NOTES AND RECORDS

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CREDIBILITY, PEER REVIEW, AND *NATURE*, 1945–1990

by

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This paper examines the refereeing procedures at the scientific weekly *Nature* during and after World War II. In 1939 former editorial assistants L. J. F. Brimble and A. J. V. Gale assumed a joint editorship of *Nature*. The Brimble–Gale era is now most famous for the editors' unsystematic approach to external refereeing. Although Brimble and Gale did sometimes consult external referees, papers submitted or recommended by scientists whom the pair trusted were often not sent out for further review. Their successor, John Maddox, would also print papers he admired without external refereeing. It was not until 1973 that editor David Davies made external peer review a requirement for publication in *Nature*. *Nature*'s example shows that as late as the 1960s a journal could be considered scientifically respectable even if its editors were known to eschew systematic external peer review.

Keywords: *Nature* (journal); peer review; refereeing; scientific journals; editorial policy

INTRODUCTION

In modern science, external refereeing is often considered the essential mechanism that protects the quality and trustworthiness of the scientific literature. As the physicist John Ziman put it in a frequently quoted passage from his 1968 book *Public knowledge*, 'The referee is the lynchpin about which the whole business of Science is pivoted.' According to Ziman, a scientific journal article does not simply contain 'the opinions of its author; it bears the *imprimatur* of scientific authenticity' by virtue of having been vetted by experts. In other words, refereeing is the reason that researchers, policy makers and the public can trust the claims put forward in a scientific article.[1] Today many observers are reluctant to accept a scientific finding unless the research has been evaluated by external referees. When *Physics Letters B* accepted a paper from CERN on the detection of the Higgs boson, for example, one journalist reported CERN's success with the headline 'CERN's Higgs boson discovery passes peer review, becomes actual science.'[2]

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Peer review's centrality to modern scientific practice has led many observers to assume that external refereeing has been a part of scientific publishing for centuries. In most accounts of the history of the scientific journal, it was Henry Oldenburg, the legendary secretary of the Royal Society of London, who introduced external refereeing to the newly created *Philosophical Transactions* in the late seventeenth century. Oldenburg, the story goes, wisely saw that he needed to consult experts to judge the quality of manuscripts, and thus the external referee has been part of science since the first scientific journal printed its very first issue.[3]

However, the history of refereeing at scientific periodicals is not nearly this straightforward.[4] Research on the early publishing history of the Royal Society has shown that the seventeenth-century *Philosophical Transactions* cannot be said to have employed any process resembling modern peer review.[5] In fact, *Philosophical Transactions* did not have a formalized refereeing process until 1752, when the Society created the Committee of Papers to vet submissions to *Transactions*.[6] Finally, Alex Csiszar's work has shown that while some nineteenth-century journals employed external refereeing, researchers at the time did not see such refereeing as a guarantee of accuracy or a requirement for scientific respectability.[7] Far from springing full-grown from the head of Henry Oldenburg, refereeing did not become a consistent feature of scientific journals until well after journals became the scientific community's site for establishing knowledge claims during the nineteenth century.[8]

Well into the twentieth century, many renowned scientists went their entire careers without having a paper refereed—and were not always enthusiastic when introduced to the practice. In 1936, for instance, Albert Einstein was extremely offended when he learned that the editor of *Physical Review* had sent his submitted paper to an external referee. In a terse note to the editor, John Tate, Einstein wrote that he and his co-author

> had not authorized you to show [our manuscript] to specialists before it is printed. I see no reason to address the—in any case erroneous—comments of your anonymous expert. On the basis of this incident I prefer to publish the paper elsewhere.[9]

Furthermore, many high-profile journals did not adopt external refereeing until the 1960s or even later. One especially striking example is that of the prestigious scientific weekly *Nature*, which did not consult referees for every paper it printed until 1973.

This paper uses *Nature* as a case study through which to examine the relationship between refereeing and scientific credibility in the mid-to-late twentieth century. L. J. F. Brimble and A. J. V. Gale, who assumed a joint editorship of *Nature* just before the outbreak of World War II, would generally accept articles without review if they came recommended by the right people. Their dynamic successor, John Maddox, did much to overhaul and speed up *Nature*'s review process, but he too retained the editor's absolute right to admit papers that he found interesting, with or without referees' opinions. Not until David Davies came to *Nature* in 1973 did external refereeing become a prerequisite for being published in *Nature*. *Nature*'s example shows that as late as the 1970s, a journal could be considered scientifically respectable even if its editors were known to eschew systematic external peer review (a term that did not come into widespread use until the late twentieth century)—which in turn suggests that the view of external refereeing as the 'lynchpin of science' originated later, and spread more slowly, than many observers have assumed. Furthermore, even after *Nature* began requiring multiple referee reports, the editor could still exert tremendous influence over which papers reached *Nature*'s pages.

