



A History of Scientific Journals: Publishing at the Royal Society, 1665–2015. By Aileen Fyfe, Noah Moxham, Julie McDougall-Waters and Camilla Mørk Røstvik. University College London Press, 2022. Pp. 664. Open access, <https://doi.org/10.2307/j.ctv2gz3zp1>.

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Keywords: book reviews; scientific journals; history.

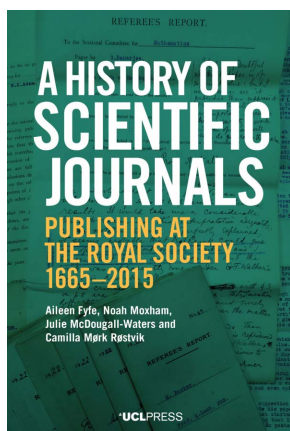
The book commences with an *Introduction: Origin myths*. This book was nine years in the making. It was inspired by the 350th anniversary of the Royal Society and the start of the first journal, *Philosophical Transactions*. At the celebrations, Sir Paul Nurse, Nobel Prize winner, observed that Henry Oldenburg had started the journal in 1665; Oldenburg had described four essential elements that its introduction would bring for scientists from all over the world, namely registration, verification, dissemination and archiving. That said, the authors of the present book are clear that whilst the core facts are true, Oldenburg started it and the year was 1665, it is not the case that the needs of modern scientists and modern scientific publications were anticipated back then. More specifically, in a section entitled *Going beyond the myths*, it is pointed out that it was a further 87 years before *Philosophical Transactions* was to become an official publication. The authors conclude their opening chapter with the observation that their book matters not least because ‘the structures of academic publishing are being consciously renegotiated [today]’.

The book is then organized in five parts, each corresponding to a designated time period. Part I is entitled *Invention, 1665–1750*.

In Chapter 1, entitled *The first Philosophical Transactions, 1665–1677*, we learn that Oldenburg was invited in 1664 to be regional correspondent to a *Journal des Scavans* to be based in Paris. He launched his idea for a more scientifically focused journal, the *Philosophical Transactions*, within the Royal Society of which he was Secretary. This section gives an account of the contents of the journal and also dispenses with the notion that peer review prepublication happened in this period. It also describes the lack of direct involvement of the Royal Society in the journal, because it saw its activities as primarily involving demonstrations of discoveries and experiments in the weekly meetings of its fellows rather than ensuring an accurate written record. Yet the Royal Society is documented as having a link with the journal, which in effect it had not really asserted on a regular basis. Oldenburg’s death in 1677 was obviously a milestone for the journal, causing the Royal Society to contemplate the journal’s future carefully.

Chapter 2 is entitled ‘*Repeated reinventions, 1677–1696*’. The Royal Society did act and appointed successively five of its fellows to be responsible for the journal. The content and style varied. Most interesting was the emergence of thematic collections (*Philosophical Collections*), of which there were three: on comets, microscopy and elasticity. It is also interesting that there was no notion of copyright. Instead the printer owned the content, as a working practice. Robert Hooke as Secretary of the Royal Society was replaced by the Oxford-based Robert Plot who ‘revived’ the *Transactions*. This next period obviously went well for five years until an unpleasant falling out over criticisms of some of Robert Hooke’s research work, including resignations of those in charge of the *Transactions*. The Royal Society then for the first time employed someone to look after the *Transactions*, namely Edmund Halley. The authors describe in detail the basically haphazard nature of the period up to the end of the 17th century or as they call it ‘repeated reinventions’.

Chapter 3 is entitled *Stabilising the Transactions, 1696–1752*. In this period the authors observe that ‘key Royal Society fellows shared a group commitment to the *Transactions*, that would ultimately be formalised in 1752’. A significant fact, it seems to me, is the



finding that ‘In the mid-1690s, fellows had authored only around a third of items in the *Transactions*, but that had risen to over 70 per cent by 1709.’ Though this indicates a strong link between the journal and the Society it was not sustained at this level. This chapter documents nicely the pros and cons of more external-to-the-society articles versus internal-to-the-society ones.

