** 1908-1914 **Oxford** 1919-1956
H. G. Lyons 1864-1944				**Military Engineering and surveying** (1882-1896) **Geological survey of Egypt** (1896-1912) **Science Museum** (1912-1933)	
T. R. Merton 1888-1969			1905-1906 1916-1919		**Oxford** 1906-1910 1919-1935

J. W. Nicholson 1881-1955	1904-1912	c.1904-1912	1912-1921		**Manchester** 1898-1901 **Queens University, Belfast** c.1904-1912 **Oxford** 1921-1930
O. W. Richardson 1879-1959	1897-1906	1897-1906	1914-1924[2]		**Princeton** 1906-1914
E. Rutherford 1895-1937	1895-1898 1919-1937	1895-1898 1919-1937			**University of New Zealand** 1890-1895 **McGill University** 1898-1907 **Manchester** 1907-1919
G. C. Simpson 1878-1965				**Indian Meteorological Office** (1906-1917) **Indian Munitions Board** (1917-1919) **Meteorological Office** (1920-1938)	**Manchester** 1897-1902 **Göttingen** 1902-1905 **Manchester** 1905-1906
F. E. Smith 1876-1970			c.1896-1900	**National Physical Laboratory** 1900-1920 **Admiralty** 1920-1929 **DSIR** 1929-1939 **Anglo-Iranian Oil**[3] 1939-1955	
S. W. J. Smith 1871-1948	1891-1896	1894-1896	1887-1890 1896-1919	**Birmingham gas examiner** 1934-1946	**Birmingham** 1919-1936
R. J. Strutt 1875-1947	1894-1906	1894-1906	1908-1920		
F. Twyman 1876-1959			1892-1897	**Scientific instrument maker**[4] 1898-1946	
G. T. Walker 1868-1958	1886-1903		1884-1886 1924-1934	**Observatories in India** 1904-1924	
C. T. R. Wilson 1869-1959	1888-1936	1895-1914			**Manchester** 1884-1888

1 Messrs W. Butler & Co., Wolverhampton
2 From then he was a Royal Society Yarrow Professor, a research only grant which freed him from 3 teaching duties.
3 Later British Petroleum
4 Adam Hilger Ltd.

Table 6.1 Subject divisions at the Science Museum, 1930

Information taken from 'Science Museum Annual Reports for 1931', SMD

Division I
Industrial Machinery and Manufactures including –
Electrical Engineering
Electrical Communication
Glass Technology
Hand and Machine Tools
Lighting and Illumination
Papermaking
Typewriting and Printing
Textile Machinery
Mining
Ore Dressing
Agricultural Implements and Machinery
Metallurgy

Division II
Power Production, Land Transport, Pumps and Civil Engineering including –
Water Supply
Building Construction
Sewage and Refuse Disposal
Heating
Weighing and Measuring
Locomotives and Rolling Stock
Railway Construction and Working
Carts, Carriages and Cycles
Mechanical Road Vehicles
Roads and Bridges
Pumping Machinery
Power Transmission
Motors other than Heat Engings
Stationary Steam Engines
Internal Combustion Engines
Engine Details
Land Boilers and Details

Division III
Air and Water Transport including –
Aeronautics
Marine Engineering
Ship Construction

Division IV
Optical Instruments, Chemistry including –
Astronomy
Optical Instruments
Chemistry
Industrial Chemistry
Mathematics
Photography and Kinematography

Division V
Physical and Geophysical Instruments including
Electrical and Magnetic Instruments
Thermal Instruments
Acoustical Instruments
Geodesy and Surveying
Geophysical Instruments, including those for –
Meteorology, Seismology, Terrestrial Magnetism, Tidal Phenomena and Gravitational Surveys
Time Measurement

Figures

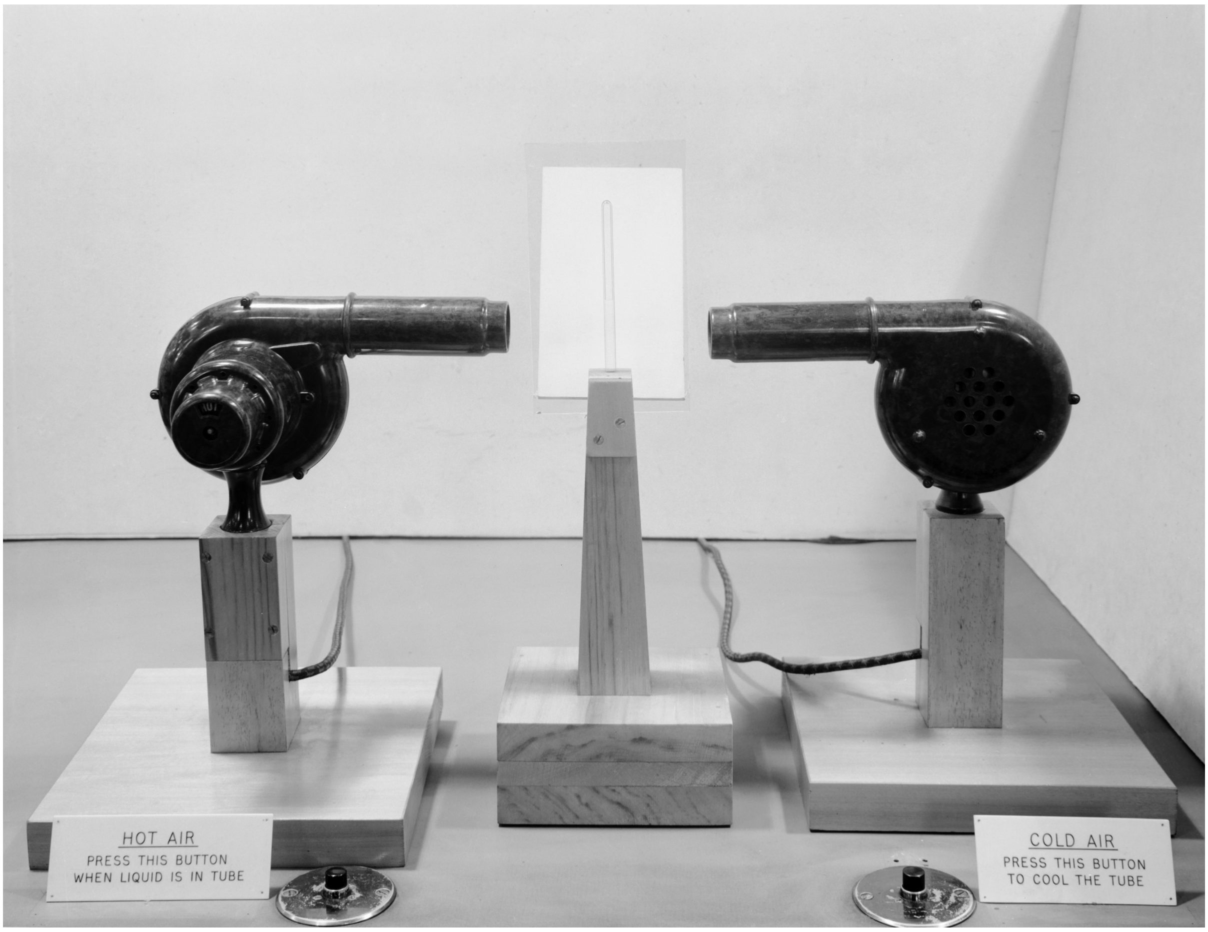
Figure 6.1: Very Low Temperatures Exhibition

Science Museum, 1936

Science Museum, London

Figure 6.2 Liquefaction and Solidification

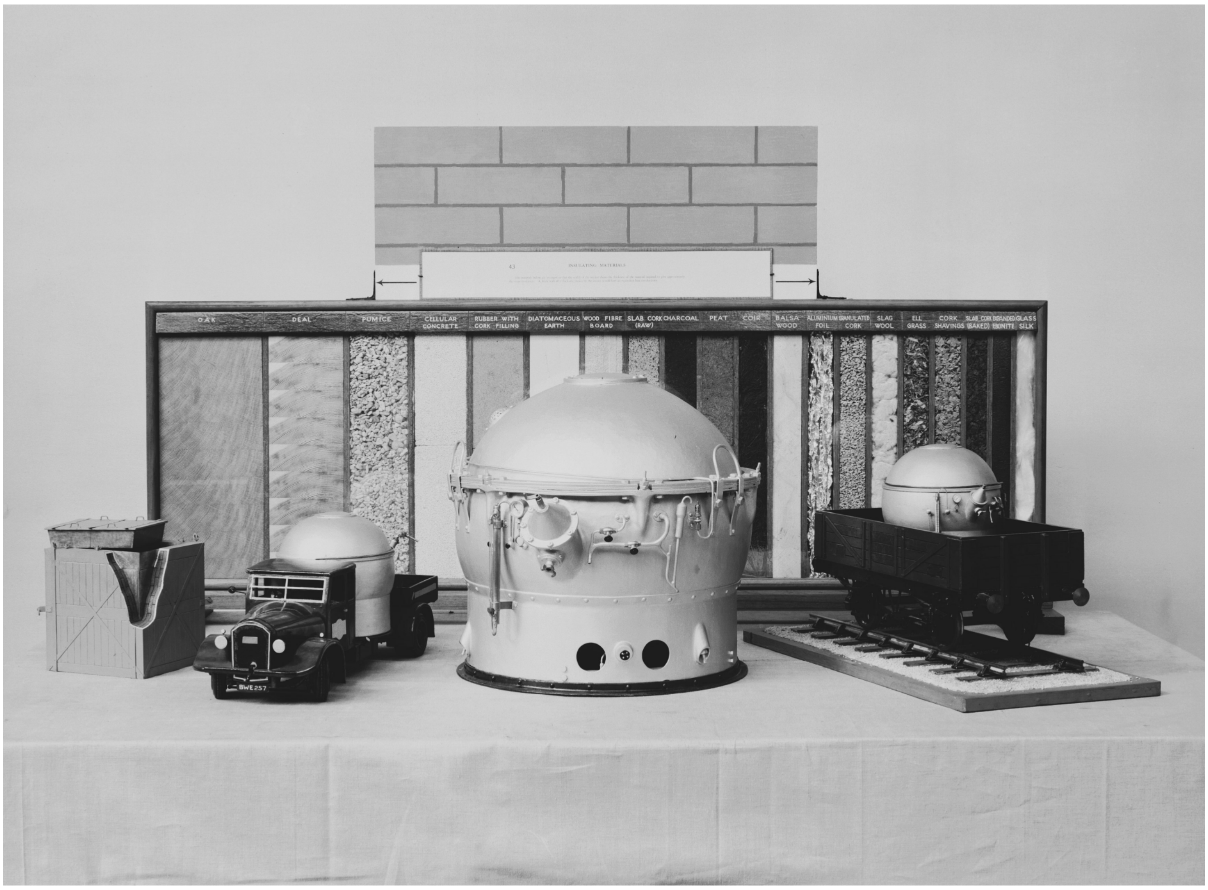
Working model at the Very Low Temperatures Exhibition, Science Museum, 1936



