A pre-history of 'peer review': refereeing and editorial selection at the Royal Society

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ABSTRACT

Despite being coined only in the early 1970s, 'peer review' has become a powerful rhetorical concept in modern academic discourse, tasked with ensuring the reliability and reputation of scholarly research. Its origins have commonly been dated to the foundation of the *Philosophical Transactions* in 1665, or to early learned societies more generally, without much consideration of the intervening historical development. It is clear from our analysis of the Royal Society's editorial practices from the seventeenth to the twentieth centuries, that the function of refereeing, and the social and intellectual meaning associated with scholarly publication, has historically been quite different from the function and meaning now associated with peer review. Refereeing emerged as part of the social practices associated with arranging the meetings and publications of gentlemanly learned societies in the late eighteenth and nineteenth centuries. Such societies had particular needs for processes that, at various times, could create collective editorial responsibility, protect the institutional finances, and guard the award of prestige. The mismatch between that context and the world of modern, professional, international science, helps to explain some of the accusations now being levelled against peer review as not being 'fit for purpose'.

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Introduction

> 'In one form or another, peer review has always been regarded as crucial to the reputation and reliability of scientific research.'
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> UK House of Commons committee on Science and Technology, 2011[1]

Public discourse on scientific and medical research places significant emphasis on the process known as 'peer review': it is seen as crucial to building the reputation both of individual scientists and of the scientific enterprise at large, and it is believed to certify the quality and reliability of research findings. It promises supposedly impartial evaluation of research, through close scrutiny by subject-specialists, and is widely used by journal editors, grant-making bodies and government. In recent decades, the effectiveness of peer review in both of these roles has been attacked, by those – particularly in the bio-medical sciences – who point to failures to detect error and fraud, and by those who identify inappropriate bias due to the social dynamics of the process.[2] The term 'peer review' was itself coined only in the early 1970s, but it 'has been elevated to a "principle" – a unifying principle' for widely diverse fields of research.[3] In all fields of academia, reputations and careers are now expected to be built on peer-reviewed publication; concerns with its efficacy and appropriateness thus seem to strike at the heart of scholarship.

In both public and scholarly discourse, peer review is routinely taken to be as old as the scientific enterprise, and its origins usually located at the Royal Society of London, in 1665,

[1] Science and Technology Committee, 'Peer review in scientific publications. Eighth Report of Session 2010-12', (London, 2011), p. 3.
[2] Since the early 1990s, the International Congress on Peer Review and Biomedical Publishing has drawn attention to the supposed failings of peer review, http://www.peerreviewcongress.org/index.html [accessed 12 May 2016]. Carole J. Lee, Cassidy R. Sugimoto, Guo Zhang and Blaise Cronin, 'Bias in peer review', *Journal of the American Society for Information Science and Technology*, 64 (2013), pp. 2-17 is a useful review of the wide variety of studies of peer review bias.
[3] Mario Biagioli, 'From Book Censorship to Academic Peer Review', *Emergences: Journal for the Study of Media & Composite Cultures*, 12 (2002), pp. 11-45, at p. 34. The *Oxford English Dictionary* gives 1967 for the first usage (in the context of the evaluation of hospitals), and 1971 for the academic usage.

with the *Philosophical Transactions*, the world's oldest scientific journal.[4] One of the most influential early studies of research evaluation was that published by Harriet Zuckerman and Robert Merton in 1971, and they recognised that that 'the referee system did not appear all at once' but 'evolved'. However, their discussion of the early Royal Society (reflecting Merton's earlier work on science in seventeenth-century England), followed by a leap to the twentieth century, has resulted in this paper being widely cited to support the invention of peer review in 1665.[5] We argue that this ahistorical treatment of peer review misunderstands both the nature of early modern editorial practice, and the significant ways in which editorial practice evolved in the three centuries after 1665, before 'refereeing' was rebranded 'peer review'.

Historians who have looked more closely at the history of editorial practices locate the origins of refereeing in the learned societies in the first half of the eighteenth century.[6] They suggest that refereeing then came to be used at (a few) independent scientific journals in the late nineteenth century, with widespread adoption occurring only in the later twentieth century.[7] Historians of science have recently begun to investigate surviving referees' reports, but these resources have so far been used to uncover the hidden dynamics of intellectual communities at a particular time and place, rather than to investigate long-term development.[8] An analysis of how and why learned societies should have felt it necessary to develop distinctive forms of editorial practice, including the use of referees and committees, is still lacking.

[4] See, for instance, the writings of senior publisher, Michael Mabe, and those that draw upon him, for instance Michael Mabe, 'Does journal publishing have a future?', in Robert Campbell, Ed Pentz and Ian Borthwick, eds., *Academic and Professional Publishing* (2012), pp. 413-40, at pp. 416-417. But see also Lee, Sugimoto, Zhang and Cronin, 'Bias in peer review'.

[5] Harriet Zuckerman and Robert K Merton, 'Patterns of evaluation in science: Institutionalisation, structure and functions of the referee system', *Minerva*, 9 (1971), pp. 66-100, at p. 68. For Merton's norms of science (including 'organised scepticism'), see Robert K Merton, 'Science and technology in a democratic order', *Journal of Legal and Political Sociology*, 1 (1942), pp. 115-26.

[6] David A. Kronick, 'Peer Review in 18th-Century Scientific Journalism', *Journal of the American Medical Association*, 263 (1990), pp. 1321-2.

[7] J C Burnham, 'The evolution of editorial peer review', *Journal of American Medical Association*, 10 (1990), pp. 1323-9; and Melinda Baldwin, 'Credibility, peer review, and Nature, 1945–1990', *Notes and Records of the Royal Society*, 69 (2015), pp. 337-52.

[8] Sloan Evans Despaux, 'Fit to print? Referee reports on mathematics for the nineteenth-century journals of the Royal Society of London', ibid.65 (2011), pp. 233-52; Melinda Baldwin, 'Tyndall and Stokes: correspondence, referee reports and the physical sciences in Victorian Britain', in Bernard Lightman and Michael S. Reidy, eds., *The Age of Scientific Naturalism: Tyndall and his Contemporaries* (London, 2014), pp. 171-86; Imogen Clarke, 'The Gatekeepers of Modern Physics: Periodicals and Peer Review in 1920s Britain', *Isis*, 106 (2015), pp. 70-93; Roberto Lalli, ''Dirty work', but someone has to do it: Howard P. Robertson and the refereeing practices of Physical Review in the 1930s', *Notes and Records of the Royal Society*, 70 (2016), pp. 151-74.

In this paper, we use the rich archives of the Royal Society to investigate the development of editorial practices at the *Philosophical Transactions*. We show that refereeing was one element within a wider set of practices which shaped the selection and evaluation of papers for publication; and we argue that the distinctive editorial practices of learned societies arose from the desire to create forms of collective editorial responsibility for publications which appeared under institutional auspices. We start in the seventeenth century and end in the twentieth, but our key focus is on what happened in between. We show how the Society transformed the *Transactions* from a periodical in the charge of a single editor into one run by a committee. We then show how that committee came to 'refer' papers to particular individuals for closer scrutiny, and how a practice that was informal (and oral) in the late eighteenth century turned into something routine, documented and written, justified by a need for expertise, in the nineteenth century. We structure the paper around three episodes when changes of editorial practice at the Royal Society were formalised in response to criticism of current practice: the move away from sole editorship (formalised in 1752); the use of expert referees (formalised in 1832); and changes to the broader gatekeeping processes (formalised in 1896).

We argue that refereeing and the associated editorial practices of the Royal Society were intended, initially, to protect the reputation of the eighteenth-century society; sometimes, to protect the society's finances; and, by the later nineteenth century, to award prestige to members of the nascent profession of natural scientists. Contrary to some modern claims for peer review, the committees and referees of the Royal Society were rarely concerned with anything that might be termed the 'reliability of scientific research'. Indeed, the Society formally distanced itself from any claims to be awarding 'the *imprimatur* of scientific authenticity' (as John Ziman would later describe refereeing[9]). By the early twentieth century, the growing professionalisation and internationalisation of scientific research was changing the dynamics and function of editorial processes that had developed in the context of a gentlemanly learned society in the late eighteenth and early nineteenth century. This mismatch between historical development and contemporary context may explain some of peer review's alleged failings.

[9] John M Ziman, *Public Knowledge: an essay concerning the social dimension of science* (Cambridge, 1968), p. 111.