Nature's early history and the Brimble–Gale editorship

Nature, a weekly for-profit journal that publishes research from across scientific disciplines, was founded in 1869 under the leadership of the astronomer and war office bureaucrat J. Norman Lockyer with the backing of the London publishing house Macmillan and Company. Its format included—and still includes—not only research articles but also news on recent scientific developments, opinion pieces about issues affecting science, and active correspondence columns. *Nature* is thus quite different from both research journals affiliated with scientific societies and most other commercial scientific journals, both because of the diversity in its format and because its articles are far shorter than those published in journals such as *Philosophical Transactions* or *Philosophical Magazine*.[10]

Lockyer would remain as *Nature*'s editor-in-chief until 1919, 50 years after *Nature*'s first issue was published. He handed the reins over to his editorial assistant, Richard Gregory, who had worked at the journal since 1893.[11] Like Lockyer before him, when Gregory decided to retire in 1938 he encouraged the Macmillans to promote from within the *Nature* staff. He specifically recommended his assistants L. J. F. 'Jack' Brimble and A. J. V. Gale for a joint editorship. 'M^{r} Gale & M^{r} Brimble are quite capable of carrying on and of maintaining the high reputation of the journal', Gregory told Daniel Macmillan in May 1938;

> We always have sufficient reviews & articles in type or reserve to last at least three months, & enough 'Letters to the Editor' to fill the correspondence columns for a month or two. ... Current topics & events vary in number & amount week by week, but Gale knows exactly how to deal with these, & when he & Brimble are in charge they should be given complete responsibility.[12]

Gale and Brimble had both been with *Nature* for several years. Gale had graduated from Selwyn College, Cambridge, with a degree in agriculture, and spent World War I in military service. In 1920 he accepted a position as Gregory's assistant with a salary of £200 per year for two days' work per week.[13] Brimble, nine years Gale's junior, came to the journal in 1931. He had earned his BSc from the University College of Reading and spent several years as a lecturer in botany, first at the University of Glasgow and then at the University of Manchester. After Brimble wrote a book review for *Nature* that Gregory admired, the Macmillans offered Brimble the opportunity to work for the journal as Gregory's second assistant.[14] In November 1938 Gale wrote to Gregory to say that he had discussed the joint editorial appointment with Daniel Macmillan, who had officially confirmed the position and offered him and Brimble increases in salary. 'Mr. Dan also promised not to interfere with Nature editorially', Gale assured Gregory. 'That was a useful point I think.'[15]

Daniel Macmillan probably felt that there was little risk in promising Brimble and Gale editorial autonomy. Unlike their predecessor, Gregory, who had leveraged his position as *Nature*'s editor into a successful career as a scientific spokesman, neither Gale nor Brimble had much interest in wielding wider public influence. In the absence of an editorial archive it is difficult to determine exactly how Brimble and Gale divided their responsibilities, but it is clear that the new editorial team was invested in maintaining *Nature*'s status quo.[16]

Brimble and Gale's steadiness would soon face a major challenge. On 1 September 1939 Germany invaded Poland. Two days later France and Britain declared war on Germany. For the second time in 25 years, Europe was at war.

World War II had a significant impact on *Nature*'s new editors. The *Nature* offices themselves escaped damage during the London Blitz, but Brimble was injured in a midwar bombing raid and would never fully recover. Furthermore, wartime paper restrictions limited *Nature*'s size—its length was cut from more than 40 pages per issue to less than 30—and postal problems delayed the printing and mailing of many issues.[17] Government restrictions on printing and electric power continued to delay issues of *Nature* for years after the war's end.[18] It was not until mid-1948 that *Nature*'s issues returned to their average prewar length of 40 pages or more.

The wartime experience permanently affected the way in which Brimble and Gale edited *Nature*. Paper restrictions and postal problems led them to publish a stripped-down version of *Nature* that contained little high-maintenance debate or discussion. Instead, wartime *Nature* ran many book reviews, reprints of lectures, and Letters to the Editor detailing new theories and recent experimental findings, and placed far less emphasis on up-to-date news coverage or commentary. These characteristics carried forward into the rest of Brimble and Gale's editorship. The controversies and debates that had made prewar *Nature* distinctive among specialist scientific periodicals in Britain almost vanished during the Brimble–Gale era. Avoiding heated discussions seems to have been an explicit policy rather than an inadvertent omission. In 1950, for example, the 'News and Views' column informed readers that a recent article by Julian Huxley on Soviet genetics 'could clearly not be allowed to become the subject of debate in the correspondence columns of this journal'—a sentiment that would have seemed very odd to anyone who remembered *Nature* under Lockyer or Gregory.[19]

Mary Sheehan, who joined the journal in 1966 as the assistant to the new editor, John Maddox, recalls thinking that *Nature* 'almost came out on its own in a funny sort of way.'[20] Brimble and Gale (and, after 1961, just Brimble) had remade the journal so that it required little active editorial management; arguments were avoided, Rainald Brightman, Chief Librarian for the Dyestuffs Group at Imperial Chemical Industries, wrote almost all of the editorials, 'News and Views' was put together from institutional press releases, and any research article or Letter that looked reasonable was likely to be accepted, especially if it came from a well-known laboratory. As a result, *Nature* lost much of the liveliness and sense of immediacy that it had possessed under Lockyer and Gregory. David Davies, who became *Nature*'s editor in 1973, would aptly call *Nature* of the 1950s 'worthy but dull'.[21]