Chapter 4 is entitled *The Transactions and the wider world, c. 1700–1750*. In this chapter ‘the varied interactions between the *Transactions* within the wider world’ are considered. The authors start by describing the reactions which led to critiques of the *Transactions*, as the view developed that the ‘Secretary-Editor’ role was basically not detached from the Royal Society. The chapter emphasizes this by describing the way that Isaac Newton, who was President of the Royal Society, was able during the 1710s to personally control it sufficiently, via ‘biddable allies’, to promote his claims over the discovery of calculus before Leibnitz. In a damning summing up the authors state that ‘During the calculus dispute, the Society’s leadership endeavoured to maintain an appearance of editorial neutrality, but equally made certain that the *Transactions* was absolutely at their disposal when occasion required.’ To this day some journal editors in my experience consistently show failure at ensuring complete reference lists in a publication, a proper way of documenting priority of discovery, ideas or insight. Of course, such needs also require in practice the regular publication of a journal, which *Philosophical Transactions* struggled with as this chapter makes clear. For example, infrequent publication limited its ability to disseminate information to the public on the power of inoculation against the smallpox epidemic of the time, compared with a daily newspaper. An interesting, indeed important, development was, in the absence of a copyright law, the occurrence of abridgements (digests) of articles between 1705 and 1708. These were published to increase the accessibility of articles to a wider readership of the ‘generally curious’ versus scholars. The chapter has a nice photograph in Fig. 4.2 of the three abridged volumes. Indeed different abridgements were compiled, e.g. one organized by subject areas and another by timeline. The abridged versions sold a lot more copies than the full journal volumes. They also facilitated translations from English at a time when English was not common outside the British Isles.

Part II is entitled *Maturity and institutionalisation, 1750–1820*.

This commences with Chapter 5 which is entitled *For the use and benefit of the Society, 1750–1770*. In 1752 the Society took control of the journal and initiated a collective responsibility, beyond the single individual editor of previous times. This chapter scrutinizes the new practices of the Society within this collective responsibility. Fig. 5.3 shows the ‘advertisement’ at the front of Volume 47 of the *Transactions* that the ‘truth of the facts, the soundness of the reasoning, or the accuracy of calculation’ of any publication rested still ‘with the authors, not the Society’. This was in contrast with the Paris Académie Royale’s journal, where experiments were even redone to ensure the accuracy of a publication’s results. This latter

procedure could mean that publication was two to three years after submission. These details are of course very interesting in the context of modern debates on the reproducibility of science, for example the excellent report of the US National Academies of Science, Engineering, and Medicine (2019) or the UK Parliamentary Committee on Science and Technology enquiry (2022, report in preparation). One modern trend is a compromise between the Paris and London approaches of the 1750s, namely checking the underpinning data and software of a publication during prepublication peer review and thereby testing the soundness of its narrative [see also e.g. Helliwell (2019) for a description of data science skills for referees of biological crystallography]. Post-publication peer review could of course include repeating experiments. In the 1750s an outcome was that London was accused of only publishing incremental science whereas Paris could publish leaps in discovery and even approve patents. That said, the Society claimed to publish papers of ‘importance’: echos, then, of today where a journal’s referees are routinely asked to evaluate the significance of a submission typically on a scale of 1 to 5. This can be a difficult thing to assess and is a source, upon rejection on such grounds, of authors feeling unfairly treated. These procedural issues, and matters of epistemological principle, impinged on the costs and benefits to the Society, which are described in detail with a bottom line that it would take two centuries before the *Transactions* showed a profit for the Society. This is a very interesting chapter of this book, I think.

Chapter 6 is entitled *Sociability and gatekeeping, 1770–1800*. It addresses the way that ‘editorial processes were embedded in the sociability of its formal and informal activities’. After a detailed provision of factual cases, the authors conclude that the ‘evaluation [of an article] was as much about the personality of the author as it was about the intellectual content’.

