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The peer-review process

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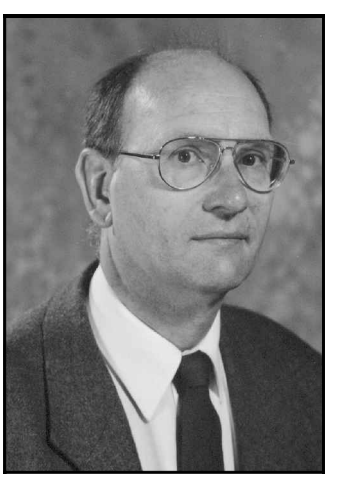
ABSTRACT: The recent literature about peer review of scholarly articles is reviewed, with particular emphasis on the cost of the peer-review process. Possible impacts of electronic scholarly publishing upon peer reviewing are discussed. Opinion among academics in their roles as authors, editors and referees seems likely to insist upon preservation of a pre-publication refereeing system in most disciplines. As the administration of any such system seems to have a cost of about $400 per published article, any scholarly publishing system will need to locate financial support to at least that extent, and a system of lump-sum payment by the authors' funders is best placed to cover this cost while providing universal free access to scholarly material.

Introduction

The peer-review process has been discussed at length in most of the established texts on the scholarly communication process, such as Ziman (1968), Ravetz (1973) and Meadows (1974). These authors agree that the four main functions of the scholarly literature are dissemination of current knowledge, archiving of the canonical knowledge base, quality control of published information, and assignment of priority and credit for their work to authors. The key position of peer review in fulfilling all four of these functions, but especially of course quality control, is acknowledged by these authorities.

The peer-review process is applied to a number of scholars' activities, but the paradigm is in the publication of scholarly journal articles. Other areas where peer review is applied include submission of proposed papers to conferences, the publication of scholarly monographs, and importantly the award of research grants and contracts. This report will concentrate on the peer-review process as applied to scholarly journals, because there is a fuller literature about peer review in this application. The process has recently been well described by Meadows (1998, pp. 177–94). When a submitted report first arrives at the editorial office of a journal, it is first vetted by the editor, who may reject it out of hand, either because it is 'out of scope' (not dealing with the right subject matter for that journal) or because it is manifestly of such low quality that it cannot be considered at all. Papers that pass this first hurdle are then sent to experts in the field of the paper, usually two, who are generally asked to classify the paper as publishable immediately, publishable with amendments and improvements, or not publishable. The middle decision is the commonest, and in that case the referees suggest the nature of the improvements that they



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consider to be required. It is widely agreed that this improving function by referees is of value in maintaining the overall quality of the scholarly literature; as many as 80% of published papers receive some revision (Lock, 1985). If the two referees disagree, the paper may be sent to a third, or the editor may adjudicate between them (effectively acting as the third referee). The referees may be members of the journal's editorial board, if they have sufficiently specialized knowledge of the paper's field, or may be others from a list of experts known to the editor.

Only quite recently has the paradigmatic 'editor plus two referees' system become widespread

Although it is commonplace to assume that peer review has taken place ever since the beginning of scholarly journals in the 17th century, Weller (2001, pp. 3–8) points out that prior to the Second World War the process was often quite uncodified, and editors frequently made all decisions themselves with only informal advice from colleagues. Only quite recently has the paradigmatic 'editor plus two referees' system become widespread.

Typically the author does not know the identity of the referee but the referee does know that of the author. There have been experiments with both 'double-blind' refereeing, where the author's identity is hidden from the referees, and open refereeing, where the referees' identity is known to the author. The former tends not to work because the authorship is often obvious to a knowledgeable reader from context (e.g. from the reference list) (Ceci and Peters, 1984), but both it and the latter have been seriously attempted and are discussed later in this report (see the section on alternative mechanisms below).

Several authoritative books about the scholarly communication process have been published recently (Peek and Newby, 1996; Page *et al.*, 1997; Meadows, 1998; Tenopir and King, 2000; Fredrikson, 2001; Weller, 2001; Abel and Newlin, 2002), of which both Frederikson and Abel and Newlin sought to review the changes in the process over the 20th century. Each of these has discussed peer review, with valuable references, though some have given this topic more coverage than others. In Peek and Newby's book, Harnad (1996) devotes an entire chapter, which is discussed later in this report, to the implications of electronic publication for peer review. Harnad followed up his chapter with a later paper (Harnad, 1999) updating his arguments.