Scrutiny in the Seventeenth Century

The first durable scientific societies emerged in the later seventeenth century: the Academia Naturae Curiosorum in Schweinfurt (1652), the Royal Society in London (1660), and the Académie Royale des Sciences in Paris (1666). These new and privileged spaces afforded (to varying degrees) official recognition and reward for enquiry into natural phenomena and processes, and new opportunities for collective discussion, comment and critique. The Royal Society's motto, 'nullius in verba', devised by John Evelyn and usually rendered as 'take no man's word for it', carried the implied promise that its Fellows would turn their critical gaze as ruthlessly upon each other as upon the rest of the learned world. The obvious and immediate manifestation of the Royal Society's collective basis was its weekly London meeting. In its early years, meetings involved both the devising and witnessing of experiments, and the critical discussion of experiments and observations reported by members and the natural philosophical community at large. The Society's role as a proving ground for early modern claims to natural knowledge is not in dispute, nor its significance as a space for free and open discussion. But it has been too rarely appreciated how distinct the practices of early Royal Society meetings were from the editorial practices of the *Philosophical Transactions*, run by Henry Oldenburg as a private venture. Letters from Oldenburg to Boyle in 1664 are cited as proof that Oldenburg was already envisioning the so-called 'four key functions' of the modern academic journal, yet those letters actually concerned the role of the Society and the function of its manuscript records, not the as-yet-unlaunched periodical.[10]

Close examination of Oldenburg's practices as editor of the *Transactions*, from 1665 until his death in 1677, reveal how different his role was from that of the modern scholarly journal editor. He did not receive submissions from authors and choose among them on the basis of intellectual merit, let alone engage in systematic consultation about those merits. Rather, Oldenburg worked hard to secure copy, drawing on his very wide correspondence with the learned men of Europe and his exceptional command of languages. He translated letters from foreign correspondents, often reporting natural-philosophical news at second or third hand. He made extracts and summaries of books, pamphlets and periodicals printed in England and on the Continent. He recombined communications or letter-excerpts from different correspondents to create articles on a shared topic. He incited correspondents working on

[10] Mabe, 'Does journal publishing have a future?'. This claim is repeated in 'Scholarly Communication and Peer Review: the current landscape and future trends', (London, 2015), p. 6.

related topics to criticise and respond to one another in print. He used its pages to promote the activity and interests of his patron and former employer, the Anglo-Irish chemist Robert Boyle.[11] This intense editorial activity reflects Oldenburg's interest in promoting communication, in making foreign material accessible to English scholars, and in earning a living.

This is not to say that Oldenburg was unconcerned with the quality of what was published; only that he articulated no clear set of standards, and only occasionally referred to any judgement other than his own. The sheer formal variety and the high degree of visible editorial mediation characterising items in the early *Philosophical Transactions* are almost in themselves proof that Oldenburg had no pretensions to an objective, systematic review process, since one of the effects of standardised modes of evaluation is to stabilise the form in which research is presented for ease of evaluation.[12] Most of what appeared in Oldenburg's periodical does not conform to a standard.[13] Nor can it be said to represent knowledge sanctioned by the Royal Society, since the *Transactions* was published under Oldenburg's independent control, and he was careful to distinguish between its contents and the activity of the Royal Society – a point missed by many of his contemporaries and some modern historians. Despite the Society's palpable approval of Oldenburg's project, in that early period, the research sponsored by the Society was published, not in the *Transactions*, but in separate books and treatises.[14]

Some historians have pointed to the Society's right – under its foundational charter – to license books for publication on its own authority as evidence of collective scrutiny and sanction.[15] This legal requirement entailed the perusal of a work prior to printing by at least two members of the Council and the approval of the Council as a whole, and was part of a wider mechanism of state censorship intended to ensure the proscription of politically seditious

[11] On Oldenburg's editorial practices, see Adrian Johns, 'Miscellaneous Methods: Authors, Societies and Journals in Early Modern England', *British Journal for the History of Science*, 33 (2000), pp. 159-86; Niall Hodson, 'Henry Oldenburg's *Philosophical Transactions* as a repository of translations', paper presented to *Publish or Perish* conference (Royal Society, March 2015).
[12] Alan G. Gross, Joseph E. Harmon and Michael S. Reidy, *Communicating Science: the scientific article from the seventeenth century to the present* (New York, 2002).
[13] Ellen Valle, ''Reporting the Doings of the Curious': Authors and Editors in the Philosophical Transactions of the Royal Society of London', in Nicholas Brownlees, ed., *News Discourse in Early Modern Britain: Selected Papers of CHINED 2004* (Bern, 2006), pp. 71-90.
[14] Noah Moxham, 'Fit for print: developing an institutional model of scientific periodical publishing in England, 1665–ca. 1714', *Notes & Records*, 69 (2015), pp. 241-60.
[15] Adrian Johns, *The Nature of the Book: print and knowledge in the making* (Chicago, 1998); Biagioli, 'From Book Censorship to Academic Peer Review'. Biagioli argues that the Royal Society in London and the *Académie royale* in Paris were incorporated into mechanisms of state oversight of the book trade.

or religiously heterodox material.[16] Mario Biagioli has suggested that the responsibility of licensing in both the English and French contexts simultaneously made the new scientific societies instruments of government, and thus made them communities of 'peers' in a legal sense. The associated burden, of policing works for seditious or heterodox material they were unlikely to contain in the first place, was largely notional, but created space, according to Biagioli, for an institution to turn the imprimatur into a means of defining what kind of science it approved of.

Between 1665 and 1708, the Royal Society licensed the publication of issues of the *Transactions*, and about fifty books. Pre-publication scrutiny was usually casual, and in the case of the *Transactions*, there are rarely traces of any at all. Furthermore, any simple conflation of early modern book censorship with the endorsement of intellectual claims is undermined by the Royal Society's own uncertainty about the implications of its privilege: it sought legal advice before using it for the first time in 1663.[17] On that occasion, the newly-chartered Society was eager to associate itself with John Evelyn's *Sylva* (1664), a practical treatise responding to a crown commission on the best way to secure the kingdom's supply of shipbuilding timber. But within a year, this precedent had become problematic: when the Society tried and failed to persuade Robert Hooke to omit some of the more speculative flights in *Micrographia* (1665), the Council insisted he include a disclaimer absolving the Society of responsibility for them. At the early Royal Society, licensing represented less an endorsement or an intellectual evaluation of particular research claims, and more a judgement of how far association with a given work would redound to the Society's credit.[18] Similarly, and despite Biagioli's plausible argument that licensing at the Royal Society was better at excluding than at selecting for specific, positive, criteria, there is no unambiguous evidence of any work being denied the imprimatur, nor of any intended contribution to the *Transactions* being rejected on the Council's say-so. The Royal Society's scrutiny for licensing purposes was, according to the best available evidence, neither rigorous nor systematic nor (strictly speaking) collective, since works were often licensed on the word of the presiding officer, apparently without debate.

[16] Peter Hinds, *'The Horrid Popish Plot': Roger L'Estrange and the Circulation of Political Discourse in Late Seventeenth-Century London* (Oxford, 2010).
[17] Thomas Birch, *The history of the Royal Society of London for improving of natural knowledge, from its first rise. In which ... papers ... hitherto not ... published, are inserted in their proper order, as a supplement to the Philosophical Transactions by Thomas Birch* (London, 1756-57), vol. 1 pp. 344, 346-7.
[18] For a fuller development of this argument see Noah Moxham, 'The uses of licensing: Publishing strategy and the imprimatur at the early Royal Society', in Mordechai Feingold and Giulia Giannini, eds., *Institutionalisation of sciences in early modern Europe* (Leiden, 2016 forthcoming).

It is, therefore, difficult to argue that the editorial and licensing mechanisms of the seventeenth-century *Philosophical Transactions* amounted either to a positively articulated protocol for choosing among particular knowledge-claims, or to a seal of collective approval establishing standards for natural-philosophical print. These distinctions were not always appreciated by contemporaries, and Oldenburg repeatedly had to deny that the *Transactions* was an official publication of the Society. After his death in 1677, the *Transactions* was edited for 75 years by the secretaries to the Society, and few observers recognised that they were doing so in a private capacity.[19] The editor's independence became increasingly difficult to sustain as the content of the *Transactions* became more straightforwardly identified with the activity of Society meetings.[20] This left the Society vulnerable to the imputation of failing to enforce adequate standards in the *Transactions*, yet with no obvious means of exercising control, and little hope of being believed when it tried to deny responsibility.[21]

1752: Editing by Committee

By the mid-eighteenth century, even though responsibility for the *Transactions* remained with individual editors, its contents derived entirely from meetings of the Society, and it had become the chief public manifestation of the Society's work. The inescapable link between the Society's reputation and that of the *Transactions* lay at the root of crucial statutory changes to the periodical's management in 1752. The coincidence of a new series of attacks on the Society and the *Transactions* at a time of difficult personal circumstances in the Society's leadership, resulted in a new model of collective editorship. This had the effect of tacitly incorporating a good deal of existing practice in the guise of a major change.