Chapter 7 is entitled *Circulating knowledge, c. 1780–1820*. It covers not only the *Transactions* but also the output of other learned societies around the world and new, independent, ‘commercial’ journals. A good idea of where the *Transactions* went can be had from the print runs peaking at 1000 copies and the number of fellows being 540 in the 1780s to 1790s. Also, the quality of the print and paper used were a concern, as the Society wished to impress recipients such as learned societies abroad. Fig. 7.2 shows maps, of Europe and the world, of the ‘institutions to which presents of the *Transactions* were sent in 1816’. The chapter concludes with sections on reprints and the appearance of more specialized scientific societies each with their own journal. The reprints raised complications when these appeared before an issue of the *Transactions* as they were in danger of financially undermining the finances of the journal.

Part III is entitled *The professionalisation of science, 1820–1890*.

Chapter 8 is entitled *Reforms, referees and the Proceedings, 1820–1850*. The chapter kicks off by citing Charles Babbage’s (1830) criticisms of science in Britain in general and the Royal Society in particular. This book’s independent views are captured by its statement ‘The Royal Society responded to

Babbage's criticisms with its long-perfected but fundamentally inadequate formula of dignified outward silence.' That said, the chapter documents steady reforms of the Royal Society and the launch of a new journal: *Proceedings of the Royal Society*. Written referee reports on articles, for example, were introduced as well as subject-specific sub-committees. Fig. 8.5 shows the procedure and Fig. 8.6 shows examples of the 300 preserved reports from the 1830s and 1840s on about 230 papers. Authors' names were not concealed from referees but referees' names were concealed from authors, a procedural aspect which attracted some public criticism. The *Transactions*, which cost more money to produce because of its full-paper content, was of more prestige than the *Proceedings* consisting of abstracts.

Chapter 9 is entitled *Editing the journals, 1850s–1870s*. This chapter provides numerous, rather delightful, examples of submitted articles and the efforts of referees and the editor in this period. Whilst they more or less applied the procedures developed earlier in these cases, an interesting and important point arises of anxieties about the extent of revisions to a submission and the need for clear record keeping in case of a dispute over priority (Note 101). Also, the fact that the *Proceedings* were published much more speedily than the *Transactions* is documented.

Chapter 10 is entitled *Scientific publishing as patronage, c. 1860–1890*. We immediately learn that the two journals were not the biggest publishing efforts of the Royal Society. This was in fact a *Catalogue of Scientific Papers*, started in 1858 and completed in 1925. This was mentioned to emphasize the point that the aim of the Society was scholarship with no particular regard to commercialization, unlike the *Philosophical Magazine* (published by Taylor and Francis) or *Nature* (published by MacMillan). There are many interesting details of the operations of the Society in this chapter in regard to its extensive efforts, in the form of patronage, in the circulation of new knowledge, with modest regard to cost or securing income. This was unlike the *modus operandi* of the commercial publishers of science who decided a fixed price for a year and accepted to publish a fixed number of articles/pages per year. This was irrespective of the number of submissions, generally escalating at a sustained rate as the sciences grew in activity.

Part IV is entitled *The growth of science, 1890–1950*.

Chapter 11 is *The rise of the Proceedings, 1890–1920s*. This chapter opens with an extensive analysis of the challenges faced by the Society journals as the number of submissions rose. The faster publication of the *Proceedings* became an advantage for many authors. The major issue for the Society and its Council was the increase of specialization of the sciences in general and a considerable competition then arising from the estimated 25000 subject-specific journals available to scientists as a whole. So, should the Society's journals focus on the interdisciplinary topics? As a practical step, the *Proceedings* split into parts covering physical sciences (series 'A') and biological sciences (series 'B'). A section on referees and refereeing reads much as it would today: a need for refereeing and often the pressures to find referees. Fig. 11.5

shows the respective number of pages published in the Society's journals from 1890 to 1960 and a major shift towards the *Proceedings* being preferred by authors over the *Transactions*.

Chapter 12 is entitled *Keeping the publications afloat, 1895–1930*. In 1894 the Society's Treasurer warned that he would have to sell £1000 of the Society's investments to cover the costs of the journals that year, and that 'it was not sustainable to eat into capital regularly'. This chapter looks at the shifting balance between philanthropic and commercial distribution and income. The history of the deficits year on year is intricate, the challenge being seriously exacerbated by the First World War. Fig. 12.4 gives a glimpse of the situation with a breakdown of the finances of the Society's journals in 1930. Table 13.1 in the next chapter gives a further glimpse of the finances via sales and free distribution figures.