Weller (2001) devoted an entire book to the topic of scholarly journal reviewing. She provides a review of published studies of editorial peer review in the following broad categories: general studies of rejection rates, studies of editors, studies of authors, and studies of reviewers. Her study is immensely detailed and provides a comprehensive reference list up to 1997. It is, however, written from the point of view of the academic or researcher (as author, editor and referee), and not from that of the publisher, and is silent on the issue of costs. Although it covers the issue of peer review throughout scholarship, there is a substantial emphasis on clinical medicine, presumably because that is the area where the greatest amount of research about the refereeing process has been undertaken. There is a lesser emphasis on other biomedical sciences, psychology and social sciences, but relatively little about either engineering or the humanities.

Disappointingly, Tenopir and King's immensely detailed work does not explicitly discuss peer review, but the data in their book can be interpreted to provide reliable information on the costs of running peer-review systems, and this topic is dealt with below. In Fredrikson's book, Sandewall (2001) provides a more recent discussion of similar issues to Harnad, and most usefully appends *Defining and Certifying Electronic Publication and Science*, a proposal made by an expert working group to the International Association of STM Publishers. Peer review is covered in a more cynical way by de Vries (2001), who calls it the 'holy cow of science'. In Abel and Newlin's book the topic is covered only briefly, by Henderson (2002), who does also, however, provide a very full bibliography on scholarly scientific journals throughout the 20th century. Meadows's (1998) contribution has been discussed above, and also contributes to the discussion of flaws and abuses in the refereeing system given below. Meadows's other recent work written with Gillian Page and Robert Campbell (Page *et al.*, 1997) was

more concerned with the business of scholarly journal publishing and its management, but it does contain on pages 46–53 a succinct description of the peer-review process seen from the publisher's perspective.

Another landmark publication in this field was *Subversive Proposal* (Okerson and McDonnell, 1995), to which many leading commentators in the field of scholarly electronic publishing contributed. It should also be mentioned that Bailey (2002) provides a regularly updated bibliography on all issues concerning electronic scholarly publishing; version 42 appeared in the Spring of 2002. Another similar project is Dworaczek's (2002) *Subject Index to Literature on Electronic Sources of Information*.

Workers in the biomedical field (e.g. Wood, 1998) have made a particular study of the peer-review process, not surprisingly since in their field dependable quality-controlled information can be literally a matter of life and death, a point that I have made previously (Rowland, 1997). In particular, the staff of the *BMJ* (formerly the *British Medical Journal*) have been making a study of the merits and limitations of peer review over a number of years, under the editorships of Stephen Lock and Richard Smith (Lock, 1985; Goldbeck-Wood, 1999; Smith, 1999b, 2001; van Rooyen *et al.*, 1999; Williamson, 2002). See the section below on the problems of peer review.

Scholars' views on peer review

Whenever surveys of scholars' opinions about scholarly publishing have taken place, the maintenance of peer-review systems has been a top-priority requirement (The Royal Society, 1981; Rowland, 1982; McKnight and Price, 1999; ALPSP,1999, 2001, 2002).

A questionnaire survey addressed to scholars in The Royal Society's (1981) study (also described by Rowland, 1982) listed various possible changes that might occur in the scholarly publication process and sought respondents' views on the likelihood and desirability of the changes. The abolition of peer review was rated as the least desirable of all possible changes, and the retention of it the most important priority in forward planning. Unfortunately, the second Royal Society study (The Royal Society, 1993) did not include a question on this topic. As electronic publishing was at that time more imminent than it had been in 1981, this omission was perhaps regrettable, but no doubt those responsible for the 1993 survey regarded the retention of peer review as inevitable and thus not worth asking about.

In McKnight and Price's study, 94% of respondents said that peer review was important in printed journals; only 46% said this about electronic journals but 42% did not answer the question, perhaps because they were unaware of any electronic-only journals in their field.

