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On peer review and preprint publication in the sciences

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Summary

Since the latter half of the twentieth century, pre-publication peer review has become ubiquitous and *de rigueur* for journal publishing in the sciences. Pre-publication peer review, in all of its manifestations, delays scientific communication, as the process of review takes time, yet it does not offer much of a credible filter for veracity, accuracy, or quality of the articles published, as little is known about how well these qualities compare to those of un-peer-reviewed, i.e. unfiltered articles. There is nothing wrong with peer review itself, of course. In fact, some form of on-going review by fellow scientists, peers, is a feature that defines the very nature of the scientific discourse.

However, there is little need – especially in the online publishing environment of today, with its much more modest volume-dependent costs than print – for pre-publication peer review as a way to prevent publication of unworthy or irrelevant manuscripts (and so limit costs). This is particularly the case since most manuscripts are eventually published anyway, even after several rounds of being rejected, as they go down the 'cascade' of the journal ranks.

Therefore, so-called 'preprint' publication (publication before peer review has taken place) deserves to be propagated and widely accepted, as long as the means and incentives are simultaneously provided to have articles (preferably openly and not anonymously) peer reviewed once they have been made public, so that research results are made available without delay to whomever needs them. The articles may subsequently be published in a peer-reviewed journal, but that should be an option, an extra, if researchers or their employers so desire, and not a requirement for authors to be cited, make career advances, or be funded.

Introduction

Peer review has become a standard ritual in science communication via publication in the form of journal articles, the prevailing method in the physical and medical sciences and mathematics. But peer review is a poorly defined concept. “Peer review is an imprecise term and varies across disciplines: at its widest it is, ‘the assessment by an expert of material submitted for publication.’ ” (Olson 1990)[1]

And the practice hasn’t sufficiently changed with the changes in the way science is being done, where speed of publication and the sharing of data have become much more important than they were in the past, which is particularly the case in the natural sciences and medicine.

It is not unambiguously clear that pre-publication peer review benefits scientific progress enough to justify its high cost, in terms of money (the cost of journals) and of time (the opportunity cost of time spent reviewing manuscripts, many of them more often than once).

Alternative approaches need to be sought and found, in order to rejuvenate scientific communication, for the benefit of contemporary research, but also for the future, in which the amount of scientifically valuable information is bound to grow.

Current status and trends of scientific communication, peer review and preprints.

What is peer review? And, importantly, what is it not?

Cameron Neylon, then Advocacy Director at Public Library of Science, is recorded as saying at a conference in 2015 celebrating the 350th anniversary of the Philosophical Transactions (the world’s oldest scientific journal, published by the Royal Society): “We talk about peer review as if it’s this unitary thing. [...] Peer-review is radically different from domain to domain, from discipline to discipline.” (Gielas 2015)[2]

To which we can add that journals often have their own concept of what peer review should be, and should be for.

Perhaps it is best to start with the second of the two questions: what is peer review not? Peer review is no guarantee of a paper’s validity or quality. This is not always understood. In fact, it is often misunderstood, even by scientists themselves. Peer review cannot guarantee the correctness of the results. Incorrect assertions are not just the result of fraud, but also of unfortunate, unintended errors. As Marc H. Brodsky, executive director of the American Institute of Physics, states “Referees cannot determine if data is falsified, nor are they expected to”. Nor is it likely a referee will be in a position to spot an error as a consequence of a mislabelled drug, as in the case of the Ecstasy research retraction. (Mulligan 2004)[3]

The danger inherent in this misunderstanding is that it might undermine scientific skepticism. Putting the label ‘peer- reviewed’ on an article may give it more than its due authority, especially in the eyes of a less or inexperienced reader. (Velterop 2015)[4]

Nonetheless, peer review is commonly perceived to have two important functions:

1. Catching errors and omissions (for this, reviewers could be anonymous). This is the only sort of peer review that is clearly beneficial before publication, as it protects the authors, the journals, as well as any readers from potential misinformation. As said above, this perceived function warrants a degree of caution.

2. Starting the scientific discussion (contrary to 1., this is best done in the open: with signed and published reviews as well as published authors' responses). This sort of peer review can easily – and should – take place after publication and continue to take place as long as a scientific issue is worth discussing. ***In fact, the whole scientific discourse, initially and subsequently, is a form of peer review.***

Scientific research has evolved, and a number of pressures in the system has made it necessary for scientific communication to co-evolve, in particular journal publishing and peer review. Among the pressures on the system are

- The number of submissions, growing year on year (~2.5m in 2014, increasing by about 3% annually) (STM Report 2014)[5]. The report[6] from the 2016 'Information Overload Workgroup' of the OSI (Open Scholarship Initiative, not to be confused with the Open Society Institute, also known as OSI, the convener of the original open access initiative known as the Budapest Open Access Initiative in 2001[7]) references a post in Nature's Newsblog from 2014 by Richard Van Noorden[8], who reports a doubling of scholarly output every nine years. An annual growth of 3% and a doubling in nine years (requiring an annual increase of ca. 9%) don't tally, though one is referring to submissions and the other to output, which presumably means publications, so there may be an explanation but the central message that growth is relentless remains. That said, there is a big difference between 3% and 9% so it would be good to investigate this further. Looking at abstracts in PubMed, the number added in 2014 was almost double (x1.9) the number added in 2005. So it seems that Richard van Noorden's estimate of doubling in nine years is much closer to the mark, at least in the life sciences, than the STM report's estimate of 3% per year. Of course, the life sciences may not be representative of all of science.