Science Museum, London

Figure 6.3 Container for transporting solid carbon dioxide and liquid nitrogen

Model at the Very Low Temperatures Exhibition, Science Museum, 1936



Science Museum, London

Figure 6.4 Oxygen Evaporator

Model at the Very Low Temperatures exhibition, Science Museum, 1936

Science Museum, London

Figure 6.5 Diagram showing proportions of atmospheric spectra

Model at the Very Low Temperatures exhibition, Science Museum, 1936

Science Museum, London

Figure 6.6 Atom Tracks Exhibition - Entrance

Science Museum, 1937

Science Museum, London

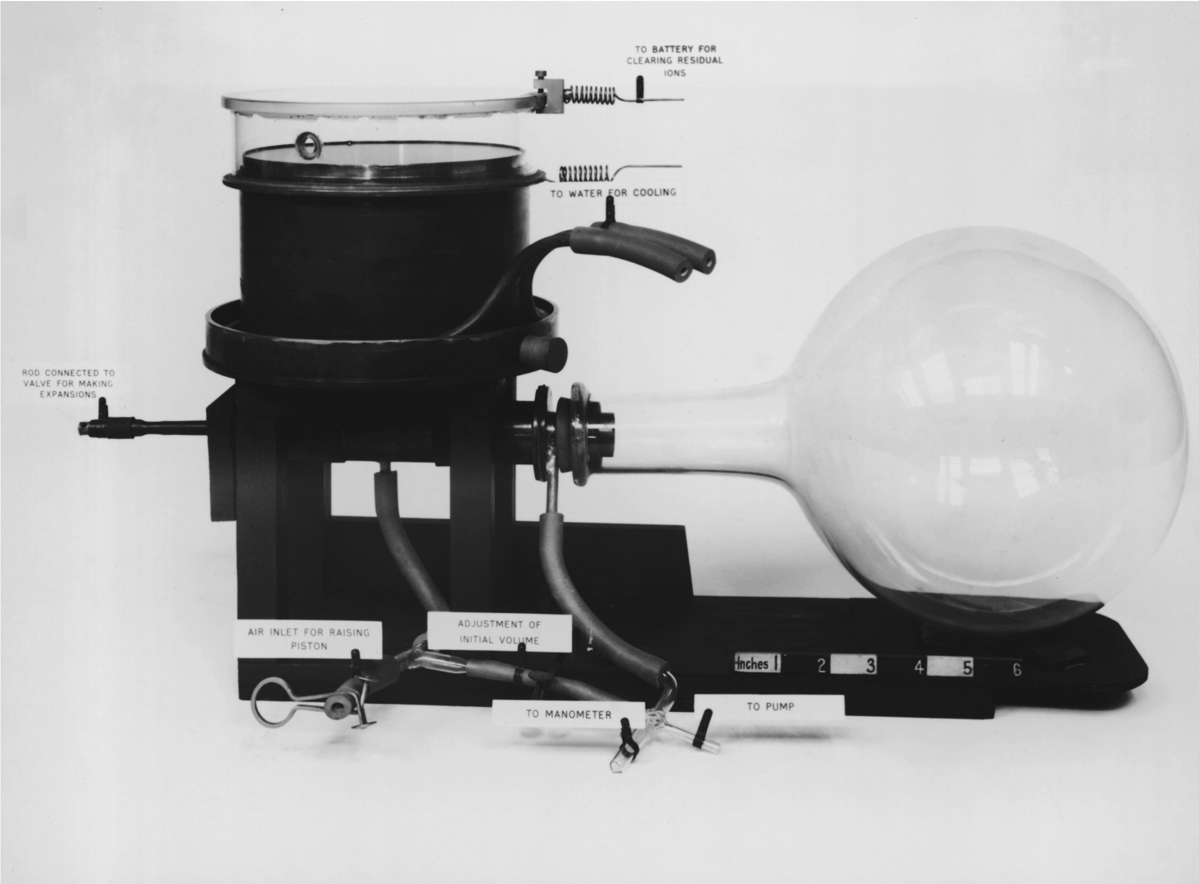
Figure 6.7 Atom Tracks Exhibition - Gallery

Science Museum, 1937

Science Museum, London

Figure 6.8 C. T. R. Wilson's Cloud Chamber

Atom Tracks Exhibition, Science Museum, 1937



Science Museum, London

Bibliography

Archives

ARM: Henry Edward Armstrong papers, Imperial College Archives

BRAGG: William Henry Bragg papers, Royal Institution Archives

GUARD: Guardian (formerly Manchester Guardian) Archive, John Rylands University Library, Manchester University

HILL: The Papers of Professor A V Hill, Churchill Archives Centre, University of Cambridge

JOLY: The Papers of John Joly, Manuscripts and Archives Research Library, Trinity College, Dublin

LAR: Joseph Larmor Papers, St. John's College Library, University of Cambridge

LIV: Liverpool University Archives

LODGE: Lodge Papers, University College London Special Collections

NEWS: News International Archive and Record Office

NORTH: Papers of Alfred Charles William Harmsworth (Viscount Northcliffe), Manuscript Collections, British Library

OWR: Papers of Owen Willans Richardson, Archive for the History of Quantum Physics

RS: Royal Society Archives

RS-LAR: Larmor Papers, Royal Society Archives

SM: Science Museum Archives

SM-ATOM: Special Exhibitions: Atom Tracks Exhibition, 1937-1938 (ED79/44), Science Museum Archives

SM-PHYS: Schemes of Development: Physical Phenomena, 1923-1938 (ED79/138), Science Museum Archives

SM-SPE: Special Exhibitions: General File, 1929-1945 (ED79/42), Science Museum Archives

THOM: Papers of Sir Joseph John Thomson, Department of Manuscripts and University Archives, Cambridge University Library

Primary Published Sources

'Abstract of the Minutes of the Proceedings of the Aristotelian Society for the Thirty-Seventh Session' (1915-1916): *Proceedings of the Aristotelian Society* (New Series) 16: 364-366

D'Albe, Edmund Edward Fournier (1914): 'The Radiation Problem', *Nature* 92 (19 February): 689-691

Andrade, Edward Neville da Costa (1923): *The Structure of the Atom* (London: Bell)

Andrade, Edward Neville da Costa (1927): *The Atom* (London: Benn)

A[rmstrong], H[enry] E[dward] (1913): 'The Mystery of Radioactivity', *Science Progress* 7 (28): 648-655

Armstrong, Henry Edward (1914): 'Sir Oliver Lodge, Intolerant, Infallible', *Bedrock* 2(4): 411-422

van der Bijl, Hendrik Johannes (1920): *The thermionic vacuum tube and its applications* (London: McGraw-Hill)

Bonacina, Leo Claude Wallace (1921): 'Relativity, Space and Ultimate Reality', *Nature* 107 (27 April): 171-2

Bragg, William Henry (1912): *Studies in Radioactivity* (London: Macmillan and Co.)

Bragg, William Henry (1913): 'Radiations old and new', *Nature* 90 (16 January): 557-560

Bragg, William Henry (1932): 'Physical Laboratories and Social Service', *Nature* 129 (2 April): 495-497

Broughton Edge, A. B. (1931): 'Applied Geophysics', *Nature* 127 (23 May): 783-785

Broughton Edge, A. B., and T. H. Laby (eds.) (1931): *The Principles & Practice of Geophysical Prospecting: Being the Report of the Imperial Geophysical Experimental Survey* (Cambridge: Cambridge University Press)

Campbell, Norman Robert (1907): *Modern electrical theory* (Cambridge: Cambridge University press)

Campbell, Norman Robert (1909): 'The study of discontinuous phenomena', *Proceedings of the Cambridge Philosophical Society* 15: 117-137

Campbell, Norman Robert (1913): *Modern Electrical Theory* (Cambridge: Cambridge University Press)

Campbell, Norman Robert (1914): 'The Structure of the Atom', *Nature* 92 (22 January): 586-587

Campbell, Norman Robert (1921a): ‘Theory and Experiment in Relativity’, *Nature* 106 (17 February): 804-6

Campbell, Norman Robert (1921b): ‘”Space” or “Aether”?’, *Nature* 107 (21 April): 234

Crawhall, T. C. (1937): *Very low temperatures* (London: H.M.S.O.)

Crawhall, T. C., and O. Kantorowicz (1937): *Very low temperatures. Bk.2 An illustrated descriptive account of the exhibits in a special exhibition held in the Science Museum from March to June 1936* (London: H. M. S. O.)

Crawhall, T. C. (ed.) (1937): *Very low temperatures. Bk.3 A symposium of lectures delivered in connection with a special exhibition held in the Science Museum from March to June, 1936* (London: H. M. S. O.)

Crommelin, Andrew Claude de la Cherois (1921): ‘Relativity and the Motion of Mercury’s Perihelion’, *Nature* 106 (17 February): 787-9

Curtis, William Edward (1926): ‘New series in the secondary hydrogen spectrum’, *Philosophical Magazine* (Series 7) 1(3): 695-700

D., D. (1921): ‘Introduction’, *Nature* 106 (17 February): 781

Dale, Henry (1943): ‘Anniversary Address by Sir Henry Dale’, *Proceedings of the Royal Society of London A* 181: 211-226

Donkin, Bryan (1913): ‘Psychical Research III – Science and Spiritualism’, *Bedrock* 1(4): 493-503

Dyson, Frank (1921): ‘Relativity and the Eclipse Observations of May, 1919’, *Nature* 106 (17 February): 786-7.

Eddington, Arthur Stanley (1916): 'Gravity and the Principle of Relativity', *Nature* 98 (28 December): 328-30

Eddington, Arthur Stanley (1918): *Report on the Relativity Theory of Gravitation* (London: Fleetway Press Ltd.)