In the early 1750s, a failed candidate for the Fellowship, the botanist, actor and apothecary John Hill, launched a series of public attacks upon the Society, criticising the conduct of its meetings; the intelligence and character of its members in general (and of the President, Martin Folkes, in particular); and, most damagingly, the *Philosophical Transactions*. Hill took advantage of the perceived association between the Society and the periodical to dredge up, and mock, weak papers dating all the way back to 1665. Hill's critique was satirical

[19] All but one of the editors were (honorary) secretaries; the exception was Edmond Halley who, during his first stint as editor (1686-92) was the Society's paid clerk.
[20] Moxham, 'Fit for print'.
[21] There were, for instance, satirical critiques on the *Transactions* in 1700 and 1711: see Joseph M. Levine, *Dr. Woodward's Shield: History, Science, and Satire in Augustan England* (Ithaca, 1991). Some Fellows thought that it was futile to keep denying responsibility; for instance, see John Harris to Sir John Hoskins, 27 February 1700, British Library Sloane MSS 4026 f. 254.

as much as it was philosophical and he had particular fun excoriating the self-evidently absurd or trivial. He solemnly proposed, for instance, a string of escalatingly ludicrous improvements to a 1703 paper on a Ceylonese technique of hunting waterfowl that involved the hunter wading into the water up to his neck with a clay pot over his head, and pulling the bird under by the feet.[22] In other cases Hill objected to the space and precedence granted to minor natural-historical observations by people he despised as cronies of the President, and in still others he raised more substantive criticisms. In each instance, however, the basic force of the critique came from his ability to amplify and exploit the assumption that everything published in the *Transactions* had in some way passed the Society's scrutiny, and that the Society was therefore intellectually responsible for the contents. Hill cemented the perceived link by calling his critique *A Review of the Works of the Royal Society* (1751).

Shortly after Hill's attack, Cromwell Mortimer, the editor (and Secretary of the Society), died suddenly. Combined with the long-term incapacity of the President, this afforded the Council an opportunity to reform the existing system without making scapegoats of its official leadership. Their response was to enact precisely the kind of collective editorial responsibility that Hill had insinuated. In January 1752, the Royal Society took on both financial and editorial management of the *Transactions*. From this point on (until 1990), the *Transactions* officially had no editor. The members of the Council acted as a Committee of Papers, charged with deciding collectively which of the papers communicated to the Society to publish.

This was not a necessary or obvious step. Individual decision-making by editors, or even groups of editors, was a widespread and successful model for editing a periodical, and by assuming financial responsibility, the Society's Council acquired a means to control any editor it appointed.[23] By involving more people in the editorial process, the Society protected itself from the incompetence or idleness of individual editors; and by making decisions through committee voting, it protected the President and Secretaries from *ad hominem* attacks on their judgement. These processes for collective editorial responsibility would deflect many of Hill's criticisms, though the Society never officially acknowledged Hill's attack.

[22] John Hill, *A Review of the Works of the Royal Society* (London, 1751), pp. 9-11. The original paper was published in *PT* **72** (1703), pp. 1094-96, as 'Observations made in the island of Ceilan...by Mr Strachan'.
[23] On individual editorial responsibility, even within teams, see David A. Kronick, 'Authorship and authority in the scientific periodicals of the seventeenth and eighteenth centuries', *Library Quarterly*, 48 (1978), pp. 225-75.

The new statutes of 1752 laid down that the Committee should consider all papers communicated to the Society, in the order in which they had been read at meetings.[24] At each committee meeting, the members were furnished with abstracts of the papers, usually between 500 and 2,000 words in length, and were supposed to reach their decision by secret ballot without discussion.[25] This contrasts with the practices of both the Parisian Académie Royale, where the *rapporteurs* assigned to consider submissions by outsiders were expected to produce jointly-authored reports, and of the Royal Society of Edinburgh (f.1783), whose statutes would explicitly allow conversation about the merits of the papers.[26] The Royal Society of London's system sought collective judgement not by seeking consensus, but on the basis of a group of equally-weighted individual judgements. The 'no discussion' rule was avowedly intended to prevent the committee decision from being unduly swayed by any particular individual. How this system worked in practice is hard to assess, since some of the rules of procedure were apparently contradictory or, at best, deeply impractical. In particular, it is difficult to see how the decision to postpone particular papers, apparently because the Committee members gathered on that particular occasion did not have the appropriate expertise, could have been arrived at in silence.[27] Thus the post-1752 decision-making process remained opaque to outsiders, but the written procedures gave the appearance of probity, and produced judgements that were hard to contest.

The practice of the Committee of Papers seems on balance to have been more concerned to weed out unsuitable papers than to proactively select the best for publication. In the decades around 1800, around 65% of papers read to the Society were later published in some form in the *Transactions*.[28] Editorial practice was governed by several factors: first, the question of whom the periodical should most benefit; second, the relationship between the Society's meetings and papers; and third, the Society's established reluctance to adjudicate claims to knowledge. According to the founding editorial statement of the post-1752 *Transactions*, it was officially to be run 'for the sole use and benefit of this Society', a statement with a range of possible meanings covering reputational or financial benefit to the institution as a whole, or

[24] Royal Society Council Minutes Original (henceforth RS CMO), vol. 4
[25] Royal Society Committee Minute Book (henceforth RS CMB) vol. 90/1.
[26] James McClellan III, 'Specialist Control: The Publications Committee of the Académie Royale des Sciences (Paris), 1700-1793', *Transactions of the American Philosophical Society*, 93 (2003), pp. 1-134; Minutes of Proceedings of the Royal Society of Edinburgh, 17 July 1784, National Library of Scotland, Acc.10000/1.
[27] The minute book of the Committee of Papers (RS CMB/90/2, 1780-1820, *passim*) shows that it was common for two or three papers at each meeting (and not just the last on the agenda) to be deferred.
[28] From our examination of the minute books of the Committee of Papers, RS/CMB/90/2.

utility to the fellows. We have already seen how, by imbuing its publishing decisions with collective authority, the Society aimed to protect its reputation and to shield individuals from external criticism. It is equally clear that the Society did not benefit financially from the takeover.[29] More copies of each issue were reserved for the fellowship than were put on general sale, making profit unlikely, and from 1799 onward, impossible (due to a new method of setting the sale price).[30] One obvious respect in which the *Transactions* constituted a benefit to the fellows, many of whom could not or did not attend meetings in London, lay in giving the wider fellowship access to the matters communicated at meetings. The value of the periodical to such an audience lay rather in being broadly representative than in showcasing the very best papers. It is also relevant that the volume of papers passing through the Society meetings was not yet large enough to put pressure on the amount of paper and print that the Society was willing to pay for. The *Transactions* had no page limits, and its length could vary to suit each session's crop of papers.

Second, the relationship between the *Transactions* and the meetings of the Society is key to understanding the editorial practice: although the procedures of the Committee of Papers provided a semi-public justification for the Society's publication decisions, they masked the fact that the main filtering of papers had, in fact, occurred (silently) much earlier.[31] Papers would only be presented at a meeting of the Society if 'communicated' (in effect, vouched for) by a fellow. This early gate-keeping was often the point at which obvious self-refuting nonsense, such as proposals for perpetual motion machines and squaring the circle, was weeded out, but it also gave the selection of papers a social dimension. The Society's existence as a gentlemanly association introduced a degree of delicacy into its publication protocols: to refuse a paper was to imply a criticism of the judgement of the Fellow who had communicated it in the first place. More broadly, if the Committee routinely declined to publish many papers, it ran the risk of implying that the meetings were filled with material too dull or too weak to appear in print; this would have risked alienating both the scientifically active Fellows, and the gentlemen who made up the bulk of the dues-paying membership.

[29] Aileen Fyfe, 'Journals, learned societies and money: Philosophical Transactions, ca. 1750–1900', *Notes and Records of the Royal Society*, 69 (2015), pp. 277-99.
[30] RS CMO/8 pp. 152, 172, envisaged a break-even point of 600 sales of an edition of 900, yet no more than 350 copies were actually available to be sold publicly.
[31] Aileen Fyfe and Noah Moxham, 'Making Public ahead of Print: Meetings and Publications at the Royal Society, 1752-1892', *Notes and Records of the Royal Society*, 70 (2016 forthcoming).