- The number of researchers, growing year on year (also by about 3% annually, currently standing at an estimate between 7 and 9 million, depending on definition) (STM Report 2014)[9]

- The increasing practice of making data available, sometimes as 'supplementary material', and data access is sometimes mandatory as per the journal's or publisher's policies.

- The number of journals available (journal titles, too, are typically growing in number by ca. 3% per year)

- The number of submission cycles (submission—peer review—rejection—repeat until acceptance) which put a burden on the availability of reviewers

> *In written evidence (2011) submitted to the UK House of Commons Science and Technology Committee for their Report on Peer Review in Scientific Communications, the Publishers Association of the UK stated that: "Publishers receive over three million article submissions per year, currently rising at 5–10%."* (UK Publishers Association)[10]
>
> *The Publishers Association also state, in the same document, that ca. 50% of submissions are rejected. — If 3 million submissions result in 2.5 million published articles and 50% of submissions are rejected, this implies that most articles must have been submitted at least twice to different journals. (If a journal requires 2 peer reviewers per submitted article and a submission goes through 2 cycles before being accepted for publication, at least 4 peer reviewers in total were needed).*

- The increasing 'interdisciplinarity' of research, with discipline boundaries becoming 'fuzzier'.
- The increasing desire for speed in the publication process, particularly on the part of authors
- The increasing possibilities of the internet to present more information, more formats, more functionality associated with scientific articles than the classic article format (e.g. semantics, data, visualizations, video, audio, active software code, etc.)
- The resulting difficulties in finding appropriate – i.e. true expert – peer reviewers, who are available and willing to undertake the requested peer review (see also Schumann[11]).

Lilian Nassi-Calò puts it like this: "The steady increase of the number of journals and articles worldwide, driven mainly by online publication, has not been accompanied to the same extent by the number of researchers, which led to the saturation of the thorough peer review work. Thus, it is increasingly difficult to get good reviews within the deadlines prescribed by the journals – and wished by the authors." (Nassi-Calò 2015)[12] – This presumes, of course, that the doubling of articles in 9 years mentioned above is correct, or at least the increase is materially larger than 3%.

The value of peer review

Peer review is a rather vague concept. Mike Taylor scathingly describes it as follows: "the principal value of peer-review is that it provides an opportunity for authors to demonstrate that they are prepared to undergo peer-review." He likens it to a 'hazing' ritual (an initiation ritual that involves harassment, abuse, or humiliation) (Taylor 2012)[13].

His views may be not be shared by everyone (it must be said that some see this comment as somewhat over-the-top), although there is undoubtedly an element of truth in it, particularly for young, early career scientists, but in view of the developments mentioned about scientific research evolving, it would make sense to consider the value of modern peer

review as separated into two, or even three elements: 1) the *value for science*; 2) *the value for scientists*; and 3) *the value for society at large.*

It seems that the value for science appears quite limited. Peer review may even promote mediocrity: "research suggests that evaluative strategies that increase the mean quality of published science may also increase the risk of rejecting unconventional or outstanding work." (Siler et al. 2015)[14] Most likely for that reason, many ground-breaking studies in science end up in lower-tier journals. (Balietti et al. 2016)[15] TheScientist highlighted a few examples in 2010[15], and Faculty of 1000 uses the term 'hidden jewels', which is mostly applied to exceptional articles in lower-tier journals[16].

Peer review as usually practiced may be suitable for what has been termed 'normal science' (Kuhn 2012)[17], but we may, collectively, be overlooking important scientific breakthroughs. Jian Wang, Reinhilde Veugelers, and Paula Stephan observe that "Research based on an unusual or novel approach may lead to important breakthroughs in science, but peer evaluators are often overly cautious in evaluating such work. [...] The researchers find that novel studies are less likely to be published in high-impact-factor journals, but eventually have a much higher chance of being in the top one percent of highly cited research, and that they are more likely to lead to important follow-up research." (World Economic Forum 2016)[18]

Sometimes one hears the argument that genuinely novel breakthroughs may be beyond conventional judgment. If that were indeed the case, and it may, peer review would clearly not be an appropriate way of assessing that kind of work. However, since almost all articles intended for publication are being published in some journal or other, the fact that truly novel studies are less likely to be published in prestigious journals is of diminishing relevance, as they will be available in the literature anyhow. In practice, this phenomenon may be more of a problematic issue for proposals submitted to funders[19] than for the appearance of novel results in the scientific literature.

