FIT TO PRINT? REFEREE REPORTS ON MATHEMATICS FOR THE
NINETEENTH-CENTURY JOURNALS OF THE ROYAL SOCIETY OF LONDON

by

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The Royal Society was one of the first British scientific societies to establish a peer review process for papers submitted to its journals. Initially, its peer review procedures were at best informal, but by the 1830s they became a formal, required gateway for all Royal Society submissions. This paper focuses on referee reports of mathematical papers submitted to the Society from 1832 to 1900, years covered in the first 15 volumes of referee reports archived at the Royal Society Library. Besides judging the content of papers, mathematical referees during this period discussed issues of professionalization and politics in their reports.

Keywords: referee reports; nineteenth-century British mathematics; Royal Society of London; journals; Philosophical Transactions

SETTING STANDARDS

As the first journal of its kind in Britain, Philosophical Transactions and its publication procedures set a standard for other scientific societies to follow. Although ‘the printing of ...[Transactions] was always, from time to time, the single act of the respective Secretaries’¹ for its first 46 volumes, Transactions became the official journal of the Society in 1752. Along with the journal’s official status, the Royal Society’s Council created the Committee of Papers, a group of Council members that managed the selection, editing, and publication of papers in Philosophical Transactions.² The Society’s statutes of 1752 state that this Committee ‘shall be at liberty to call in to their assistance ...any other members of the Society who are well skilled in any particular branch of Science that shall happen to be the subject-matter of any paper which shall be then to come under deliberation’.³ This referral option, however, was at best an informal one during the eighteenth century:

the earliest mention which has been found in the Society’s records of a paper being technically ‘referred’ is on May 25, 1780, when a paper by Mr. Ludlow as ‘referred to

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Mr. Cavendish and Dr. Hutton.' There does not appear to be a similar record until March 21, 1831, when a paper by Prof. Davy was referred to Mr. Faraday.4

The Committee of Papers felt no need to follow formal refereeing procedures, and at the same time it was hampered in judging the quality of papers accurately because of the lack of scientific training of its members.5 Moreover, the state of the Committee of Papers failed to improve during the more than 40-year tenure of Sir Joseph Banks as President of the Royal Society; the controlling body of Philosophical Transactions had devolved into a perfunctory clearing house and a sometime arbitrary decision maker.6

A call for change in the execution of Philosophical Transactions became part of a wider reform movement that began after Banks’s death in 1820. In one of the most critical tracts concerning the Society during this period, Augustus Bozzi Granville called for the Committee of Papers, which he described as a ‘secret tribunal’, to be restructured. Granville, a Fellow of the Society and an Italian ex-patriot, described the Committee’s administrative faults in his 1830 anonymous pamphlet, Science Without a Head; or, The Royal Society Dissected:

In proportion as we get nearer to our own times, the importance of the papers rejected seems to be in the inverse ratio of the scientific character of the deciding members of the committee; —and subsequent events have proved that those members have as often decided wrong when they decided for the rejection of papers—as they would have decided right had they not admitted some of the papers which appear now in the Transactions, but which are fit only for insertion in magazines and other periodical publications.7

Granville suggested that the fellowship of the Society be divided into classes by disciplines, including a ‘Free Class’ for non-scientific fellows, and that each class should be responsible for the selection of papers in its respective discipline.8 This plan would ensure that a paper’s judges would be well versed in its subject; it also excluded non-scientific members from the referral process entirely.

Although Granville’s plan to separate the fellows by discipline was not adopted by the Society, his call for the reform of the publication procedures of Philosophical Transactions received attention. The Duke of Sussex used his second presidential address to the Royal Society to outline an 1832 Council resolution crafted

[to] increase the usefulness and to uphold the credit of the Royal Society; I mean the Resolution . . . to allow no Paper to be printed in the Transactions of the Royal Society, unless 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.9

He cited earlier refereeing practices of Continental societies as worthy of emulation:

It has long been the custom of many Foreign Societies, and particularly the Academies of Science and of Medicine at Paris, to require written Reports upon every paper submitted to them, from a Committee of their Members: as the persons who are selected for this duty are frequently veterans in their respective sciences, who have earned by their labours an European reputation, the Reports which are thus produced prove often more valuable than the original communications upon which they are founded.10

Besides citing the Continental society model, the Duke’s justification of the Society’s new resolution described referees as

a powerful stimulus to the exertions of the genuine cultivators and lovers of science, who feel assured that their labours will be properly examined and appreciated by those who are
most competent to judge of their value; whilst at the same time, they tend to keep under
the obtrusive and turbulent pretensions of those who presume to claim a rank as men of
science, for which they possess no just title or qualification.11

With this Council resolution, referee reports became part of the modus operandi of the Royal
Society’s journals.12

Luckily for historians, these reports are well preserved and catalogued in the Royal
Society Library. This paper focuses on referee reports of mathematical papers submitted
to the Society from 1832 to 1900, years covered in the first 15 volumes of referee reports
archived in the Library. I present a variety of reports of mathematical papers, illustrating
the different fates that a submitted paper could meet and the issues of professionalization
and politics that the referees weighed alongside the actual mathematics they were judging.

SETTING CASE LAW: GEORGE PEACOCK AS REFEREE

The first entry in the volumes of referee reports is from the Cambridge mathematician,
George Peacock, on Richard Abbott’s submission on the variation of a triple integral. The
format of the report, a letter to the Royal Society’s Secretary, Samuel Hunter Christie,
was used by many of the nineteenth-century referees. In his opinion, Peacock wrote:

I hardly think that it can be considered as having any important additions to our
knowledge of the calculus of variations such as to make it worth insertion in the Royal
Society’s Transactions . . . the memoir is certainly the production of a good
mathematician, but it appears to be too brazen a piece of analysis for our Transactions,
though otherwise well worthy of being published.13

In his report, Peacock also makes the following point: ‘I think it proper however to
observe that this is a department of analysis which I have never much studied & that I am
not therefore very well qualified to give an opinion upon the merits of this paper.’14 This
sentiment, perhaps stated for the sake of politeness, is a persistent feature of the
mathematicians’ reports. In the end, Abbott’s paper was archived, an inconvenient state in
a time before easy copying.15 With this report, Peacock was acting as a judge,