Eddington, Arthur Stanley (1919): ‘Einstein’s Theory of Space and Time’, *Contemporary Review* 116: 639-643

Eddington, Arthur Stanley (1920): *Space Time and Gravitation: An outline of the general relativity theory* (Cambridge: University Press)

Eddington, Arthur Stanley (1921a): ‘The Relativity of Time’, *Nature* 106 (17 February): 802-804

Eddington, Arthur Stanley (1921b): ‘”Space” or “Aether”?’, *Nature* 107 (14 April): 201

Eddington, Arthur Stanley (1928): *The Nature of the Physical World* (Cambridge: Cambridge University Press)

‘Editorial Notes’ (1921): *Discovery* 2 (June): 137-139

‘Editorial Notes’ (1929a): *Discovery* 10 (January): 1

‘Editorial Notes’ (1929b): *Discovery* 10 (February): 35

Einstein, Albert (1905a): ‘On the Electrodynamics of Moving Bodies’, *Annalen der Physik* 17: 891-921

Einstein, Albert (1905b): ‘On a Heuristic Viewpoint Concerning the Production and Transformation of Light’, *Annalen der Physik* 17: 132-148

Elliott, Hugh S. (1912a): *Modern Science and the Illusions of Professor Bergson* (London: Longmans, Green and Co.)

Elliott, Hugh S. (1912b): ‘Modern Vitalism’, *Bedrock* 1(3): 312-332

Ferguson, Allan (1936): ‘Trends in Modern Physics’, *Nature* 138 (7 November): 785-9

Flint, H. T. (1928): 'Relativity and the Quantum Theory', *Proceedings of the Royal Society of London. Series A, Containing Papers of a Mathematical and Physical Character* 117(778): 630-637

Flint, H. T., and J. W. Fisher (1928): 'The Fundamental Equation of Wave Mechanics and the Metrics of Space', *Proceedings of the Royal Society of London. Series A, Containing Papers of a Mathematical and Physical Character* 117(778): 625-629

Flint, H. T., and O. W. Richardson (1928): 'On a Minimum Proper Time and Its Applications (1) to the Number of the Chemical Elements (2) to Some Uncertainty Relations', *Proceedings of the Royal Society of London. Series A, Containing Papers of a Mathematical and Physical Character* 117(778): 637-649

Glazebrook, Richard (1896): *James Clerk Maxwell and modern physics* (London: Cassell)

Green, G. (1920): ‘A fluid analogue for the æther’, *Philosophical Magazine* (Series 6) 39(234): 651-659

Hargreaves, Richard (1901): *Arithmetic* (Oxford: Clarendon Press)

Hargreaves, Richard (1922): ‘Atomic systems based on free electrons, positive and negative, and their stability’, *Philosophical Magazine* (Series 6) 44(264): 1065-1105

Hartree, Douglas R. (1923): 'On the propagation of certain types of electromagnetic waves', *Philosophical Magazine* (Series 6) 46(273): 454-460

Hill, Arthur (1912): ‘Fair Play and Common Sense in Psychical Research’, *Bedrock* 1(3): 342-350

Jeans, James Hopwood (1908): *Mathematical Theory of Electricity and Magnetism* (Cambridge: Cambridge University Press)

Jeans, James Hopwood (1914): *Report on Radiation and the Quantum-Theory* (London: “The Electrician” Printing & Publishing Co. Ltd.)

Jeans, James Hopwood (1919): ‘The Quantum Theory and New Theories of Atomic Structure’, *Journal of the Chemical Society, Transactions* 115: 865-871

Jeans, James Hopwood (1921): ‘The General Physical Theory of Relativity’, *Nature* 106 (17 February): 791-3

Jeans, James Hopwood (1924): *Report on Radiation and the Quantum-Theory*, Second Edition (London: Fleetway Press)

Jeans, James Hopwood (1929): *The Universe Around Us* (Cambridge: Cambridge University Press)

Jeans, James Hopwood (1930): *The Mysterious Universe* (Cambridge: Cambridge University Press)

Jeans, James Hopwood (1943): ‘Newton and the Science of To-Day’, *Proceedings of the Royal Society of London A* 181: 251-262

Jones, T. (1954): *A diary with letters, 1931-1950* (Oxford: Oxford University Press)

Lankester, E. Ray (1912): *The Kingdom of Man* (London: Watts and Co)

Larmor, Joseph (1900): *Aether and matter : a development of the dynamical relations of the aether to material systems, on the basis of the atomic constitution of matter : including a discussion of the influence of the Earth's motion on optical phenomena : being an Adams Prize Essay in the University of Cambridge* (Cambridge: Cambridge University Press)

L[armor], J[oseph] (1922): ‘A Type of Ideal Electric Atoms’, *Nature*, 110 (30 December): 873

Larmor, Joseph (1923): ‘On the nature and amount of the gravitational deflexion of light’, *Philosophical Magazine* (Series 6) 45(265): 243-256

Leighton, J. A. (1910): ‘On Continuity and Discreteness’, *The Journal of Philosophy, Psychology and Scientific Methods* 7(9): 231-238

Lewis, W. C. M., and James Rice (1918): *A system of physical chemistry* (London: Longmans, Green)

Lockyer, Norman (1894): *The dawn of astronomy: a study of the temple-worship and mythology of the ancient Egyptians* (London: Cassell)

Lodge, Oliver (1897): ‘Presidential Address’, *Proceedings of the Physical Society of London* 16: 343-386

Lodge, Oliver (1906): *Electrons; or, The nature and properties of negative electricity* (G. Bell and Sons)

Lodge, Oliver (1907): ‘The density of the aether’, *Philosophical Magazine* (Series 6) 13(76): 488-506

Lodge, Oliver (1909): *The Ether of Space* (New York; London: Harper & Brothers)

Lodge, Oliver (1912a): ‘Becquerel Memorial Lecture’, *Journal of the Chemical Society, Transactions* 101: 2005-2042

Lodge, Oliver (1912b): ‘Uncommon Sense as a Substitute for Investigation’, *Bedrock* 1(3):333-341

Lodge, Oliver (1913a): ’On Telepathy as a Fact of Experience: A Reply to Sir Ray Lankester’, *Bedrock* 2(1): 57-64

Lodge, Oliver (1913b): ‘Prof. Armstrong and Atomic Constitution’, *Nature* 91 (31 July): 558

Lodge, Oliver (1913c): ‘Atomic Theory and Radioactivity’, *Science Progress* 8 (30): 197-201

Lodge, Oliver (1916): *Raymond: or, Life and death: with examples of the evidence for survival of memory and affection after death* (London: Methuen)

Lodge, Oliver (1919a): ‘The New Theory of Gravity’, *Nineteenth Century and After* 86: 1189-1201

Lodge, Oliver (1919b): ‘On a possible means of determining the two characteristic constants of the Æther of space’, *Philosophical Magazine* (Series 6) 37(221): 465-471

Lodge, Oliver (1920a): ‘The Ether Versus Relativity, *Fortnightly Review* 107(637): 54-9

Lodge, Oliver (1920b): ‘Popular Relativity and the Velocity of Light’ *Nature* 106 (4 November): 325-326

Lodge, Oliver (1920c): ‘Note on a possible structure for the ether’, *Philosophical Magazine* (Series 6) 39(230): 170-174

Lodge, Oliver (1921a): ‘The “Philosophical” Magazine’, *Nature* 108 (1 September): 12

Lodge, Oliver (1921b): ‘The Geometrisation of Physics and its Supposed Basis on the Michelson-Morley Experiment’, *Nature* 106 (17 February): 795-800

Lodge, Oliver (1921c): ‘Ether, light, and matter', *Philosophical Magazine* (Series 6) 41(246): 940-943

Lodge, Oliver (1924): *Atoms and Rays: An Introduction to Modern Views on Atomic Structure and Radiation* (London: Benn)

Lodge, Oliver (1925): *Relativity: A Very Elementary Exposition* (London: Methuen)

Lodge, Oliver (1927): *Modern scientific ideas, especially the idea of discontinuity: being the substance of the talks on "Atoms and worlds" broadcast during October and November, 1926* (London: Benn)

Lodge, Oliver (1929): ‘The New Outlook in Physics’, *Discovery* 10 (April): 109-112

Maxwell, James Clerk (1873): *A Treatise on Electricity and Magnetism* (Oxford: Clarendon Press)

MacAlpine, T. W. (1925): 'Correspondence', *Journal of Scientific Instruments* 2(4): 143

McDougall, W. (1913): ‘Modern Materialism’, *Bedrock* 2(1): 24-41

McLaren, Samuel B. (1913): ‘The theory of radiation’, *Philosophical Magazine* (Series 6) 25(145): 43–56

Meksyn, D. (1927): 'The physical form of ether', *Philosophical Magazine* (Series 7) 4(21): 272-300

Mitchell, Peter Chalmers (1915): *Evolution and the War* (London: J. Murray)

Mitchell, Peter Chalmers (1937): *My Fill of Days* (London: Faber and Faber)

Moseley, H., and C. G. Darwin (1913a): ‘The Reflection of the X-rays’, *Nature* 90 (30 January): 594

Moseley, H., and C. G. Darwin (1913b): ‘The Reflexion of the X-rays’, *Philosophical Magazine* (Series 6) 26(151): 210-232

‘News and Views’ (1927): *Nature* 119 (4 June): 824-9

‘Notes’ (1920): *The Observatory* 43: 375-6

‘Notes’ (1921): *The Observatory* 44: 312-324

The Physical Laboratories of the University of Manchester: a record of 25 years' work prepared in commemoration of the 25th anniversary of the election of Dr. Arthur Schuster, F. R. S., to a professorship in the Owens College, by his old students and assistants (1906): (Manchester: Manchester University Press)

Planck, Max (1949): *Scientific Autobiography and Other Papers* (trans. Frank Gaynor) (Greenwood Press, Publishers)

Plummer, William G. (1925): ‘The crystalline structure of hexachlorobenzene and 2 hexabromobenzene’, *Philosophical Magazine* (Series 6) 50(300): 1214-1220

Lucien Poincaré (1907): *The new physics and its evolution; Being the authorized translation of "La physique moderne son evolution* (London: K. Paul, Trench, Trübner, & co.)

Poincaré, Henri (1905): *Science and Hypothesis* [trans. W. J. G.] (London: Walter Scott Publishing Co.)

Poincaré, Henri (1912) ‘L'hypothese des Quanta', *Journal de Physique* 2: 5-34

Poincaré, Lucien (1907): *The New Physics and its Evolution (Being the authorized translation of "La physique moderne, son évolution)* [trans. Paul Kegan] (London: Trench, Trübner & co.)