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The Association of Learned and Professional Society Publishers (ALPSP) has in recent years become a major source of training, expertise and data about scholarly publishing. The ALPSP (1999) study of authors' views, *What Authors Want*, has recently been updated by a second survey, ALPSP (2002), undertaken by the same consultants. The first survey looked at authors' views about the scholarly publication process generally, while the second concentrated more specifically on the changes likely to result from the swing to electronic publication. In between, ALPSP (2001) also carried out a survey specifically about peer review addressed to editors, members of editorial boards and referees. Together these three surveys provide a most valuable source of data on scholars' attitudes to the refereeing process during the transition period to electronic publishing.

In the ALPSP (1999) survey, almost 70% of authors were 'satisfied' or 'very satisfied' with the current system of peer review, although in another question regarding 'obstacles to achieving their publishing aims', 52% said that peer review was an obstacle, presumably because it prevents them publishing poor papers! Those who indicated dissatisfaction with peer review were asked to expand on their reasons. A separate set of questions asked respondents to place themselves in the role of referee instead of author. About half said that the number of papers they were asked to referee annually was about right, but about one in six said they were overloaded. And although payment to referees is exceptional, almost

20% felt that academic workloads were now such that payment for refereeing would eventually become necessary if journals were going to find enough people to do this work.

The ALPSP (2001) survey on peer review was conducted in collaboration with the European Association of Scientific Editors (EASE). They discovered that the majority of respondents did refereeing work for only one journal; this evidence conflicts with the literature surveyed by Weller (2001, p. 155), which showed that the majority of referees worked for more than one journal. The ALPSP/EASE study also found that more than three-quarters of the journals represented in the sample refereed all papers; the modal number of submissions per year to these journals was in the range 100–500, the modal acceptance rate was 25–50%; and about 40% used double-blind refereeing, but 88% of them kept the referees' identities secret. Surprisingly, in late 2000 almost half still communicated with referees largely by mail rather than electronic means.

The ALPSP (2002) survey asked respondents to distinguish between their views as authors and their views as readers of electronic journals. Their opinion of peer review in both roles was solicited. As found by Rowland (1982) and ALPSP (1999), peer review remained important to these respondents, with virtually no difference between the number rating it important as authors (81%) and those regarding it as important to them as readers (80%). However, when asked to predict what would be the most common form of quality control in five years' time, only a bare majority answered 'traditional peer review' (with referees' identities withheld); 27% said 'traditional peer review supplemented by post-publication commentary', and 16% said that the referees would no longer be anonymous. Importantly, though, only 1% minorities opted for post-publication commentary only and for no peer review of any kind. However, 45% expected to see some changes in the peer-review system within the next five years.

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Studies of peer review and its problems

As mentioned above, the peer-review system has been extensively studied in the biomedical field, and indeed there is a regular series of International Congresses on Peer Review in the Biomedical Sciences, which are reported in special themed issues of the *Journal of the American Medical Association (JAMA)* (1990, 1994, 1998). Following the publication of the book by Lock (1985), then editor of the *BMJ*, his successor Richard Smith established Locknet in 1994 as an electronic forum for the discussion of issues related to peer review (van Rooyen, 1998). A number of *BMJ* and *JAMA* staff have become well known for their work in this field, including Susan van Rooyen (1998, 1999), Alex Williamson (2002), Fiona Godlee (Godlee and Jefferson, 1999) and Drummond Rennie (Smith, 2001). Their objective has been to develop an international and collaborative programme of research into peer review in medicine, in order to raise the quality of medical publications. The members of Locknet divide themselves into several groups: the decision-making, authors', scientific integrity, industry, specialist journals, and dissemination and outcomes groups, depending on their affiliation and interests.

Meadows (1998, pp. 177–94) reviewed the literature up to that point on peer review, how it works in practice and its difficulties. Among issues he noted were bias in refereeing, against authors from minor institutions, or against female authors, for example, undetected falsification of results by authors (scientific fraud), and referees stealing authors' results or ideas (plagiarism) or deliberately delaying publication in order to publish first themselves. These are, however, relatively rare abuses, as Meadows (1998) notes. Weller (2001, pp. 207–46) also devotes a chapter to reviewer bias.