This delicate balance was usually negotiated ahead of meetings, rather than at the (post-meeting) committee stage. The Society was not obliged to grant time at a meeting to every paper submitted to it, and decisions were in the gift of the President and officers. The protocols for deciding what would feature at meetings remained, to outsiders, dauntingly opaque; much depended on the interests and prejudices of the individuals concerned, especially during the presidency of Joseph Banks (1778-1820). The President sometimes informally sought a second opinion on the intellectual merits of a paper, but was under no obligation to follow the advice he received. Thus, in 1796 Banks unilaterally turned down Edward Jenner's description of his technique of vaccination against smallpox, despite positive evaluations from two medical colleagues. Banks appears to have been more concerned about the reputational risk to the Society of appearing to promote a treatment that might not live up to the claims made for it.[32] Surviving correspondence and diaries from the late eighteenth century demonstrate that such unofficial consultations were common, both before and after a paper was formally read to a meeting.[33]

The third significant factor governing the broadly inclusive tendency of the Society's editorial dispensation was its habitual reluctance to appear to be endorsing the truth of what was contained in the *Transactions*. What was published appeared in good faith, but the Society admitted no ultimate responsibility for the claims advanced in the periodical. Thus, while reputational control demanded that trivial papers not be published, anything else of interest might be. In an 'advertisement' printed at the front of every part of *Transactions* from 1752 until 1959, the Society explicitly distanced itself from the types of judgements contained in the official reports on patents and discoveries produced by the Paris Académie.[34] It insisted that 'it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any subject, either of Nature or Art, that comes before them'. Appearing in the *Transactions* signified only the committee's collective recognition of 'the importance and singularity of the subjects, or the advantageous manner of treating them', and should in no way be taken to imply that the Society answered 'for the certainty of the facts, or propriety of the reasonings …, which must still rest on the credit or judgment of their respective authors'.[35] This refusal to adjudicate the epistemic status of claims to knowledge was already

[32] Jenner's paper thus appeared as a privately printed pamphlet. See Richard B. Fisher, *Edward Jenner, 1749-1823* (London, 1991).
[33] Fyfe and Moxham, 'Making Public ahead of Print'.
[34] *The Record of the Royal Society: supplement 1940-1989* (London, 1992), PAGE.
[35] e.g. 'Advertisement', *Philosophical Transactions* 64 (1774), iii-iv.

apparent in the discussion about licensing Hooke's *Micrographia* in 1664. By denying that it made public epistemic judgements, the Society avoided tying its reputation to any particular knowledge-claim, but also sought to prevent unscrupulous authors and projectors from using the Society's name for their own advantage. The 1752 advertisement complained about the practice of claiming in the 'public news-papers' that particular 'projects, inventions, and curiosities' had 'met with the highest applause and approbation' from the Royal Society. Such acknowledgements as were tendered, the council claimed, were no more than 'a matter of civility', and to presume they meant anything more was potentially 'to the dishonour of the Society'.[36]

This helps explain why a committee-based editorial system, which could have been used as a way of expressing the collective, corporate opinion of the fellowship as a whole, actually sought to *prevent* its judgements from being read that way. The *Transactions* was not supposed to be a repository of officially-sanctioned knowledge, but of interesting or intriguing phenomena that were worthy of further consideration. This remained the official understanding of the meaning of the editorial process until the mid-twentieth century, although, in practice, it would shift significantly with the introduction of referees, and, in particular, with the introduction of a second Society periodical.

1832: Referees

In the late 1820s and 1830s, the Royal Society was, once more, facing criticism. This time, it came from a group of younger fellows, disenchanted with the Society's conduct under the recently-deceased Joseph Banks. Influenced by developments in France, the reformers sought to turn the Society into a smaller, more elite organisation, made up of members with active research interests, more like the Paris Académie, yet still voluntary. As Charles Babbage's *The Decline of Science in England* (1830) made clear, the reformers treated publication in the *Transactions* as an indication of the author's suitability for membership of such an organisation.[37] This approach had the potential to change the meaning of publication decisions, which would no longer merely imply that a published paper would be of some interest to readers, but would be a positive recommendation of the author as a man of science.

[36] Ibid.
[37] Charles Babbage, *Reflections on the Decline of Science in England* (London, 1830), pp. 33-4. See also Marie Boas Hall, *All Scientists Now: The Royal Society in the Nineteenth Century* (Cambridge, 2002), Ch. 3.

With individual reputations to rest on publication, the reformers proposed changes to the editorial system. *Science without a Head* (published anonymously by Augustus Bozzi Granville in 1830) argued that the increasing specialisation of scientific research meant that the Committee of Papers, limited by statute to twenty-one members and whose meetings were seldom fully attended, was not qualified to decide the fate of the wide variety of papers received by the Society.[38] In 1827, several of the reformers had served on a committee whose report emphasised the need for the committee to have 'sufficient time… to examine [papers] carefully' and to communicate directly with the authors when necessary, implicitly critiquing the habit of relying on abstracts of papers, and voting with no discussion or opportunity for revision.[39] In his anniversary address as President, in November 1832, the Duke of Sussex announced in his presidential address to the Society that, for almost a year, the Committee of Papers had now been approving papers for publication in the *Transactions* only if 'a written report of its fitness shall have been previously made by one or more members of the Council, to whom it shall have been especially referred for examination'.[40]

The series of referees' reports in the Royal Society's archive runs continuously from 1832; and the Society nowadays proudly cites 1832 as the invention of refereeing. However, there were precedents. The 1752 statutes allowed the Committee of Papers to summon any other fellow, who was 'knowing and well-skilled in the particular branch of Science', to deliver an opinion of a paper on whose merits the Committee felt itself unqualified to decide.[41] The surviving minute books contain few records of referrals under this statute: five between 1780 and 1815, and not many more thereafter. Yet Granville, having used his Fellow's privilege to examine these same records in 1830, nonetheless asserted that 'every communication is supposed to have been previously […] referred to the judgment of some competent member who reports his opinion'.[42] Granville's confident assertion suggests that there was assumed to be, and may actually have been, far more use of oral reporting at the Committee of Papers prior to 1832 than either the statutes required, or the minute-books recorded. Granville's testimony, and the scant evidence for earlier instances of 'referring' papers, suggests that the Committee

[38] [Augustus Bozzi Granville*, *Science without a head; or, The Royal Society dissected. By one of the 687 F.R.S.* (London, 1830), p. 122.
[39] Committee for Limiting the Fellowship, reported to Council on 11 June 1827, RS CMB/1/20/2, pp.167-8, quoted in Babbage, *Decline of Science in England*, p. 164
[40] Frederick Augustus Duke of Sussex, '[Presidential Address 1832]', *Abstracts of the Papers printed in Philosophical Transactions of the Royal Society of London*, 3 (1830-1837), pp. 140-55, at p. 141. Although Sussex claimed this trial had been resolved by Council, there is no mention of it in the Council minute books.
[41] *Diplomata et Statuta Regalis Societatis Londini* (London: Samuel Richardson, 1752), p. 109.
[42] Ibid., p. 54.

customarily worked around the statutory provisions for calling upon outside help and forbidding discussion in meetings. Furthermore, in the few recorded instances, papers were usually referred to someone already on the Committee of Papers, indicating that, in late eighteenth-century practice, 'referring' was used to ensure that someone had actually taken the time to scrutinise the full paper (not just the abstract), rather than to bring in additional expertise.

According to the Duke of Sussex, the Royal Society's 1832 move to (mostly) written reports was in emulation of 'many Foreign Societies', but particularly the Parisian Académie, which required 'written Reports… from a Committee of their Members'. He claimed the key virtues of the French reports were, first, that they expressed the judgment of 'veterans… who have earned by their labours an European reputation', and second, that they were made public. Those sitting in judgment had 'an authority sufficient to establish at once the full importance of a discovery, to fix its relation to the existing mass of knowledge, and to define its probable effect upon the future progress of science', and their public reports were 'often more valuable than the original communications upon which they are founded'.[43] By continuing to use only members of Council as referees, the Royal Society in 1832 was imitating this top-down model of evaluation; and it further imitated the French by making some of the reports (those 'of a favourable nature') public at Society meetings and in print. As in France, some of these early reports were collaborative, with referees expected to reach consensus and issue a joint report. As Alex Csiszar has shown, this quickly proved problematic, especially when referees disagreed about both the paper's precise merits and the purpose of their report.[44] Within a year, the Society abandoned both the requirement of a joint verdict and the publishing of reports. Written refereeing continued, but the referees henceforth reported independently and their reports (and names) were treated as confidential.[45]

Why did the 1832 experiment with open, collaborative refereeing fail? Sussex acknowledged that it would call for 'the occasional sacrifice both of time and labour'.[46] And codes of politeness meant that reports were only ever published when the referees felt able to offer ringing endorsements. It was potentially an excellent way of pointing up and even adding

[43] Ibid., 142.
[44] Alex Csiszar, 'Peer review: troubled from the start', *Nature*, 532 (2016), pp. 306-8. For the French model, see McClellan III, 'Specialist control'.
[45] There is an 1835 exception.
[46] Sussex, '[Presidential Address 1832]', p. 142.

value to outstanding papers, but a significant waste of 'time and labour' if the paper were bad or merely mediocre. Publishing a reasoned and expert justification of a paper could also have been seen as running counter to the 1752 'advertisement' that the *Transactions* still carried.