Local networks and the DNA papers

Despite the comparative 'dullness' of the postwar years at *Nature*, Brimble and Gale were responsible for printing some of the most renowned papers ever to appear in *Nature*'s pages. Most famously, Brimble and Gale approved the publication of James Watson and Francis Crick's 1953 article 'A structure for deoxyribonucleic acid', now considered one of the signal scientific achievements of the twentieth century.[22] The Watson–Crick paper serves as a useful illustration of two important features of *Nature*: first, its reputation for relatively speedy publication, and second, the extent to which Brimble and Gale relied on prominent scientists—particularly British ones—to recommend *Nature*'s content.

The Watson–Crick paper on the structure of DNA has a famously contentious backstory. Watson was an American who had come to Cambridge's famed Cavendish Laboratory specifically to work on nucleic acid structures; his collaboration with the British Crick began shortly after his arrival in England. As Watson would tell the story in his autobiography *The double helix*, nothing mattered more than being the first to solve the puzzle of DNA's structure—especially if it meant beating Linus Pauling at the California Institute of Technology, who was also working on the same question.[23] Watson and Crick were also 'racing' against another group of researchers in England: a team at King's College, London, that included the biophysicist Maurice Wilkins and the crystallographer Rosalind Franklin.

The Watson–Crick paper was actually one of three papers about DNA that *Nature* published on 25 April 1953; the other two papers came from their rivals at King's. Wilkins, who would share the Nobel Prize for DNA's structure with Watson and Crick, was the lead author of the second paper, 'Molecular structure of deoxypentose nucleic acids'.[24] Franklin, who had moved to Birkbeck College earlier that year, was the lead author of 'Molecular configuration in sodium thymonucleate'.[25] This paper included Franklin's famous photograph of the DNA B form, 'Photo 51'—which had been shown to Watson without her knowledge or permission, and which had been a key inspiration for Watson and Crick's model.[26]

Watson, Crick and Wilkins have each written personal recollections of the events leading up to the publication of the DNA papers.[27] Interestingly, none of them mention why they chose to send their findings to *Nature* instead of another journal. In *The double helix*, the choice of journal appears obvious to the authors; Watson simply says, '*Nature* was a place for rapid publication', and a week after the crucial insight, 'the first drafts of our *Nature* paper got handed out'.[28] Wilkins recalls that Watson and Crick decided to submit 'a short paper to be published quickly in *Nature*', and that 'consultation and negotiation with the editor gave King's a week or two' to write accompanying papers detailing their own results.[29] It seems almost a foregone conclusion that Watson, Crick, Wilkins and Franklin would submit their results to *Nature*.

There are several reasons why *Nature* would have seemed a clear choice for the DNA papers. *Nature* had become known as a venue for the fast publication of new results in the early twentieth century.[30] The journal's weekly publication schedule meant that submissions could be published faster than in monthly or quarterly journals, and by the 1930s scientists from across disciplines and national backgrounds were sending their most exciting work to *Nature* in the hope that it would appear in print quickly and establish priority claims.[31] Given Watson's interest in priority for the findings, *Nature*'s weekly publication schedule was probably the most important reason behind the DNA team's choice of journal. Wilkins's comment about Watson and Crick's desire for a note that could be 'published quickly' supports this interpretation, as does Watson's observation about *Nature* being a place for fast publication. Furthermore, by the early 1950s *Nature* had become one of the major publications for scientists working in the field of nucleic acid research, making it a natural choice for papers on DNA.[32]

The assumption that results would be sent to *Nature*, however, was not just about *Nature*'s international reach in the field of molecular biology. In fact, local scientific networks seem to have been equally (if not more) important in determining that the DNA papers would be printed in *Nature*. Sir Lawrence Bragg, the head of the Cavendish Laboratory, had long-standing connections with Brimble and Gale. Watson's recollection of Bragg's first look at their famous paper suggests that Bragg both approved of the choice of *Nature* and felt that his own endorsement of the paper was likely to improve its chances of acceptance: 'After suggesting a minor stylistic alteration, [Sir Lawrence] enthusiastically expressed his willingness to post it to *Nature* with a strong covering letter.'[33] Personal connections with

Nature helped the King's College group, too. John Randall, head of the King's College, London, laboratory, was a member of the Athenæum along with Brimble. This social connection prompted Brimble to alert Randall to the forthcoming Cambridge publication; Brimble wanted to make certain that King's was aware of the Cambridge paper and had the opportunity to publish their work as well.[34]

All three papers were submitted to *Nature* in early April. Whether *Nature* would accept their articles apparently did not worry Watson, Crick or Wilkins much. In their retrospective accounts, none of them recalls any anxiety over the manuscript's fate or any excitement when news of forthcoming publication came from the *Nature* editors. Nor should they have been anxious—recommendations from Bragg and Randall were enough, in Brimble and Gale's view, to justify printing the papers without further review. On 25 April all three articles were in print.[35] Watson, Crick and their competitors got their wish for speedy publication—largely because the editors were willing to approve their papers without a time-consuming refereeing process.