A more widespread concern is the actual effectiveness of peer review in ensuring quality control, and it is this issue that mainly concerns the *BMJ/JAMA* group. Williamson (2002) in the most recent report from this group divides up the problems into the following headings: subjectivity, bias, abuse, detecting defects, and fraud and misconduct. The group, and Locknet generally, carry out research projects in these areas. 'Subjectivity' concerns summary rejections by the editor without sending the paper to referees, and the choice of referee by the

editor (choosing, for example, a known harsh referee for a paper the editor wishes to see rejected). 'Bias' concerns discrimination against authors because of their nationality, native language, gender or host institution. It can also cover occasions when the referee and author are competitors in some sense, or when they belong to warring schools of thought. 'Abuse' by authors includes salami publishing (producing far too many articles out of one piece of research) or duplicate publication, and also omission or downgrading of junior staff by senior authors who effectively steal their subordinates' work. 'Abuse' by referees includes plagiarism (stealing others' as yet unpublished work that has been sent to them for peer review) and deliberately delaying publication of potentially competing work. 'Detecting defects' concerns referees' ability to spot errors in papers. 'Fraud and misconduct' concerns authors who fabricate results, falsify data or claim authorship of results that they know not to be their own. On the basis of the substantial number of research projects carried out in this area over the years (e.g. Godlee *et al*., 1998; van Rooyen *et al*., 1999) in which various changes were instituted to the normal procedure and the effects of these on referees' decisions monitored, Williamson (2002) makes a number of suggestions for improvement. Open peer review (where the referee is identified to the author) may lead to fewer abusive reviews, would give some recognition to referees, and would help to prevent stealing of authors' work. But some scholars might refuse to referee openly. The *BMJ* itself uses open peer review but its specialist journals do not, as the British Medical Association has been unable to convince the editors to change.

Subjectivity, bias and failure to detect errors can be minimized by training reviewers and by using standard checklists rather than letting the referee simply give a report in their own words (Weller, 2001, pp. 160–6). The use of Web-based reviewing systems (see next section) can speed up the process, and also increases the number of potential referees because there is no objection to using referees in distant countries. From the BMJ group, van Rooyen (2001) has also summarized the various research projects carried out in recent years, under similar headings to Williamson (2002).

Use of electronic communication to facilitate traditional peer-review systems

The arrival of electronic journals during the 1990s first newly established electronic-only journals, and later the addition of electronic alternative versions of existing print titles, naturally led to speculation and experimentation regarding the operation of peer review in an electronic environment. The earliest and least radical idea was aired by Campbell (1993), who described how an efficient publisher would organize their system for handling 'compuscripts' in a wholly electronic manner even if the end-product was still a print product. This idea had been discussed in principle many years earlier by Tony Woodward with the concept of an 'Editorial Processing Centre', a shared high-tech facility serving many small not-for-profit publishers, though at the time the technology to support this concept was not sufficiently mature (Woodward, 1976). By now, however, it is likely that a large proportion of scholarly publishers (especially the large ones with significant numbers of staff) use email and email attachments for receiving papers from authors, sending them to referees and receiving the referees' reports, returning the 'typescripts' to authors for amendment, and ultimately for sending them to typesetters (if still used instead of in-house desktop publishing) or printers. Large publishers like the American Geophysical Union have given conference presentations about their computerized refereeing administration systems, although for reasons of commercial confidentiality they are less willing to describe these in print. Some software houses are now developing refereeing control systems which smaller publishers can buy off-the-peg. It is also possible for publishers to use World Wide Web technology in its Intranet form; papers for refereeing can be mounted on a dedicated website to which only editorial board members and accredited referees have access. I have personal experience of using this technology, as it was used for the refereeing by the Programme Committee of the papers

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submitted to the 2001 ICCC/IFIP Electronic Publishing Conference (Hübler *et al.*, 2001), of which I was a member. Friedman (1995) described the computerized referee administration system in use at the *American Journal of Roentgenology*.