‘The President’s Address’ (1926): *Monthly Notices of the Royal Astronomical Society* 86: 262-270

Press, A. (1925): 'Maxwell's electromagnetic æther and the Michelson-Morley experiment', *Philosophical Magazine* (Series 6) 50(298): 809-812

Pullin, V. A. (1928): ‘Benn’s Sixpenny Library: First Scientific Titles’, *Discovery* 9(100): 163-5

Rankine, A. O. (1932): ‘Some Aspects of Applied Geophysics’, *Nature* 130 (17 September): 421-424.

‘Reviews’ (1912): *Bedrock* 1(2): 274

‘Reviews of Books: Atoms and Rays’ (1924): *Discovery* 5 (September): 228

Rice, James (1914): ‘Note on the form assumed by the red corpuscles of the blood, or by the suspended particles in a lecithin emulsion’, *Philosophical Magazine* (Series 6) 28 (167): 664-670

Rice, James (1915a): ‘On the form of a liquid drop suspended in another liquid, whose density is variable’, *Philosophical Magazine* (Series 6) 29(169): 149-154

Rice, James (1915b): 'An elementary account of the quantum theory', *Transactions of the Faraday Society* 11: 1-18

Rice, James (1923a): *Relativity: A Systematic Treatment of Einstein’s Theory* (London: Longmans, Green and Co.)

Rice, James (1923b): ‘The velocity constant of a unimolecular reaction’, *Philosophical Magazine* (Series 6) 46(272): 312-320

Rice, James (1925): ‘On Eddington's natural unit of the field, and possible relations between it and the universal constants of physics’, *Philosophical Magazine* (Series 6) 49(290): 457-463

Rice, James (1926): 'Note on the radiation theory of chemical reaction', *Transactions of the Faraday Society* 21(February): 494-503

Rice, James (1927): *Relativity: An Exposition Without Mathematics* (London: Ernest Benn)

Rice, James (1928): *An introduction to physical science* (London: Ernest Benn)

Rice, James (1930): *Introduction to statistical mechanics for students of physics and physical chemistry* (London: Constable)

Royal Society (1921): *Year book of the Royal Society* (London: Royal Society)

Royal Society (1924): *British empire exhibition 1924: handbook to the exhibition of pure science : Galleries 3 and 4 British Government Pavilion* (London: Royal Society)

Royal Society (1925): *Phases of modern science : published in connexion with the science exhibit arranged by a committee of the Royal Society in the pavilion of His Majesty's Government at the British Empire Exhibition, 1925* (London: Royal Society)

Rutherford, Ernest (1904a): *Radio-activity* (Cambridge: Cambridge University Press)

Rutherford, Ernest (1904b): 'Disintegration of the radioactive elements', *Harper's Monthly Magazine* 108: 279-284

Rutherford, Ernest (1905): 'Radium – the cause of the earth's heat', *Harper's Monthly Magazine* 110: 390-396

Rutherford, Ernest (1911a): 'The scattering of alpha and beta particles by matter and the structure of the atom', *Philosophical Magazine* 21: 669–688.

Rutherford, Ernest (1911b): 'Conference on the Theory of Radiation', *Nature* (16 November): 82-83.

R[utherford], E[rnest] (1913): 'Modern Physics', *Nature* 90 (27 February): 694-695

Rutherford, Ernest (1927): 'Address of the President, Sir Ernest Rutherford, O.M., at the Anniversary Meeting, November 30, 1927', *Proceedings of the Royal Society of London A* 117: 300-316

Rutherford, Ernest, F. A. B. Ward, and C. E. Wynn-Williams (1930): 'A New Method of Analysis of Groups of Alpha-Rays. (1) The Alpha-Rays from Radium C, Thorium C, and Actinium C', *Proceedings of the Royal Society of London A* 129(809): 211-234.

Rutherford, Ernest, F. A. B. Ward, and W. B. Lewis (1931): 'Analysis of the long-range alpha-particles from, Radium C', *Proceedings of the Royal Society of London A* 131: 684-703

Schuster, Arthur (1911): *The Progress of Physics During Thirty-Three Years (1875-1908)* (Cambridge: Cambridge University Press)

Severini, Gino (1913): 'The Futurist Exhibition', *The Cambridge Magazine* (3 May): 516

Shaw, H., and E. Lancaster-Jones (1922a): 'The Eötvös Torsion Balance', *Proceedings of the Physical Society of London* 35: 151-166

Shaw, H., and E. Lancaster-Jones (1922b): 'Application of the Eötvös Torsion Balance to the Investigation of Local Gravitational Fields', *Proceedings of the Physical Society of London* 35: 204-212

Shaw, H., and E. Lancaster-Jones (1923): ’The Eötvös Torsion Balance and its Use in the Field’, *Nature* 111 (23 June): 849-851.

Shaw, H. (1931): *Applied Geophysics: A Brief Survey of the Development of Apparatus and Methods Employed in the Investigation of Subterranean Structural Conditions and the Location of Mineral Deposits* (London: Science Museum)

Silberstein, L. (1920): ’The recent eclipse results and Stokes-Plack's æther’, *Philosophical Magazine* (Series 6) 39(230): 161-170

‘Societies and Academies’ (1926): *Nature* 117 (12 June): 841-844

Soddy, Frederick (1912): *The interpretation of radium: being the substance of six free popular experimental lectures delivered at the University of Glasgow* (London: John Murray)

Soddy, Frederick (1913): ‘Modern Physical Ideas and Researches’, *Nature* 92 (20 November): 339-340

Synge, E. H. (1922): 'A definition of simultaneity and the æther', *Philosophical Magazine* (Series 6) 43(255): 528-531

Thomson, J. J., Max Planck, Albert Einstein, Sir Joseph Larmor, Sir James Jeans, William Garnett, Sir Ambrose Fleming, Sir Oliver Lodge, Sir R. T. Glazebrook and Sir Horace Lamb (1931): *James Clerk Maxwell: A Commemoration Volume, 1831-1931* (Cambridge: Cambridge University Press)

Thomson, J. J. (1939): ‘Electron Waves’, *Philosophical Magazine* 27: 1-32.

Thomson, William and Peter Guthrie Tait (1867): *Treatise on Natural Philosophy* (Oxford: Clarendon Press)

Tuckett, Ivor Ll. (1912): `Psychical Researchers and “The Will to Believe”’, *Bedrock* 1(2): 180-204

‘Very Low Temperatures: Exhibition at the Science Museum’ (1936): *Nature* 137 (14 March): 466

Ward, F. A. B. (ed.) (1937): *Catalogue of the atom tracks exhibition (November 1937-February 1938): The results of 25 years of research by Professor C.T.R. Wilson's expansion chamber method, in which the tracks of individual atoms and electrons are rendered visible and photographed* (London: H.M. S. O.)

Weiss, H. (1925): ‘The Application of X-Rays to the Study of Alloys’, *Proceedings of the Royal Society of London A* 108: 643-654

Secondary Sources

Abir-Am, Pnina G., and Clark A. Elliott (eds.) (1999): ‘Commemorative Practices in Science: Historical Perspectives on the Politics of Collective Memory’ [Special Issue], *Osiris* 14

Allibone, T. E. (1984): ‘Metropolitan-Vickers and the Cavendish’, in John Hendry (ed.), *Cambridge Physics in the Thirties* (Bristol: Adam Hilger): 150-73

Anonymous (1938): ‘Mr D. Baxandall’, *Nature* 141 (5 March): 402

Anonymous (1939): ‘Dr. A. C. D. Crommelin’, *Nature* 144 (30 September): 578

Anonymous (1971): ‘Obituaries’, *British Medical Journal* 4 (4 December): 628

Anonymous (1972): ‘Dr. A. S. Russell’, *Nature* 237 (12 May): 120-121

Atkinson, D. (1999): *Scientific discourse in sociohistorical context: the Philosophical transactions of the Royal Society of London, 1675-1975* (Mahwah; London: L. Erlbaum Associates)

Badash, Lawrence (1972): 'The Completeness of Nineteenth-Century Science', *Isis* 63(1): 48-58

Badash, Lawrence (1978): ‘Radioactivity and the Popularity of Scientific Discovery’, *Proceedings of the American Philosophical Society* 122(3): 145-154

Badash, Lawrence (1979): ‘The Origins of Big Science: Rutherford at McGill’, in Mario Bunge and William R. Shea (eds.), *Rutherford and Physic at the Turn of the Century* (New York: Dawson and Science History Publications)

Badash, Lawrence (1985): *Kapitza, Rutherford and the Kremlin* (New Haven; London: Yale University Press)

Barnes, Barry, David Bloor, and John Henry (1996): *Scientific Knowledge: A Sociological Analysis* (Chicago: University of Chicago Press)

Bauer, Martin W. (2007): 'The public career of the 'gene' - trends in public sentiments from 1946 to 2002', *New Genetics and Society* 26(1): 29-45

Bauer, Martin W., Nick Allum, and Steve Miller (2007): 'What can we learn from 25 years of PUS survey research? Liberating and expanding the agenda', *Public Understanding of Science* 16: 79-9

Bawn, C. E. H. (1963): 'Morris William Travers. 1872-1961', *Biographical Memoirs of Fellows of the Royal Society* 9: 301-313

Beller, Maria (2001): *Quantum Dialogue: The Making of a Revolution* (University of Chicago Press)

Bergson, Henri (1911): *Creative Evolution* (trans. Arthur Mitchell) (London: Macmillan)

Berman, R. (1987): 'Lindemann in Physics', *Notes and Records of the Royal Society of London* 41(2): 181–189.