Besides concerns about the quality and technicality of a mathematical paper, Royal
Society politics could influence the outcome of Peacock’s reports. In 1848, he refereed a
paper by Augustus De Morgan concerning the Newton–Leibniz calculus controversy.
Although Isaac Newton had first discovered the special, inverse relationship between
finding tangents to curves and finding areas beneath curves, Gottfried Leibniz was the first
to put this discovery in print. Although the two mathematicians are today considered as
independent discoverers of the calculus, in the eighteenth and nineteenth centuries issues
of priority and accusations of plagiarism about the discovery were heavily laced with
nationalism. De Morgan considered the Commercium Epistolicum, a 1712 Royal Society
report on the controversy. He found changes to the second edition of the report, published
13 years later, which ‘hardened the language of the report against Leibniz, making it
appear to accuse him of plagiarism’.16 In his referee report to Secretary Christie, Peacock
did not dispute the truth of De Morgan’s paper and classified its author as ‘the most
accurate & learned of all modern writers on the History of Mathematics’.17 However,
Peacock believed that ‘if the question of repairing a wrong done 140 years ago be entertained it must be entertained in a much more formal & solemn manner: I should recommend therefore Mr. De Morgan to withdraw his Paper & to publish it through some other channel than the RST[ransactions].’18 Peacock also suggested a further option, but he indicated that this extra measure would not give De Morgan any satisfaction:

If he thinks that the question should be entertained by the Council, then a distinct application should be made to the Council for this purpose. If however such an application should be made to the Council, I should not recommend the Council to entertain it… the Council as a body would appear to me to act wisely in abstaining from the expression of any official opinions whatever affecting the acts either of contemporaries or predecessors.19

The Royal Society had expressed earlier an entirely different attitude when it printed in its Philosophical Transactions De Morgan’s first paper on the Commercium Epistolicum, which ‘actually cleared up a point in favour of Newton’.20 However, when De Morgan put Leibniz in a more favourable light, his work was relegated to the archives of the Royal Society instead of the pages of one of its journals.21

Handled with care: the case of Sir Frederick Pollock

As Peacock’s treatment of De Morgan illustrates, concerns beyond the mathematical content of a paper could occupy the minds of referees. In the case of the Philosophical Transactions aspirant Sir Jonathan Frederick Pollock, the extra-mathematical concerns of the referees focused on the author’s stature outside mathematical circles. As a student at Trinity College, Cambridge, Pollock showed considerable mathematical promise and became both Senior Wrangler and First Smith’s Prizeman in 1806. However, he soon left Cambridge to follow a familiar path for high-ranking Wranglers—a career in law. He soon proved to be as successful in law as he had been at Cambridge. He began a parliamentary career in 1831; he was knighted in 1834 and served as attorney-general under Sir Robert Peel. In 1841, he was appointed as Lord Chief Baron of the Exchequer; he served in this role until his retirement in 1866, when he received a baronetcy.22

Although he had left Cambridge for the law, he had not completely left mathematics behind. A Fellow of the Royal Society since 1816, Pollock repeatedly tried to get his mathematical work published in the Society’s journals. His first submissions date from the early 1840s, but at best only abstracts of them appeared in Abstracts of Papers Printed in the Philosophical Transactions of the Royal Society of London, a Royal Society journal later known as Proceedings of the Royal Society of London. He repeatedly withdrew his submissions, although some were archived. An 1850 cover letter to a report by Peacock illustrates the problems that most referees had with Pollock:

I enclose a Report on Pollock’s paper, which I have found a… task to read and understand: it is so confused & so inaccurately worded:… it is a subject about which he has worked hard and thought a good deal… [but] I think he has no ground but analogy for [his propositions]… to be so. I do not understand one word of his suggestions towards a proof.23

Peacock further grumbled, ‘The really good mathematics are sent to the Cambridge [and Dublin Mathematical] Journal, the best of its kind in Europe: all the interesting papers seem
to miss the Royal Society. Pollock withdrew this paper, although an abstract of it was printed in *Proceedings*. Pollock finally got a paper on number theory published in *Philosophical Transactions* in 1854; he repeated the feat in 1859 and 1861. The 1861 paper discussed Fermat’s Theorem of Polygonal Numbers, which states that every natural number can be written as the sum of four squares. Specifically, Pollock sought to discover the properties of these four squares, or decomposition. The referee reports on a second communication by Pollock on Fermat’s Theorem illustrate the gruelling path of review that a submission by the Lord Chief Baron could take.

The first referee of the paper, Henry J. S. Smith, was an obvious choice as referee of a paper on number theory. Smith was the new Savilian Professor of Geometry at Oxford, and at the time of Pollock’s submission he had published half of a codifying series of reports on number theory for the British Association for the Advancement of Science (BAAS). In Smith’s BAAS reports, he discussed Pollock’s area of interest, the decomposition of natural numbers into squares. He reported that by 1770 the French mathematician Joseph Louis Lagrange had proved that every natural number could be decomposed into four squares. Smith considered the generalized question of the number of ways in which a given natural number can be decomposed into any given number of squares. Smith reported that the German mathematician Carl Jacobi’s work on the theta function solved the question for two, four, six and eight squares. Carl Friedrich Gauss had earlier solved the question for three squares. Five and seven were open questions, which Smith soon answered. In fact, he formulated a method that would in theory handle any given number of squares; however, in practice, the calculations involved were extremely laborious. Smith published these findings in *Proceedings* in 1867. Five years earlier, in his first of many referee reports on Pollock’s paper, Smith tactfully wrote, ‘his researches, though curious and interesting as they stand, are not, in the paper submitted to me, brought to a sufficiently definite result to fit them for immediate publication.’ The second referee called to weigh in on this paper was Arthur Cayley, who was practising at that time as a lawyer but who would soon assume the Sadlerian Chair at Cambridge. Cayley was likewise unimpressed with the memoir, writing that ‘the mere algebraical identities... upon which the reasoning [of Pollock’s paper] is in effect based, do not and cannot lead to any valuable result as regards the decomposition of an integer number in general.’