One of the research projects carried out within the UK Electronic Libraries (eLib) programme in the late 1990s, the ESPERE (Electronic Submission and Peer Review) project (Wood, 1998), was also directed at this area with the intention of helping smaller publishers to adopt these methods. At the time the journals seemed to be dragging their feet in the online refereeing area, but four years later such systems are commonplace. They do, however, come at a cost in terms of hardware, software and staff training. There is a corresponding saving in postal costs, but the major benefit is saving of time.

Referees are generally unpaid

Costs of peer review

Referees are generally unpaid, though editors and sometimes other members of editorial boards receive an honorarium (Page *et al.*, 1997). Authors generally receive no payment from publishers for their scholarly articles either, though they may be paid for commissioned review articles, for example. Many commentators such as Harnad (1996, 1999) have therefore concluded that the cost of refereeing is low, much lower than is claimed by traditional publishers (see Okerson and McDonnell, 1995, for a debate on these costs issues).

However, all scholarly journals need management. It is essential, for example, that all papers submitted are recorded in a formal filing system (paper or electronic) and that every subsequent transaction regarding the paper is also recorded in its file. The date of each transaction is also important, since dates of submission, of acceptance and of publication are relevant to any arguments about priority, and editors will also be concerned to monitor delays. With much electronic handling of papers today, even if the final product is printed, version control is also an important consideration. It is necessary to identify each version of the paper (initial submission, changes proposed by referees, changes made by authors in response to referees' suggestions, final accepted version). All of these administrative requirements lead to a need for people's time (Rowland, 1996, 1997). In the case of very small journals with only a few dozen articles submitted annually, it may be possible for the academic editor to handle all this work. Normally, though, editing even a quite small journal will lead to a level of clerical work that necessitates some paid assistance. This can be organized in a large number of different ways (Singleton, 1980) but unless the editor's host institution is willing to pay for the clerical assistance, which is increasingly unlikely as budgets tighten, it needs to be paid for by the journal in some way. The same applies to any honorarium paid to the academic editor.

Some journals, a large proportion in some subject areas, are published by large organizations which employ significant numbers of staff at various levels from senior managers down to editorial, production and clerical assistants. Some of the journals produced by such organizations (in both the for-profit and the not-for-profit sectors) are very large, and some publishers have a stable of hundreds of titles. The staff and premises costs of these publishing operations are substantial. A journal such as the *Journal of Biological Chemistry*, for example, published by an American learned society publisher, which produces 30,000 pages per annum, will inevitably require a number of staff regardless of its medium of publication. The question, however, is what proportion of these costs can be assigned to the peer-review process, since it may be assumed that this is an element of the cost that will not be saved even if printing ceases altogether. It is particularly difficult to differentiate between the costs of the quality-control element and those of the straightforward record-keeping and progress-chasing administrative tasks.

Page *et al.* (1997) estimate that the amount paid to editors (honoraria plus support costs) amount to 3–5% of the subscription income of a journal (pp. 63–4). Using their typical figures (p. 277), a journal publishing 600 pages/year, say 60 papers, would have an income of about £150,000, so 3–5% would

represent £4,500-7,500, or a cost of £75-125 per published paper.

Donovan (1998) reported results from a number of journals where serious attempts had been made to cost the refereeing process. One important variable is the rejection rate. The amount of work (and thus cost) entailed in rejecting a paper is essentially the same as in accepting one. So if a journal has a rejection rate of 80% and each paper costs £100 to referee, the refereeing cost per paper *published* is £500. Another important factor is the treatment of overheads. Many organizations, especially large ones, place an overhead figure of say 100% on to staff costs to cover premises costs, costs of the senior management and governance structure of the company, specialist departments such as accounts and personnel, and so on. Small organizations on the other hand may incur fewer of these costs or may have hidden subsidy in the form of (for example) free office accommodation for the editor's paid assistant. The figures obtained by Donovan from different publishers may therefore be comparable only approximately. The range of refereeing costs per paper submitted was £33–200, though the lowest figure (£33) did not include either overheads or 'other editorial costs' which included editorial board honoraria. Excluding this low outlier, the range was £50–200, but when one alters the basis to cost per paper published, taking differing rejection rates into account, it becomes £100–400, excluding again the lowest figure.

