The Gatekeepers of Modern Physics

Periodicals and Peer Review in 1920s Britain

*By Imogen Clarke**

ABSTRACT

This essay analyzes the processes behind the publication of physics papers in two British journals in the 1920s: the *Proceedings of the Royal Society of London: Series A* and the *Philosophical Magazine*. On the surface, it looked as though the *Philosophical Magazine* was managed very informally, while the *Proceedings* had in place a seemingly rigid system of committee approval and peer review. This essay shows, however, that in practice the two journals were both influenced by networks of expertise that afforded small groups of physicists considerable control over the content of these prestigious scientific publications. This study explores the nature of peer review, suggesting how a historical approach can contribute to contemporary debates. In studying these relationships, the essay also considers the interplay of "classical" and "modern" ideas and physicists in 1920s Britain and cautions against an anachronistic approach to this classification.

"IMAGINE A WORLD without peer review."[1] This apparently straightforward suggestion, posed by Fiona Godlee and Tom Jefferson in their introduction to *Peer Review in the Health Sciences*, is far from simple to execute. The practice has become such a ubiquitous part of scientific funding and publishing, and a basis for much of science's claims to objectivity, that it is difficult to separate from the idea of modern science itself. And yet peer review has only recently been subject to careful examination, much of it critical.[2] A 2002 review of published studies into the effects of editorial peer-review processes in biomedical journals concluded that the practice "should be regarded as an

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[1] Fiona Godlee and Tom Jefferson, eds., *Peer Review in Health Sciences*, 2nd ed. (London: Wiley, 2003), p. xiii.

[2] This critical approach to peer review can be seen in many of the articles cited below. The International Congress on Peer Review and Biomedical Publications is now in its seventh year.

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untested process with uncertain outcomes." In 2006, the journal *Nature* featured a web debate on peer review, and more recently Richard Smith, former editor of the *British Medical Journal*, has proposed a number of drastic changes.[3] The 2009 "Climategate" scandal brought many of the potential problems with peer review to the attention of a wider nonscientific public, revealing that a small number of climate scientists had been discussing their desires to reject papers presenting views they disagreed with and planning to boycott a journal edited by a "well known sceptic." Godlee and Jefferson suggest that peer review is "a process with so many flaws that it is only the lack of an obvious alternative that keeps the process going."[4]

Given the increasing interest in its current usage, there has been surprisingly little attention paid to the historical development of peer review.[5] This is in spite of a vast literature on the development of popular and professional scientific publications.[6] Peer review is certainly ripe for this kind of analysis, particularly following Adrian Johns's exposure of the contingency of what we regard as essential elements of print. In a study of the early history of the Royal Society's *Philosophical Transactions*, Johns argues that

[3] T. Jefferson, P. Alderson, E. Wager, and F. Davidoff, "Effects of Editorial Peer Review: A Systematic Review," *Journal of the American Medical Association*, 2002, *287*:2784–2786, on p. 2785; "*Nature*'s Peer Review Debate," http://www.nature.com/nature/peerreview/debate/index.html, 2013 (accessed 1 Sept. 2013); and R. W. Smith, "In Search of an Optimal Peer Review System," *Journal of Participatory Medicine*, 2009, *1*. In addition to removing the anonymity of reviewers, Smith argues that peer review should take place in an open online environment that would allow anybody to read and comment on both the paper and the reviews.

[4] House of Commons Science and Technology Committee, *The Reviews into the Univ. of East Anglia's Climatic Research Unit's E-mails: First Report of Session 2010–11* (London: Stationery Office, 25 Jan. 2011), p. 25; and Godlee and Jefferson, eds., *Peer Review in Health Sciences* (cit. n. 1), p. xiii.

[5] John C. Burnham, "The Evolution of Editorial Peer Review," *J. Amer. Med. Assoc.*, 1990, *263*:1323–1329; Burnham, "How Journal Editors Came to Develop and Critique Peer Review Procedures," in *Research Ethics, Manuscript Review, and Journal Quality*, ed. Henry R. Mayland and R. E. Sojka (Madison, Wis.: American Society of Agronomy, Crop Science Society of America, and Soil Science Society of America, 1992), pp. 55–62; Drummond Rennie, "Editorial Peer Review: Its Development and Rationale," in *Peer Review in Health Sciences*, ed. Godlee and Jefferson, pp. 3–13; and Mario Biagioli, "From Book Censorship to Academic Peer Review," *Emergence*, 2002, *12*:11–45. This limited literature is, however, shortly to be enhanced, with Melinda Baldwin's upcoming research on peer review and the Arts and Humanities Research Council project "Publishing the *Philosophical Transactions*."

[6] Ruth Barton, "Just before *Nature*: The Purposes of Science and the Purposes of Popularization in Some English Popular Science Journals of the 1860s," *Annals of Science*, 1998, *55*:1–33; Aileen Fyfe, *Science and Salvation: Evangelical Popular Science Publishing in Victorian Britain* (Chicago: Univ. Chicago Press, 2004); Bernard Lightman, *Victorian Popularizers of Science: Designing Nature for New Audiences* (Chicago: Univ. Chicago Press, 2007); James A. Secord, *Victorian Sensation: The Extraordinary Publication, Reception, and Secret Authorship of* Vestiges of the Natural History of Creation (Chicago: Univ. Chicago Press, 2000); G. N. Cantor, ed., *Science in the Nineteenth-Century Periodical: Reading the Magazine of Nature* (Cambridge: Cambridge Univ. Press, 2004); Cantor and Sally Shuttleworth, eds., *Science Serialized: Representations of the Sciences in Nineteenth-Century Periodicals* (Cambridge, Mass.: MIT Press, 2004); A. J. Meadows, *Communication in Science* (London: Butterworths, 1974), Ch. 3; Meadows, ed., *Development of Science Publishing in Europe* (New York: Elsevier, 1980); Thomas Broman, "Periodical Literature," in *Books and the Sciences in History*, ed. Marina Frasca-Spada and Nicholas Jardine (Cambridge: Cambridge Univ. Press, 2000), pp. 225–238; David McKitterick, ed., *The Cambridge History of the Book in Britain*, Vol. 6: *1830–1914* (Cambridge: Cambridge Univ. Press, 2009), pp. 443–474; Alex Csiszar, "Seriality and the Search for Order: Scientific Print and Its Problems during the Late Nineteenth Century," *History of Science*, 2010, *48*:399–434; and Csiszar, "Broken Pieces of Fact: The Scientific Periodical and the Politics of Search in Nineteenth-Century France and Britain" (Ph.D. diss., Harvard Univ., 2010). There is very little work on science publishing in the twentieth century, but see Peter Bowler, *Science for All: The Popularization of Science in Early Twentieth-Century Britain* (Chicago: Univ. Chicago Press, 2009). Brock and Meadows's history of Taylor & Francis continues into the twentieth century, as does Atkinson's brief overview of the development of the *Philosophical Transactions of the Royal Society*. See William H. Brock and Meadows, *The Lamp of Learning: Two Centuries of Publishing at Taylor & Francis* (London: Taylor & Francis, 1998); and Dwight Atkinson, *Scientific Discourse in Sociohistorical Context: The* Philosophical Transactions of the Royal Society of London, *1675–1975* (Mahwah, N.J.: Erlbaum, 1999), Ch. 2.

periodicals are not an inevitable feature of science.[7] Neither, it would seem, is peer review an inevitable feature of periodicals. While the Royal Society established a form of peer review as early as 1752, when it set up a Committee of Papers to manage the *Philosophical Transactions*, the practice did not become widespread until after World War II, and John Burnham has found no clear pattern in its development.[8] Melinda Baldwin's analysis of *Nature*'s "Letters to the Editor" section reveals the benefits for 1920s physicists of a much faster system of publication than is allowed for by the lengthy peer-review process.[9] Historians can contribute to contemporary discussions of peer review by examining the variety of ways in which publication decisions were made in the early twentieth century, a period in which our current system of peer review was by no means the only option.

In this essay, I look at such decisions made by those responsible for managing two physics journals in the 1920s: the *Proceedings of the Royal Society of London: Series A* and the commercially owned *Philosophical Magazine*. The Royal Society had a large bureaucratic system in place, with papers approved by a committee and sent for review, but in practice the management of the *Proceedings* functioned in a manner very similar to that of the *Philosophical Magazine*, where publication decisions were in the hands of individual editors. In both cases small networks of appointed experts were afforded the authority to judge the work of their peers. These "gatekeepers" were alone responsible for deciding the content of two prestigious scientific journals, at a time when the discipline was undergoing dramatic changes.[10]

By examining the roles these networks played, I reconsider how we categorize the discipline and its practitioners during this period. The developments of the late nineteenth and early twentieth centuries have retrospectively been characterized as a shift from "classical" to "modern" physics.[11] The use of these terms is somewhat anachronistic, however, and recent scholarship has explored their development, revealing that these categories were under negotiation during the early twentieth century, with no clear distinction in place even by the 1930s.[12] The perceived dichotomy has nonetheless resulted

[7] Adrian Johns, *The Nature of the Book: Print and Knowledge in the Making* (Chicago: Univ. Chicago Press, 1998); and Johns, "Miscellaneous Methods: Authors, Societies, and Journals in Early Modern England," *British Journal for the History of Science*, 2000, *33*:159–186.