One way to lessen the load of refereeing was to spread it more widely among the fellowship. From 1833, various *ad hoc* subject committees were established to adjudicate the award of the Society's Royal Medals, and these committees rapidly assumed an editorial function. From 1838, they were formally established as Sectional Committees (each covering a different disciplinary field), and charged with delivering recommendations to the Committee of Papers about what to publish and what not. For the next decade, these Sectional Committees sometimes came to a collective decision amongst themselves, and sometimes referred papers to one or two individual members. The committee members thus became a pool of subject-specialist referees, involving a wider circle of fellows in decision-making, and potentially deflecting criticism aimed at a Council clique.

It is clear that, during the 1830s and 1840s, the way refereeing fitted into editorial practices had not yet standardised. The number of referees varied, reports were not necessarily delivered in writing, and varied from single sentences to twenty closely-written pages. Referees were unsure whether they were to offer criticism and suggestions, or even a positive recommendation about whether or not to publish.[47] Those who offered a recommendation were not necessarily dogmatic about it: in June 1833, one referee sent a letter full of criticisms of David Brewster's paper on the crystalline structure of the eye, but was happy to leave it to his fellow referee to 'draw up such a report as you think necessary for the occasion, and on your better judgement I shall most willingly rely'. (The paper was published.)[48] In some cases we have only one surviving report for a paper, in others two; in some cases the two referees agreed on a joint decision, and in others they submitted their reports separately. It was up to the Sectional Committees or the Committee of Papers to make sense of the form in which the reports happened to be submitted.

In early 1831, the Royal Society had also created a new periodical, which changed the perceived role of the *Transactions* and the refereeing process associated with it. The *Proceedings* was issued monthly during the Society's session, in contrast to the twice-yearly

[47] Some preferred not to make a positive recommendation: see Thomas Wharton Jones's 24 June 1841 report on a paper by JM Ferrall, RS Referees' Reports (henceforth RS RR) 1/64.
[48] RS RR/1/30 and 31.

parts of *Transactions*. It reported on each meeting of the Society, including lists of gifts received, elections of new Fellows, and annual reports, as well as summaries of the papers read.[49] By 1833, the initial *Proceedings* print run of 750 copies (enough for the fellowship, plus a hundred more) had been increased to 1500.[50] *Proceedings* thus assumed the function of representing the Society's meetings to the fellowship and to the wider public.

The post-1831 *Transactions* thus became more selective in what it published: by the 1850s, *Transactions* published only around 30% of the papers submitted to the Society.[51] The more systematic use of referees, which had been introduced shortly after the launch of *Proceedings*, was specifically for the *Transactions*. Only around half of the papers communicated to the Society were sent to referees for possible consideration for the *Transactions*, indicating that some pre-selection was being done by the Committee of Papers. From early on, referees were drawing distinctions between papers suitable for the *Transactions* and those that might be adequately reported in short form in the *Proceedings*. Reports recommending publication in the *Transactions* frequently commended scope, originality and significance, much the same evaluation criteria as those advocated by the Duke of Sussex in his 1832 address.

The greater attention paid to decisions for *Transactions* – as evidenced by the use of refereeing – suggests that publication in *Transactions* carried greater consequences for the Society. With the 1752 advertisement still in place, there was no endorsement of the knowledge claims put forward in either periodical. But a *Transactions* paper represented a financial commitment from the Society (because these papers were lengthy and well-illustrated), and a mark of prestige for both the Society (because of the glory potentially reflected on the Society for having recognised and published important research) and the author (from 1840, authorship of a paper in *Transactions*, but not *Proceedings*, was seen as sufficient evidence of scientific merit to justify a discount on the life membership fee for fellows).[52] Given that the pool of

[49] Council resolved to print abstracts of the papers read at meetings on 16 December 1830 (RS CMO/12 pp. 144-6); the first issue covered the meetings of 18 November to 16 December 1830; though the date entry on the Royal Society's account for the first issue does not appear in the printer's records until 25 February 1831: Taylor and Francis Journal (St Bride's Library) 1830-40. The issues of the new periodical were titled *Proceedings of the Royal Society*, but the early bound volumes have a title page *Abstracts of the papers printed in the Philosophical Transactions* for continuity with the retrospective series of abstracts covering 1800-1830.

[50] Taylor and Francis Journal (St Bride's Library) 1830-40, 21 March 1833. The *Transactions* print run at the time was 1,000.

[51] From our analysis of the RS Register of Papers, MS/421.

[52] Fellows could either pay annual fees, or 'compound' their future fees by paying a hefty £60; this 'compounded' fee was reduced to £40 for those with a *Transactions* paper, see *The Record of the Royal Society of London* (London, 1912), p. 170. This practice (with its bias towards *Transactions* as the only eligible form of publication) was discontinued in 1887, Ibid., p. 275.

papers deemed worthy of being read at a meeting could now be seen in *Proceedings*, the publication decisions for *Transactions* could potentially be scrutinised as never before. The refereeing process could be seen (internally) as protecting the Society's reputation and finances and (externally) as mechanism for generating expert evaluation of research, thus justifying prestige.

After the variety of practice in the 1830s and 1840s, the Society's refereeing practices stabilised. After the sectional committees were disbanded in 1847, amidst a scandal over the award of a Royal Medal, referees were drawn from the entire fellowship. Papers were usually sent to two referees, one after the other, to save the labour of recopying what might be a very substantial manuscript. Although papers as published in *Transactions* were supposed to be substantively the same as when read to the Society, referees often recommended stylistic changes: flabby introductions and overly speculative conclusions were vigorously targeted for cutting.[53] Referees' enthusiasm for offering such suggestions, often at great length, may reflect the long-standing tradition of sociability and collegiality associated with Royal Society meetings: the written report permitted referees to respond at more considered length than was possible at a meeting, as well as enabling distant fellows to engage with the research presented at the London meetings. Thanks to the Victorian postal system, William Thomson was one of the most active referees in the 1860s and 1870s, despite being based in Glasgow. (His colleague W.J.M Rankine was also active, as was Henry Roscoe in Manchester, and many fellows based in London, Oxford and Cambridge.) This improving-and-mentoring function for refereeing was cultivated by long-serving secretary George Gabriel Stokes (1854-85). Stokes mediated between author (or communicator) and referees, passing on the official decision and usually sharing some of the referees' remarks.[54] By 1894, a guidance letter codified the dual role now expected of referees, advising that 'the guidance [for] the Committee of Papers' be kept 'separate from any detailed criticisms, or suggestions intended to be communicated to the author'.[55]

Once the brief experiment with published referees' reports ended, referees' identities and reports were kept confidential, just as Joseph Banks had always done with the informal

[53] For more detailed discussion of refereeing practices in this period, see Despaux, 'Fit to print?' and Baldwin, 'Tyndall and Stokes'.
[54] Baldwin, 'Tyndall and Stokes'. Julie McDougall-Waters, 'Peer review in the nineteenth century', paper presented to *Publish or Perish* conference (Royal Society, March 2015).
[55] RS CMP/7, 6 December 1894.

advice he received.[56] Thus, the 1894 guidance allowed referees to request that their comments be transcribed before forwarding to the author.[57] The secrecy of this process occasionally led to complaints, and in 1878, one rejected author had railed against 'accursed… Secret Committees, secret members, [and] secret judgements'. Yet he admitted that he had been told the gist of the referees' complaints, and although the secretary refused to reveal the referees' names, the fact that they were fellows of the Society meant that their credentials were to some extent known.[58] Authors, on the other hand, were not permitted to be anonymous, because the Society wished to be able to evaluate the credentials (social and intellectual) of its contributors.