The major source of detailed data on scholarly publishing is the work of Donald King and Carol Tenopir. Tenopir and King (1997) estimated the total direct first-copy costs per published article at about $2,000, though it is not clear whether their figure takes any note of staff time used on papers ultimately rejected. Their book (Tenopir and King, 2000) goes into greater detail. Although they do not isolate refereeing costs, they use a variable C_2 defined as the 'cost per page of receiving, processing and reviewing a manuscript' (p. 256) and they produce a figure, derived from American Chemical Society data, of about $20 per page for this parameter. Another parameter, C_3, is defined as 'cost per page associated with editing and proofing articles', and data from six sources suggest that this figure might be around $50/page. If we assume that refereeing is subsumed within C_2 rather than C_3 since the 'editing' mentioned there is probably copy-editing rather than academic editing and we use the $20/page figure, then for a ten-page paper the refereeing cost appears to be about 10% of the total first-copy cost per article. Again, assuming that a paper is ten pages long on average, their figure of $200 per article is well within Donovan's (1998) range of £50–200, or that derived above from Page *et al.*'s (1997) figures of £75–125.

Another source of useful information on costs is Holmes's (1997) article. Aldyth Holmes is Director of the NRC Research Press, the major Canadian publisher of scholarly journals, and her figures come from the NRC's own records; they are averages across all their journals. She maintained that the refereeing element of costs would be unaffected by the medium of publication (electronic, print or both) and quoted a 1996 figure (in Canadian dollars) of $41.80 per published page in 'editorial office costs'. These include the editors' honoraria and the costs of editorial assistants based at the editors' institution, but no overheads. This amount represented about one-quarter of the total direct costs per published page ($169.93), to which was then added an approximately 100% level of overhead to make a final cost per published page of $331.49. As her figures are per *published* page they must include costs associated with rejected papers, so the base figure of $41.80 needs to be approximately halved to arrive at a figure per submitted page of say Can$21. This would then be in fair agreement with Tenopir and King's (2000) figure of US$20.

We may conclude from these figures that a very bare estimate of the cost of operation of a conventional refereeing system (inclusive of the cost of an article administration system, with which it is inextricably intertwined) is US$40 per published page, which includes allowance for the costs associated with those papers not published, at a rejection rate of 50%. If a paper is typically ten pages long, then the cost per paper is about $400. This agrees with the Institute of

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Physics (IoPP), who charge $500 for including a paper in their electronic-only *New Journal of Physics*, which is funded by author payments and makes no charge to readers. Halliday and Oppenheim (2000) thought that IoPP's figure was too low, but in my opinion they overestimate editorial staff costs, by underestimating the number of small journals that one staff editor can simultaneously handle. Wood (1998), reporting on the ESPERE project, believed that an all-electronic system for the administration process would save money (as well as time) and bring the figure below the estimates of Donovan (1998), but she produced no numbers to back up this claim.

Figures from the *BMJ* (Williamson, 2002) dividing up the costs of their stable of specialist journals between 'selecting', 'editing', 'pagination' and 'web', showed that the first-named of these, selecting, which incorporates the peer-review process, cost £1.5 million per annum, against less than £1 million per annum in total for the other three headings. This would suggest that the proportion of the costs of a journal that can be assigned to the peer-review process is considerably higher than the figure of only 10% derived above from the results of Tenopir and King (2000).

existing review systems perpetuate an outdated approach to the distribution of research results

There is a countervailing view from Walker (1998, 2002) who has been responsible for the publishing programme of the Florida Entomological Society (FES) as well as being involved in the larger Entomological Society of America (ESA), which publishes several journals. The FES expects to charge authors only $100 per article for inclusion of their paper in the society's journals, which will then be made available free of charge to readers on the Internet; print subscribers pay a figure that covers printing costs but not first-copy costs. One has to assume that this figure will cover the FES's costs, including those of peer review. The larger ESA, which has a professional staff, has resisted Walker's arguments and charges subscribers for its electronic versions. One may conclude from Walker's data that the costs are much lower for a small society publisher operating on a semi-amateur basis.