The intermediary between the referees and Pollock, Royal Society Secretary George Gabriel Stokes, suggested to the author that he withdraw the paper and try again. Pollock agreed to the suggestion, writing to Stokes:

> [although] I expressly disclaimed then presenting it as a proof of Fermat’s theorem—I am desirous now of presenting it—as such proof—which in my judgment it is—almost daily the matter grows under my observation & new & (I think) very interesting matter comes out—I have no copy of the paper—I put it together without even a draft of it—desirous that it might be known, (crude as it was, compared to what I have since done)—I have no doubt I could now make it shorter & clearer—& come at once to the general theorem.

In October 1863, Cayley and Smith were again brought in to referee the revised paper. Smith wrote that the paper was still not appropriate for *Philosophical Transactions* because its chief result was already presented in Pollock’s 1861 *Philosophical Transactions* paper. Cayley similarly did not think much of the revised paper, writing ‘I
am not sure that I rightly apprehend the argument... The intermediate theorem... is certainly not brought out with sufficient clearness.’ Interestingly, he concluded his report with the following suggestion: ‘considering the paper as the concluding one of a series of papers already printed, I think that it may without impropriety be published in the Transactions of the Society.’

Eight days after Cayley made his comments, Smith again reported on the paper so that he and Cayley could come to a consensus:

The point which seemed so doubtful to Cayley is precisely the same as that which had struck me. I have... again carefully examined the papers, and I do think that, if the auxiliary proposition to which Cayley refers were really proved in the paper, I should have been able to make out what the proof is; which, at present, I cannot do.

With his report, Smith also included a very accommodating and tactful Memorandum to Pollock in which I have tried to state with distinctness the objection which seems to me to be against the demonstration... Whether the objection can be sustained or not, (and I do not forget that it may after all be only a mistake of my own) I think the Chief Baron will be glad of an opportunity of reconsidering, with reference to it, the details of his proof.

The inclusion of this memorandum, the self-effacing language employed in his report, and the speed of his response to Cayley’s report all portray Smith as a conscientious and politically sensitive referee.

Pollock withdrew the paper again; however, on 19 January 1865, Cayley wrote another report for yet another version of this paper. Perhaps fatigued by the matter, he reviewed it favourably, writing that ‘the proof... appears to me... to be conclusive’. Smith, however, noticed a problem in a theorem, and outlined his objection in a note, which Stokes shared with Pollock. Pollock in turn wrote to Stokes that ‘The copy you have sent me of the remarks of “the second” does not notice the reciprocity of the 2 expressions.’ Pollock supplied improvements and wished that Stokes would ‘shew this to “the first fellow”... but I w[oul]d add tho’ it may fail to perfect the proof... there is much proved in the paper which is worth of notice in my judgment as being quite new—and very interesting.’ Although Pollock perceived the ‘first Fellow’, Cayley, as a bit more accommodating, Cayley had soon agreed with Smith that there was a fundamental error in Pollock’s argument. He wrote to Stokes the day after Pollock’s note of 10 April 1865 that Pollock’s improvements had done nothing to fix the problem. One day after that, Pollock wrote to Stokes, saying that after turning ‘the matter over, in what “I call my mind”’, he realized that he had forgotten to put an assumption in his paper, that the second fellow was absolutely right that there was a problem without it, and that the addition of it ‘really takes up the stitch’. Not quite. A few weeks later, Smith wrote to Stokes:

I do not know that I can add anything to what I have said before with reference of the Chief Baron’s Paper; nor do I think that the objection which has been made to the demonstration of that Paper, and which is stated by Professor Cayley in his note of April 11 with the utmost possible clearness, is in the least weakened by the considerations urged by the Chief Baron in his two notes dated April 10 and 12... I am unable therefore to alter the opinion I have already expressed that the Paper ought not to appear, in its present form, in the Transactions of the Royal Society.
After this formal rejection, a very interesting private postscript follows:

It seems to me that if, without really annoying the Chief Baron, you can induce him to withdraw the Paper, it would be the best thing for himself and for the Society; if you cannot, ... it might be possible to publish it with some slight modification, not as a ‘Demonstration of’ but as ‘Considerations relating to’ Fermat’s Theorem. ... There was a mathematician, whose name I forget, who tried to prove the Theorem upon the principle of Sufficient Reason. His paper was published by the Berlin Academy while Lagrange was still at Berlin; and I really think the Lord Chief Baron’s paper, though not good, at least as good as the ‘sufficient reason’ one. I agree with Cayley that it would not do any great harm to print it, if it were a little altered: and if this seems to you the best way out of the difficulty, I do not see why the Council should not take it. 

In the end, Pollock’s paper appeared only in abstract in Proceedings. He did make it back into Philosophical Transactions in 1868, with the paper ‘On the Mysteries of Numbers alluded to by Fermat. Second Communication’ (the first communication appeared only in abstract in Proceedings). The delicate treatment of Pollock illustrates the care that the referees and the Secretary sometimes employed when dealing with mediocre mathematicians who possessed great reputations outside mathematics.

A MATHEMATICIAN ON BOTH SIDES OF THE PROCESS: THE CASE OF ARTHUR CAYLEY

What issues did referees and the Secretary confront when great mathematicians submitted to the Society mathematics badly fitted for Philosophical Transactions? Arthur Cayley, whom I introduced above as a referee, put his own referees in such a position. Often, Fellows called on to referee Cayley’s work would decline because it was too technical for them to understand. When they did not bow out, they were very cautious when suggesting anything other than publication in Philosophical Transactions.

For example, one referee report of Cayley’s work authored by George Boole, Professor of Mathematics at Queen’s College, Cork, reveals some serious reservations and backpedalling. Boole’s pioneering work on invariant theory of 1841 had inspired Cayley to further the subject, thereby producing ‘the most important idea of his mathematical life’. Cayley, in turn, had proposed Boole for Royal Society membership less than two years before Boole was saddled with the task of evaluating Cayley’s submission ‘On certain formulae of differentiation’ for publication in Philosophical Transactions. Boole wrote in his report:

the extreme complexity of the analysis which Mr. Cayley has employed and which does not even suffice to enable him to verify both of the identities in question, raised a doubt whether the proper method ought not to be sought from another direction. ... On these grounds I am somewhat doubtful (I wish it to be understood that I am only doubtful) whether to recommend the publication of the memoir in the Philosophical Transactions. I think it right however to add that the memoir does possess a certain independent value from its containing the solution of the exceedingly difficult equation of partial differences.