Chapter 13 is entitled *Why do we publish? 1932–1950*. This question was posed by Sir William Henry Bragg in his presidential address of 1938. The question impinged on the Society's expectations of a submitted paper. To be publishable the paper should 'contain methods or results of critical importance' and be 'of value to others than specialists in the particular subject'. These criteria would 'avoid [the] unnecessary expense' of publishing routine work. Where to draw the boundary was obviously difficult, as illustrated by W. H. Bragg trialling the archiving of data for X-ray crystallography results where 'even Bragg admitted that most of these data would only be needed by those 'very few readers' who wanted to 'check the detail of the work''. Note 96 will be of specific interest to crystallographers, being a reference to Bragg's aforementioned address to the Royal Society which includes a substantive overview of our field from page 303 onwards (Bragg, 1939). There is also a nice description of Kathleen Lonsdale getting elected as a fellow, one of the first women to be elected.

Part V is entitled *The business of publishing, 1950–2015*.

Chapter 14 is entitled *Selling the journals in the 1950s and 1960s*. Like in Chapter 12, the details are intricate. A major change to the science publishing landscape was the substantial rise in commercial journals. Price increases in the Society's journals became 'a regular feature', summarized in Table 14.2. A wide-ranging set of topics are then covered, the common basis being the need to balance the finances. One such section, on refereeing, commends Kathleen Lonsdale for her refereeing work which was substantial. An example of her reports is shown in Fig. 14.4.

Chapter 15 is entitled *Survival in a shrinking, competitive market, c. 1970–1990*. The focus is on generating a financial surplus in journals operation despite declining subscription numbers. A reality check of sorts was that in 1987 the journal impact factors ranked the *Proceedings* at 73rd and the *Transactions* 'not in the top 150' while the equivalent US publication the *Proceedings of the National Academy of Sciences* was 12th. This chapter undertakes a determined, and very interesting, analysis to understand the subject-specific content trends within the context of the Society envisaging its journals as multidisciplinary. Another Publications Policy Committee was formed in 1987, at which the physical sciences

secretary, Sir Roger Elliott, advised that the committee consider how the Society's journals could best serve the scientific community and provide financial benefit to the Society. (I served with Sir Roger on the International Council of Scientific and Technical Information, he representing the International Union of Pure and Applied Physics and myself the IUCr.) Fig. 15.2 is a copy of the promotional leaflet for the new opportunities for authors in a revamp that sought to make the journals more attractive to readers. To Sir Roger's two admonitions above, the third aspect that was emphasized in the relaunch, as ever, was for the publications 'to be worthy of the Society'. Perhaps most basically, in terms of attracting authors, a big focus of this chapter, I would observe that the two words 'transactions' and 'proceedings' are synonyms of each other. So, those unfamiliar with how the Royal Society organized its business would not know which journal to submit to. Also, as someone working at the boundary between physical sciences and biological sciences, it is not obvious to which I would submit an article, Series A or B. I have published with the Royal Society though, in several categories, and it was a highly professional, *i.e.* very good, experience. The outcome of the revamp described in the next chapter was a 67% rise in submissions, faster publication times, papers from all over the world, 'from a wider mix of subjects' and 'several papers of outstanding international importance', but it did not solve the problem of the decline in subscriptions.