[8] Atkinson, *Scientific Discourse in Sociohistorical Context* (cit. n. 6), Ch. 2; and Burnham, "Evolution of Editorial Peer Review" (cit. n. 5). Burnham argues that widespread adoption was the result of the increasing specialization and quantity of submissions and notes that this developed independently from grant peer review.

[9] Melinda Baldwin, "'Keeping in the Race': Physics, Publication Speed, and National Publishing Strategies in *Nature*, 1895–1939," *Brit. J. Hist. Sci.*, 2014, *47*:257–279. See also Baldwin, "The Successors to the X Club? Late Victorian Naturalists and *Nature*, 1869–1900," in *Victorian Scientific Naturalism: Community, Identity, Continuity*, ed. Bernard Lightman and Gowan Dawson (Chicago: Univ. Chicago Press, 2014), pp. 288–308.

[10] K. Barzilai-Nahon, "Gatekeeping: A Critical Review," *Annual Review of Information Science and Technology*, 2009, *43*:1–79. The concept of gatekeeping appears frequently in contemporary peer-review discussions. See, e.g., M. Hojat, J. S. Gonnella, and A. S. Caelleigh, "Impartial Judgment by the 'Gatekeepers' of Science: Fallibility and Accountability in the Peer Review Process," *Advances in Health Sciences Education*, 2003, *8*:75–96; and M. B. Kovera and B. D. McAuliff, "The Effects of Peer Review and Evidence Quality on Judge Evaluations of Psychological Science: Are Judges Effective Gatekeepers?" *Journal of Applied Psychology*, 2000, *85*:574–586.

[11] See, e.g., Helge Kragh, *Quantum Generations* (Princeton, N.J.: Princeton Univ. Press, 1999); David M. Knight, *Public Understanding of Science: A History of Communicating Scientific Ideas* (London: Taylor & Francis, 2006), Ch. 12; and Laurie M. Brown, Abraham Pais, and Brian Pippard, *Twentieth Century Physics*, Vol. 1 (Bristol: Institute of Physics Publishing; New York: American Institute of Physics Press, 1995).

[12] Richard Staley, "On the Co-Creation of Classical and Modern Physics," *Isis*, 2005, *96*:530–558; Staley, *Einstein's Generation: The Origins of the Relativity Revolution* (Chicago: Univ. Chicago Press, 2008); Imogen Clarke, "Negotiating Progress: Promoting 'Modern' Physics in Britain, 1900–1940" (Ph.D. diss., Univ. Manchester, 2012); and Graeme Gooday and Daniel Mitchell, "Rethinking 'Classical Physics,'' in *Oxford Handbook of the History of Physics*, ed. Jed Z. Buchwald and Robert Fox (Oxford: Oxford Univ. Press, 2013), pp. 721–764.

in an approach to the history of early twentieth-century physics that situates developments in relation to the rise of modern physics as it has come to be defined.[13] Physicists—and institutions—that do not fit into this narrative are often retrospectively labeled as classical and out of date, but they were not necessarily perceived as such at the time. The physicists responsible for managing the *Proceedings* and the *Philosophical Magazine* in the 1920s were in the process of establishing the relations between old and new traditions in their discipline. In this essay, I focus in particular on the editorial role of two physicists whom we might now define as classical and modern, respectively: Oliver Lodge and James Jeans.[14] An examination of the processes behind publication decisions helps to uncover the heterogeneous approaches to physics and scientific progress in the 1920s. This case study of early twentieth-century physics provides insight into the negotiations under way when a discipline is undergoing rapid change, highlighting how the past is reconciled with the present. With these issues for physicists playing out in two journals with very different styles of management, we can see the role of networks in attempts to control the textual output of a discipline, with or without peer review.

I begin with an overview of the publishing landscape for physicists in the 1920s, paying particular attention to the structure and status of the *Philosophical Magazine*, of which Oliver Lodge was an editor. I then examine the organizational structure of the Royal Society's Physical Committee and the role of the Physical Secretary, James Jeans. I trace the different routes taken by papers submitted to the Royal Society and, considering the roles played by select communicators and referees, explore the influence of physicists with leanings toward modern theories and approaches. Following this, I analyze two case studies, papers by Richard Hargreaves and Joseph Larmor, a mathematician and a mathematical physicist, respectively, and explore the criteria against which these papers were judged by both journals. In addition to the referees interpreting these papers in relation to their own approaches to physics, and assigning them value accordingly, publication decisions were also based on the relative prestige of the author. This essay explores how these sometimes competing influences directed the content of the *Proceedings* and the *Philosophical Magazine*.

[13] See, e.g., Jed Z. Buchwald, *From Maxwell to Microphysics: Aspects of Electromagnetic Theory in the Last Quarter of the Nineteenth Century* (Chicago: Univ. Chicago Press, 1988); Buchwald and Andrew Warwick, eds., *Histories of the Electron: The Birth of Microphysics* (Cambridge, Mass.: MIT Press, 2001); and Olivier Darrigol, *Electrodynamics from Ampère to Einstein* (Oxford: Oxford Univ. Press, 2000). In a development closely related to the emphasis on the modern, the historiography has also been affected by what Hughes terms "bomb historiography," in which the physics of the early twentieth century is reinterpreted and emphasized in relation to its use in the war effort; see Jeff Hughes, "Radioactivity and Nuclear Physics," in *The Cambridge History of Science*, Vol. 5: *The Modern Physical and Mathematical Sciences*, ed. Mary Jo Nye (Cambridge: Cambridge Univ. Press, 2002), pp. 350–374, on p. 351. For a recent overview of changing historiographies of physics see Richard Staley, "Trajectories in the History and Historiography of Physics in the Twentieth Century," *Hist. Sci.*, 2013, *51*:151–177.

[14] For Lodge see Bruce J. Hunt, *The Maxwellians* (Ithaca, N.Y.: Cornell Univ. Press, 1991); Peter Rowlands, *Oliver Lodge and the Liverpool Physical Society* (Liverpool: Liverpool Univ. Press, 1990); and David B. Wilson, "The Thought of Late Victorian Physicists: Oliver Lodge's Ethereal Body," *Victorian Studies*, 1971, *15*:29–48. For Jeans see E. A. Milne, *Sir James Jeans: A Biography* (Cambridge: Cambridge Univ. Press, 1952); Rob Hudson, "James Jeans and Radiation Theory," *Studies in History and Philosophy of Science, Part A*, 1989, *20*:57–76; and Helge Kragh, "Resisting the Bohr Atom: The Early British Opposition," *Physics in Perspective*, 2011, *13*:4–35. Jeans has retrospectively been labeled a "classical physicist," but, as Staley has pointed out, he is unlikely to have been viewed as such by his contemporaries. See Staley, "On the Co-Creation of Classical and Modern Physics" (cit. n. 12), p. 552.

THE *PHILOSOPHICAL MAGAZINE* AND THE LANDSCAPE OF PERIODICAL PUBLISHING IN 1920S BRITAIN

What options were available in the 1920s to a British physicist seeking to publish his work? There were a number of highly specialized journals for specific purposes: for example, *Transactions of the Faraday Society* published physics with a chemical leaning, the *Journal of the Röntgen Society* provided a home for research into X-rays, and the *Journal of Scientific Instruments* was founded by the Institute of Physics in 1923. Of the more general physics journals, the *Philosophical Transactions of the Royal Society* was the oldest, having been first published in 1665. This was split into two separate journals in 1887, with Series A publishing mathematical and physical papers. In 1832 the Royal Society established a second journal, its *Proceedings* (split into Sections A and B in 1905), initially intended as a medium for Society news and abstracts of papers published in the *Transactions*. By the twentieth century this had become a journal in its own right, with the two publications differentiated mostly on the basis of the length of the submissions. The *Transactions* was now the choice for longer papers, often with elaborate illustrations, and its longer times to publication and less frequent issues gradually turned it into the Society's secondary publication for physicists. Beyond the Royal Society, and for a wider audience and faster turnaround, *Nature* was a popular destination, and a letter published there could quickly generate further debate.[15]

Scope for discussion was also a factor in choosing the *Proceedings of the Physical Society of London*, where all contributions were required to be read out in a meeting. Established by Frederick Guthrie in 1873, the Physical Society and its journal were intended to serve as a home for the type of physics research rejected by the Royal Society's publications but relevant to many in the physical community, including those in Guthrie's laboratory at the Royal School of Mines.[16] London institutions such as this have been somewhat overlooked in the historiography of early twentieth-century physics, with Cambridge and its prestigious Cavendish Laboratory taking a central role. As a result, British modern physics has to some extent been equated with microphysics, arising from the regime of experimental explorations into the particulate structure of matter established by J. J. Thomson. However, recent scholarship has revealed the nature of Cambridge physics to have been far more varied than earlier characterizations suggest.[17] In addition to this, institutions and contexts outside of Cambridge have

[15] For an overview of the development of Royal Society journals see Atkinson, *Scientific Discourse in Sociohistorical Context* (cit. n. 6). Jeans noted that a paper in the *Proceedings* would "secure more rapid publication and much better publicity" than one in the *Transactions*: J. H. Jeans to O. W. Richardson, 27 Oct. 1922, Papers of O. W. Richardson, Archive for the History of Quantum Physics (microfilm) (hereafter cited as **Richardson Papers**). In 1925, William Lawrence Bragg explicitly requested that a paper he had cowritten, "The Crystalline Structure of Chrysoberyl," appear in the *Proceedings* and not the *Transactions*: Jeans to Richardson, 12 Nov. 1925, Richardson Papers. On the advantages of publishing in *Nature* see Baldwin, "'Keeping in the Race'" (cit. n. 9).