Bloor, David (1976/1991): *Knowledge and Social Imagery.* 2nd ed. (Chicago: University of Chicago Press)

Bowler, Peter J. (2001): *Reconciling Science and Religion: The Debate in Early-Twentieth Century Britain* (Chicago: University of Chicago Press)

Bowler, Peter J. (2009): *Science for All: The Popularisation of Science in Early Twentieth-Century Britain* (Chicago: University of Chicago Press)

Bowler, Peter J., and Iwan R. Morus (2005): *Making Modern Science* (Chicago: University of Chicago Press)

Bown, Nicola, Carolyn Burdett, and Pamela Thurschwell (2004): *The Victorian Supernatural* (Cambridge: Cambridge University Press)

Boudia, Soraya (1997): 'The curie laboratory: Radioactivity and metrology', *History and Technology* 13(4): 249-265

Brock, William H., and A.J. Meadows (1998): *The lamp of learning: two centuries of publishing at Taylor & Francis* (London: Taylor & Francis)

Brock, William H. (2002): 'Exploring the Hyperarctic: James Dewar at the Royal Institution', in. F. A. J. L. James (ed.), *'The Common Purposes of Life': Science and society at the Royal Institution of Great Britain* (Aldershot: Ashgate): 169-190

Brock, William H. (2008): *William Crookes (1832-1919) and the Commercialization of Science* (Aldershot: Ashgate)

Broks, Peter (1993): 'Science, Media and Culture: British Magazines, 1890-1914', *Public Understanding of Science* 2: 123-139

Broks, Peter (1996): *Media Science before the Great War* (London: MacMillan)

Broks, Peter (2006): *Understanding Popular Science* (Milton Keynes: Open University Press)

Brown, Laurie M., Abraham Pais, and Brian Pippard (1995): *Twentieth Century Physics - Volume 1* (Bristol; New York: Institute of Physics Publishing; American Institute of Physics Press)

de Bruyne, Norman (1984): 'A Personal View of the Cavendish 1923-30', in John Hendry (ed.), *Cambridge Physics in the Thirties* (Bristol: Adam Hilger): 81-89

Bucchi, Massimiano (2008): ‘Of deficits, deviations and dialogues: theories of public communication of science’, in Massimiano Bucchi and Brian Trench (eds.), *Handbook of Public Communication of Science and Technology* (Abingdon: Routledge): 57-76

Buchwald, Jed Z. (1988): *From Maxwell to Microphysics: Aspects of Electromagnetic Theory in the Last Quarter of the Nineteenth Century* (Chicago: University of Chicago Press)

Buchwald, Jed Z, and Andrew Warwick (eds.) (2001): *Histories of the Electron: The birth of microphysics* (Cambridge, MA: MIT Press)

Bud, Robert (2010): ‘Infected by the Bacillus of Science: The Explosion of South Kensington’, in Peter J. T. Morris (ed.), *Science for the Nation: Perspectives on the History of the Science Museum* (Basingstoke: Palgrave Macmillan): 11-40

Bud, Robert (2012): ‘Life, DNA and the model’, *British Journal for the History of Science* (FirstView Article, Cambridge Journals Online): 1-24

Burnham, John C. (1987): *How Superstition Won and Science Lost* (London: Rutgers University Press)

Burwick, Frederick, and Paul Douglass (1992): *The Crisis in modernism: Bergson and the vitalist controversy* (Cambridge: Cambridge University Press)

Butler, Christopher (1994): *Early Modernism: Literature, Music, and Painting in Europe, 1900–1916* (Oxford: Clarendon Press)

Büttner, Jochen, Jürgen Renn, and Matthias Schemmel (2003): ‘Exploring the Limits of Classical Physics: Planck, Einstein, and the Structure of a Scientific Revolution’, *Studies in the History and Philosophy of Modern Physics* 34: 37–59

Cantor, G. N., and M. J. S. Hodge (1981): *Conceptions of Ether: Studies in the History of Ether Theories, 1740-1900* (Cambridge: Cambridge University Press)

Carey, John (2002): *The Intellectuals and the Masses* (Chicago: Academy Chicago Publishers)

Caroe, G. M. (1978): *William Henry Bragg, 1862-1942: man and scientist* (Cambridge: Cambridge University Press)

Cathcart, Brian (2004): *The Fly in the Cathedral* (London: Penguin)

Chayut, Michael (1991): 'J. J. Thomson: The discovery of the electron and the chemists', *Annals of Science* 48(6): 527-544

Clarke, Bruce, and Linda Dalrymple Henderson (2002): ‘Ether and Electromagnetism: Capturing the Invisible’, in Bruce Clarke and Linda Dalrymple Henderson (eds.), *From energy to information: representation in science and technology, art, and literature* (Stanford, CA: Stanford University Press): 95-97

Cloitre, Michel, and Terry Shinn (1985): 'Expository Practice: Social, Cognitive and Epistemological Linkage', in. T. Shinn and R. Whitley (eds.), *Expository Science: Forms and Functions of Popularisation* (Dordrecht: D. Reidel Publishing Company): 31-60

Cockcroft, J. D. (1954): 'Rutherford: Life and Work after the Year 1919, with Personal Reminiscences of the Cambridge Period', in *Rutherford: by those who knew him, being the collection of the first five Rutherford lectures of the Physical Society* (London: Physical Society)

Collins, Harry (1985): *Changing Order* (Chicago: University of Chicago Press)

Collins, Harry, and Trevor Pinch (1993): *The Golem* (Cambridge: Cambridge University Press)

Collins, Peter (1981): 'The British Association as Public Apologist for Science, 1919-1946', in. Peter Collins and Roy MacLeod (eds.), *The Parliament of Science* (Northwood: Science Reviews, Ltd): 211-236

Cooter, Roger, and Stephen Pumfrey (1994): 'Separate Spheres and Public Places: Reflections on the History of Science Popularization and Science in Public Culture', *History of Science* 32: 237-267

Cottrell, Alan (1972): ‘Edward Neville da Costa Andrade. 1887-1971’, *Biographical Memoirs of Fellows of the Royal Society* 18: 1-20

Crook, D. P. (1989): ‘Peter Chalmers Mitchell and Antiwar Evolutionism in Britain during the Great War’, *Journal of the History of Biology* 22(2): 325-356

Crowther, J. G. (1970): *Fifty Years with Science* (London: Barrie and Jenkins)

Dale, Henry (1944): ‘Henry George Lyons’, *Obituary Notices of Fellows of the Royal Society* 4(13): 795-809

Dangerfield, George (1935): *The Strange Death of Liberal England* (London: Constable)

Darrigol, Olivier (2000): *Electrodynamics from Ampere to Einstein* (Oxford: Oxford University Press)

Darrigol, Olivier (2001): ‘The Historians’ Disagreements over the Meaning of Planck’s Quantum’, *Centaurus* 43: 219–239

Darwin, Charles G. (1962): 'Ezer Griffiths. 1888-1962', *Biographical Memoirs of Fellows of the Royal Society* 8: 41-48

van Delft, Dirk (2008): 'Zero-Point Energy: The Case of the Leiden Low-Temperature Laboratory of Heike Kamerlingh Onnes', *Annals of Science* 65(3): 339-361

Dolby, R. G. A. (1976): 'Debates over the Theory of Solution: A Study of Dissent in Physical Chemistry in the English-Speaking World in the Late Nineteenth and Early Twentieth Centuries', *Historical Studies in the Physical Sciences* 7: 297-404

Donnan, F. G. (1936): ‘James Rice’, *Nature* 137 (16 May): 807-8

Douglas, A. V. (1956): *The Life of Arthur Stanley Eddington* (Thomas Nelson and Sons Ltd.)

Duncan. P. D. (1980): *Newspaper science: The presentation of science in four British newspapers during the interwar years, 1919-1939* M.Phil (University of Sussex)

Earman, J., and C. Glymour (1980): 'Relativity and Eclipses: The British Eclipse Expeditions of 1919 and their Predecessors', *Historical Studies in the Physical Sciences* 11(1): 49-85

East, W. Norwood (1928): 'Sir Horatio Bryan Donkin, M.A., M.D.Oxon., F.R.C.P.', *British Journal of Psychology* 74: 1-12

Eccles, W. H. (1945): 'John Ambrose Fleming. 1849-1945', *Obituary Notices of Fellows of the Royal Society* 5(14): 231-242

Eddington, Arthur Stanley (1942): 'Joseph Larmor. 1857-1942', *Obituary Notices of Fellows of the Royal Society* 4(11): 197-207

Edgerton, David (2005): *Warfare State: Britain, 1920-1970* (Cambridge: Cambridge University Press)

Eley, G. (1993): 'Nations, publics, and political cultures', in C. Calhoun (ed.), *Habermas and the Public Sphere* (Cambridge, MA: MIT Press): 289-339

Eve, A. S. (1935): 'Sir John Cunningham McLennan. 1867-1935', *Obituary Notices of Fellows of the Royal Society* 1(4): 577-583

Everdell, William R. (1997): *The first moderns: profiles in the origins of twentieth-century thought* (Chicago: University of Chicago Press)

Falconer, Isobel (1987): 'Corpuscles, Electrons and Cathode Rays: J. J. Thomson and the 'Discovery of the Electron'', *The British Journal for the History of Science* 20(3): 241-276

Falconer, Isobel (1989): 'J J Thomson and 'Cavendish' Physics', in Frank A J L James (ed.), *The Development of the Laboratory* (London: Macmillan): 104-117

Falconer, Isobel (2001): 'Corpuscles to Electrons', in Jed Z. Buchwald and Andrew Warwick (eds.), *Histories of the Electron: The birth of microphysics* (Cambridge, MA: MIT Press): 77-100

Fara, Patricia (2002): *Newton: The Making of Genius* (London: Macmillan)

Feldberg, W. S. (1970): 'Henry Hallett Dale. 1875-1968', *Biographical Memoirs of Fellows of the Royal Society* 16: 77-174

Follett, David (1978): *The rise of the Science Museum under Henry Lyons* (Science Museum)

Forman, Paul (1971): ‘Weimar Culture, Causality, and Quantum Theory, 1918–1927: Adaptation by German Physicists and Mathematicians to a Hostile Intellectual Environment’, *Historical Studies in the Physical Sciences* 3: 1–116.

Forman, Paul (1979): ‘The Reception of an Acausal Quantum Mechanics in Germany and Britain’, in Seymour H. Mauskopf (ed.), *The Reception of Unconventional Science* (Boulder, Colorado: Westview Press), 11-50.