Besides softening his criticism by emphasizing his uncertainty and the paper’s ‘independent value’, Boole wrote in a postscript: ‘Since writing the above report I have again examined the paper and formulae of differentiation, and I am now disposed to request the committee to submit it to another examiner communicated if it is thought desirable the observations which I have made upon it.’
William Fishburn Donkin, professor of astronomy at Oxford, was called in as a second referee. In his report he applauded Cayley’s effort, but was unsure of the value of his conclusions:

The cases here examined lead to very complicated calculations, which to ordinary mathematicians would most likely have seemed inextricable, and I believe few besides Mr. Cayley would have arrived at any available result. I do not know what may be the value of the conclusions actually obtained at all events the processes might probably to some extent serve as a guide or pattern for similar investigations. Considered in this light, and as a specimen of unusually complex analysis conducted with great skill, I think [the] paper deserves a place in the Transactions; though I should regret its omission less than that of either of the two former memoirs.46

Donkin had refereed the ‘two former memoirs’ in the same report. Cayley was a tireless producer of mathematics; in fact, his Collected Mathematical Papers occupy 13 volumes. Cayley was so prolific that his referees often reviewed more than one of his papers at a time in their reports; indeed, Boole’s report, cited above, had reviewed two of Cayley’s papers. In the end, this paper appeared in Proceedings in abstract only.47

Two weeks later, the English mathematician William Spottiswoode reported on four more of Cayley’s submissions. The first three gave him no pause, but

About the remaining paper ‘On the Surface which is the Envelope &c’, I have some hesitation. [It is] without doubt interesting & masterly; but, as it relates rather to a special problem than to a general principle, & does not introduce any distinctly new method, it is perhaps better suited for a mathematical periodical than for the Philosophical Transactions.48

The second referee, Bartholomew ‘Bat’ Price, the Sedleian Professor of Natural Philosophy at Oxford, agreed with Spottiswoode, writing that the paper ‘appears to be characterised by the great ability and ingenuity which belongs peculiarly to Mr. Cayley: and it is also a good example of the application of his method: yet I think that it is a paper better adapted to a Mathematical Journal than to the R.S. Transactions.’49 Although these two referees were in agreement, the Council brought in yet another, the Scottish mathematician and lawyer Archibald Smith, who got to the heart of the matter: whether a skilful but not truly novel mathematical result has its proper place in Philosophical Transactions. Smith wrote that if ‘the question of the publication of this paper in our transactions depended on its intrinsic merits or even on its mathematical interest, I should have no hesitation in recommending it for publication.’50 However, he continued:

it appears to me that there is a more important question of principle involved of which the Council at large can judge as well as the referees, and that is whether the solution of such a mathematical problem as is proposed in this paper, viz., the discussion of a particular Curve or surface, which as far as I am aware has no interest beyond itself, and in particular no physical or historical interest, nor even such general Mathematical interest as the discussion of a class of curves or surfaces would possess, and which discussion neither introduces nor exemplifies any new mode of investigation finds its most appropriate place in our transactions, or whether it is not rather better fitted for publication in some exclusively Mathematical Journal.51

In the end, only an abstract of this paper appeared in Proceedings.52
In the opinion of his biographer Tony Crilly, Cayley was most probably aware that some of his papers were not well fitted for *Philosophical Transactions*, but he used the journal as an outlet for the publication of longer articles that would not be readily accepted by independent mathematical journals. For example, Cayley’s choice of *Philosophical Transactions* for his ‘Memoirs on Quantics’, a codifying series of papers on the new area of invariant theory,

was probably dictated by the anticipated length and nature of the memoirs. It was not a specialized mathematical journal and few Royal Society fellows would gain much benefit, since even the selected referees found the material difficult and unfamiliar. In its favor, the Philosophical Transactions could accommodate bulk while the editor of *Crelle’s Journal* [*Journal für die reine und angewandte Mathematik*] in whose journal he placed shorter works, would surely have demurred.\(^{53}\)

In admitting difficulties in understanding his work, Cayley’s referees provided vivid impressions of their mathematical colleague. In reporting on Cayley’s ‘On the Porism of the In-And-Circumscribed Polygon’, the Scottish mathematician Phillip Kelland gave an ‘undoubtable opinion that it ought to be printed.’\(^{54}\) However, he expressed frustration with being unable to connect the geometrical ideas of the problem with Cayley’s analysis of it, remarking:

> Mr. Cayley appears to me to be one of those who are powerful in throwing out blocks for the next generation to work up. Their present value would be increased a hundred fold, were some fragments from them chiselled and made fit for ordinary hands.\(^{55}\)

**A NEW OPTION FOR REFEREES: PROCEEDINGS**

As is seen in the section above, Cayley’s referees suggested that some of his prolific mathematical output should appear in *Proceedings*. This option, albeit under a different title as discussed below, opened to Royal Society referees in 1839, and it rescued many papers that would have remained unpublished despite the quality of the results contained in them. The foundation of *Proceedings* can be seen as a product of the same Royal Society reform movement that formalized the refereeing process. Both Augustus Bozzi Granville, whose concerns about the Committee of Papers were discussed above, and the mathematician and Fellow of the Royal Society Charles Babbage recognized a need to widen the publication venue of the Society. In his *Reflections on the Decline of Science in England*, Babbage advocated the publications of abstracts to papers that had appeared in *Philosophical Transactions* so as to increase the circulation of ideas contained within them and to present them in a more streamlined form: ‘[p]erhaps two or three volumes octavo, would contain all that has been done in this way during the last century.’\(^{56}\)

The concerns of both Granville and Babbage were addressed in 1832 with the establishment of *Abstracts of Papers Printed in the Philosophical Transactions of the Royal Society of London*. While the title of this new journal aptly described the content of its first two volumes, by its third the journal had added further material: accounts of Society business, abstracts of papers never published in *Philosophical Transactions*, the President’s annual address, a report of the Council and Secretary, and a listing of medal winners and deceased Fellows (some with eulogies).\(^{57}\) A change of title to *Abstracts of the Papers Communicated to the Royal Society* in the fifth volume, published in 1851,
reflected the growing use of the journal as a repository for results overlooked by *Philosophical Transactions*. The change of the journal’s title to the familiar *Proceedings of the Royal Society of London* in 1856 also heralded further changes in the format of the publication. As the 1897 *Record of the Royal Society* explained:

> many papers were published in full in this and the subsequent volumes which were not published in the ‘Philosophical Transactions’ at all. These papers were for many years only the briefer or less important communications, the more bulky or more valuable papers being reserved for the quarto form. In time even this distinction became less marked, some papers of great importance appearing only in the ‘Proceedings’.58

As a referee, H. J. Stephen Smith used the potential of *Proceedings*. Recall that in the case of Pollock, he was sensitive to the ‘difficulties’ inherent in shelving certain articles. As his refereeing career progressed, Smith began to see *Proceedings* as a way of sidestepping political ‘difficulties’ but also of getting good mathematics printed and out of the archives. Again and again, Smith suggested *Proceedings* as a place in which to publish articles *in extenso*.