[16] For an uncritical, institutionally sponsored history of the Physical Society see John L. Lewis, *125 Years: The Physical Society and the Institute of Physics* (Bristol: Institute of Physics, 1999). The Physical Society has also been discussed in Russell Moseley, "Tadpoles and Frogs: Some Aspects of the Professionalization of British Physics, 1870–1939," *Social Studies of Science*, 1977, *7*:423–446; and Graeme Gooday, "Precision Measurement and the Genesis of Physics Teaching Laboratories in Victorian Britain" (Ph.D. diss., Univ. Kent, 1989), pp. 8.35–8.51.

[17] On the central role of Cambridge and the Cavendish see Hughes, "Radioactivity and Nuclear Physics" (cit. n. 13), pp. 350–351. Hughes has countered the idea of the early twentieth-century Cavendish as defined by a "sealing-wax and string" approach of small-scale, benchtop laboratory work, pointing out the many connections the laboratory had with engineering and the radio industry: Jeff Hughes, "Plasticine and Valves: Industry, Instrumentation, and the Emergence of Nuclear Physics," in *The Invisible Industrialist: Manufactures and the Production of Scientific Knowledge*, ed. Jean-Paul Gaudillière and Ilana Löwy (London: Macmillan, 1998), pp.

only recently been assessed on their own terms, rather than as comparative failures in relation to the apparently more successful Cavendish Laboratory.[18] These alternative institutions often promoted the study of practical, *useful* physics as an alternative to the esoteric explorations under way in parts of the Cavendish.

Just as these institutions are being reconsidered, so too can we rethink the status of journals that don't fit into the microphysics lineage. As Graeme Gooday has noted, in contrast to Russell Moseley's characterization, the Physical Society was of interest not only to "second order" experimental physicists but also to senior academic physicists.[19] Indeed, 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: Bragg noted that, if Pealing preferred, he could certainly get the piece into the *Philosophical Magazine* or the Royal Society's *Proceedings* with ease—although it should be noted that Bragg was then president of the Physical Society and thus not entirely impartial.[20] However, Bragg's reference to the *Philosophical Magazine* and the Royal Society's *Proceedings* seems to indicate that these two journals held a lofty reputation for physicists.

Of the two, the *Philosophical Magazine* was by far the more prolific, publishing nearly twice as many papers as the *Proceedings* in 1910. While this gap had closed considerably by 1930, following James Jeans's vigorous campaign to revitalize the Royal Society's physics output, the *Philosophical Magazine* could certainly boast superiority in terms of quantity throughout the first half of the twentieth century.[21] First published in 1798, the *Philosophical Magazine* was the older of the two journals and, rather than being under the management of a learned society, was owned by a family-run publishing company, Taylor

58–101. Warwick has explored the divergent traditions of experimental and mathematical physics at Cambridge and revealed differences in approach even within the Cavendish, where the older tradition of precision measurement coexisted with Thomson's new regime. See Andrew Warwick, *Masters of Theory: Cambridge and the Rise of Mathematical Physics* (Chicago: Univ. Chicago Press, 2003); Warwick, "Cambridge Mathematics and Cavendish Physics: Cunningham, Campbell, and Einstein's Relativity, 1905–1911, Pt. 1: The Uses of Theory," *Stud. Hist. Phil. Sci. Pt. A*, 1992, *23*:625–656; and Warwick, "Cambridge Mathematics and Cavendish Physics: Cunningham, Campbell, and Einstein's Relativity, 1905–1911, Pt. 2: Comparing Traditions in Cambridge Physics," *ibid.*, 1993, *24*:1–25.

[18] Robert Fox and Graeme Gooday, eds., *Physics in Oxford, 1839–1939* (Oxford: Oxford Univ. Press, 2005); Romualdas Sviedrys, "The Rise of Physics Laboratories in Britain," *Historical Studies in the Physical Sciences*, 1976, *7*:405–436; Gooday, "Precision Measurement and the Genesis of Physics Teaching Laboratories in Victorian Britain," *Brit. J. Hist. Sci.*, 1990, *23*:25–51; and Gooday, "Precision Measurement and the Genesis of Physics Teaching Laboratories in Victorian Britain" (Ph.D. diss.) (cit. n. 16).

[19] Gooday, "Precision Measurement and the Genesis of Physics Teaching Laboratories in Victorian Britain" (Ph.D. diss.), pp. 8.35–8.51. Moseley characterized the early Physical Society as the home of "second order" experimental physicists and of little interest to senior academic physicists: Moseley, "Tadpoles and Frogs" (cit. n. 16).

[20] W. H. Bragg to H. Pealing, 11 Mar. 1921, William Henry Bragg Papers, Royal Institution Archives, London. Similarly, Csiszar has shown how specialist journals were often chosen over more prestigious ones, such as the *Philosophical Transactions*, in order to ensure wide readership by the most relevant people: Csiszar, "Seriality and the Search for Order" (cit. n. 6).

[21] Brock and Meadows, *Lamp of Learning* (cit. n. 6), p. 144. Publication statistics are calculated from data in Science Abstracts; Jeans's work was praised in Ernest Rutherford, "Address of the President, Sir Ernest Rutherford, O.M., at the Anniversary Meeting, November 30, 1927," *Proceedings of the Royal Society of London, Part A*, 1928, *117*:300–316, esp. p. 305. The rise in number of papers was partly down to Jeans's efforts, but increased financial contributions from government also played a significant role, as noted in Rutherford's 1929 address: Rutherford, "Address of the President, Sir Ernest Rutherford, O.M., at the Anniversary Meeting, November 30, 1929," *ibid.*, 1930, *126*:184–203, esp. p. 190; and Jeff Hughes, "'Divine Right' or Democracy? The Royal Society 'Revolt' of 1935," *Notes and Records of the Royal Society*, 2010, *64*(Suppl. 1):S101–S117, esp. p. S103.

& Francis. In its early history, the *Philosophical Magazine*'s remit had been fairly broad, but it naturally found its output bent toward experimental and mathematical physics as other journals took the share of biological and chemical papers. The journal became home to the work of prestigious physicists, with Lord Kelvin serving as the editor from 1871 until 1907 and Ernest Rutherford and Niels Bohr both choosing the *Philosophical Magazine* as the destination for their now-famous papers on atomic structure.[22]

The *Philosophical Magazine* had no official refereeing system but instead was managed more informally by a small group of editors. In the 1920s, this consisted of two representatives of the publishing company, William, Jr., and Richard Francis, along with the Dublin geophysicist John Joly, former director of the Cavendish Laboratory J. J. Thomson, and Oliver Lodge, a recently retired physicist with a firm commitment to the now decidedly classical concept of the ether.[23] William and Richard were responsible for distributing submitted papers to members of the editorial team, or other suitable scientific men, for their approval or rejection. This management style was not universally popular, and the journal was subject to some criticism throughout the early twentieth century. 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 in the pages of *Nature*, insisting 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 that 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."[24] As I shall show, Lodge and his friends certainly profited from this more flexible management style.

Lodge was not alone in his belief that a less bureaucratic system was beneficial, and even critics of the *Philosophical Magazine* did not always recommend the establishment of a refereeing process. In 1910, Joly invited Joseph Larmor to join him as an editor. Larmor declined, suggesting that while the *Philosophical Magazine* now had some good content, it also had "much more that is poor." Crucially, Larmor felt that "much will depend on the success of the new volume of the Phys. Soc." He thus saw the Physical Society—not the Royal Society—as the journal's competition. 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 representing a number of British societies. Lord Rayleigh also felt that this was the best option, but it was not adopted by Taylor & Francis.[25]

The characterization of the *Philosophical Magazine* that emerges is perhaps one of an

[22] Brock and Meadows, *Lamp of Learning*; Ernest Rutherford, "The Scattering of Alpha and Beta Particles by Matter and the Structure of the Atom," *Philosophical Magazine*, 1911, *21*:669–688; and Niels Bohr, "I: On the Constitution of Atoms and Molecules," *ibid.*, 1913, *26*:1–25.