Forman, Paul, John L. Heilbron, and Spencer Weart (1975): 'Physics circa 1900: Personnel, Funding, and Productivity of the Academic Establishments', *Historical Studies in the Physical Sciences* 5: 1-185

Fox, Robert, and Anna Guagnini (1999): *Laboratories, workshops, and sites: concepts and practices of research in industrial Europe, 1800-191* (Office for History of Science and Technology, University of California, Berkeley)

Friedman, Alan J., and Carol C. Donley (1985): *Einstein as Myth and Muse* (Cambridge: Cambridge University Press)

Gale, A. J. V. (1972): 'Thomas Martin', *British Journal for the History of Science* 6(1): 105

Galison, Peter, and Alexi Assmus (1989): 'Artificial clouds, real particles', in. D. Gooding, T. Pinch and S. Schaffer (eds.), *The Uses of Experiment* (Cambridge: Cambridge University Press): 225-276

Galison, Peter (1983): 'Re-Reading the Past from the End of Physics: Maxwell's Equations in Retrospect', in. L. R. Graham, W. Lepenies and P. Weingart (eds.), *Functions and uses of disciplinary histories* (Dordrecht: D. Reidel): 35-52

Galison, Peter (2003): *Einstein's Clocks, Poincaré's Maps: Empires of Time* (London: Hodder and Stoughton)

Gamwell, Lynn (2002): *Exploring the Invisible: Art, Science, and the Spiritual* (Princeton: Princeton University Press)

Gardner, Lloyd C. (1987): *Safe for Democracy: The Anglo-American Response to Revolution, 1913-1923* (Oxford: Oxford University Press)

Gay, Hannah (2000): 'Association and Practice: The City and Guilds of London Institute for the Advancement of Technical Education', *Annals of Science* 57(4): 369-398

Gay, Hannah (2007): *The History of Imperial College London 1907-2007* (London: Imperial College Press)

Gay, Peter (2008): *Modernism: The Lure of Heresy: from Baudelaire to Beckett and Beyond* (New York: W. W. Norton)

Gere, Cathy (2009): *Knossos and the Prophets of Modernism* (Chicago: University of Chicago Press)

Gillies, M. A. (1996): *Henri Bergson and British modernism* (McGill-Queen's University Press)

Glasgow, Eric (1998): 'Publishers in Victorian England', *Library Review* 47(8): 395 – 400

Glazebrook, R. T. (1932): 'Dr. E. H. Griffiths, F.R.S', *Nature* 129 (26 March): 461-462.

Goldberg, Stanley (1970): 'In Defense of Ether: The British Response to Einstein's Special Theory of Relativity, 1905-1911', *Historical Studies in the Physical Sciences* 2: 89-125

Golinski, Jan (1998): *Making Natural Knowledge* (Cambridge: Cambridge University Press)

Gooday, Graeme (1989): *Precision measurement and the genesis of physics teaching laboratories in Victorian Britain* PhD (University of Kent)

Gooday, Graeme (1990): 'Precision Measurement and the Genesis of Physics Teaching Laboratories in Victorian Britain', *The British Journal for the History of Science* 23(1): 25-51

Gooday, Graeme (1991): `Teaching telegraphy and electrotechnics in the physics laboratory: William Ayrton and the creation of an academic space for electrical engineering 1873-84', *History of Technology* 13: 73-114

Gooday, Graeme, and Daniel Mitchell (2012): 'Rethinking 'classical physics'', in Jed Buchwald and Robert Fox (eds.), *Oxford Handbook to the History of Physics* (Oxford University Press, in preparation)

Goodeve, Charles F. (1972): 'Frank Edward Smith. 1876-1970', *Biographical Memoirs of Fellows of the Royal Society* 18: 525-548

Gorham, Geoffrey (1991): 'Planck's principle and Jeans's conversion', *Studies In History and Philosophy of Science Part A* 22(3): 471-497

Gregory, Jane, and Steve Miller (1998): *Science in Public* (Cambridge, MA: Perseus Publishing)

Griffiths, Ezer (1941): 'George William Clarkson Kaye. 1880-1941', *Obituary Notices of Fellows of the Royal Society* 3(10): 881-891

Hakfoort, C. (1992): 'Science deified: Wilhelm Osstwald's energeticist world-view and the history of scientism', *Annals of Science* 49(6): 525-544

Habermas, Jürgen ([1962], 1989): *The Structural Transformation of the Public Sphere* (Cambridge, MA: MIT Press)

Haimes, Gerard Francis (1982): *Sir Oliver Lodge's Unpublished Physics Book* M.Sc. (University of Liverpool)

Hall, A. (1969): *The Cambridge Philosophical Society: a history, 1819-1969* (Cambridge: Cambridge Philosophical Society)

Harris, Alexandra (2010): *Romantic Moderns: English Writers, Artists and the Imagination from Virginia Woolf to John Piper* (London: Thames & Hudson)

Harris, Jose (1994): *Private Lives, Public Spirit: Britain 1870-1914* (London: Harmondsworth)

Harrison, Thomas J. (1996): *1910, the emancipation of dissonance* (Berkeley: University of California Press)

Heilbron, John L. (1968): 'The scattering of a and ß particles and Rutherford's atom', *Archive for History of Exact Sciences* 4(4): 247-307

Heilbron, John L. (1974): *H.G.J. Moseley: The Life and Letters of an English Physicist, 1887-1915* (Berkeley: University of California Press)

Heilbron, John L. (1982): '*Fin-de-siècle* physics', in Carl Gustaf Bernhard, Elisabeth Crawford, Per Sörbom (eds.), *Science, Technology and Society in the Time of Alfred Nobel* (Oxford: Pergamom): 51-73.

Henderson, Linda Dalrymple (2002): 'Vibratory Modernism: Boccioni, Kupka, and the Ether of Space', in Bruce Clarke and Linda Dalrymple Henderson (eds.), *From energy to information: representation in science and technology, art, and literature* (Stanford: Stanford University Press): *126-149*

Hennessy, Brian, and John Hennessy (2005): *The emergence of broadcasting in Britain* (Southerleigh)

Henry, Holly (2003): *Virginia Woolf and the Discourse of Science: The Aesthetics of Astronomy* (Cambridge: Cambridge University Press)

Hicks, G. Dawes (1930): 'Mr. Hugh S. R. Elliot', *Nature* 125 (24 May): 786-787

Hiebert, Erwin N. (1971): 'The Energetics Controversy and the New Thermodynamics', in Duane H. D. Roller (ed.), *Perspectives in the History of Science and Technology* (Norman: University of Oklahoma Press): 67-86

Hiebert, Erwin N. (1982): 'Developments in physical chemistry at the turn of the century', in. C. G. Bernhard, E. Crawford and P. Sörbom (eds.), *Science Technology and Society in the time of Alfred Nobel* (Oxford: Pergamom): 97-115

Hiebert, Erwin N. (1996): 'Discipline Identification in Chemistry and Physics', *Science in Context* 9: 93-119

Hill, J. A. (1932): *Letters from Sir Oliver Lodge. Psychical, Religious, Scientific and Personal* (London: Cassell)

Hindle, E. (1947): 'Peter Chalmers Mitchell, 1864-1945', *Obituary Notices of Fellows of the Royal Society* 5(15): 367-372

Hirsh, Richard F. (1981): 'A Conflict of Principles: The Discovery of Argon and the Debate over Its Existence', *Ambix* 28(3): 121-130

Hudson, Rob (1989): 'James Jeans and radiation theory', *Studies In History and Philosophy of Science Part A* 20(1): 57-76

Hughes, Jeff (1998): ‘Plasticine and Valves: Industry, Instrumentation and the Emergence of Nuclear Physics’, in Jean-Paul Gaudilliere and Ilana Löwy (eds.), *The Invisible Industrialist: Manufactures and the Production of Scientific Knowledge* (London: Macmillan): 58-101

Hughes, Jeff (2002a): ‘Radioactivity and nuclear physics’, in Mary Jo Nye (ed.), *The Cambridge History of Science Volume: 5 The Modern Physical and Mathematical Sciences* (Cambridge: Cambridge University Press): 350–374

Hughes, Jeff (2002b): ‘Craftsmanship and Social Service: W. H. Bragg and the Modern Royal Institution’, in Frank A. J. L. James (ed.), *‘The Common Purposes of Life’: Science and Society at the Royal Institution of Great Britain* (Aldershot: Ashgate): 225-248

Hughes, Jeff (2005): 'Redefining the Context: Oxford and the Wider World of British Physics, 1900-1940', in. R. Fox and G. Gooday (eds.), *Physics in Oxford 1839 – 1939* (Oxford: Oxford University Press): 267-300

Hughes, Jeff (2007): ‘Insects or neutrons? Science news values in interwar Britain’, in M.W. Bauer and M. Bucchi (eds.), *Journalism, Science and Society: Science Communication between News and Public Relations* (New York; Abingdon: Routledge): 11-20

Hughes, Jeff (2009a): ‘A ‘Wholesome Alliance’?: British Atomic Physicists and the Press, 1925-1935’ (unpublished draft)

Hughes, Jeff (2009b): 'Making isotopes matter: Francis Aston and the mass-spectrograph', *Dynamis* 29: 131-165.

Hughes, Jeff (2010): ‘‘Divine right’ or democracy? The Royal Society ‘revolt’ of 1935’, *Notes and Records of the Royal Society* 64 (Supplement 1): S101-S117

Hull, Andrew (1999): 'War of words: the public science of the British scientific community and the origins of the Department of Scientific and Industrial Research, 1914–16', *British Journal for the History of Science* 32: 461-81

Hunt, Bruce J. (1991): *The Maxwellians* (Ithaca: Cornell University Press)

Jaki, Stanley L. (1986): *Lord Gifford and his lectures: a centenary retrospect* (Edinburgh: Scottish Academic Press)

Jenkin, John (2004): 'William Henry Bragg in Adelaide: Beginning Research at a Colonial Locality', *Isis* 95(1): 58-90

Jenkin, John (2008): *William and Lawrence Bragg, Father and Son: The Most Extraordinary Collaboration in Science* (Oxford: Oxford University Press)

Jenkin, John (2011): ‘Atomic Energy is “Moonshine”: What did Rutherford Really Mean?’, *Physics in Perspective* 13(2): 128-145

Jones, R. V., and William S. Farren (1961): ‘Henry Thomas Tizard, 1885-1959’, *Biographical Memoirs of Fellows of the Royal Society* 9(1): 313-48

Jones, Thomas (1954): *A Diary with Letters, 1931-1950* (Oxford: Oxford University Press)

Kaiser, David (ed.) (2005): *Pedagogy and the Practice of Science: Historical and Contemporary Perspectives* (Cambridge, MA: MIT Press)

Kauffman, George B. (ed.) (1986): *Frederick Soddy (1877-1956): early pioneer in radiochemistry* (Dordrecht; Lancaster: Reidel)

Keller, Alex (1983): ‘Continuity and Discontinuity in Early Twentieth-Century Physics and Early Twentieth-Century Painting’, in M. Pollock (ed*.), Common denominators in art and science: the proceedings of a discussion conference held under the auspices of the School of Epistemics, University of Edinburgh, November 1981* (Aberdeen: Aberdeen University Press)

Kern, Stephen (2003): *The culture of time and space, 1880-1918: with a new preface* (Cambridge, MA: Harvard University Press)

Kevles, Daniel J. (1995): *The physicists: the history of a scientific community in modern America* (Cambridge, MA: Harvard University Press)

Kim, Dong-Won (2002): *Leadership and creativity: a history of the Cavendish Laboratory, 1871-1919* (Dordrecht; London: Kluwer Academic Publishers)