By the late nineteenth century, the Royal Society had a well-established set of editorial practices, with referees consulted specifically for expensive and high-prestige publication. The fact that refereeing was not deemed necessary for selecting papers to be read at meetings, or for short-form publication in the *Proceedings*, suggests that the long-standing, tacit and social processes for winnowing papers ahead of meetings – which relied on the judgement of the Fellows acting as 'communicators' and those serving on the Committee of Papers (i.e. Council) – were still felt to be working adequately well. By the 1890s, however, these gate-keeping practices were under pressure from finances and from the shifting demographic of what had become the scientific profession.

1896: Gate-Keeping

In his anniversary address in November 1896, Joseph Lister, then President of the Royal Society, introduced a major overhaul of the Society's procedures. Lister explained the changes were intended to 'increase the interest of the meetings' and to achieve a 'greater rapidity in the publication'. The first aim would be achieved by reading a limited subset of the papers received, thus freeing up time at meetings for commentary and discussion. Secondly, new 'Sectional Committees' were to be 'entrusted' with 'reviewing the communications' received by the Society. By delegating the initial editorial evaluation to men knowledgeable in the

[56] Banks was happy for the substance of the comments to be passed on but insisted that authors should not be given the referee's exact words nor his identity. See David Philip Miller, 'The usefulness of natural philosophy: the Royal Society and the culture of practical utility in the later eighteenth century', *British Journal for the History of Science*, 32 (1999), pp. 185-201.
[57] RS CMP/7, 6 December 1894.
[58] C. Piazzi Smyth, 'Solar Science at the pleasure of *Secret* Referees' *Nature* April 13, 1878, pp. 468-469. Smyth was the Astronomer Royal for Scotland and had previously resigned as a Fellow of the Royal Society.

distinct sections of knowledge, Lister hoped the committees would produce 'a more secure, and, at the same time, more rapid judgment as to the value of communications'.[59]

These restored Sectional Committees and their chairmen became the *de facto* guardians of the editorial process, though the Secretaries and Council retained ultimate responsibility. The committees organised referees for papers being considered for the *Transactions*, and they provided input into decisions about publication in *Proceedings* and selection for Discussion Meetings. The committees did most of their business by correspondence, often meeting in person just once a year; and it was the six chairmen who took on the bulk of the editorial work. Each chairman looked after his 'section' of natural knowledge, reporting to either the Physical Secretary or the Biological Secretary, as appropriate; and the papers recommended by his committee would appear alongside those recommended by the other committees in *Proceedings* or *Transactions*.[60] There is no evidence of any efforts to achieve a balance between subjects. Thus, in some respects, the Sectional Committees and their chairs might be seen as the editors and editorial boards of separate journals, linked only by the shared institutional branding of the printed product. Perhaps unsurprisingly, adding an extra layer of bureaucracy did not speed up decision-making: the median time taken from receipt of a paper to decision rose from 62 days in the decade 1865-75 to 100 days in 1905-1915.[61]

Despite the changes in management, the practice of refereeing continued largely unaffected through the 1890s. The new 1894 letter of guidance for referees codified the intellectual distinction between *Proceedings* and *Transactions* that referees had been working with for decades, stating that while every paper published by the Society should be 'of such a character that it should be accepted', those for the *Transactions* should 'mark a distinct step in the advancement of Natural Knowledge'.[62] Publication in the *Proceedings* was still seen as more routine: 'short' papers (of less than twelve pages) and abstracts could be printed there on the authority of the Secretary and the Chairman of the relevant Sectional Committee, without necessarily consulting the other members of the committee.

[59] Joseph Lister, 'Address of the President', *Year-book of the Royal Society, 1896-1897* (London, 1896), pp. 119-37, at 124. Lister made no mention of the earlier incarnation of Sectional Committees.
[60] From 1887, *Transactions* was issued in two series, A for physical sciences and B for biological sciences. *Proceedings* was split in 1905.
[61] From our analysis of RS Register of Papers, MS/421-422.
[62] RS CMP/7, 6 December 1896.

There was, however, one newly prominent aspect to the refereeing process: money. In spring 1894, John Evans, the Society's Treasurer, had reported to Council on 'the difficulties in which we are placed' due to the soaring cost of the publications.[63] The cost of producing the publications had risen steeply in the early 1890s, and Evans feared it would become unsustainable.[64] He therefore made a series of recommendations to Council, the brunt of which was tighter editorial controls. He wanted limits on the length of individual papers and the cost of the accompanying illustrations, and greater scrutiny of all submissions at an earlier point in the process. The Council's response was initially lukewarm, but it eventually passed a resolution approving the limits on pages and illustrations for *Transactions*, with loopholes that would be regularly exploited.[65] And the Sectional Committees created in 1896 would assist with the earlier scrutiny.

The financial concerns were clear in the new guidance to referees, who were now asked specifically about length and illustrations. Should papers 'be published in full or in an abridged form'? Should 'any portions be omitted as being unnecessary' (or as 'liable to give offence')? And, most explicitly, could the illustrations 'be reduced in number or extent without actual injury to the paper, with a view to economy?'[66] Referees had, from time to time, suggested possible cuts for economic reasons, but the scale of the underlying problem was new and was not resolved by the Treasury grant-in-aid of publications, first awarded in 1895.[67] Thus, new procedures for the Committee of Papers in 1896 specified that it was to consider estimated costs alongside the referees' reports, and from 1907, referees were also informed of the estimated costs.[68] Evans's memorandum of 1894 had thus inaugurated a practice of weighing financial implications against intellectual merit, though how referees were expected to do this remained unclear.[69]

[63] RS CMP/7, 26 April 1894.
[64] Fyfe, 'Journals, learned societies and money'.
[65] RS CMP/7, 6 December 1894. The limits were 40 pages quarto for papers in *Transactions*, and no more than £35 of illustrations; but exceptions were allowed if agreed on two separate occasions by the Committee of Papers.
[66] RS CMP/7, 6 December 1894.
[67] For example, RS RR/1/71 and 72, in which W.H. Allen and Charles Daubeney respectively suggest that a paper by the Edinburgh geologist JD Forbes ought to be abridged. On the grant (different from that for supporting scientific research), see Fyfe, 'Journals, learned societies and money'.
[68] RS CMP/7, 21 May 1896. Committee of Papers report, 24 Oct 1907, CMB/90/6. The practice of sending costs appears to have lapsed during the war, but had resumed by the 1920s.
[69] By the 1960s, the estimate of costs had been replaced by an estimate of pages and images, which may have been an easier measure for referees to evaluate. For a different instance of evaluating a mixture of intellectual and commercial issues, consider the publishers' readers discussed in Sylvia Nickerson, 'Referees, Publisher's Readers and the Image of Mathematics in Nineteenth Century England', *Publishing History*, 71 (2012), pp. 27-67.

The role referees played as stewards of the Society's finances helps us to understand an otherwise puzzling element of the editorial history of *Proceedings*: If refereeing were simply about judging merit – whether a paper was 'fit and proper' for a Royal Society periodical – then why did refereeing not become standard practice for *Proceedings* when, in 1914, its papers were, despite their shorter length, granted equivalent intellectual status to those *Transactions*?[70] One key difference was that the page limit for *Proceedings* papers was more rigorously enforced. Even with an increase to 24 pages, that constraint meant that the financial implications were limited. The 40-page limit for *Transactions*, on the other hand, was routinely breached, and the Committee of Papers sometimes approved papers of more than a hundred pages plus images.[71] Thus, the financial implications of approving a paper for *Transactions* were far more variable (and potentially far higher) than for *Proceedings*. Using referees would have acted against the Society's desire for *Proceedings* to publish papers rapidly; and the Society appears to have been content that a lighter-touch editorial regime was still adequate for *Proceedings*.

Nonetheless, the involvement of the Sectional Committee chairmen – in addition to the communicator and the Secretary – suggests some concern about whether the Society's long-established gate-keeping procedures were completely adequate. By the early twentieth century, only 12% of papers submitted to the Society appeared in *Transactions*, while around 75% were published in *Proceedings*.[72] A large element of the Society's public reputation thus rested on the *Proceedings* and those who controlled access to its pages. The requirement that papers be 'communicated' by a Fellow acted as a filter, both social and intellectual, on submissions, and helps to explain the low overall rejection rate for papers received by the Society. But the growing number of scientific researchers, combined with the more restrictive admissions policy that the Society had been operating since 1847, resulted in an increase in the number of papers communicated on behalf of a non-Fellow. Such papers had accounted for barely 40% of submissions in 1865-75 but had risen to over 60% by the early twentieth century.[73] A Fellow

[70] From 1914, the difference was notionally nothing more than page length and number of illustrations. RS CMP/10, 21 May 1914, clauses 36 and 52. For a more detailed discussion of editorial practice at *Proceedings*, see Clarke, 'Gatekeepers of Modern Physics'.
[71] The average length of a paper in *Transactions* in the 1910s was 44 pages; but for 112 pages of comparative anatomy, see Elizabeth A. Fraser and J. P. Hill, 'The Development of the Thymus, Epithelial Bodies, and Thyroid in the Marsupialia. Part I. Trichosurus vulpecula', *Philosophical Transactions of the Royal Society of London B: Biological Sciences*, 207 (1916), pp. 1-85; Elizabeth A. Fraser, 'The Development of the Thymus, Epithelial Bodies, and Thyroid in the Marsupialia. Part II. Phascolarctos, Phascolomys, and Perameles', ibid., pp. 87-112.
[72] From our analysis of the RS Register of Papers, RS MS/422.
[73] From our analysis of the RS Register of Papers, RS MS/421-422.

acting as a communicator had long been expected to 'satisfy himself that the paper is a fit and proper one to be communicated to the Society, and has not been previously published elsewhere', but already in 1894, John Evans was worried that this did not lead to adequate scrutiny.[74] He proposed that all papers by outsiders – even for *Proceedings* – should be examined by referees; but Council had rejected the idea.