[23] For Lodge and the ether see Richard Noakes, "Ethers, Religion, and Politics in Late-Victorian Physics: Beyond the Wynne Thesis," *Hist. Sci.*, 2005, *43*:415–455; Stanley Goldberg, "In Defense of Ether: The British Response to Einstein's Special Theory of Relativity, 1905–1911," *Hist. Stud. Phys. Sci.*, 1970, *2*:89–125; and Hunt, *Maxwellians* (cit. n. 14). For a history of changing conceptions of the ether from 1740 to 1900 see G. N. Cantor and M. J. S. Hodge, *Conceptions of Ether: Studies in the History of Ether Theories, 1740–1900* (Cambridge: Cambridge Univ. Press, 1981).

[24] Oliver Lodge, "The 'Philosophical' Magazine," *Nature*, 1921, *108*:12. The "referees" mentioned here are the editors. In the front pages of the *Philosophical Magazine*, they are listed as those whom the journal is "conducted by."

[25] Joseph Larmor to John Joly, 6 Nov. 1910, Papers of John Joly, Manuscripts and Archives Research Library, Trinity College, Dublin.

out-of-touch journal, in terms of both content and management style. Indeed, owing to the particular theoretical commitments of Oliver Lodge, the *Philosophical Magazine* became the home of ether physics in the 1920s.[26] Furthermore, as I shall show, the journal often published articles rejected by the Royal Society. For those more aligned with the values of the *Proceedings*, such as the former Cavendish experimental physicist Patrick Maynard Stuart Blackett, the *Philosophical Magazine* was desperately in need of a formal refereeing system.[27] Others, such as J. J. Thomson, felt by the mid 1930s that the journal needed a paid editor who was energetic, active, and businesslike, with a "wide knowledge of physics including modern physics."[28] However, these criticisms cannot necessarily be applied to the 1920s as well. Charles Galton Darwin, the Cambridge-trained quantum theorist, declared in 1924 that the *Proceedings* and the *Philosophical Magazine* were the only two British physics journals worth reading.[29] Furthermore, it is clear that at no point was there consensus on formal peer review as the best practice. I propose that while the *Philosophical Magazine* may appear decidedly old-fashioned when viewed through the lens of modern approaches to both physics and publishing, a more contextually aware analysis frames the periodical as a respected alternative to the *Proceedings* in the 1920s. In addition, as I shall show, in practice the Royal Society's formal management did not differ dramatically from the more relaxed approach in place at the *Philosophical Magazine*. For both journals, power lay in the hands of the few.

THE ORGANIZATIONAL STRUCTURE OF THE ROYAL SOCIETY'S PHYSICAL COMMITTEE: THE INFLUENCE OF THE MODERN

At the Royal Society, all papers for publication were required to be communicated by a Fellow, before being considered by a Sectional Committee (of mathematics, physics, or chemistry, with the latter two combined until 1919) that would also choose the appropriate journal. The Sectional Committee for Physics consisted of nine members, with three retiring each year; a retired committee member was then ineligible for election the following year.[30] 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

[26] Oliver Lodge, "On a Possible Means of Determining the Two Characteristic Constants of the Aether of Space," *Phil. Mag.*, 1919, Ser. 6, *37*:465–471, esp. p. 471; L. Silberstein, "The Recent Eclipse Results and Stokes-Planck's Aether," *ibid.*, 1920, Ser. 6, *39*:161–170; Lodge, "Note on a Possible Structure for the Ether," *ibid.*, pp. 170–174; G. Green, "A Fluid Analogue for the Aether," *ibid.*, pp. 651–659; Lodge, "Ether, Light, and Matter," *ibid.*, 1921, Ser. 6, *41*:940–943; E. H. Synge, "A Definition of Simultaneity and the Aether," *ibid.*, 1922, Ser. 6, *43*:528–531; Douglas R. Hartree, "On the Propagation of Certain Types of Electromagnetic Waves," *ibid.*, 1923, Ser. 6, *46*:454–460; A. Press, "Maxwell's Electromagnetic Aether and the Michelson-Morley Experiment," *ibid.*, 1925, Ser. 6, *50*:809–812; and D. Meksyn, "The Physical Form of Ether," *ibid.*, 1927, Ser. 7, *4*:272–300.

[27] P. M. S. Blackett to Bragg, 8 Nov. 1937, Papers of Professor A. V. Hill, Churchill Archives Centre, University of Cambridge. Blackett turned down an offer of editorship on the grounds that the journal had "lost its great position that it used to hold and should still hold." He noted that in his view "the lack of an effective refereeing system was a serious disability."

[28] J. J. Thomson to Anon., ca. 1935, Papers of Sir Joseph John Thomson, Department of Manuscripts and University Archives, Cambridge University Library. The recipient is presumably Taylor & Francis. Thomson wrote that unless the journal improved, he no longer wanted to be associated with it.

[29] C. G. Darwin to Richardson, 29 Apr. 1924, Richardson Papers. Darwin noted that he read "practically no other papers" beyond those in the *Philosophical Magazine*, the *Proceedings*, the German *Zeitschrift fur Physik*, and the American *Physical Review*.

[30] "Minute Books of the Physics and Chemistry/Physics Sectional Committee," Royal Society Archives, London (hereafter cited as **Royal Society Archives**); and *Year Book of the Royal Society* (London: Royal Society, 1921) (one-year ineligibility rule).

1922, Owen Willans Richardson from 1925, Alfred Fowler from 1927, and Richardson again from 1929 to 1930.[31] 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 had some connection with Cambridge, and eleven had researched at the Cavendish. (See Table 1 for the institutional affiliations of members of the committee in the 1920s.)

However, a variety of alternative institutions were also represented within the Physical Committee. Thirteen members were, or had been, affiliated with one of the London universities, which were generally more industrially minded than Cambridge. Furthermore, nine committee members either were or had been based in nonacademic institutions, including Greenwich Observatory, a brewery, the Department of Scientific and Industrial Research, the military, and the National Physical Laboratory. There were, of course, considerable overlaps between all of these categories. 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.[32] Glazebrook also spent time there, 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. There were a variety of institutions and backgrounds represented within the Physical Committee and, consequently, multiple approaches to early twentieth-century developments in physics.

One final position completed the managerial structure of the *Proceedings*, and this was the Physical Secretary, a position external to the committee itself and not subject to the same rules of retirement. Originally, this secretary (along with his biological counterpart) held sole authority over the contents of the Society's journals, but this had changed with the 1896 establishment of Sectional Committees.[33] From 1919 to 1929, the position of Physical Secretary was held by the Cambridge mathematician James Jeans. By the 1920s, Jeans was a strong advocate of quantum theory; he had produced his *Report on Radiation and the Quantum-Theory* for the Physical Society in 1914. This *Report* had a considerable impact on British physics, playing a significant role in the acceptance of quantum theory by Jeans's peers.[34] On his appointment as Physical Secretary, Jeans's influence grew still further, as 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 the

[31] Information about the members of the committee is taken from the "Minute Books of the Physics Sectional Committee," Royal Society Archives.

[32] On Fowler's work with Rayleigh at the Royal College of Science see Hannah Gay, *The History of Imperial College London, 1907–2007* (London: Imperial College Press, 2007). Many committee members worked for the military during World War I, but I have placed them in the nonacademic category only if they were also employed by the military in peacetime—e.g., Lyons and F. E. Smith.

[33] Similar committees had first been established in the early days of the Society, but they were short-lived. Later, "Scientific Committees" were set up in 1838 but abolished in 1849. See Henry Lyons, *Record of the Royal Society of London* (London: Royal Society, 1940), pp. 102–103.

[34] James Hopwood Jeans, *Report on Radiation and the Quantum-Theory* (London: "The Electrician" Printing & Publishing, 1914); and Geoffrey Gorham, "Planck's Principle and Jeans's Conversion," *Stud. Hist. Phil. Sci. Pt. A*, 1991, *22*:471–497, esp. pp. 473–474.