'I felt frustrated with the system': *Nature* and seafloor spreading

Bragg and Randall were not the only scientists who held significant sway over which papers *Nature* would print. Brimble and Gale's most influential contacts tended to be British, but some prominent scientists abroad might enjoy similar influence, such as Maurice Ewing, the charismatic director of the Lamont Geological Observatory at Columbia University. Ewing encouraged an extensive internal review process before any papers from Lamont were submitted for publication. If Ewing did not personally approve of a Lamont paper, he would often call a journal's editor and ask that it be rejected—a request that Brimble and Gale apparently honoured.[36]

Personal relationships may also account for another striking editorial decision made under Brimble in 1963 (two years after Gale retired): the acceptance of Frederick Vine and Drummond Matthews's paper on magnetic 'stripes' on the sea floor and the rejection of a very similar paper by the Canadian geophysicist Lawrence Morley. In the 1960s, *Nature* was one of the major publication venues for a revolution in the Earth sciences that produced modern plate tectonic theory. The Vine–Matthews paper and Morley's letter both proposed that alternating 'stripes' of normal and reversed magnetic polarity over oceanic ridges might be evidence in favour of seafloor movement—and in favour of continental drift.[37]

Vine had joined the Department of Geodesy and Geophysics at Cambridge as a PhD candidate in October 1962. He began developing computer-based methods for reconstructing the possible effects of reversing magnetization on the ocean floor. When Matthews returned from an expedition to the Carlsberg ridge in the Indian Ocean, Vine used his computer model to interpret the magnetic data and found magnetic 'stripes' of normally and reversely magnetized oceanic floor running parallel to the ridge. From there, Vine and Matthews began to develop the idea that would be referred to as the Vine–Matthews hypothesis: that seafloor spreading combined with reversible magnetization of the oceanic crust would produce 'stripes' of normally and reversely magnetized ocean floor at oceanic ridges. The pair decided to write up their theory and submit their paper to *Nature*. Maurice Hill, one of Cambridge's senior geologists, read the paper and felt that *Nature*'s editor would want more physical evidence in favour of their hypothesis. He gave

Vine and Matthews permission to publish magnetic data from ridges in the North Atlantic and northwest Indian oceans to provide a stronger empirical underpinning to their arguments.[38]

But unbeknown to Vine and Matthews, another geophysicist had already submitted a paper very similar to theirs. In February 1963 Lawrence Morley at the Geological Survey of Canada sent a letter to *Nature* suggesting that magnetic patterns around oceanic ridges could support a model of seafloor spreading. Going a step further than Vine and Matthews, Morley also suggested that better knowledge of the chronology of the magnetic reversals could help geologists calculate the rate of seafloor spreading.

Two months later Morley received a rejection letter from the editor of *Nature*. According to Morley, the letter simply said that the editor 'did not have room to print' his communication.[39] Morley would go on to submit his piece to *Journal of Geophysical Research*, where it was again rejected. When Morley saw the Vine and Matthews piece in September, he knew his paper would no longer be considered novel within the geological community; he even worried that if he persisted with trying to publish his own version, he might be accused of plagiarizing the Cambridge geologists. In 1970 Morley moved out of geophysics and accepted a position managing the Canadian Centre for Remote Sensing.

In a 1979 interview with the historian Henry Frankel, Morley seemed sceptical of *Nature*'s explanation for his rejection. He noted that his article would have taken up three-quarters of a page of *Nature*.[40] However, Morley submitted the piece as a Letter to the Editor. For that section of the journal, three-quarters of a page was by no means a trivial amount of space—most letters were less than a page, and many took up half a page or less. Furthermore, the backlog of submissions to the popular Letters to the Editor column was quite substantial by 1963.[41] But if lack of space was the primary reason for the rejection of Morley's letter, why would the *Nature* staff have accepted Vine and Matthews's very similar piece?

Because *Nature*'s archives have not survived and we do not have access to referee reports or in-house communications about various papers, it is difficult to determine the exact rationale behind the editorial decisions on the Morley and Vine–Matthews papers; however, there are a few possible explanations. The first, offered by Vine himself, is that there were significant differences between Morley's rejected letter and Vine and Matthews's accepted piece.[42] Morley's article was almost entirely speculative and theoretical, offering no new data. Vine and Matthews's paper, in contrast, did contain new data—and in fact, their colleague Maurice Hill had urged them to include more data because he thought it would strengthen the article's chances of acceptance. In later interviews Vine indicated that he thought he and Matthews had benefited from Hill's good instincts about what would impress the editorial staff at *Nature*.[43]