If communicators could not always be trusted, then greater responsibility would fall onto the shoulders of the two Secretaries. But by the late nineteenth century, two men not hope to be knowledgeable on all possible subjects; and moreover, the Secretaries also had responsibility for an increasing range of Royal Society activities (such as grant administration and advice to government) in addition to carrying out their obligations as senior academics within their own universities.[75] Getting the chairmen of the Sectional Committees to assist the Secretaries was a compromise which ensured someone with knowledge of the general field was involved in the decision-making, without slowing things down as much as full refereeing would have done. Lister had certainly hoped that the new committees would free the Council and officers from the minutiae of publications, and enable them to devote more attention to 'matters of larger policy'.[76] Like the earlier use of committees and referees, the Sectional Committees could be presented as a mechanism for closer and more expert scrutiny, but they also enabled the Society to demonstrate institutional (not individual) editorial responsibility for both its periodicals, and to spread the workload.

The Twentieth Century

As others have shown, it is clear that few proprietors of independent journals in the late nineteenth or early twentieth century felt any need to adopt such complex processes for editorial scrutiny.[77] What 'refereeing' there was tended to take the form of informal consultations with trusted acquaintances, and editors relied strongly on their own instincts, and on the reputations of the individuals and institutions concerned – much as Joseph Banks had

[74] The rule about communication was the very first item in the explanatory notes issued from 1896, though the practice dated to at least the eighteenth century. See, 'Explanatory notes on the procedure relating to the reading and publication of papers', *Year-book of the Royal Society 1897-98* (London: Harrison & Sons, 1898), 67. For Evans's concern, see RS CMP/6, 26 April 1894.
[75] The expanding remit of the Society is described in Hall, *All scientists now*.
[76] Lister, 'Address of the President', at p. 124.
[77] An exception was the *British Medical Journal*, which used refereeing from 1870, see Burnham, 'Evolution of editorial peer review', p. 1325.

done.[78] The lack of interest in systematic refereeing at the independent journals is further confirmation that refereeing was originally part of an editorial system distinct to the learned societies.[79]

Learned societies perceived their periodicals in the context of their members' needs, and an institutional mission to support scholarship; but the editors and publishers of independent journals always had to think about sales, and thus about the interests of paying customers. Thus, they usually had a fixed page length and price, were issued with regularity, and focused on short accounts of the latest discoveries and observations. By the late nineteenth century, researchers in fast-moving fields like physics valued these journals as routes for rapid publication of what would later be called 'preliminary results'; they had become a key route to priority. Particularly significant discoveries might then be written-up in a lengthy paper for one of the learned societies, both as a means of fully documenting the discovery, and in the search for personal prestige. The reputation of an independent journal was tightly connected to that of its named editor, which meant that, whereas the Royal Society's committees and officers always had to consider the corporate reputation of the Society and its Fellows, independent journal editors could follow their own instincts and interests. Together, this meant that the proprietors of independent journals tended not to need mechanisms to create collective editorial responsibility; and their desire for speedy publication was better served by editors making executive decisions, than by referees.

For most of the twentieth century, therefore, the use of refereeing (and communicators and committees) continued to be a practice peculiar to learned societies, associated with collective responsibility and the publication of lengthy research memoirs. However, just as they had done in the eighteenth and nineteenth century, the societies' editorial practices continued to adapt to changing circumstances. The editorial system developed by the Royal Society to protect the prestige of a very old organisation, much of whose conduct was still

[78] For an account of this at *Nature*, see Melinda Baldwin, *Making "Nature": the history of a scientific journal* (Chicago, 2015), and Baldwin, 'Credibility, peer review, and Nature'. For the *Philosophical Magazine*, see Imogen Clarke and James Mussell, 'Conservative attitudes to old-established organs: Oliver Lodge and Philosophical Magazine', ibid., pp. 321-36.

[79] We know of equivalent systems used at the Royal Society of Edinburgh and the American Physical Society, as well as at the (London) Geological Society and Astronomical Society. Except for Lalli's work on the journal of the APS (Roberto Lalli, ''Dirty work', but someone has to do it: Howard P. Robertson and the refereeing practices of Physical Review in the 1930s', ibid.70 (2016), pp. 151-74), and work in progress on the RSE by Sian Burkitt and Aileen Fyfe, little is known about the common trends or the idiosyncrasies of learned society editorial practice.

rooted in the idea of gentlemanly civility, responded – gradually – to the needs of professional, international scientific research.

For the Royal Society, the key issues revolved around human resources. Firstly, the Society in the twentieth century became increasingly involved in grant-making, scientific diplomacy and, eventually, policy and education, and thus the small pool of active Fellows and Officers faced an increasing number of responsibilities.[80] This was partly dealt with by employing more paid assistants, but editorial evaluation and decision-making remained a matter for Fellows. But while the fellowship in the nineteenth century might arguably have been seen as the mainstream of British science, by the twentieth century, the growing number of scientific researchers, coupled with the Society's election policy, meant that the fellowship had become an elite echelon. Moreover, it was distinctly British, while science was becoming increasingly international. This meant that the social dynamics underlying the editing of the Society's periodicals were significantly different from the days when the majority of authors, readers and referees were all, equally, Fellows of the Society.

The editorial practices instituted in 1896 came under criticism as early as 1902, when a member of Council complained the new arrangements were 'complicated', that the continued use of refereeing was 'an anachronism', and that the Society should consider 'the appointment of an Editor'.[81] But it was not until the late 1960s that any major reforms were discussed: in 1967, the system of editorial management was described as 'outdated and cumbersome',[82] and in the reforms which followed – as in the subsequent reforms in 1990 – the aim was to make the Society's procedures more effective and streamlined. From 1969, the editorial work done by the chairmen of Sectional Committees was transferred to a new (larger) group of Associate Editors. Those Associate Editors were still nominally under the authority of the Secretaries and Committee of Papers, but positive recommendations were to be 'automatically endorsed by the appropriate secretary';[83] and from 1990, editors were appointed with full responsibility for each of the Society's journals. After 238 years, the Committee of Papers was disbanded, and the Secretaries relinquished their role in managing the Society's publications. The Society's

[80] Peter Collins, *The Royal Society and the Promotion of Science since 1960* (2015).
[81] Memorandum by H.E. Armstrong, in CMP/8, 6 November 1902.
[82] RS CMP/22, 15 June 1967. The proposals passed on 9 May 1968 and were implemented on 1 January 1969.
[83] RS CMP/22, 15 June 1967. See also 'Notes for the Guidance of Associate Editors' [1969], RS.

corporate interests are now represented by a Fellow acting as editor, and by the Fellows who serve on the advisory Editorial Boards.