Table 1. Institutional Affiliations of Members of the Royal Society's Physical Committee, 1921–1930

Committee Member	Cambridge	Cavendish	London	Nonacademic	Other University
F. W. Aston 1877–1945	1910–1945	1910–1945		**Brewery** *(Messrs W. Butler & Co., Wolverhampton)* 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 **Royal Institution** 1923–1942		**Adelaide** 1885–1908 **Leeds** 1909–1915
W. L. Bragg 1890–1971	1909–1914 1945–1953	ca. 1910–1914 1938–1953	**Royal Institution** 1938–1971		**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	ca. 1901–1914	1914–1946		**Birmingham** ca. 1896–1901

Table 1. Continued

Committee Member	Cambridge	Cavendish	London	Nonacademic	Other University
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	ca. 1904–1912	1912–1921		**Manchester** 1898–1901 **Queens University, Belfast** ca. 1904–1912 **Oxford** 1921–1930
O. W. Richardson 1879–1959	1897–1906	1897–1906	1914–1924 (Then Royal Society Yarrow Professor, a research-only position)		**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

Table 1. Continued

Committee Member	Cambridge	Cavendish	London	Nonacademic	Other University
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			ca. 1896–1900	**National Physical Laboratory** 1900–1920 **Admiralty** 1920–1929 **DSIR** 1929–1939 **Anglo-Iranian Oil** 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** *(AdamHilgerLtd.)* 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

secretary often worked alone. This was certainly true in 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." 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.[35] Only those papers opposed during this process, or earlier flagged by Jeans or the chair, were properly discussed at a meeting. For most papers, the first destination was an independent referee, with a system for reviewing put in place in 1752: the committee could call on "any other members of the Society who are knowing and well skilled in that particular branch of Science that shall happen to be the subject matter of any paper which shall be then to come under their deliberations." These "well skilled" experts were, in the 1920s, chosen by Jeans and the chair, but for some submissions this stage was bypassed altogether. Of the sixty-one of his own papers that Robert Strutt communicated between 1906 and 1926, all but one was passed for publication without review. Such treatment was not reserved only for Lords: Sydney Chapman, a Cambridge graduate employed at Imperial College, also had his work cleared for publication, as he was trusted by Jeans to be "always very careful and reliable in all his work."[36]

Confidence 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 refereeing if it was communicated by a physicist with access to the Royal Society's inner circle. For such researchers, it was thus extremely beneficial to be part of this network. In 1925, Jeans noted that future chair Alfred Fowler was "always careful in what he communicates" and so didn't send his communications on for review. Rutherford, director of the Cavendish Laboratory from 1919, received similar treatment. In 1925 he sent the Society three papers produced by Cavendish researchers and accompanied by a covering letter noting that he had spent "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.[37] By building up a relationship and reputation with the Physical Secretary, physicists were able to ensure the continued acceptance of their work—or even, as in the case of Rutherford, the work of an entire research school—into the Royal Society. As such, Rutherford's particular style of reductionist modern physics was ensured publication in a prestigious journal.[38]

Clearly the relationship between the communicator and committee members greatly influenced the decision to accept a paper. A communicator's influence within this relationship could be earned over time by continually sending in papers of a high quality, and certain communicators developed reputations for being more careful than others. How-

[35] Jeans to Richardson, 2 Jan. 1925, Richardson Papers; and *Year Book of the Royal Society* (cit. n. 30) (regarding Standing Order 43).

[36] Lyons, *Record of the Royal Society* (cit. n. 33), p. 93 (the first recorded referral took place in 1780); "Register of Papers," Royal Society Archives (for the passing of Strutt's papers); and Jeans to Richardson, 25 Feb. 1926, Richardson Papers (regarding the reliability of Chapman).

[37] Jeans to Richardson, 9 July 1925, Richardson Papers (he quotes Rutherford's covering letter as well as praising Fowler).

[38] Rutherford's approach differed somewhat from that of his predecessor, J. J. Thomson. It was less mathematical (as the training of experimental physicists in Cambridge became distinct from the training of mathematicians) and dedicated to the study of microscopic particles themselves, rather than their relations with an ether, which was now of little interest experimentally. See John L. Heilbron, "The Scattering of α and β Particles and Rutherford's Atom," *Archive for History of Exact Sciences*, 1968, *4*:247–307; and Isobel Falconer, "J. J. Thomson and 'Cavendish' Physics," in *The Development of the Laboratory*, ed. Frank A. J. L. James (London: Macmillan, 1989), pp. 104–117, esp. p. 109.

ever, 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 as well. Indeed, the status of work could be affected by the institution it came from. In one instance, after receiving a paper written by the Sorbonne physicist H. Weiss and sent in by the Cambridge zoologist Cresswell Shearer, Jeans noted that he knew "nothing of [Shearer's] reliability as a communicator." Upon discovering that the paper arose out of 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." The paper subsequently appeared in the *Proceedings* as communicated not by Shearer but by Bragg, suggesting that the communicator was partly responsible for convincing the reader of a paper's value.[39]

For those papers that *were* sent on to a referee, the reviewer was usually decided upon by Jeans and the chair. These referees were given the authority to decide not only if a paper was of a sufficiently high standard but also if the kind of physics it presented was appropriate for the pages of the *Proceedings*. Choice of referee was often limited: for example, 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 Trinity College graduates, became the "arbiters of quantum physics." 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.[40] In a similar position to Fowler and Darwin was Arthur Stanley Eddington. As an early supporter of relativity, and a Fellow of the Royal Society from 1914, Eddington was perfectly suited to become something of an arbiter himself for this area of physics.[41] As I shall explore, having committed relativists and quantum theorists as appointed experts influenced the types of physics accepted into the *Proceedings*. If a paper did not conform to the views of the chosen referees, then it was subject to considerable scrutiny.

Networks of experts played a central role in the management of the *Proceedings*, with certain referees and communicators empowered to make judgments as to 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 James Jeans, various committee members, and 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 was not sufficiently modern and up to date for the circle of Fellows entrusted to judge it. This is found in the case of a paper written by Richard Hargreaves and communicated to the Royal Society by Joseph Larmor in 1921.

[39] Jeans to Richardson, 21 Apr. 1925, Richardson Papers; and H. Weiss, "The Application of X-Rays to the Study of Alloys," *Proc. Roy. Soc. Lond. Pt. A*, 1925, *108*:643–654.

[40] Jaume Navarro, "'A Dedicated Missionary': Charles Galton Darwin and the New Quantum Mechanics in Britain," *Studies in the History and Philosophy of Modern Physics*, 2009, *40*:316–326, on pp. 321, 320. The word "arbiters" is Navarro's, and there is no indication of contemporary usage.

[41] For Eddington see Matthew Stanley, *Practical Mystic* (Chicago: Univ. Chicago Press, 2007); and A. V. Douglas, *The Life of Arthur Stanley Eddington* (London: Nelson, 1956).

JUDGING THE VALUE OF RICHARD HARGREAVES: "CLASSICAL MECHANICS LEAD[S] NOWHERE AT ALL"

Joseph Larmor was Lucasian Professor of Mathematics from 1903 to 1932, a prestigious Cambridge position once held by Isaac Newton, and had been a Fellow of the Royal Society since 1892. His groundbreaking 1900 *Aether* and *Matter* had formed a significant part of Cambridge electrodynamics pedagogy at the beginning of the twentieth century.[42] However, given his firm commitment to the ether, and a skeptical approach to many developments in the discipline, by the 1920s Larmor's scientific outlook differed considerably from that held by James Jeans and many others on the Physical Committee. And yet, with his elevated status in the physical community, he could command substantial respect from his peers even as his intellectual positions began to appear out of date. This was not the case with Hargreaves. While both men had studied at St. John's College, Cambridge, and placed in the top ten in the Mathematical Tripos, Larmor had performed considerably better, earning the title of Senior Wrangler. Unlike Larmor, Hargreaves then chose 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 to the Cambridge Philosophical Society for publication.[43] In the late nineteenth century he had been influenced by J. J. Thomson's vortex theory of ether and matter. By 1920, he was still considering the structure of matter and, now employed as a lecturer at 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.[44]

This area of physics was undergoing rapid advances in the 1920s. Rutherford had discovered artificial disintegration in 1919 and was now working on a model of the nucleus, while also delivering lectures on the subject to the Royal Institution and the Physical Society. Meanwhile, Bohr's quantum theory was acquiring increasing credibility in Britain, particularly with the help of Jeans's promotional work. From 1919, Jeans was proposing the quantum work produced by Bohr and his school as the only viable option for understanding radiation.[45] Larmor, however, ignored such developments, conceiving of Hargreaves's 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

[42] Joseph Larmor, *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 Univ. of Cambridge* (Cambridge: Cambridge Univ. Press, 1900); this work is discussed in Warwick, *Masters of Theory* (cit. n. 17), pp. 376–381. On the Lucasian Professorship and its holders see Kevin C. Knox and Richard Noakes, *From Newton to Hawking: A History of Cambridge University's Lucasian Professors of Mathematics* (Cambridge: Cambridge Univ. Press, 2003).

[43] Richard Hargreaves, *Arithmetic* (Oxford: Clarendon, 1901). Hargreaves noted in a letter to Larmor that he usually sent his mathematical physics to these two destinations: Richard Hargreaves to Larmor, 30 Sept. 1921, Joseph Larmor Papers, St. John's College Library, University of Cambridge (hereafter cited as **Larmor Papers, Cambridge**). Regarding Larmor's and Hargreaves's relative success in the Mathematical Tripos see Warwick, *Masters of Theory*, pp. 515–516.