Furthermore, although Morley's rejection letter from *Nature* was not particularly illuminating, a second rejection letter from another journal suggests that the theoretical aspects of Morley's letter did strike some readers as problematic. When *Journal of Geophysical Research* rejected Morley's paper, the editor enclosed a note from a referee that made it clear the anonymous reviewer considered the letter too speculative, more appropriate 'over Martinis' than in a communication to *Journal of Geophysical Research*.[44] And yet the speculation-versus-data explanation seems unsatisfactory, or at least incomplete, given that *Nature* had printed other speculative pieces in the field of geophysics.[45]

Given Brimble and Gale's reliance on local networks of scientific authority, and in particular their strong ties to Cambridge, we must ask whether there was institutional or national bias at work in the rejection of Morley's piece. The idea that the Cambridge-affiliated Vine and Matthews received preferential treatment from *Nature* would certainly be in line with North American geologists' impressions of *Nature* under Brimble and Gale. The Lamont geologist Bruce Heezen once suggested that Brimble and Gale were favourably disposed to speculative papers from Cambridge or Oxford but regarded 'speculation from a redbrick university in the United States [as] bullshit'—an especially striking statement given that Lamont's director Ewing had a collegial relationship with Brimble and Gale.[46] Morley's case has been cited in Canada as an example of an apparent lack of international respect for Canadian science, an accusation applied equally to the British *Nature* and the Americans who ran *Journal of Geophysical Research*.[47]

Morley, certainly, seems to believe that his British competitors had an advantage. In his interview with Frankel, Morley claimed that the Cambridge connection had worked against him and that the person who read magnetic geology articles for *Nature* wanted to ensure priority for the Cambridge geologists:

> I found out that the reason it was rejected was that the reader for *Nature* on magnetic methods knew at that time through verbal communications that Vine and Matthews were hoping to publish their paper and for that reason he did not want my letter to scoop their paper.[48]

Morley softened his take on his rejection for a 2001 collection of retrospectives on the development of plate tectonics, saying only,

> I felt frustrated with the system. I knew that when a scientific paper was submitted to a journal, the editors choose reviewers who are experts on the topic being discussed. But the very expertise that makes them appropriate reviewers also generates a conflict of interest: they have a vested interest in the outcome of the debate. We could call this the 'not invented here syndrome': scientists may be biased against good ideas emerging from someone else's lab. In retrospect, that is exactly what happened.[49]

Morley ultimately seemed to conclude that his paper had been the victim of a silent bias against new ideas from different institutions. His more serious charge, that an unnamed *Nature* reviewer hindered his letter because he knew about Vine and Matthews's work, is difficult to substantiate, especially in the absence of that person's name or institutional affiliation. However, given the *Nature* staff's strong reliance on institutional and personal connections, Morley's complaint seems uncomfortably plausible.

The stories behind the DNA papers and the Vine–Matthews and Morley papers show that Brimble and Gale did not employ systematic peer review and placed great power in the hands of influential laboratory heads when deciding what to print and what to reject. This was not inconsistent with how Lockyer and Gregory had run the journal; neither of the previous editors had systematically solicited outside opinions. However, it seems unlikely that the combative Lockyer or the debate-loving journalist Gregory would have rejected an interesting piece because a laboratory head said so. But Brimble and Gale were more retiring than their predecessors and more willing to be influenced. As a result, Brimble and Gale are usually seen as affable but low-energy, the heads of a regime in which editorial decisions were as likely to be made over dinner at the Athenæum as they were in the *Nature* offices.

Comments from contemporaries make it clear that British contributors knew (or at least suspected) that the refereeing process at *Nature* was somewhat lax. Walter Gratzer, who became *Nature*'s molecular biology correspondent in 1966, recalls:

> I published a few things in *Nature* when I was a PhD student [in the 1960s] and almost anything could get into it at the time, if it wasn't actually wrong. Refereeing was pretty erratic and I think they took more notice of where it came from than the content.[50]

Similarly, in 1979 Fred Vine recalled a light-hearted conversation with his colleagues about his now-famous 1963 *Nature* paper:

> it must have been in June or July '63. . . . Somebody said, 'Do you know if *Nature* gets their articles reviewed, or do they publish almost anything?' I said, 'Well, we're just about to find out because, you know, I just put my paper in, and if they publish that they'll publish anything.'[51]

Morley's 1979 comments about 'the reader on magnetic methods' who rejected his article suggests that North Americans, too, knew that *Nature* relied on a small number of local opinions.

Crucially, unsystematic refereeing was not unusual for commercial journals during the nineteenth and early twentieth centuries; many of these journals placed their trust in editors or editorial boards rather than in external reviewers.[52] Although journals affiliated with scientific societies often employed more consistent and rigorous refereeing procedures, this was primarily a method of choosing between a large number of contributions and was not seen as a special guarantee of scientific accuracy.[53] *Nature*'s contributors and readers do not seem to have considered *Nature*'s unsystematic peer review process a reason to distrust the scientific claims made in the journal. Well into the second half of the twentieth century, *Nature* could still be considered a legitimate place to publish scientific findings even in the absence of systematic external peer review.