Alternative mechanisms for peer review in electronic publication

It has been noted above that the *BMJ* has moved to open peer review (Williamson, 2002). Conversely, the American Psychological Association (2002) practices double-blind refereeing. Both of these approaches are to some extent novel and are probably a consequence of the shift to electronic publishing.

There has been much debate, on relevant email discussion lists and elsewhere, about altogether new approaches to publication in the electronic era (Rowland, 1999). Much of the debate has been driven by Stevan Harnad (1996, 1999) who is a staunch supporter of peer review despite his espousal of free-of-charge scholarly communication, and who has experience as editor of both a conventional journal, *Behavioral and Brain Sciences*, and a free electronic journal, *Psycoloquy*. Harnad identifies the difference between peer review (in which a small number of individuals are specifically asked to pass judgement on the paper), and peer commentary, where after refereeing and publication other scholars may append notes or comments to a paper. *Behavioral and Brain Sciences* practices open peer commentary. Roberts (1999) has also identified these new kinds of post-publication refereeing procedures. Weller (2000, 2001, pp. 321–2) considered a number of new approaches to reviewing in the electronic environment, but concluded that something similar to traditional refereeing would continue. The advocates of change believe that existing review systems perpetuate an outdated approach to the distribution of research results, that they were needed in order to 'ration' space in print journals, and that in the near cost-free environment of the Internet, they are unnecessary because authors can post all their material and allow the readers to sort out what they would like to read. In some fields this view is acceptable but in many, notably the biomedical and chemical fields, it is not, owing to the need for quality control of information. However, post-publication commentary can provide quality control so long as reputable scholars take part and express their views honestly.

The consensus appears to be that open peer commentary after publication is a valuable adjunct to traditional refereeing but not a replacement for it. A major objection to non-refereeing (or post-publication commentary) is that no one has the time to read all the poor material to find the occasional good paper; referees save the rest of us time, by sorting the material out into an order of descending quality.

A related approach is to mount papers online, identified as unrefereed, and then solicit comment on them. On the basis of comments received, the author then revises and improves the paper. It is then refereed in its improved form in the conventional way. Finally it is published as a refereed paper (after which it does not get further changed), in a print version, if the journal has one, as well as the electronic version. This overcomes many of the objections to non-refereeing. In effect this is what happens in the physics community, owing to the existence of the e-print archive http://arXiv.org/ which mounts both unrefereed and published papers (each identified as such). Physicists send comments about the unrefereed papers, and authors take these into account when finally submitting their papers to conventional journals for publication. After refereeing and publication the preprint version is usually removed and replaced by the published version, or with a reference to the published version. This system has been held up as a model to other disciplines for some time, but has been slow to catch on in fields other than those adjacent to physics (mathematics and computer science), though attempts have been made in both economics and psychology. There are clearly significant differences of attitude between the cultures of different subjects.

The STM group's guidelines on Defining and Certifying Electronic Publication in Science, appended to Sandewall (2001), attempt to codify these new situations, and in particular cover the issue of just when, in an iterative process such as that described above, a paper becomes a true 'scholarly publication' worthy of archiving as such, and thus should not be further amended.

There have also been suggestions that, on the Web, a journal issue, or even a journal as such, has no real meaning, and each paper should be regarded as a separate entity. ALPSP held a seminar in London in November 2001 entitled *The Article Economy,* looking into some of these possibilities. The general consensus is that the journal title itself is valuable, since it tells the reader what to expect in the way of both subject matter and standard, but that in the electronic era the concept of a journal issue is meaningless and papers can be published electronically as soon as they are ready, even if they have to wait to be gathered together into a printed issue.