While both sets of twentieth-century reforms were principally about management, they incorporated some changes to the procedures of communication and refereeing. The end-result was the removal of the privileged role of Fellows in the editorial process. The Duke of Sussex in 1832 had felt it entirely appropriate that publication decisions be made by those who 'have earned by their labours an European reputation', but by the 1960s and 1970s questions might have been raised about the fairness of a self-selecting group of senior scientists, mostly male and mostly British, sitting in judgement on the work of researchers of all genders, ages and nationalities.[84] However, the rationale behind the Society's reforms appears to have been practical effectiveness, rather than an attempt to dispel any accusations of unfairness.[85]

Refereeing had become standard practice for both Society periodicals by the mid-1930s, and in the 1960s, it was clarified that this meant 'at least one independent referee other than the communicator'.[86] The role of the fellowship as 'so critical and helpful a body' of referees was seen as an asset that could be matched by 'no Journal in the world'.[87] Refereeing had come to be seen as a form of quality control, and a 1967 suggestion to publish un-refereed papers in *Proceedings* (as had been done before the 1930s) was dismissed as risking 'a degeneration of standards for presenting new scientific knowledge', despite its advantages for speedy publication.[88] Other than drafting new guidance and revising the printed report form from time to time, the Society made few changes to the actual practice of refereeing during the twentieth century. For instance, it continued to keep referees' names confidential but to share the identity of the authors, even though its referees had been accused in 1922 of being 'anonymous and irresponsible', and even though, from the mid-1950s, some journals began anonymising authors as a means to protect them from the perceived prejudices of referees.[89] The ongoing use of 'single-blind' refereeing at the Society – and in the sciences more generally

[84] Sussex, '[Presidential Address 1832]', 142.
[85] We have yet to find any such critiques of Royal Society editorial practice, but for changing sensibilities in the social sciences and humanities, see David Pontille and Didier Torny, 'The Blind Shall See! The Question of Anonymity in Journal Peer Review', *Ada: A Journal of Gender, New Media, and Technology*, 4 (2014).
[86] RS CMP/22, 6 May 1965.
[87] [Alfred Egerton?], 'A note on Proceedings and Transactions A' [1945], RS Egerton Papers.
[88] RS CMP/22, 15 June 1967.
[89] '*Philosophical Magazine*: Report by Research Committee', *The Scientific Worker*, 29-30, (11 March, 1922), 29, quoted in Clarke and Mussell, 'Conservative attitudes to old-established organs', 321. For the mid-1950s (mostly in social sciences and humanities), see Pontille and Torny, 'The Blind Shall See!'. See also David Pontille and Didier Torny, 'From Manuscript Evaluation to Article Valuation: The Changing Technologies of Journal Peer Review', *Human Studies*, 38 (2015), pp. 57-79.

– illustrates the enduring legacy of nineteenth-century learned society practices. However, it did relax its rules about the involvement of non-Fellows.[90] The guidelines drawn up for the new Associate Editors in 1969 included explicit provision for dealing with referees who were not Fellows, and even those who were resident overseas.[91] Like so many earlier reforms, widening the pool of potential referees could be presented as a means of ensuring appropriate expertise, but it also helped to spread the load on busy Fellows: in 1950, one had complained that 'if I get much more heavy refereeing like this, it is goodbye to any chance of doing real scientific work myself.'[92]

Referees were seen as an asset to the Society's periodicals, but communicators were a more ambiguous legacy. The 1890s worries about whether they were screening submissions carefully enough, continued. In 1936, for instance, one Fellow blamed the 'increasing bulk' of submissions of 'routine research' – which he deemed inappropriate for the Society – on Ph.D. supervisors who were too willing to put their students forward. He claimed that these Fellows had forgotten that communication involved 'a duty as well as a privilege'.[93] And if communicators could not be relied upon, more responsibility devolved on the referees. Hence, one referee in 1950 complained of his time being wasted by Fellows sending in weak papers by their students in the hope of gaining 'a little extra prestige' from a Society publication. He wished such Fellows 'would only take the trouble, exercise their undoubted critical powers and have the papers put into proper shape, on in some cases stopped, before sending them in'.[94] Such complaints hint at the challenge for senior scientists in balancing loyalties to their universities and their students as well as to the Royal Society.

A further problem with the long-standing insistence on communication by a Fellow was whether it had become an obstacle to the Society's receiving the best papers. Even before the

[90] The Standing Orders in use from 1899 admitted the possibility of 'special reasons' why it would be desirable to consult referees who were not Fellows. See *Year-book of the Royal Society 1901* (London: Harrison & Sons), p. 65. Most of the known exceptions involved people who went on to become Fellows shortly thereafter. For instance, the physicist Charles Galton Darwin acted as referee shortly before his 1922 election to the fellowship; and the botanist Agnes Arber was consulted in 1939, and became the third female Fellow in 1946.
[91] 'Notes for the Guidance of Associate Editors' [1969], RS.
[92] N.K. Adam to D.C. Martin, 15 July 1950, regarding paper A128, RS Referee Reports Withdrawn 1950. See also Camilla Mørk Rostvik, ''I am seriously tempted to burn some of the papers which reach me for an opinion'', *Times Higher Education* (2016), https://www.timeshighereducation.com/features/workload-survival-guide-for-academics [accessed 24 August 2016].
[93] Memo by L.N.G. Filon to RS Council, 9 July 1936, RS/CMP14.
[94] N.K. Adam to D.C. Martin, 15 July 1950, in RS RR/Withdrawn_AB_1950 / A128.

First World War, the majority of papers were being submitted by non-Fellows;[95] but by definition, those papers were all coming from researchers who somehow knew a Fellow. But were there other good papers which were not reaching the Society? After all, as a 1954 memorandum noted, many good scientists 'are not working under the direction of Fellows', particularly those 'from the Commonwealth and abroad'.[96] In 1973, Council approved a 'relaxation' in the rules to permit submission directly to the editorial office.[97] Yet, when the experiment was evaluated four years later, the effects were described as 'not good'. The result had been too many 'exceptionally troublesome' papers, and not much increase in good papers.[98] Thus it seemed to the Society in the 1970s that, despite legitimate concerns, communicators played a useful winnowing role after all. The preference for papers to be communicated by Fellows was only dropped in 1990.

It was in 1971, just as the Royal Society's reformed editorial system was bedding in and its officers were worrying about the role of communicators, that one of the first (and most influential) investigations of the function and power politics of journal evaluation was published: Harriet Zuckerman and Robert Merton's analysis of the editorial archive of *The Physical Review*.[99] While their work surely reflects contemporary concern about the efficacy of the system, its message was ultimately reassuring. It opened with John Ziman's 1968 assertion that a published article 'bears the imprimatur of scientific authenticity, as given to it by the editor and the referees he may have consulted', discussed the practices of the Royal Society in the 1660s and of the *Physical Review* in the 1950s, and ended by concluding that, although errors of judgement undoubtedly do occur from time to time, the refereeing system was essentially sound, and provided 'an institutional basis for the comparative reliability and cumulation of knowledge'.[100]

When David Davies became editor of *Nature* in 1973, he made refereeing a standard practice, seeing it as a way to raise the journal above accusations of cronyism and elitism, and during the cold fusion episode in 1989, his successor John Maddox would trumpet peer review

[95] Around 70% of submitted papers were from non-Fellows by 1938-45, see [Alfred Egerton?], 'A note on Proceedings and Transactions A' [1945], RS Egerton papers. This contrasts with our analysis of the RS Register of Papers for 1865-75, when it had been only 40%.
[96] R. A. McCance, 'Memorandum on the Proceedings of the Royal Society, Series B', [1954] RS/Q/20(54).
[97] RS CMP/24, 8 Nov. 1973 [*CMR to double-check that it really did happen at this meeting, as planned*]
[98] AE (AB)3(77), Associate Editors for the biological and mathematical & physical sciences minutes (joint meeting 28 Jan 1977), 1: Communication of papers.
[99] Zuckerman and Merton, 'Patterns of evaluation in science'.
[100] Ibid., p. 66 (Ziman), p. 100 (closing quotation).

as an essential process for scrutinising scientific research before announcing it.[101] Thus, by the 1990s, a process which was once an oddity of learned societies had come to be seen as a normal and essential practice for all scholarly journals. Even though John Burnham's 1990 survey of editorial peer review noted that it had 'been essential to science and medicine' only for 'at least two generations', Zuckerman and Merton's discussion of the early Royal Society has enabled many subsequent commentators to project 'peer review' back onto the 1660s.[102] By glossing over the intervening three centuries, scholars have ignored the period in which refereeing and collective decision-making actually developed, and whose legacy is still apparent in current practice.

For the Royal Society in the late eighteenth and nineteenth centuries, refereeing was a luxury – a possibility afforded to an organisation with a unique position in the history of science and in British scientific organisation, one strongly aware of that position, and possessing both a captive population of scholars obligated to serve the Society's ends and sufficient financial resources to promote scholarship (mostly) for its own sake. The mismatch between the context of the gentlemanly learned society (in a national context) and modern, professional, international science, helps to explain some of the accusations now being levelled against peer review as not being 'fit for purpose'. If our aim, therefore, has been to show the complexity, contingency, and historical specificity of peer review's origins, our ambition is to start a scholarly conversation about which of its attributes still seem desirable, whose interests it serves, and what the realistic limits of its pretensions might be.

[101] Baldwin, 'Credibility, peer review, and Nature'.
[102] Burnham, 'Evolution of editorial peer review', p. 1323.