Editorial reforms under Maddox and Davies

After Brimble's death in 1965, the Macmillans approached the former physics lecturer and science journalist John Maddox about taking over as *Nature*'s editor. Notably, after he came to the journal in 1966, the editor's right to approve papers without referee opinions was one of the few things that the dynamic Maddox did *not* overhaul. Which is not to say that he made no changes to the refereeing process at *Nature*. When he arrived, Brimble's office was piled high with old manuscripts still awaiting final judgement on acceptance or rejection. As Maddox described it,

> [The *Nature* office] was an open-plan space without much of a plan. A window facing West ran 10 metres along the room and the broad window-ledge supported the famous backlog. That was arranged in piles, one for each month, providing a histogram of Brimble's problem, soon to be mine. There were fourteen monthly piles when I first saw them.[54]

Maddox immediately began working to clear the backlog.[55] He had to give three months' notice at the Nuffield Science Teaching Project before starting at *Nature*, but even before leaving Nuffield, Mary Sheehan recalled, Maddox 'used to go into the *Nature* office every day and pick up a suitcase full of manuscripts and take them home and take them back the next day.'[56] Once he officially began the job at *Nature*, Maddox began holding a daily editorial meeting about manuscripts sent for consideration.[57] He even tried an innovative experiment to speed up the refereeing process: he collected a group of referees around a table piled high with manuscripts, hoping they would come to 'instant decisions' about the submissions. The process worked less well than he had hoped. It was difficult to get all of the referees together at once and many were unhappy at making decisions with the speed Maddox expected. One colleague recalled that a particular referee 'would immerse himself in the first paper and couldn't be shifted until the whole thing was over.'[58] The experiment was ceased after only half a dozen referee meetings.

Under Maddox, some unsuitable papers were rejected outright, others were sent out for referee opinions, and some Maddox simply accepted on his own authority without sending them out for referees' comments. Walter Gratzer, who became the journal's molecular biology correspondent in 1966, thought that Maddox 'didn't worry too much about refereeing' early in his tenure; his priority was quick publication and a compelling journal. The galvanic Maddox found a great deal of value in having such wide scope to print unusual, controversial or speculative articles based solely on his own authority. Gone were the days when an influential laboratory director might stop *Nature* from printing an article. Lamont's Maurice Ewing, who had been able to halt the publication of papers from his laboratory with a phone call to Gale or Brimble, found that the new regime was not nearly so accommodating. Maddox, in the words of one anonymous geophysicist, 'just wouldn't put up with pressure from the establishment to stop something.'[59]

It was not until 1973, when David Davies was hired as *Nature*'s editor, that peer review became a requirement for every paper printed there. Davies, a Cambridge-educated geophysicist, was the head of the Seismic Discrimination Group at Massachusetts Institute of Technology (MIT) and had experience in scientific publishing; he had edited *Geophysical Journal of the Royal Astronomical Society* and had been *Nature*'s geophysics correspondent since 1968. When Davies arrived at *Nature*, one of his first goals was, as he put it, 'getting the refereeing system beyond reproach.'[60] Before he left MIT, Davies embarked on a series of visits to scientists in the Boston area to discuss their impressions of *Nature*. 'I thought of it as sort of a lap of honour but it turned out to be exactly the opposite', he later recalled. 'They all were complaining.' The complaints were largely about *Nature*'s perceived British bias: 'They all said ... it's a very British establishment journal. I bet all your referees are London and Cambridge.'[61] So, unlike Maddox, who had felt perfectly comfortable accepting a paper because he found it interesting, Davies admitted nothing without reports from at least two referees, even in his own field of geophysics. Mindful of the criticism that *Nature* was a 'British establishment journal', Davies also worked to expand *Nature*'s base of referees, as well as its news-gathering apparatus, outside the UK's borders.

Referee opinions and editorial influence: the 1988 INSERM paper

Davies remained at *Nature* for seven years, bringing a deft sense of humour to the lead editorials and successfully expanding *Nature*'s news-gathering and refereeing network outside of Great Britain.[62] When Davies stepped down in 1980, Maddox returned to *Nature* for a second stint in the editor's chair, with his appetite for controversy still intact. Maddox continued Davies's policy of obtaining referee reports for every scientific paper *Nature* printed, but referee opinions alone might not dissuade Maddox from printing a paper.