Knight, Donald R., and Alan D. Sabey (1984): *The Lion Roars at Wembley: British Empire Exhibition, 60th Anniversary 1924-1925* (New Barnet: D. R. Knight)

Knight, David M. (2006): *Public Understanding of Science: A History of Communicating Scientific Ideas* (London: Taylor & Francis)

Knox, Kevin C., and Richard Noakes (2003): *From Newton to Hawking: A History of Cambridge University’s Lucasian Professors of Mathematics* (Cambridge: Cambridge University Press)

Knudsen, Ole (2004): 'O. W. Richardson and the Electron Theory of Matter', in. Jed Z. Buchwald and Andrew Warwick (eds.), *Histories of the Electron: The birth of microphysics* (London: MIT Press): 227-253

Kostecka, K. (2011): *Morris William Travers - a Lifetime of Achievement* (Xlibris Corporation)

Kragh, Helge (1990): *Dirac: A Scientific Biography* (Cambridge: Cambridge University Press)

Kragh, Helge (1999): *Quantum Generations* (Princeton: Princeton University Press)

Kragh, Helge (2002): 'The Vortex Atom: A Victorian Theory of Everything', *Centaurus* 44: 32-144

Kragh, Helge (2011): 'Resisting the Bohr atom: The early British opposition', *Physics in Perspective* 13: 4-35

Kuhn, Thomas S. (1970): *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press)

Kuhn, Thomas S. (1978): *Black-Body Theory and the Quantum Discontinuity, 1894-1912* (Oxford: Oxford University Press)

Kurti, N. (1958): 'Franz Eugen Simon. 1893-1956', *Biographical Memoirs of Fellows of the Royal Society* 4: 224-256

LaFollette, Marcel C. (1990): *Making Science Our Own: Public Images of Science 1910 – 1955* (Chicago: University of Chicago Press)

Latour, Bruno (1987): *Science in Action: How to follow scientists and engineers through society* (Cambridge, MA: Harvard University Press)

Latour, Bruno, and Steve Woolgar (1986): *Laboratory Life: The Construction of Scientific Facts* (Princeton: Princeton University Press)

Lavine, Matthew (2008): *A Cultural History of Radiation and Radioactivity in the United States, 1895-1945* PhD (University of Wisconsin-Madison)

Lelong, Benoit (2005): 'Translating Ion Physics from Cambridge to Oxford: John Townsend and the Electrical Laboratory, 1900-24', in Robert Fox and Graeme Gooday (eds.), *Physics in Oxford 1839 - 1939* (Oxford: Oxford University Press): 209-232

Lewis, John L. (1999): *125 years: the Physical Society and the Institute of Physics* (Institute of Physics Pub.)

MacLeod, Glen (1999): 'The Visual Arts', in Michael Levenson (ed.), *The Cambridge Companion to Modernism* (Cambridge: Cambridge University Press): 194 – 216

Macleod, Christine, and Jennifer Tann (2007): 'From engineer to scientist: reinventing invention in the Watt and Faraday centenaries, 1919-31', *The British Journal for the History of Science* 40(3): 389-411.

Magnello, Eileen (2000): *A century of measurement: an illustrated history of the National Physical Laboratory* (Bath: Canopus)

Malley, Marjorie (1979): 'The Discovery of Atomic Transmutation: Scientific Styles and Philosophies in France and Britain', *Isis* 70(2): 213-223

Mazda, Xerxes (1996): *The changing role of history in the policy and collections of the Science Museum, 1857-1973* MA (London Centre for the History of Science, Technology and Medicine)

McCormmach, Russell (1966): 'The atomic theory of John William Nicholson', *Archive for History of Exact Sciences*, 3(2): 160-184.

McCormmach, Russell (1991): *Night Thoughts of a Classical Physicist* (Cambridge, MA: Harvard University Press)

McGucken, William (1984): *Scientists, Society, and State* (Colombus: Ohio State University Press)

McQuail, Denis (1987): *Mass Communication Theory: An Introduction* (London: Sage)

Mendelssohn, Kurt (1977): *The Quest for Absolute Zero: the meaning of low temperature physics* (London: Taylor & Francis)

Merricks, Linda (1996): *The World Made New: Frederick Soddy, Science, Politics, and Environment* (Oxford: Oxford University Press)

Miller, Arthur I. (2002): *Einstein, Picasso: space, time and the beauty that causes havoc* (New York: Basic Books)

Milne, E. A. (1941): 'Augustus Edward Hough Love. 1863-1940', *Obituary Notices of Fellows of the Royal Society* 3(9): 467-482

Milne, E. A. (1952): *Sir James Jeans: A Biography* (Cambridge: University Press)

Milner, S. R. (1935): 'William Mitchinson Hicks. 1850-1934', *Obituary Notices of Fellows of the Royal Society* 4: 393-399

Moore, H. (1938): 'Sir Herbert Jackson. 1863-1936', *Obituary Notices of Fellows of the Royal Society* 2(6): 307-314

Morrell, Jack (1992): 'Research in Physics at the Clarendon Laboratory, Oxford, 1919-1939', *Historical Studies in the Physical and Biological Sciences* 22 (2): 263-307

Morrell, Jack (2005): 'The Lindemann Era', in R. Fox and G. Gooday (eds.), *Physics in Oxford 1839 – 1939* (Oxford: Oxford University Press): 233-266

Morris, Peter J. T. (ed.) (2010): *Science for the Nation: Perspectives on the History of the Science Museum* (Basingstoke: Palgrave Macmillan)

Morris, Peter J. T. (2010b): ''An Effective Organ of Public Enlightenment': The Role of Temporary Exhibitions in the Science Museum', in Peter J. T. Morris (ed.), *Science for the Nation: Perspectives on the History of the Science Museum* (Basingstoke: Palgrave Macmillan): 212-249.

Morrisson, Mark S. (2002): 'Why Modernist Studies and Science Studies Need Each Other', *Modernism/modernity* 9(4): 675-82

Morus, Iwan Rhys (2005): *When Physics Became King* (Chicago: University of Chicago Press)

Morton, Alan Q. (2000): 'The Electron Made Public: The Exhibition of Pure Science in the British Empire Exhibiition, 1924-5', in. Bernard Finn, Robert Bud and Helmuth Trischler (eds.), *Exposing Electronics* (Amsterdam: Harwood Academic): 25-44

Moseley, Russell (1977): 'Tadpoles and Frogs: Some Aspects of the Professionalization of British Physics, 1870-1939', *Social Studies of Science* 7(4): 423-446

Moseley, Russell (1978): 'The origins and early years of the National Physical Laboratory: A chapter in the pre-history of British science policy', *Minerva* 16(2): 222-250

Mott, Nevill F. and C. F. Powell (1962): 'Arthur Mannering Tyndall. 1881-1961', *Biographical Memoirs of Fellows of the Royal Society* 8: 159-165

Navarro, Jaume (2005): 'J. J. Thomson on the Nature of Matter: Corpuscles and the Continuum', *Centaurus* 47(4): 259-282

Navarro, Jaume (2009): ‘’A dedicated missionary’. Charles Galton Darwin and the new quantum mechanics in Britain’, *Studies in the History and Philosophy of Modern Physics* 40: 316–326

Needell, Allan (1988): ‘Introduction’, in Max Planck, *The Theory of Heat Radiation* (Los Angelos; New York; Tomash: American Institute of Physics): xi–xlv

Noakes, Richard (2005): ‘Ethers, Religion and Politics in Late-Victorian Physics: Beyond the Wynne Thesis’, *History of Science* 43: 415-455

Noakes, Richard (2008a): 'The ‘world of the infinitely little’: connecting physical and psychical realities circa 1900', *Studies In History and Philosophy of Science Part A* 39(3): 323-334

Noakes, Richard (2008b): ‘The historiography of psychical research: lessons from histories of the sciences’, *Journal of the Society for Psychical Research* 72(2): 1-20

Nye, Mary Jo (1996): *Before Big Science: The Pursuit of Modern Chemistry and Physics, 1800-1940* (New York: Twayne Publishers)

Oliphant, M. (1972): *Rutherford Recollections of the Cambridge Days* (Amsterdam; New York: Elsevier)

Oppenheim, Janet (1988): *The Other World: Spiritualism and Psychical Research in England, 1850-1914* (Cambridge: Cambridge University Press)

Peters, Hans Peter (2008): 'Scientists as Public Experts', in. Massimiano Bucchi and Brian Trench (eds.), *Handbook of Public Communication of Science and Technology* (Abingdon: Routledge): 131-146

Pickering, Andrew (ed.) (1992) *Science as Practice and Culture* (Chicago: University of Chicago Press)

Pike, William S. (2000): ‘Meteorologists Profile: Leo Claude Wallace Bonacina’, *Weather* 55: 45-53

Price, Katy (2005): ‘Flame far too hot: William Empson's non-Euclidean predicament’, *Interdisciplinary Science Reviews* 30(4): 312-322

Price, Katy (2008): ‘On the Back of the Light Waves: ‘Novel Possibilities in the “Fourth Dimension”’, in Sharon Ruston (ed.), *Literature and Science* (Woodbridge: Boydell & Brewer): 91-110

Price, Katy (2012): *Loving Faster than Light: Romance and Readers in Einstein’s Universe* (Chicago: University of Chicago Press)

Pursell, Caroll (1974): ''A Savage Struck by Lightning’: The Idea of a Research Moratorium, 1927-1937', *Lex et Scientia* 10: 146-158

Pyatt, Edward C., and Paul Dean (1983): *The National Physical Laboratory: a history* (Bristol: A. Hilger)

Rankine, A. O. (1952): ‘Prof. Allan Ferguson’, *Nature* 169 (5 January): 14-15

Rayleigh and F. J. Selby (1936): 'Richard Tetley Glazebrook. 1854-1935', *Obituary Notices of Fellows of the Royal Society* 2(5): 29-56

Rieger, Bernhard (2005): *Technology and the Culture of Modernity in Britain and Germany, 1890-1945* (Cambridge: Cambridge University Press)

Robinson, H. R. (1954): ‘Rutherford: Life and Work to the Year 1919, with Personal Reminisces of the Manchester Period’, in *Rutherford: by those who knew him, being the collection of the first five Rutherford lectures of the Physical Society* (London: Physical Society)

Root. John D. (1978): 'Science, Religion, and Psychical Research: The Monistic Thought of Sir Oliver Lodge', *The Harvard Theological Review* 71(3/4): 245-263