For example, in 1864, Smith recommended this course of action for a ‘Memoir on the Calculus of Symbols—Fourth Memoir’, by the 1851 Cambridge graduate William Henry Leighton Russell. Smith recommended that ‘the Paper be printed at full in the Proceedings; because the investigation which it contains is an useful one, not hitherto published, and relating to an important Problem.’59 Although the first three papers in this series had been published in *Philosophical Transactions*, Smith felt that *Proceedings* was the most fitting place for the fourth instalment:

> I presume that the Committee would expect a Paper printed in the Transactions to contain some important accession to Mathematical knowledge, either as introducing some new ideas and principles into the science, or as overcoming some acknowledged difficulty, or (at the least) as containing such a complete treatment of its subject as to make it of permanent value. And I do not think that Mr. Russell’s Paper is of a character to satisfy these higher requirements.60

Not only did the abstract of the paper appear in *Proceedings*, but also, in a later number, the full paper.61

The minutes of the Committee of Papers show that it first exercised this option of printing a paper *in extenso* in *Abstracts of Papers Printed in the Philosophical Transactions of the Royal Society of London* (an earlier title for *Proceedings*) on 7 February 1839. As the nineteenth century progressed, this option was employed increasingly (figure 1). For the period 1832–1900, the minutes accounted for 3500 paper submissions that were not published in *Philosophical Transactions*. Although the total number of papers submitted grew over the period, from 169 in the 1830s, to 339 in the 1850s, to more than 700 per decade for the last 30 years of the century, the number of archived papers was fairly constant, around 100 per decade. The option of publishing in *Proceedings* kept the number of archived papers low as the total number of submitted papers grew. The same pattern can be seen in the 338 papers that I classify as mathematical (figure 2).62

Despite the increased use of *Proceedings* for publishing full papers, even near the end of the nineteenth century referees were instructed that it was the decision on suitability of the submission for *Philosophical Transactions* that was their primary decision. The first question in a standard report form from 1899 asked the referee ‘Whether the paper should or should
Figure 1. Papers not accepted for Philosophical Transactions, 1832–1900.

Figure 2. Papers on mathematics not accepted for Philosophical Transactions, 1832–1900.
not be published in the “Philosophical Transactions”’. These formal printed sheets for referees had been introduced a year previously. Also in 1898, the anonymity of the referees was finally guaranteed by leaving their names out of the Society’s journal book.

**AN ELITE CIRCLE: MATHEMATICAL REFEREES FOR THE ROYAL SOCIETY**

Not surprisingly, the pool of mathematical referees for Royal Society publications was restricted to the Fellowship. Was the pool even more restrictive? The answer to this question is convoluted and reflects the arduous process of reform in the nineteenth-century Royal Society.

As mentioned above, the Committee of Papers in the eighteenth century had the option of asking any Fellow for assistance in its deliberations. The 1831 Statutes continued this practice:

> [papers] failing to gain a majority vote on two meetings of the Committee were rejected, but the Committee could call upon any Fellow to present a written Report to assist the process of deliberation before the second meeting. This system of assessment commenced in December 1831, and soon became the norm for most papers, although the Reports were not necessarily presented in person.

However, the description by the Duke of Sussex of the 1832 Council resolution ‘to allow no Paper to be printed in the Transactions of the Royal Society, unless 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’ seems to imply that the referees must have come from the Council membership. In 1838, spurred on by accusations that the Council was treating the paper of a physiologist unfairly, Sectional Committees were formed for physiology and for six other scientific subjects, including mathematics. These committees then supplied referees and also made recommendations for the awarding of the Royal and Copley Medals. Using distinct groups separated by discipline to consider the publication of papers was a realization of one of Granville’s proposed reforms; however, allegations of bias, especially in the Sectional Committee for physiology, caused the committees to dissolve in 1849. In 1896, the Sectional Committees were resurrected ‘and have continued in increasing number ever since.’

A ‘Note on the History of the Statutes of the Society’ appearing in *Proceedings* for 1891–92 stated:

> the custom of the Committee [of Papers] is now, and for a long time has been, to ‘call in to their assistance’ two or more Fellows, by asking for written reports, and such Fellows so assisting are generally spoken of as ‘referees’… For some time the name of the person (or persons) to whom the paper was referred is stated in the Minutes of the Committee of Papers, and in all these cases…the persons in question were members of the then Council. Very soon, however, the name was omitted, the entry being simply ‘referred’. There seems to be no means of ascertaining when ‘referees’ outside the Council were first had recourse to…’

Even under the freer criterion that referees were to come from the Society Fellowship, the number of mathematicians eligible to referee papers was limited. In his history of the administration of the Royal Society to 1940, Sir Henry Lyons lists only 12 Fellows (8% of the scientific Fellows) as mathematicians in 1800, 21 (9.6%) in 1830, and 28 (8.5%) in
1860. If we add astronomers and physicists to this count, we still only obtain 27 (18\%) such Fellows in 1800, 48 (23\%) in 1830, and 76 (23\%) in 1860.\textsuperscript{69} The limited size of this pool of eligible mathematical referees implies that a Victorian mathematician who could sign his name with ‘FRS’ was part of a very elite group within his discipline given the privilege and the great burden of guarding the gates to publication in the Royal Society’s journals. An obituary notice for James Clerk Maxwell gives a sense of this burden and privilege:

Nor must we omit another species of work always performed by Maxwell kindly and conscientiously, professorial work, surely, of the very highest kind, that, viz., of reading and reporting on papers contributed to learned societies by young aspirants to scientific fame. This kind of work, of which much fell to Maxwell’s share, is but little known to the outside world, but involves when carefully performed a vast expenditure of time and trouble even on the part of the most accomplished specialist.\textsuperscript{70}

THE REFEREEING PROCESS IN OTHER BRITISH SCIENTIFIC SOCIETIES

As noted in the quote above, Maxwell acted as a referee for a variety of learned societies. Indeed, other British scientific societies adopted refereeing procedures that were variants of the successive versions followed by the Royal Society. From the first volume of the Royal Society of Edinburgh’s Transactions, for example, the Council selected the papers for publication. By at least the mid-nineteenth century, this body began appointing ‘“members conversant with the subject” to act as referees’.\textsuperscript{71} For the Manchester Literary and Philosophical Society, the concept of a council was synonymous with a group who would select papers for publication. In fact, the original name of the Manchester society’s leadership group was the Committee of Papers. This body decided which papers to publish in the Manchester society’s Memoirs and even ‘select[ed] parts of a paper for publication if the whole be deemed unsuitable.’\textsuperscript{72} Although it automatically included the Society’s officers, the 14-member Committee also included six members of the Society outside the ruling circle.\textsuperscript{73}

Although scientific societies employed refereeing committees to provide a more objective system in which to evaluate papers, these groups were not always capable of recognizing outstanding submissions. For example, as a result of the Royal Irish Academy’s refereeing practices, ‘probably the most significant contribution ever submitted to the Academy was at first rejected.’\textsuperscript{74} In 1824, Henry H. Harte, Dionysius Lardner and a Dr MacDonnell were assigned by the Academy to evaluate a paper on optics by an undergraduate of Trinity College, Dublin. In their report of the paper submitted in June 1825, the committee wrote:

the results at which the author has arrived are novel and highly interesting, and… considerable analytical skill has been manifested in the investigations which lead to them. But we conceive that the discussions included in the Memoir are of a nature so very abstract, and the formulæ so general, as to require that the reasoning by which some of the conclusions have been obtained should be more fully developed, and that the analytical process by which some of the formulæ have been obtained should be distinctly specified. This we conceive to be necessary in order to render the publication of the Memoir generally useful.\textsuperscript{75}

Although probably not happy with these opinions, the young author, William Rowan Hamilton, rewrote and expanded his original work, ‘On Caustics’, and resubmitted it two
years later to the Academy as ‘Theory of Systems of Rays’.\textsuperscript{76} This enlarged paper, in the opinion of Hamilton’s first biographer, Robert Percival Graves, ‘became the foundation of his mathematical fame’.\textsuperscript{77}

British societies devoted to science in general were not the only ones to adopt refereeing procedures similar to those of the Royal Society. The London Mathematical Society established a rigorous peer review process for its \textit{Proceedings}. An early and sustained member of the London Mathematical Society’s council, James Glaisher, recalled:

Every paper was invariably considered by two referees, who sent in written reports which were read to the Council; and when the reports differed the paper was sent to a third referee. Every paper was balloted for, to decide whether it should be printed... At the \textit{[Royal] Astronomical Society}, on the contrary, it was rarely that a paper was refereed, and a verbal report from a single referee was generally accepted.\textsuperscript{78}

It is not surprising that the refereeing procedures of the London Mathematical Society should have mirrored those of the Royal Society. Glaisher reported that Thomas Archer Hirst, Henry Smith and Arthur Cayley all performed the ‘rather irksome duty’\textsuperscript{79} of refereeing papers; these three mathematicians and Glaisher all served as referees for the Royal Society.

\textbf{CONCLUSIONS}

Besides serving as a model for their own society, the refereeing process gave mathematical Fellows of the Royal Society active roles in controlling the content and quality of the mathematics published in the Society’s journals. The referee reports, preserved in the Royal Society Library, are a valuable and intriguing record of this activity. Between and sometimes squarely on the lines, one can find intrigue, politics, personal concern and humorous asides. Although not free from forces outside mathematics, mathematical referees could guide and encourage the research in their discipline while setting limits on the type and depth of mathematics appearing in a Society journal.

Through the reports, one can see that refereeing mathematics for the Royal Society during the nineteenth century was not simply an exercise of judging papers as good or bad. A referee might decide to shelve a piece of ‘good’ mathematics because it was too specialized for \textit{Philosophical Transactions}, and he might decide to have published a piece of ‘bad’ (or, at least not good) mathematics because of the stature of its author. This is not to say that the referees did not take their jobs seriously. In spite of other obligations, they dedicated large amounts of time and energy to carefully reading, understanding, and educating the committee about papers on increasingly specialized topics. These referees wanted to support mathematics, to see it on the pages of \textit{Philosophical Transactions}, but they wanted this mathematics to be at a certain level of novelty, clarity and sophistication. They often used a considerable amount of ink in trying to make the submitted papers better. They spent time outlining corrections and asking for clarification and better citations. If a paper did get archived, this decision was usually reached after a good amount of deliberation.

With the emergence and development of \textit{Proceedings}, referees of mathematical papers gained a valuable new option. Significant pieces of mathematical research, while not general enough or novel enough for \textit{Philosophical Transactions}, could proceed directly to \textit{Proceedings} instead of being archived. As more mathematics was published in
Proceedings, it became less of a home for second-class papers and more of an alternative venue for publication. Proceedings kept the number of archived papers low as the number of submitted papers grew. In this way, mathematicians could participate more fully in the publication function of the Royal Society, which communicated research beyond the walls of the meeting room to the wider British mathematical publication community.80

The mathematical referees of the Royal Society helped build and keep the gates guarding the journals in which publication led to distinction. These referees saw their jobs as a duty to the Society, and a duty to British mathematics. Despite the politics inherent in their roles, they in effect established case law for what level and type of mathematics should appear in the premier scientific journal in Britain.