[44] Hargreaves to Larmor, 5 Oct. 1920, Larmor Papers, Cambridge. For the influence on Hargreaves of Thomson's vortex theory see Helge Kragh, "The Vortex Atom: A Victorian Theory of Everything," *Centaurus*, 2002, *44*:32–144, esp. p. 43.

[45] Roger H. Stuewer, "Rutherford's Satellite Model of the Nucleus," *Historical Studies in the Physical and Biological Sciences*, 1986, *16*:321–352; Kragh, "Resisting the Bohr Atom" (cit. n. 14), p. 29; James Hopwood Jeans, *Report on Radiation and the Quantum-Theory*, 2nd ed. (London: Fleetway, 1924); and Jeans, "The Quantum Theory and New Theories of Atomic Structure," *Journal of the Chemical Society: Transactions*, 1919, *115*:865–871.

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.[46] However, Hargreaves's finished paper, "Atomic Systems Based on Free Electrons, Both Positive and Negative, and Their Stability," did not take quantum theory into consideration and, as such, was not well received at the Royal Society. Neither Larmor nor Hargreaves held views on atomic structure that matched up to the referees' idea of credible, up-to-date physics. Hargreaves was now subject to the networks of expertise 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 he 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. A friend of Eddington's, the two men having studied together at Manchester, Nicholson was also, perhaps more notably, cited frequently in Niels Bohr's 1913 series of articles on the quantum theory of atoms and molecules.[47] While Nicholson criticized Bohr's quantum model of the atom, he did so in a way that displayed substantial knowledge of the new kinds of physics involved. Despite this, Helge Kragh depicts Nicholson as a "classical physicist," and Russell McCormmach describes his work as "starting with an attempt to understand the constitution of matter by wholly classical laws." I suggest, however, that the case of Nicholson demonstrates one of the difficulties of characterizing physicists of this period in such a way. Indeed, when judging Hargreaves's work, Nicholson did not recommend publication, declaring that the author was "certainly not in touch to any sufficient extent with modern developments."[48] 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.[49]

Notably, Nicholson did not say that the paper itself was bad—or even unpublishable; rather, he judged that it was not suitable for the *Proceedings*. Nicholson believed that the theory contained within the paper had been refuted by experimental results in support of quantum theory and that therefore the work had no worth. He did not see how an atomic model that did not take this into account could help further knowledge on the subject. Nicholson was thus recommending rejection on the grounds that the paper was not in

[46] Hargreaves to Larmor, 4 Nov. 1920, Larmor Papers, Cambridge.

[47] For Nicholson see Wm. Wilson, "John William Nicholson, 1881–1955," *Biographical Memoirs of Fellows of the Royal Society*, 1956, *2*:209–214; Helge Kragh, *Niels Bohr and the Quantum Atom* (Oxford: Oxford Univ. Press, 2012); and Russell McCormmach, "The Atomic Theory of John William Nicholson," *Arch. Hist. Exact Sci.*, 1966, *3*:160–184.

[48] Kragh, "Resisting the Bohr Atom" (cit. n. 14), p. 28; McCormmach, "Atomic Theory of John William Nicholson," p. 160; and "R. Hargreaves Referee Report 1921" (No. 141), Royal Society Archives.

[49] "R. Hargreaves Referee Report 1921" (No. 141), Royal Society Archives.

accord with his views on where the discipline of physics was headed, on which theories and approaches needed to be discarded on the basis of recent developments.

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. Jeans himself was a staunch modernist on the topic of atomic structure and quantum theory, and he also had confidence in his chosen referee. However, Jeans could also not dismiss Larmor, who had acquired a certain amount of regard during his lengthy career. While he was perhaps now "not in touch" with many developments in physics, Larmor maintained much of his former status. And so, when Larmor insisted that Hargreaves's paper be sent on to a second referee, Jeans complied. The second referee was Darwin, who suggested publication only with "considerable modifications." Crucially, he advised that the paper needed to take into consideration Bohr's theory, which was essential, he insisted, for defining the size of the atom. Again, a committed quantum theorist was criticizing the paper because it did not fit with his own assessment of the state of play in atomic theory. 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. Given these circumstances, it is perhaps not so surprising that publication was not recommended, and Jeans informed Larmor of the decision the following day. 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 be able to bring an unbiased mind to its consideration."[50]

Hargreaves drastically shortened the paper and sent it on to the *Philosophical Magazine*, while Larmor wrote to Lodge to detail his support for the work. 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." Larmor insisted that "the solutions of all relevant problems in true Dynamics should be recorded: and if you have room in the Phil. Mag. it will not disgrace you. It is certainly superior to most of the theory that the Roy. Soc.—or the Phil. Mag. either—now prints at great length." Lodge agreed with Larmor, noting that modern spectrum results concerned the planetary electrons and not the constitutional structure of the proton or nucleus. He put complete faith in Larmor's opinion, declaring that "I know nothing at present about Hargreaves's paper: but on your recommendation I feel sure that the Phil. Mag. will find room for it." Larmor and Lodge did not share Darwin's and Nicholson's commitment to quantum theory and were eager to explore atomic theories that could function without this concept. The paper was finally published in the December issue of the *Philosophical Magazine* and featured a note of thanks to Joseph Larmor. 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. Meanwhile, Larmor completed his role of advisor and advocate with a letter to *Nature*. Here, he enthusiastically pointed readers toward Hargreaves's "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

[50] Jeans to Larmor, 17 Nov. 1921, Larmor Papers, Cambridge; "R. Hargreaves Referee Report 1921" (No. 141), Royal Society Archives; "Sectional Committee for Mathematics," Minute Books (CMB/46/4), Royal Society Archives; Jeans to Larmor, 27 Jan. 1922, Larmor Papers, Cambridge; and Hargreaves to Larmor, 9 Aug. 1922, Larmor Papers, Cambridge.

mathematical analysis could only help to expand the range of ideas in the field.[51] Where Nicholson and Darwin believed that Hargreaves's paper could not contribute to further progress in physics, Larmor fundamentally disagreed. For him, this kind of mathematical physics *was* extremely valuable. The credibility of an older approach to physics was under negotiation.

While the Royal Society networks were detrimental to Hargreaves's publication success, those of the *Philosophical Magazine* worked in his favor. In the case of Hargreaves, we thus have a small circle of Royal Society Fellows rejecting a certain kind of physics, on the grounds that it was not sufficiently up to date; they were treating the *Proceedings* as the natural home of quantum theory. Meanwhile, the *Philosophical Magazine*, on the advice of Larmor, quickly accepted Hargreaves's paper. For both journals, the decisions were based on the advice of appointed experts. These experts were influenced by their own commitments to particular theories and views on the consequences of new developments in the discipline. But we cannot necessarily conceive of this as a clear-cut divide between modern and classical, as Larmor held 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 current scientific research. They were communicating and promoting the types of physics that Jeans and his circle now saw as 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.

DIMINISHING POWER: THE CAREFUL TREATMENT OF ESTABLISHED FELLOWS

In the case of the Hargreaves paper, we found that Larmor, despite his prominent 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 submitted on behalf of a colleague. Notably, Larmor had previously served as the Society's Physical Secretary, 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.[52] And yet now a paper he wholeheartedly recommended had been rejected. This would suggest that at the beginning of the 1920s there was a shift in Larmor's status at the Royal Society. His power within the network was fading. 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 criticized. Committee members needed to negotiate this somewhat awkward situation carefully. Crucially, Larmor's status at the Royal Society was in a period of transition, as he continued to hold a degree of influence.

[51] Larmor to Oliver Lodge, 12 Aug. 1922, Oliver Lodge Papers, University College, London, Special Collections (hereafter cited as **Lodge Papers**); Lodge to Larmor, 15 Aug. 1922, Joseph Larmor Papers, Royal Society Archives (hereafter cited as **Larmor Papers, Royal Society Archives**); Richard Hargreaves, "Atomic Systems Based on Free Electrons, Positive and Negative, and Their Stability," *Phil. Mag.*, 1922, Ser. 6, *44*:1065–1105 (see p. 1091 for Hargreaves's admission of ignorance regarding recent experimental results in physics); and J. L., "A Type of Ideal Electric Atoms," *Nature*, 1922, *110*:873.

[52] "Register of Papers," Royal Society Archives.