Rowlands, Peter (1990): *Oliver Lodge and the Liverpool Physical Society* (Liverpool: Liverpool University Press)

Rudwick, Martin S. J. (1985), *The Great Devonian Controversy: The Shaping of Scientific Knowledge among Gentlemanly Specialists* (Chicago: University of Chicago Press)

Ryan, M. (1992): ‘Gender and public access: women’s politics in nineteenth-century America’, in C. Calhoun (ed.), *Habermas and the Public Sphere* (Cambridge, MA: MIT Press): 258-88

Savage, J. P. (1979): *British Physics and Causality, 1918-27* M.Sc. (University of Manchester)

Schaffer, Simon (1992): ‘Late Victorian Metrology and Its Instrumentation: A Manufactory of Ohms’, in Bud and Cozzens (eds.), *Invisible Connections: Instruments, Institutions, and Science* (Bellingham: SPIE)

Shaw, H. (1945): ‘Mr. E. Lancaster-Jones’, *Nature* 156 (13 October): 442-443

Scheinfeldt, Tom (2010): 'The First Years: The Science Museum at War and Peace', in. Peter J. T. Morris. (ed.), *Science for the Nation: Perspectives on the History of the Science Museum* (Basingstoke: Palgrave Macmillan): 41-60

Secord, James (1986): *Controversy in Victorian Geology: The Cambrian-Silurian Dispute* (Perinceton: Princeton University Press)

Servos, John W. (1990): *Physical Chemistry from Ostwald to Pauling: The Making of a Science in America* (Princeton: Princeton University Press)

Shapin, Steven, and Simon Schaffer (1985): *Leviathan and the Air-Pump* (Princeton: Princeton University Press)

Shapin, Steven (1988): 'Following Scientists Around', *Social Studies of Science* 18(3): 533-550

Shapin, Steven (1994): *A Social History of Truth: Civility and Science in Seventeenth-Century England* (Chicago: University of Chicago Press)

Silliman, Robert H. (1974): 'Fresnel and the Emergence of Physics as a Discipline', *Historical Studies in the Physical Sciences* 4: 137-162

Simpson, R. (1983): *How the PhD came to Britain: a century of struggle for postgraduate education* (Guildford: Society for Research into Higher Education)

Sinclair, S. B. (1987): ‘J. J. Thomson and the Chemical Atom: From Ether Vortex to Atomic Decay’, *Ambix* 34(2): 89-116

Sinclair, S. B. (1988a): 'J. J. Thomson and Radioactivity: Part I', *Ambix* 35(2): 91-104

Sinclair, S. B. (1988b): 'J.J. Thomson and radioactivity: Part II', *Ambix* 35(3): 113-126

Smith, Crosbie (1998): *The Science of Energy: A Cultural History of Energy Physics in Victorian Britain* (London: The Athlone Press)

Smith, Crosbie, and M. Norton Wise (1989): *Energy and Empire: A Biographical Study of Lord Kelvin* (Cambridge: Cambridge University Press)

Smith, E. E. (1975): *Radiation Science at the National Physical Laboratory 1912-1955* (London: National Physical Laboratory)

Smith, George E. (2001): ‘J. J. Thomson and the Electron, 1897-1899’, in Jed Z. Buchwald and Andrew Warwick (eds.), *Histories of the Electron: The birth of microphysics* (Cambridge, MA: MIT Press): 21-76.

Sponsel, Alistair (2002): 'Constructing a ‘revolution in science’: the campaign to promote a favourable reception for the 1919 solar eclipse experiments', *British Journal for the History of Science* 35(4): 439-467

Staley, Richard (2005): 'On the Co-Creation of Classical and Modern Physics’, *Isis* 96: 530-558

Staley, Richard (2008a): *Einstein's Generation: The Origins of the Relativity Revolution* (Chicago: University of Chicago Press)

Staley, Richard (2008b): 'The Fin de Siècle Thesis', *Berichte zur Wissenschaftsgeschichte* 31(4): 311-330

Staley, Richard (2008c): 'Worldviews and physicists' experience of disciplinary change: on the uses of 'classical' physics', *Studies In History and Philosophy of Science Part A* 39(3): 298-311

Stanley, Matthew (2003): 'An Expedition to Heal the Wounds of War": The 1919 Eclipse and Eddington as Quaker Adventurer', *Isis* 94(1): 57-89

Stanley, Matthew (2007a): *Practical Mystic* (Chicago: University of Chicago)

Stanley, Matthew (2007b): 'So simple a thing as a star: the Eddington-Jeans debate over astrophysical phenomenology', *British Journal for the History of Science* 40(1): 53-82

Stuewer, Roger H. (1986): 'Rutherford's Satellite Model of the Nucleus', *Historical Studies in the Physical and Biological Sciences* 16(2): 321-352

Sturdy, Steve (ed.) (2002): Medicine, Health and the Public Sphere in Britain, 1600-2000 (London: Routledge)

Sviedrys, Romualdas (1976): 'The Rise of Physics Laboratories in Britain', *Historical Studies in the Physical Sciences* 7: 405-436

Sviedrys, Romualdas, and Arnold Thackray (1970): 'The Rise of Physical Science at Victorian Cambridge with Commentary and with Reply', *Historical Studies in the Physical Sciences* 2: 127-151

Thomas, H. Hamshaw (1954), 'Robert Stewart Whipple ', *Bulletin of the British Society for the History of Science* 1(10): 249-250

Thomas, J. S. G. (1949): 'Samuel Walter Johnson Smith. 1871-1948', *Obituary Notices of Fellows of the Royal Society* 6(18): 579-598

Thomson, George P. (1956): 'Alexander Oliver Rankine. 1881-1956', *Biographical Memoirs of Fellows of the Royal Society* 2: 248-255

Thomson, George P. (1963): 'Charles Galton Darwin, 1887–1962', *Biographical Memoirs of Fellows of the Royal Society* 9: 69–85

Titchmarsh, E. C. (1958): 'George Barker Jeffery. 1891-1957', *Biographical Memoirs of Fellows of the Royal Society* 4: 128-137

Tiffen, Herbert J. (1935): *A History of the Liverpool Institute Schools, 1825-1935* (Liverpool: Liverpool Institute Old Boys' Association)

Tobey, Ronald C. (1971): *The American Ideology of National Science, 1919-1930* (Pittsburgh: University of Pittsburgh Press)

Travers, M.W. (1956): *A life of Sir William Ramsay, K.C.B., F.R.S* (E. Arnold)

Trench, Brian (2008): 'Towards an analytical framework of science communication models', in. D. Cheng and M. Claessens (ed.), *Communicating Science in Social Contexts: New models, new practices* (Dordrecht; London: Springer): 119-135

D., W. C. D. (1932): 'Ernest Howard Griffiths. 1851-1932', *Obituary Notices of Fellows of the Royal Society* 1(1): 15-18

Ward, F. A. B. (1950): 'Dr. Herman Shaw', *Nature* 165 (10 June): 916-7

Ward, F. A. B. (1987): 'Physics in Cambridge in the Late 1920s', in Rajkumari Williamson (ed.), *The Making of Physicists* (Bristol: Adam Hilger): 77-85

Warwick, Andrew (1991): 'On the role of the FitzGerald-Lorentz contraction hypothesis in the development of Joseph Larmor's electronic theory of matter', *Archive for History of Exact Sciences* 43(1): 29-91

Warwick, Andrew (1992): 'Cambridge Mathematics and Cavendish Physics: Cunningham, Campbell and Einstein's Relativity 1905 - 1911 / Part I: The Uses of Theory', *Studies in History and Philosophy of Science* 23(4): 625-656

Warwick, Andrew (1993a): 'Cambridge Mathematics and Cavendish Physics: Cunningham, Campbell and Einstein's Relativity 1905 - 1911 / Part II: Comparing Traditions in Cambridge Physics', *Studies in History and Philosophy of Science* 24(1): 1-25

Warwick, Andrew (1993b): 'Frequency, Theorem and Formula: Remembering Joseph Larmor in Electromagnetic Theory', *Notes and Records of the Royal Society of London* 47(1): 49-60

Warwick, Andrew (2003): *Masters of Theory: Cambridge and the Rise of Mathematical Physics* (Chicago: University of Chicago Press)

Watson, Katherine D. (2002): ''Temporary Hotel Accommodation'? The Early History of the Davy-Faraday Research Laboratory, 1894-1823', in. Frank A. J. L. James. (ed.), *'The Common Purposes of Life': Science and society at the Royal Institution of Great Britain* (Aldershot: Ashgate): 191-223

Wheaton, Bruce R. (1983): *The Tiger and the Shark: Empirical Roots of Wave-Particle Dualism* (Cambridge: Cambridge University Press)

Whitworth, Michael (1996): 'The Clothbound Universe: Popular Physics Books, 1919-39', *Publishing History* 40: 55-82

Whitworth, Michael (2001): *Einstein's Wake: Relativity, Metaphor, and Modernist Literature* (Oxford: Oxford University Press)

Williams, Francis (1957): *Dangerous Estate: The Anatomy of Newspapers* (London: Longmans, Green and Co)

Wilson, David B. (1971): 'The Thought of Late Victorian Physicists: Oliver Lodge's Ethereal Body', *Victorian Studies* 15(1): 29-48

Wilson, David B. (1982): 'Experimentalists among the Mathematicians: Physics in the Cambridge Natural Sciences Tripos, 1851-1900', *Historical Studies in the Physical Sciences* 12(2): 325-371

Wilson, David B. (1983): *Rutherford: simple genius* (London: Hodder and Stoughton)

Wilson, Wm (1953): 'Charles Herbert Lees. 1864-1952', *Obituary Notices of Fellows of the Royal Society* 8(22): 523-528

Wilson, Wm (1956): ‘John William Nicholson. 1881-1955’, *Biographical Memoirs of Fellows of the Royal Society* 2: 209-214.

Witham, Larry (2005): *The Measure of God: our century-long struggle to reconcile science & religion* (San Francisco: Harper)

Wohl, Robert (2002): ‘Heart of Darkness: Modernism and its historians’, *Journal of Modern History* 74: 573-621

Wynne, Brian (1976): ‘C.G. Barkla and the J Phenomenon: A Case Study in the Treatment of Deviance in Physics’, *Social Studies of Science* 6: 307-47

Wynne, Brian (1979): ‘Physics and Psychics: Science, Symbolic Action, and Social Control in Late Victorian England’, in Barry Barnes and Steven Shapin (eds.), *Natural Order: Historical Studies of Scientific Culture (*London: Sage): 167-86