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NOTES

1 ‘Advertisement to the Philosophical Transactions, volume 47’; quoted in The Record of the Royal Society, 1897 (Harrison & Sons, London, 1897), p. 103.
2 Sir Henry Lyons, The Royal Society, 1660–1940 (Cambridge University Press, 1944), p. 253. The Royal Society’s officers and vice-presidents were made permanent members of this committee. For the Royal Society as well as other British scientific societies, a paper could be submitted directly if the author was a member of the society or could be ‘communicated’ through a member. Dwight Atkinson, Scientific discourse in sociohistorical context: the Philosophical Transactions of the Royal Society of London, 1675–1975 (Lawrence Erlbaum Associates, Mahway, NJ, 1999), p. 43.
3 Royal Society statues of 1752; quoted in Record, op. cit. (note 1), p. 104.
4 Record, op. cit. (note 1), p. 104.
7 Augustus Bozzi Granville, Science Without a Head; or, The Royal Society Dissected (T. Ridgway, London, 1830), pp. 54 and 57–58.
10 Ibid., pp. 141–142.
11 Ibid., p. 142.
12 According to Atkinson (op. cit. (note 2), pp. 42–43), ‘after about 1840, prior publication in the PTRS became an important criterion for membership.’ In addition, in 1834, ‘new fellows were charged £60 to join the Society if they had not published in the journal, but only £40 if they had. Although this policy was given up in 1871, a ‘P’ continued to be printed in membership lists next to the names of fellows who had contributed to the journal until 1887.’ Craik points out that not all Fellows approved of the new system. For example, David Brewster was deeply
offended that one of his papers was rejected through the anonymous refereeing process; A. D. D. Craik, ‘James Ivory’s last papers on the “Figure of the Earth” (with biographical additions)’, *Notes Rec. R. Soc. Lond.* 56, 187–204 (2002), at p. 192. Craik’s article also examines Royal Society referee reports surrounding an 1838 paper by James Ivory and one written 33 years later by Isaac Todhunter that was mercilessly critical of the former paper.


Although the paper was not published, a very short abstract of it did appear as Richard Abbott, ‘On the Variation of a Triple Integral’, *Abstr. Pap. Printed Phil. Trans. R. Soc. Lond.* 4, 42 (1837–43). When an author submitted a paper to the Royal Society, there was no guarantee that he would get it back. An archived paper was not returned to the author, although he could ask to copy it. This practice was established in 1776 with a statute ‘to the effect that the original copy of every paper read at the Society shall be considered as the property of the Society.’ (Michael Foster, ‘A Note on the History of the Statutes of the Society’, *Proc. R. Soc. Lond.* 50, 501–515 (1891–92), at p. 512.) It continued until 1984, and the Royal Society Library currently houses 1603 of these archived papers dating from 1768 to 1984 in nine boxes and 79 bound volumes. This collection may be a good source of ‘proto-refereeing’ at the Society: the library record for these archived papers states interestingly, ‘[e]arly papers in this sequence are occasionally of interest in being preserved complete with associated correspondence (pre-dating Referees Reports)’. Library record for ‘AP Archived Papers 1768–1984’ at http://royalsociety.org/Catalogues/#archive (accessed 6 February 2011).


Report by George Peacock to Samuel Hunter Christie, Sec.R.S. Royal Society Archives, MS. RR.1.57, 16 October 1847.

Rice, *op. cit.* (note 16), p. 213. In this paper, De Morgan pointed out that although it was thought to have been entirely British, the committee that wrote the *Commerium epistolicum* included two Continental members (*ibid.*, pp. 213–214). Augustus De Morgan, ‘On a Point Connected with the Dispute between Keill and Leibnitz about the Invention of Fluxions’, *Phil. Trans. R. Soc. Lond.* 136, 107–109 (1846).


H. C. G. Matthew and Brian Harrison, *Oxford Dictionary of National Biography* (Oxford University Press, 2004), under the headings ‘Pollock, Sir (Jonathan) Frederick, First Baronet’ and ‘Pollock, Sir Frederick, Third Baronet’. One of Pollock’s grandsons, Sir Frederick Pollock, Third Baronet, followed his grandfather’s career path, and was a renowned legal scholar and a close correspondent of Oliver Wendell Holmes.

George Peacock to Samuel Hunter Christie. (3 pages.) Royal Society Archives, MS. RR.2.188, 8 April 1850.

*Ibid.* Peacock’s appraisal might seem to discount the excellent Continental mathematical journals in existence at that time, such as the *Journals of Liouville* and Crelle. However, Peacock could have been ranking the *Cambridge and Dublin Mathematical Journal* among a more limited class of university affiliated mathematical journals. Nonetheless, Peacock was a former Analytical Society member and Cambridge reformer who had sought to compel Cambridge students to abandon old fluxionary methods for Continental, differential methods, and his comments about the *Journal* probably reflected a sort of paternal pride. For more on the *Cambridge and Dublin Mathematical Journal*, see Tony Crilly, ‘The *Cambridge Mathematical Journal* and its descendants: the linchpin of a research community in the early


26 Frederick Pollock, ‘On Certain Properties of Square Numbers and Other Quadratic Forms, with a Table of Odd Numbers from 1 to 191, Divided into 4, 3 or 2 Square Numbers, the Algebraic Sum of Whose Roots (Positive or Negative) May Equal 1, by means of Which Table All the Odd Numbers up to 9503 May be Resolved into not Exceeding 4 Square Numbers’, *Phil. Trans. R. Soc. Lond.* **144**, 311–319 (1854); ‘On Some Remarkable Relations Which Obtain among the Roots of the Four Squares into Which a Number May be Divided, as Compared with the Corresponding Roots of Certain Other Numbers On Some Remarkable Relations Which Obtain among the Roots of the Four Squares into Which a Number May be Divided, as Compared with the Corresponding Roots of Certain Other Numbers’, *Phil. Trans. R. Soc. Lond.* **149**, 49–59 (1859); ‘On Fermat’s Theorem of the Polygonal Numbers’, *Phil. Trans. R. Soc. Lond.* **151**, 409–421 (1861).


29 Henry J. S. Smith to G. G. Stokes. (4 pages.) Royal Society Archives, MS. RR.5.190, 6 September 1862.

30 Referee’s report by Arthur Cayley. (2 pages.) Royal Society Archives, MS. RR.5.189, 22 October 1862.

31 Frederick Pollock to George Gabriel Stokes. (4 pages.) Royal Society Archives, MS. RR.5.195, 17 January 1863. The minutes of the Committee of Papers show that the paper was withdrawn by the meeting on 19 February 1863.