Chapter 16 is entitled *Money and mission in the digital age, 1990–2015*. The Royal Society launched five new journals between 2003 and 2014. This included the journal *Interface* as an interdisciplinary initiative, but the text has no self-consciousness that the Society had always claimed interdisciplinarity. Also as Table 16.1 shows, there was a dramatic growth of submissions with a shift in balance greatly in favour of the biological sciences. There is an important description of the views of the *Transactions A* Editorial Board of the *arXiv* preprint service for physicists based at the time at Los Alamos National Laboratory, USA. I have used this several times as a 'quantitative biologist', for several reasons. This chapter introduces the open-science movement and its impact at the Society. Whilst quite detailed, the discussion misses two key points. Firstly, if one is funded by the tax payer and one's funding agency pays for open access I think one must comply with making one's work open access, and this has the major advantage for the author of more exposure of one's results. Secondly though, unfunded yet alpha-rated research proposals dominate by far, typically two-thirds of proposals being left unfunded by the agencies. For myself as a proposer I do not abandon such efforts, but then I have no funds to publish the results as open access. It can also be argued that the unfunded alphas have by far a larger fraction of innovative ideas. This chapter of the book captures well, however, the divided opinions around open access and whether learned society journals, and their good works based on their income, could survive a trend away from the subscriber finance model.

A final section, page 592, is entitled *Reflections*. The key sentence from the authors about this book is 'What we have done is to study the development of the scientific journal from

the perspective of those who ran it.' My criticisms of this valuable book for anyone in science are largely that those who run a journal have blind spots, which can change over time and circumstances, as to what a scientist experiences, which also varies over one's career timeline. As someone working across physics, chemistry and biology I am perhaps not an easy customer to cater for either. But it seems that, if I had not been captured by crystallography and the IUCr Journals, as well as the Taylor and Francis full reviews option in *Crystallography Reviews* and my science books, I should have been well suited to the Society *Transactions* or *Proceedings* journals. By contrast, as another example besides myself, I note that Max Perutz published many more crystallography articles in the Royal Society journals than IUCr Journals, about 40 versus eight. In this section's look forward two very interesting aspects are considered. Firstly, that the free-to-authors and free-to-readers model (*i.e.* diamond open access, although this is not used as a term by the book's authors) is mentioned because it would be a return to the Royal Society's original, albeit obviously not identical, operations of free publishing for its fellows and gifting journal volumes to other learned societies. Secondly, the book does not question the assumption that referees will continue to provide free refereeing. For high-profit commercial publishers, or indeed the Royal Society journals, their modern era surpluses (around 40% in the 2010s; see p. 570 and Figure 16.3) are being increasingly questioned by scientists.

There is an extensive, and meticulously detailed, list of sources used in the authors' investigations. There is also an extensive index with easy 'touch' navigation to a cited page. The back cover highlights papers in *Philosophical Transactions* from four authors: Isaac Newton, Charles Darwin, Stephen Hawking and, crystallographers will be pleased to note, Dorothy Hodgkin. The brief biographies of the book's authors show them to be distinguished historians of science.

In summary, this book is important as it captures the history of the first ever science journals and their subsequent evolution. It also reveals in detail the evolution of the Royal Society. In addition, it captures the wider context of science over these centuries in terms of the international scene and other journals from the subject-specific societies as well as from commerce. When comparing this picture with my own experience of an international group of journals in crystallography there is a yet wider role arising from the many countries involved in forming the worldwide crystallographic community. This role still means protecting standards as gatekeeper to publication but it also requires detailed and constructive dialogue with authors *from all nations* so that if at all possible science across the world is developed. To my mind the biggest gap in the book is that the Royal Society's motto *Nullius in verba* ('on the word of no one' or 'take nobody's word for it') does not get a mention, let alone discussion. I had interpreted the motto to mean that their journals would insist that the underpinning data of their publications that had data, which must surely be many, would be checked by the referees and/or editors. With the exception of W. H. Bragg in 1938 as I have mentioned (Bragg, 1939), this is not covered in the book.

There is a recurrent theme in the book of the dividing line between discipline specific (to be discouraged) and broad themes, *i.e.* multidisciplinary (to be encouraged). A compromise approach was the division of the *Proceedings* into physical sciences Series A and biological sciences Series B. Nevertheless, merit does exist in the discipline specific. As an example I mention the helical theory of diffraction which was published in *Acta Crystallographica* (Cochran *et al.*, 1952), without which the DNA double helix and its parameters would not have been characterized.

The whole book is very interesting and quite thought provoking at times for me as a working scientist. I have maybe

drawn different emphases from the book than a publisher or managing editor as reader would have.

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