In 1988, for example, Maddox authorized the publication of 'Human basophil degranulation triggered by very dilute antiserum against IgE', from a team of researchers led by the immunologist Jacques Benveniste at Paris's Institut national de la santé et de la recherché médicale (INSERM) laboratory.[63] The controversial paper seemed to present laboratory-based evidence for the effectiveness of the alternative medical practice of homeopathy. Benveniste had submitted a similar paper to *Nature* in 1986, which the journal had rejected after negative referee reports.[64] When the INSERM team submitted a heavily revised paper in 1987, however, Maddox took a strong interest in the results—but not because he thought INSERM had uncovered a major scientific breakthrough. Instead, Maddox saw an opportunity for *Nature* to evaluate results that Maddox strongly suspected were too good to be true. He agreed to publish the paper if the INSERM team would let *Nature* investigators evaluate their laboratory methods in person after its publication. Walter Stewart, a National Institutes of Health employee famous for his audits of scientific fraud, acted as one of the revised paper's referees. In the cover letter to his (largely negative) referee report, Stewart wrote, 'If you do send a team to Paris to check on the laboratory, please keep me in mind', indicating that Maddox was considering a visit to INSERM long before he agreed to publish the heavily revised Benveniste paper.[65]

The three independent investigators that *Nature* chose to observe the experiments were Stewart, Maddox himself, and James 'The Amazing' Randi, a former laboratory technician who had made his name as a stage magician and debunker of alleged psychic phenomena.[66] Less than a month after the initial publication of the INSERM paper, *Nature*'s three investigators released a report calling the results a 'delusion'. The team declared that the high-dilution experiments were 'statistically ill-controlled', said that 'no substantial effort has been made to exclude systematic error, including observer bias', and further claimed that all data in conflict with the team's hypothesis had been excluded from the paper's analysis. 'The phenomenon described', said Maddox, Randi and Stewart, 'is not reproducible in the ordinary meaning of the word.'[67]

The Benveniste paper and the editorial investigation ignited a storm of correspondence, much of it critical of Maddox's conduct. Some readers complained that *Nature* should not have printed a paper that the editor thought was so questionable; others argued that the INSERM visit created a 'circus' atmosphere that could only embarrass and discredit science.[68] However, the Benveniste episode does illustrate Maddox's continuing ability to ensure publication for papers he considered significant, even if the referee reports were somewhat less than complimentary. Benveniste's paper would almost certainly not have been printed if Maddox had not taken a personal interest in the paper's controversial claims. Significantly, the Benveniste paper ran under the special heading of 'Scientific Paper'. According to Charles Wenz, then the Coordinating Editor of *Nature*, none of Maddox's subeditors would accept responsibility for printing the paper in their own sections.[69] Even after multiple referee reports became the norm at *Nature*, an editor still had a great deal of ability to shape the journal's content.

One *Nature* reader, the biochemist Keith Snell, drew a slightly different conclusion about *Nature*'s refereeing procedures from the Benveniste episode. 'So now at last confirmation of what I have always suspected', he wrote in a letter to the Correspondence column. 'Papers for publication in *Nature* are refereed by the Editor, a magician and his rabbit.'[70]

Conclusions

External refereeing is now an expected part of scientific publishing, and when we look back at *Nature* during the 1950s it is tempting to see only the pitfalls of Brimble and Gale's system. For instance, Brimble and Gale's trust in top British laboratory officials may have helped obscure Rosalind Franklin's contributions to the DNA model; their successor, Maddox, would later claim that he 'would have smelled a rat' when he read Watson and Crick's sentence about being 'stimulated by a general knowledge' of the crystallographer's unpublished findings.[71] Lawrence Morley's case provides evidence that a journal whose editor relied on personal connections to choose articles might treat submissions from those outside the editor's network with less serious consideration than pieces from those inside it.

But the Brimble and Gale style gave *Nature* scope to print papers that might not have passed a more extensive external review—and some of those papers turned out to constitute major advances in their fields.[72] Maddox was also fond of saying that the Watson–Crick DNA paper would never have made it into print if *Nature* had employed peer review in 1953: 'It is only necessary to imagine what people would say if it reached them in the mail: "It's all model-building, just speculation, and such data as they have are not theirs but Rosalind Franklin's!"'[73] Maddox, too, found a great deal to like in retaining the editor's right to print pieces he found worthy of discussion.

In modern science, peer review is generally seen as the mechanism that allows us to trust the scientific literature. Many observers would say that authoritative scientific knowledge comes from papers published in peer-reviewed journals. And yet, even though peer review has become central to questions of trust and authority in science, very few historians have investigated when, where and why editors of scientific journals came to consult outside referees before approving papers for publication, or how the process of peer review became seen as a prerequisite for scientific respectability. Examining the history of a journal such as *Nature* shows that refereeing in the late twentieth century was much less systematic and universal than some might assume.

Furthermore, it seems significant that David Davies, the editor who made multiple referee reports a matter of policy at *Nature*, had most recently been working in the USA. Comments from British journal editors in the late twentieth century suggest that, for a time, British and American journals held somewhat different beliefs about peer review. In 1976 Ian Munro, the new editor of the prestigious medical journal *The Lancet*, expressed concern that his journal would not be taken seriously in the USA unless the journal began employing peer review. Until that point, the editor had retained the right to accept papers on his own authority.[74] Ultimately *The Lancet* decided to use referees but to limit the influence that referees had over the editor. In a 1989 editorial *The Lancet* complained that 'in the United States far too much is being asked of peer review' and proudly assured readers that at *The Lancet*, 'reviewers are advisers not decision makers.'[75] These comments suggest that there may be interesting and significant differences in the development and spread of peer review in different national contexts.