32 Referee’s report by Henry J. S. Smith. (3 pages.) Royal Society Archives, MS. RR.5.191, 14 October 1863.

33 Referee’s report by Arthur Cayley. (3 pages.) Royal Society Archives, MS. RR.5.192, 2 November 1863.

34 Henry J. S. Smith to G. G. Stokes. (7 pages.) Royal Society Archives, MS. RR.5.193, 10 November 1863.


36 ‘Note on the Chief Baron’s Paper “On Fermat’s Theorem of the Polygonal Numbers” ’ by H. J. S. Smith. (4 pages.) Royal Society Archives, MS. RR.5.201, n.d. This note was written anonymously.

37 Frederick Pollock to George Gabriel Stokes. (4 pages.) Royal Society Archives, MS. RR.5.198, 10 April 1865.

38 Frederick Pollock to George Gabriel Stokes. (4 pages.) Royal Society Archives, MS. RR.5.197, 12 April 1865. Pollock’s emphasis.

39 Henry J. S. Smith to G. G. Stokes. (6 pages.) Royal Society Archives, MS. RR.5.199, 31 May 1865. Pollock’s emphasis.
40 Ibid.
43 Ibid., p. 221.
44 Referee’s report by George Boole. Royal Society Archives, MS. RR.3.55, 29 March 1858.
45 Ibid.
46 Report by W. F. Donkin to George Gabriel Stokes. Royal Society Archives, MS. RR.3.57, 12 April 1858.
50 Ibid.
51 Ibid.
52 Arthur Cayley, ‘On the Surface which is the Envelope of Planes through the Points of an Ellipsoid at right angles to the Radii Vectors from the Centre’, *Proc. R. Soc. Lond.* 9, 171–172 (1857–59). Interestingly, an identical abstract appeared in *Phil. Mag.* (4) 16, 383–384 (1858), and the full version of the paper appeared in *Annali di matematiche pura ed applicata*, founded by Barnaba Tortolini. (‘Sur la surface qui est l’enveloppe des plans conduits par les points d’un ellipsoide perpendiculairement aux rayons menés par le centre’, *Annli Mat. Pur. Appl.* 2, 3–14 (1859), reprinted in *The collected mathematical papers of Arthur Cayley*, vol. 4, pp. 123–134 (Cambridge University Press, 1891)). Cayley was no stranger to this Italian journal, which represented a good fit for his article, because, as Cayley wrote in his abstract, ‘The consideration of the surface in question was suggested to me some years ago by Professor Stokes; but it is proper to remark, that the curve which is the envelope of lines through the points of an ellipse at right angles to the radius vectors through the centre occurs incidentally in Tortolini’s memoir “Sulle relazione”, &c., Tortolini, vol. vi. pp. 433 to 466 (1855).’
54 Referee’s report by Phillip Kelland. Royal Society Archives, MS. RR.4.45, 27 May 1861.
55 Ibid., Kelland’s emphasis. The problem involved finding conditions for two conics such that ‘a polygon may be inscribed in the one, and circumscribed about the other conic, in such a manner that any point of the circumscribing conic may be taken for the vertex of the polygon.’ Cayley had given a general formula for this problem in 1853, but sought in this paper to put the relationships between the conics in a ‘new and simple form’. Arthur Cayley, ‘On the Porism of the In-And-Circumscribed Polygon’, *Phil. Trans. R. Soc. Lond.* 151, 225–239 (1861), at p. 225. This appears in *The collected mathematical papers of Arthur Cayley*, vol. 4, pp. 292–308.
Philosophical Transactions from 1800 to 1814, and volume 2 covered the papers from 1815 to 1830. The 1850s also saw another important Royal Society initiative to increase the diffusion of scientific knowledge: the Catalogue of Scientific Papers. This enormous undertaking provided a listing of scientific papers in major journals worldwide. The first volume was published in 1867 and was followed by 18 others; the last volume was published in 1925 (three subject indexes, including one on pure mathematics, were also published). The Catalogue of Scientific Papers made it possible for almost any serious scientific worker to discover what work had been done in his field and by whom, and in many cases showed him how to gain access to it. . . . The Catalogue only ceased to be essential when the ever greater increase in science made abstracting journals in each scientific field a necessity’ (pp. 154–155).

58 Record, op. cit. (note 1), p. 165.
59 Referee’s report by Henry J. S. Smith. (4 pages.) Royal Society Archives, MS. RR.5.225, 10 April 1864.
60 Ibid.
62 Atkinson corroborates the importance of Proceedings as a publication venue of the Royal Society. In comparing submission records of the Royal Society for 1824–26 with those of 1874–76, he found that during the later period many more papers were being published: ‘the Proceedings was now publishing the lion’s share of submissions to the Society, outdistancing the PTRS by approximately three-to-one, and . . . many fewer papers were being rejected than had been earlier in the century, when the PTRS was the only vehicle of Royal Society publication. Thus, whereas about 23% of the papers considered for publication had been rejected in 1824–1826, only 10% fell into this category from 1874–1876.’ Atkinson, op. cit. (note 2), p. 43.
64 Atkinson, op. cit. (note 2), p. 44.
67 Hall, op. cit. (note 57), pp. 69 and 126.
72 Robert H. Kargon, Science in Victorian Manchester: enterprise and expertise (Johns Hopkins University Press, Baltimore, MD, 1977), pp. 7 and 14. This body’s duties also included adjudicating the awards and premiums set by the Society. It assumed the name ‘Council’ in 1822.
73 Ibid.


James W. L. Glaisher, ‘Notes on the early history of the Society’, *J. Lond. Math. Soc.* **1**, 51–64 (1926), at p. 60. Glaisher was the Secretary of the Royal Astronomical Society from 1877 to 1884, and from 1881 to the end of his secretaryship he was the editor of its publications.

Ibid, p. 63.

Viewing scientific papers contained in scientific journals as significant indicators of research, we can consider scientists who authored or read and responded in print to papers in a given area within a given group of journals to constitute a publication community. For more on this use of ‘publication community’, see Sloan Evans Despeaux, The development of a publication community: nineteenth-century mathematics in British scientific journals. PhD thesis, University of Virginia, 2002; abstract in *Dissertat. Abstr. Int.* **63** (09B), 4201 (2002).