The paper sent by Larmor to the Royal Society was intended to antagonize those who had rejected Hargreaves's 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."[53] 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 toward atomic theory, Larmor's attack was not against quantum theory; he offered, rather, a paper entitled "On the Nature and Amount of the Gravitational Deflexion of Light." Here, Larmor was responding to the 1919 eclipse expedition to measure one of Einstein's relativistic predictions, the deflection of starlight by the sun's gravitational field. While some deflection was expected according to Newton's laws, it was calculated that the effects of relativity theory should result in a deflection of roughly double that, and this was the amount observed in 1919.[54] By questioning the interpretation of the measurements made during the eclipse, Larmor was attacking relativity theory, not quantum theory. It would seem that he placed these two concepts in the same category, and it was one that divided him from the "dogmatics" of the Royal Society. For Larmor, relativity and quantum theory appear both to have been part of the same problem: the commitment of a 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.[55] 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 Einstein had not been victorious in the eclipse expedition.[56] This is indeed what Eddington seems to have gathered from his first reading of the paper, leading him immediately to recommend that it be withdrawn. 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." Eddington noted that while he would have liked to ask Larmor to remove the "innuendo

[53] Larmor to Lodge, 4 Oct. 1922, Lodge Papers.

[54] Much has been written about the eclipse expedition. See John Earman and Clark Glymour, "Relativity and Eclipses: The British Eclipse Expeditions of 1919 and Their Predecessors," *Hist. Stud. Phys. Sci.*, 1980, *11*:49–85; Matthew Stanley, "An Expedition to Heal the Wounds of War: The 1919 Eclipse and Eddington as Quaker Adventurer," *Isis*, 2003, *94*:57–89; and Alistair Sponsel, "Constructing a 'Revolution in Science': The Campaign to Promote a Favourable Reception for the 1919 Solar Eclipse Experiments," *Brit. J. Hist. Sci.*, 2002, *35*:439–467.

[55] Larmor's handwriting was "notoriously illegible": A. Rupert Hall, *The Cambridge Philosophical Society: A History, 1819–1969* (Cambridge: Cambridge Philosophical Society, 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 Mar. 1922, Lodge Papers. Lodge tried to bring to Larmor's attention the difficulty of reading his works, noting, "You are of course entitled to your own style, but it certainly is a difficult style": Lodge to Larmor, 10 June 1925, 1 May 1929, Larmor Papers, Royal Society Archives. Eddington retrospectively noted that in Larmor's later years "his style, never lucid, grew more and more involved": Arthur Stanley Eddington, "Joseph Larmor, 1857–1942," *Obituary Notices of Fellows of the Royal Society*, 1942, *4*:197–207, on p. 205.

[56] Joseph Larmor, "Einstein and Gravitation," *Times* [London], 17 Apr. 1923, p. 15. Larmor declared that he had made this clear in a recent publication, presumably his paper published in the *Philosophical Magazine* in 1923: Joseph Larmor, "On the Nature and Amount of the Gravitational Deflexion of Light," *Phil. Mag.*, 1923, Ser. 6, *45*:243–256.

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 work 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. 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 it in full. In this case, "those who understand the relativity theory can either satisfy themselves or publicly reply to the paper."[57]

This was a remarkably different approach to that taken by Nicholson and Darwin when reviewing Hargreaves's paper. This can be accounted for by the different attitudes taken when considering experimental and theoretical developments and the status that Larmor held professionally. Darwin and Nicholson disregarded Hargreaves's 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, Eddington's philosophy of science was an inclusive one: he believed in using whatever techniques produced results and worrying about an overarching theory later.[58] Furthermore, Larmor could not be dismissed as easily as Hargreaves, who was a scientist of far less distinction. Larmor did not hold sufficient authority to ensure that a paper he recommended, such as Hargreaves's, would be published, but Eddington was obliged seriously to consider Larmor's own work, even if he did not agree with the views contained therein. In the face of a paper written by a man of Larmor's status and containing no apparent technical inaccuracies, Eddington felt that he had no choice but to recommend publication.

However, the paper ultimately was not published in the *Proceedings*. After reading it carefully 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. 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 this second part "on trust," as it appeared to deal with electrodynamics, a field in which Eddington believed Larmor to be expert.[59] At the next meeting of the Sectional Committee for Mathematics, the paper was discussed by Eddington and Jeans, joined also by Nicholson, the pure mathematician J. E. Littlewood, and the Trinity College physicist G. I. Taylor. Eddington's suggestion of

[57] "J. Larmor Referee Report 1922" (No. 147), Royal Society Archives; Eddington refers to an earlier report, presumably removed from the records.

[58] Matthew Stanley, "So Simple a Thing as a Star: The Eddington–Jeans Debate over Astrophysical Phenomenology," *Brit. J. Hist. Sci.*, 2007, *40*:53–82. Stanley places Eddington in opposition to Jeans, who believed that a scientific method should be based on certainties.

[59] Eddington, "Joseph Larmor, 1857–1942" (cit. n. 55), p. 204; and "J. Larmor Referee Report 1922" (No. 147), Royal Society Archives. For Larmor's commitment to "least action" see Andrew Warwick, "Frequency, Theorem, and Formula: Remembering Joseph Larmor in Electromagnetic Theory," *Notes Rec. Roy. Soc. Lond.*, 1993, *47*:49–60.

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.[60]

Larmor was unhappy with this suggestion and decided that it was time to move on to a different journal. He confided in Lodge, admitting that he could not afford to "quarrel" with the Royal Society as long as he still had "students to help along into the world." Larmor was thus aware of the significant role the Royal Society networks could play in a scientist's career; indeed, his own status as a Fellow contributed to his current position of authority. Lodge, meanwhile, was shocked by the Society's actions, responding 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" and that he was writing at once to the publishers to see if there was space in the January issue. Larmor did withdraw the paper from consideration for the *Proceedings*, and it was quickly published in the *Philosophical Magazine*.[61]

In this episode, we see the path to a paper's publication dictated by the interplay of a variety of relationships. In the case of Eddington and Larmor, these relationships were in the midst of transition. Eddington, who had taken up a scholarship at Trinity College two years after the publication of Larmor's influential *Aether and Matter*, still held its author in high regard. He devoted considerable time and energy to reviewing Larmor's paper, reading it at least three times and eventually suggesting accepting the entire paper, despite finding parts of it incomprehensible. While Eddington made sure carefully to examine the first part of the paper, which dealt with an area of physics on which he himself was deemed expert, he was happy to place his faith in Larmor regarding the second part. For Eddington, Larmor was a reliable authority on matters relating to electrodynamics, but not relativity theory. Larmor was still an expert, in Eddington's eyes—but less so than he had been twenty years previously.

The remainder of the committee did not share Eddington's confidence in Larmor. And after their final decision, Lodge's take on the matter was that the Royal Society had no right to doubt the credibility of Larmor's work. Again we see Lodge putting complete faith in Larmor, rushing to publish the paper in the *Philosophical Magazine*. The importance of maintaining amicable working relationships also arises. While Larmor was keen not to fight with the Royal Society, for the good of his students, the Society representatives were equally careful in their correspondence with Larmor, aware that there were many things they could not ask of him. Relationships with longtime Fellows such as Larmor were treated with caution. This is evident in the Society's dealings with other prestigious but similarly classical physicists. When considering an unfavorably reviewed paper by Charles Herbert Lees, Vice Principal of Queen Mary College, London, Jeans suggested giving Lees the "benefit of the doubt" as he was a "Fellow of some standing."[62] Similarly, when William Mitchinson Hicks, a Fellow since 1885, received an equally critical review in 1931, the Physical Secretary (now Frank Edward Smith) was torn: he didn't want to do

[60] "Sectional Committee for Mathematics," Minute Books (CMB/46/4), Royal Society Archives. Two days after this meeting, Lodge referred to Jeans's criticisms in a letter to Larmor: Lodge to Larmor, 9 Dec. 1922, Larmor Papers, Royal Society Archives.

[61] Larmor to Lodge, 8 Dec. 1922, Lodge Papers; Lodge to Larmor, 9 Dec. 1922, Larmor Papers, Royal Society Archives; and Larmor, "On the Nature and Amount of the Gravitational Deflexion of Light" (cit. n. 56).

[62] "Report on Paper by C. H. Lees, on 'The Determination of the Specific Heats of Gases at Constant Pressure &c.,'" Richardson Papers. On Lees see Wm. Wilson, "Charles Herbert Lees, 1864–1952," *Obituary Notices Fellows Roy. Soc.*, 1953, *8*:523–528.

"anything wrong," but he also didn't want to "publish anything which is not useful."[63] There was thus more to consider in the publication of a paper than whether it matched up to the reader's notions of valuable physics, whether classical or modern. The professional position of the author was also taken into account.