All of this suggests that there is much yet to be uncovered about the development and spread of refereeing at scientific journals. The belief that Oldenburg invented external refereeing is clearly incorrect, but it is not yet clear exactly how peer review became seen as a requirement for findings to be 'real science'. There seems to be a wider twentieth-century pattern of placing increasing emphasis on peer review at both journals and at grant-giving organizations—a pattern that eventually made peer review a much more central part of modern science than it had been before World War II. Tracing the reasons for this shift will be an important task for scholars interested in how peer review came to assume a central function in the world of scientific publishing—and in the establishment of claims to scientific knowledge.

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Notes

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2 S. Anthony, 'CERN's Higgs boson discovery passes peer review, becomes actual science', *ExtremeTech* (10 September 2012) (http://www.extremetech.com/extreme/135756-cerns-higgs-boson-discovery-passes-peer-review-becomes-actual-science; accessed 16 May 2015).

3 Many authors have drawn this narrative from H. Zuckerman and R. Merton, 'Patterns of evaluation in science: institutionalization, structure and function of the referee system', *Minerva* **9**, 66–100 (1971). On the 'heroic narrative' of the scientific journal, and on the historical problems with this narrative, see A. Csiszar, *Broken pieces of fact: the scientific periodical and the politics of search in nineteenth-century France and Britain* (PhD thesis, Harvard University, 2010), pp. 15–22.

4 Interestingly, little scholarly work has been done on the history of scientific or scholarly peer review. John Burnham has given an extremely useful overview of the history of peer review in two pieces: J. Burnham, 'The evolution of editorial peer review', *J. Am. Med. Assoc.* **263**, 1323–1329 (1990); J. Burnham, 'How journal editors came to develop and critique peer review procedures', in *Research Ethics, Manuscript Review, and Journal Quality: Proceedings of a Symposium on the Peer Review-Editing Process* (ed. H. F. Maryland and R. E. Sojka), pp. 55–62 (ACS Miscellaneous Publications, Madison, WI, 1992).

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8 See Csiszar, *op. cit.* (note 3), on how the scientific journal became the 'principal institutional site for the representation, certification, and registration of scientific knowledge' (p. 7).

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11 On Gregory, see W. H. G. Armytage, *Sir Richard Gregory: his life & work* (Macmillan & Co. Ltd., London, 1957).

12 Richard Gregory to Daniel Macmillan, May 1938 (no day given), Sir Richard Gregory Papers, Special Collections, University of Sussex Library, Falmer (hereafter SRGP), 3/1.

13 There are several letters about Gale's appointment in the Macmillan Papers at the University of Reading. See Macmillan Papers, Special Collections, University of Reading Library, Reading (hereafter MP:UR), 75/30.

14 For Brimble's letter accepting the position, see Lionel J. F. Brimble to Daniel Macmillan, 12 July 1930, MP:UR, 127/130.

15 A. J. V. Gale to Sir Richard Gregory, 18 November 1938, SRGP, 3/1.

16 When the *Nature* staff moved offices in 1963, the vast majority of editorial correspondence was thrown away.

17 On the dating and mailing of *Nature*, see 'News and Views', *Nature* **145**, 887 (1940); 'News and Views', *Nature* **146**, 361 (1940).

18 See 'News and Views', *Nature* **159**, 224 (1947); 'News and Views', *Nature* **159**, 297 (1947).

19 'News and Views: Soviet genetics: the real issue', *Nature* **165**, 711 (1950).

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28 Watson, *op. cit.* (note 23), pp. 124–128.

29 Wilkins, *op. cit.* (note 27), p. 216. See also B. Maddox, *op. cit.* (note 26), pp. 208–209.

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31 On the history and status of *Nature*'s Letters to the Editor column, see M. Baldwin, '"Keeping in the race": physics, publication speed and national publishing strategies in *Nature*, 1895–1939', *Br. J. Hist. Sci.* **47**, 257–279 (2014).

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33 Watson, *op. cit.* (note 23), p. 129.

34 B. Maddox, *op. cit.* (note 26), p. 209. See also Wilkins, *op. cit.* (note 27), p. 216.

35 Unfortunately, *Nature* purged its collection of papers in 1963 when they moved offices, so any internal correspondence about the DNA papers has not survived. See B. Maddox, *op. cit.* (note 26), pp. 210–211.

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43 Vine, *op. cit.* (note 38), p. 58.

44 Morley, *op. cit.* (note 39), p. 84.

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46 Bruce Heezen, quoted in Stewart, *op. cit.* (note 36), p. 193.

47 See, for example, J. Lear, 'Canada's unappreciated role as scientific innovator', *Saturday Rev.* (2 September), 45–50 (1967).

48 Morley, quoted in Frankel, *op. cit.* (note 37), pp. 17–18 (fn 33).

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51 Vine, quoted in Frankel, *op. cit.* (note 37), pp. 21–22.

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63 E. Davenas *et al.*, 'Human basophil degranulation triggered by very dilute antiserum against IgE', *Nature* **333**, 816–818 (1989).

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