The question should also be asked what the value of peer review is to *society at large*. David Horrobin: "If the pronouncements of science are to be greeted with public confidence – and there is plenty of evidence to suggest that such confidence is low and eroding – it should be able to demonstrate that peer review, 'one of the sacred pillars of the scientific edifice', is a process that has been validated objectively as a reliable process for putting a stamp of approval on work that has been done. Peer review should also have been validated as a reliable method for making appropriate choices as to what work should be done. Yet when one looks for that evidence it is simply not there." (Horrobin 2001)[20]

A forteriori this applies in 2018, in the age of fake news, which is not alien to scientific and medical information that reaches the general public, as anti-vax news, for instance, demonstrates.

Horrobin even asks the question "Could the peer-review processes in both academia and industry have destroyed rather than promoted innovation?" According to him, the conclusion from Rothwell and Martyn's paper (Rothwell et al. 2000)[21] is that "the core system by which the scientific community allots prestige (in terms of oral presentations at

major meetings and publication in major journals) and funding is a non-validated charade whose processes generate results little better than does chance."

With regard to the *value to scientists*, the situation is different. Since their future funding potential and their chances of career progression depend largely on the prestige of the peer-reviewed journal they have managed to publish in, the value of peer review to them is obvious. Albeit not just any peer review. Determined authors will almost always find a peer reviewed journal willing to publish their articles, e.g. as reported by Siler et al. in the area of medical sciences (Siler et al. 2015)[22]. Therefore, in reality it is mostly the perceived prestige of journals – primarily expressed in terms of 'impact factor' – in which articles are published, rather than the mere fact that they have been peer reviewed before publication, that determines the value of a publication to a research scientist's career and funding potential.

The reliability of peer review

Whilst there are many publications criticizing (aspects of) peer review, there seems to be a paucity of literature offering evidence that peer review generally improves publications. The Open Scholarship Initiative Report from the Peer Review Workgroup offers a peer review SWOT analysis[23], which in its 'strengths' quadrant contains assertions, that, unfortunately, do not quite amount to evidence (e.g. "Peer review adds value").

- Richard Smith, ex Editor of the British Medical Journal: "We have little evidence on the effectiveness of peer review, but we have considerable evidence on its defects."

- And: "So peer review is a flawed process, full of easily identified defects with little evidence that it works. Nevertheless, it is likely to remain central to science and journals because there is no obvious alternative, and scientists and editors have a continuing belief in peer review. How odd that science should be rooted in belief." (Smith)[24] – Smith's thinking later evolved into arguing for abolition of peer review altogether. (Smith)[25]

- Richard Smith again, in a different article, entitled "The peer review drugs don't work": "[Peer review] is ineffective, largely a lottery, anti-innovatory, slow, expensive, wasteful of scientific time, inefficient, easily abused, prone to bias, unable to detect fraud and irrelevant." (Smith)[26]

- In a comment on the article mentioned above, made on June 1, 2015, Pavel Nesmiyanov says: "The peer review system works only for publishers and competitive reviewers who don't want their rivals get published. It doesn't work for science and scientists. It is no surprise that peer review has its 'social contexts' and sometimes this 'context' overweighs real scientific value of the paper."

- In What Are The Alternatives To Peer Review? William Arms says: "One problem with peer review is that many types of research cannot be validated by a reviewer. In

> the *Journal of the ACM*, the content is mainly mathematics. The papers are self-contained. A reviewer can check the accuracy of the paper by reading the paper without reviewing external evidence beyond other published sources. This is not possible in experimental areas, including clinical trials and computer systems. Since a reviewer cannot repeat the experiment, the review is little more than a comment on whether the research appears to be well done." (Arms)[27]

In spite of what seems to be peer review's lack of reliability, peer review has become a standard ritual in science communication via journal articles, the prevailing method in the physical and medical sciences and mathematics. It was not always thus. Systematic peer review prior to publication is a fairly recent phenomenon. Some, however, nonetheless speak of a "centuries-old peer-review process" – e.g. David J. Solomon in 'The Role of Peer Review for Scholarly Journals in the Information Age' (Solomon)[28] – and that notion is rather common. However, it is misleading, because whilst the practice is indeed centuries old, it started to be widely and systematically applied only in the second half of the 20th century. (Fyfe)[29]