the journal title itself is valuable

A more radical idea has come from Smith (1999a), a paper which also briefly reviews other models. John Smith's hypothetical 'deconstructed journal' was based on an earlier suggestion by Ginsparg, the originator of the physics e-print archive, that journals could be 'overlaid' on that archive. In Smith's model, an author writes a paper and places it on a server, and notifies one or more 'evaluator organizations' of its existence. They review the article, require changes in the usual way of referees, and eventually approve the paper. At this point the author notifies the relevant 'subject focal points' who select material that is relevant to their subject areas and insert links to the paper if they think it appropriate for inclusion. This model separates out the storage and provision of access to the paper (provided by the owners of the server where it is mounted), the quality control (provided by the evaluator organization) and the subject-specific grouping of documents (provided by the subject focal point). The server might well be one belonging to the institution where the author is employed, although organizations like JSTOR or even national libraries could also provide secure long-term archiving. The subject focal points are rather similar to subject gateways that already exist, but they would also be the nearest thing to an actual 'journal' existing in this model. The evaluator organizations could well be learned societies. One major question regarding Smith's model is: who pays for the different functions? So far as the evaluator organizations are concerned, he proposes that the author pays a fee for having the paper evaluated, effectively similar to

the 'author-pays' model of more conventional electronic publication. It is not clear how the subject focal points would be financed, though their costs would probably not be high as they in fact host little except links. A claimed advantage of Smith's model is that papers in cross-disciplinary fields could be included in the 'journals' (i.e. the subject focal points) of all the fields that they encompassed. They could even be separately evaluated by each field according to the refereeing standards prevalent in that particular discipline. A drawback is the lack of any clear evolutionary path from the existing system to Smith's radically different alternative. It is unlikely to come about, but it has been useful in drawing attention to the fact that the functions of the scholarly journal, identified in the Introduction above, will not necessarily all be performed by the same institution in the future.

we may expect evolutionary change to take place

An earlier suggestion that has something in common with Smith's model was made by Kochar (1986), before the start of the true electronic publishing era. He suggested that major medical journals should establish a consortium to receive and review articles – authors would submit their work to the consortium along with a list of journals in which they would prefer to see it published. He suggested that the International Committee of Medical Journal Editors would be the best organization to implement this proposal. It is perhaps not surprising that this suggestion seems to have been ignored.

Conclusions

We may conclude that peer review continues to be regarded as a high-priority requirement by the academics of most disciplines, as demonstrated by the three recent ALPSP surveys (ALPSP 1999, 2000, 2002). This is especially true in the biomedical fields, where the greatest amount of study has been given to the peer-review process, notwithstanding its manifest failings in those fields, which have been extensively documented in the *Journal of the American Medical Association* and the *BMJ*. Alternatives based on open peer commentary after publication have a number of drawbacks. Notably, those best qualified to comment on a paper might be too busy to do so, and those who do comment might be ill-informed or prejudiced. The graded quality-control system provided by a variety of journals in a 'pecking order' helps readers by enabling them to identify the most important papers, whereas an undifferentiated mass of publications on the Internet would overwhelm readers by its volume. And finally, in addition to their 'accept/reject' judgement, referees' suggestions help to improve papers before publication.

Nevertheless we may expect evolutionary change to take place, and in the ALPSP (2002) survey 45% of respondents expected to see some change in the next five years. More journals may move either to open peer review (where the author knows who the referee is) or to double-blind reviewing (where, at least in theory, the referee does not know who the author is). A symmetrical relationship in one direction or the other seems equitable.

It is difficult to disentangle the costs of a peer-review system from those of general administration of a scholarly journal, though it is easier to distinguish them from copy-editing (American 'redaction') costs. We might reasonably conclude, however, from the available data that a cost of about $200 per submitted paper, and thus $400 per published paper if the rejection rate is 50%, is a good estimate that would cover the staff necessary to undertake the work on a journal (or stable of journals) too large to be run on an amateur basis. This figure includes the overheads necessary to provide the staff with office accommodation and the hardware and software necessary to run an email-based manuscript submission, administration and communication system, and a system to assist in the choice of referees. This figure would not, however, cover the head-office overheads of a large publishing company nor provide for a profit element. In any suggested innovative system of scholarly communication one therefore needs to identify a source of revenue to cover costs of at least this magnitude. If readers or their surrogates (such as their employers or their university library) are asked to pay for electronic access, then a further cost, for systems for the collection of subscriptions and access

control, is introduced. These extra costs are avoided if free Internet access is provided, and in that case the 'author pays' model seems to have the greatest promise, though something like the $500 fee charged by the *New Journal of Physics* would have to be levied.

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