CONCLUSION

In his overview of the development of peer review, Burnham notes that as late as World War I editorial decisions at the *Annalen der Physik* were made by just two people, Max Planck and a junior colleague, although they were overseen by a board managed by the Berlin Physical Society.[64] Ostensibly, the situation at the Royal Society in this same period was very different: the autonomous secretary had been officially replaced by a committee in the eighteenth century, when a reviewing system was also set up. In this essay I have shown, however, that in the 1920s the Royal Society's *Proceedings* was managed in a manner that was effectively identical to that for the *Annalen der Physik*. James Jeans and the serving Chair of the Physical Committee discussed papers with one another, with the remainder of the committee often absent from decisions. While there was a system of peer review in place, this was an available option rather than a strict requirement. By tracing the paths of a number of submitted papers, and the people involved in the decision-making process, I have revealed the power held by the Physical Secretary in the 1920s and exposed underlying similarities between the management of the *Proceedings* and the *Philosophical Magazine*. I have shown the necessity, when conducting a history of publishing practice, of delving deeper, beyond the official procedures, and have emphasized the importance of carefully examining the actual roles of the individuals involved.

In this analysis of specific cases at the *Philosophical Magazine* and the *Proceedings*, I have explored positive and negative aspects of different styles of editorial control. Many of these issues are certainly relevant today, although managerial structures have changed. *Proceedings A* is currently managed by a single editor, along with an editorial board of fifty scientists—a considerably larger pool of expertise than was held by the much smaller committee in the 1920s. However, within this much wider network, smaller ones still play a fundamental role. Contributions are "pre-assessed by a member of the Editorial Board" before, if selected, being sent to two or more independent referees, with the final decision made by the editor, on the basis of the referee reports. Furthermore, the Royal Society continues to use single-blind reviewing, with only the referees' names concealed, a practice that could certainly bring a reviewer to place more confidence in a paper authored by a prestigious figure.[65] In addition, the problem of limited numbers of experts in particular fields can only have increased as the specialization of scientific fields continues.

[63] "W. M. Hicks Referee Report 1925" (No. 186), Royal Society Archives; F. E. Smith to Richardson, 30 Jan. 1932, Richardson Papers; and S. R. Milner, "William Mitchinson Hicks, 1850–1934," *Obituary Notices Fellows Roy. Soc.*, 1935, *4*:393–399. In the end, neither Hicks's nor Lees's papers was published, although Lees's was read at a meeting.

[64] Burnham, "How Journal Editors Came to Develop and Critique Peer Review Procedures" (cit. n. 5).

[65] Royal Society, *Proceedings A—Information for Referees*, http://rspa.royalsocietypublishing.org/site/misc/referees.xhtml, 2013 (accessed 1 Sept. 2013); and Royal Society, "Editorial Standards and Processes," *Royal Society Publishing—Policy and Ethics*, http://royalsocietypublishing.org/site/authors/policy.xhtml, 2013 (accessed 1 Sept. 2013). This practice may also have consequences for issues such as gender bias: a study of the journal *Behavioral Ecology* found that the introduction of double-blind peer review significantly increased the number of papers with women as the first authors. See Amber E. Budden, Tom Tregenza, Lonnie W. Aarssen, Julia Koricheva, Roosa Leimu, and Christopher J. Lortie, "Double-Blind Review Favours Increased Representation of Female Authors," *Trends in Ecology and Evolution*, 2008, *23*:4–6.

While the Royal Society maintains a fairly traditional approach to peer review and publication practices, there is rapid change elsewhere. The case of Hargreaves is interesting in this respect, as his article in the *Philosophical Magazine* appealed to physicists to interpret his work in light of recent experimental results. Perhaps Hargreaves would have benefited from a system of post-publication peer review, such as that used at *PLOS ONE*, an online open-access journal that publishes primary research from any scientific discipline. Here, each submission is reviewed purely on the basis of its technical proficiency, with no judgment about its value for the field. It is then the role of the readers to assess this value and make suggestions regarding the paper; these comments become part of the published record.[66] This is somewhat comparable to the approach at the Physical Society and *Nature*'s "Letters to the Editor" page in the early twentieth century, where a publication was intended to generate discussion. Many of the problems that post-publication peer review endeavors to solve have historical precedents in attempts to expand the peer-review process, allowing it to move beyond small networks. This development also addresses the problem of speed, a factor for 1920s physicists sending letters to *Nature* and for Joseph Larmor when sending papers to Lodge at the *Philosophical Magazine*.[67]

Peer review is currently in a fragile state, subject to considerable criticism and attempts to change drastically how it is implemented. But it appears necessary, in some form or other, as a method of evaluating the vast outputs of scientific research. We must place our trust in experts not merely to make publication decisions but also to appoint relevant experts, expanding the networks of controlling parties. In this essay, I have examined the role of such networks, considering the extent of their authority over the content of the two most prominent physics journals in 1920s Britain. At the Royal Society alone, multiple, overlapping networks were a crucial part of the publication process. In order for a paper to be considered in the first place, it had to be communicated by a Fellow. It would then be assessed by the much smaller network of James Jeans and the serving chair of the committee; from there it might also be appraised by a committee of nine and perhaps sent on to reviewers, handpicked from a chosen network of experts. At the *Philosophical Magazine*, the process was less formalized, but a paper either written or endorsed by somebody within the professional or personal networks of an editor would certainly attract careful consideration. While it was not entirely necessary for a physicist to be part of, or have access to, any of these networks, it was certainly beneficial, speeding up the publication process and ensuring a more positive reception of the work.

One's place in a network was not static, but carefully negotiated and constantly subject to change. Joseph Larmor built up power over a long, prestigious career, but during the 1920s this was waning. This was not the case in the context of his relationship with Lodge, who took Larmor's word very seriously and often without question. Lodge shared a similar approach to Larmor regarding the worth of modern—and, correspondingly, classical—physics, whereas many at the Royal Society did not. When submitting work to the *Proceedings*, Larmor found that the treatment he received was changing, that his classical approach was causing him to lose some of the power that his status had previously guaranteed. Authority within the discipline was thus negotiable over a person's lifetime—but I have also shown this to be the case within a single referee report. Eddington

[66] *PLOS ONE Guidelines for Reviewers*, http://www.plosone.org/static/reviewerGuidelines, 2013 (accessed 1 Sept. 2013).

[67] Baldwin, "'Keeping in the Race'" (cit. n. 9).

explicitly trusted Larmor on the subject of electrodynamics, a classical theory, but not on matters concerning modern relativity. In addition, while Eddington was happy to take certain portions of Larmor's work "on trust," he also feared that the wider scientific community might do the same. This was the motivation behind his suggestion that Larmor's paper be published in full, so that it could be critically assessed by the readers in a practice akin to modern-day post-publication peer review. Larmor's case shows that one's role in a network could be both powerful and fragile; once earned, it was not guaranteed for life. As the discipline changed, some gatekeepers were left behind.

Looking at the networks of the Royal Society and the *Philosophical Magazine* invites new ways of thinking about the discipline of physics during this period. The interactions between physicists we would now think of as classical and modern challenge the idea that there were two distinct categories of physicists in the 1920s. As I have shown, many classical physicists of an older generation were not rejected outright by modernists at the Royal Society but instead were treated very carefully and, where possible, often given the benefit of the doubt. In addition, there was a range of physicists involved in the administration of the *Proceedings*; they held different approaches to physics, and many of them were based at London institutions that favored physics with practical applications over the modern studies of the atom under way at Cambridge. Moreover, many physicists cannot be easily classified, as is clear in the case of Nicholson.

The place of the *Philosophical Magazine* in the landscape of physics publishing also points to complexities beyond a classical/modern divide. Here, Lodge gladly accepted papers that had been rejected by the Royal Society because they did not take modern developments into account; he also published numerous articles on the ether. In spite of this, Charles Galton Darwin still considered the publication worthy of his attention—unlike nearly all other British journals of physics. Furthermore, even while serving as Chair of the Royal Society's Physical Committee, O. W. Richardson also found the *Philosophical Magazine* to be of value. In 1925, he communicated to the Society a paper by William Edward Curtis, a King's College, London, physicist, entitled "New Series in the Secondary Hydrogen Spectrum." Despite Richardson's support, this paper was rejected by the committee, following Alfred Fowler's insistence that the time had come for "the publication of scraps about this spectrum [to] be discouraged." Richardson immediately sent the paper on to the *Philosophical Magazine*, where it was published, and he proudly stuck by his decision many years later.[68] The *Philosophical Magazine* and the *Proceedings* contributed to a complementary landscape of physics periodicals, where different journals fulfilled particular roles. With the *Philosophical Magazine* accepting a type of physics often rejected at the Royal Society, both classical and modern physics were able to coexist in the 1920s. When considering the state of physics in this period, the former cannot simply be dismissed. Just as peer review was not seen as the gold standard of academic publication in the 1920s, neither were the relativity and quantum theories viewed by all as the future of physics.

[68] Jeans to Richardson, 30 Nov. 1925, Richardson Papers; and William Edward Curtis, "New Series in the Secondary Hydrogen Spectrum," *Phil. Mag.*, 1926, Ser. 7, *1*:695–700. Reporting on a paper that also concerned the hydrogen spectrum, Richardson 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": Richardson to Smith, 14 July 1932, Richardson Papers.