One reason – not often mentioned – for journal publishers to implement peer review widely was a consequence of the increasing manuscript submission flow in the decades after the Second World War, combined with the increasing cost of publishing at that time: the cost of typesetting, printing, and distribution, due to the rapidly growing volume. Publishers needed to make sure (or rather, to create the impression) that what they published in their journals was 'worth publishing' for what they perceived as their audiences, as those audiences influenced librarians' decisions to subscribe. Audiences might not recommend subscriptions if the journals in question published too much material seen as 'irrelevant' to their research. However, pretty much all research that authors wanted to publish did get published anyway (if not in the journal of their first choice, then in another one – the number of journals in the same or a similar subject area proliferated) so the 'relevance' perception evolved into a 'quality' perception. The 'self-organising' nature of scientific journals evolved from a subject-organisation into a quality ranking. In the process, the fact that peer review itself is an academic community exercise, not a publisher's one (only its workflow management is), is often lost.

The fairness of peer review

Peer review has been described, e.g. by Lutz Bornmann and Hans-Dieter Daniel, as "the luck of the referee draw." This is particularly the case when reviewers are asked to 'vote' whether a manuscript should be published, instead of an editor taking that decision on the basis of substantive analyses and reviews of a paper's content. (Bornmann et al.)[30] The authors give the example of a journal using what they term the 'clear cut rule': only when two reviewers agree that the manuscript should be published, it will be. The reviewers are asked questions such as: 'How important do you consider the results?' (four response categories: very important, important, less important, unimportant); 'Do the data obtained by experiment or calculation verify the hypothesis and conclusions?' (two response categories: yes, no); 'Is the length of the manuscript appropriate to its contents?' (three response categories: yes, no – the manuscript is too short, no – the manuscript is too long);

‘Do you recommend acceptance for publication?’ (four response categories: yes – without alterations, yes – after minor alterations, yes – but only after major alterations, no).

Particularly the first question is inappropriate for a ‘clear-cut’ decision-making vote. Judgments of relevance and significance should not be made by reviewers and ideally, journal editors and publishers instruct reviewers accordingly. Such an approach can complicate, impede, or even prevent openness of the procedure and the resulting reviewer reports (see paragraph on open peer review, below).

High profile journals, such as Nature and Science (but also others), that still appear in print, yet have many submissions, and therefore inevitable space limitations, operate a very strict selection, partly using pre-peer-review rejection (via a system curiously called, at Nature in any case, ‘pre-submissions’), and partly using strict peer review, resulting in an estimated 95% + average rejection rate. The consequence of this is that a paper rejected by these journals is by no means of insufficient quality. Many a high-quality paper is simply rejected due to lack of space.

The necessity of peer review

Richard Smith, then Editor of the British Medical Journal, reports: “Stephen Lock when editor of the BMJ conducted a study in which he alone decided which of a consecutive series of papers submitted to the journal he would publish. He then let the papers go through the usual process. There was little difference between the papers he chose and those selected after the full process of peer review. (Lock 1985)[31] This small study suggests that perhaps you do not need an elaborate process. Maybe a lone editor, thoroughly familiar with what the journal wants and knowledgeable about research methods, would be enough. But it would be a bold journal that stepped aside from the sacred path of peer review.” (Smith)[32]

Taking this approach would consider any peer review merely as advice to the Editor, rather than it being used as a label of reliability, veracity or quality. The onus would solely be on the Editor, and if the experiences of Lock are more generally true, the elaborate, costly (in time as well as money), and universally adopted peer review process may to a significant degree be redundant.

Peer review practices

Different journals follow different types of peer review based on the kind of research they publish and their journal management style. There isn’t really a commonly accepted ‘best practice’ for peer review, although a few – certainly not all – publishers have guidelines for the reviewers (and editors) of their journals in that regard (e.g. Wiley[33] and Taylor & Francis[34]). The single- or double-blinded peer review is the most commonly adopted format. More recently, some journals have also started adopting open review and post publication peer review.

- **Single-blind peer review.** In a single-blind review, authors are unaware of who reviewed their paper, but reviewers are aware of the authors' identity. While this method serves to reduce chances of bias or conflict of interest, there is a possibility that making the authors' identities and their affiliations known could influence the review.

- **Double-blind peer review.** In a double-blind review both the author and peer reviewers are not aware of each other's identity. The risk here is that it may sometimes allow reviewers to give inaccurate or irresponsible feedback to authors.

- **Semi-open peer review**. In this model the reviewers' reports are open, but anonymised: the identities of the reviewers remain undisclosed to the authors of the submitted articles.

- **Open peer review.** In an open peer review, identities of both author and reviewers are known. Also, this system allows the peer reviewer comments as well as authors' responses to be published along with the final manuscript. The assumption is that open peer review is a safeguard against the perceived problems and potential conflicts of interest that may compromise the fairness of anonymous peer review. That said, some reviewers that are invited by journals that have open peer review, are reluctant to engage. This is particularly true of early career scientists, as they fear that openly reviewing (unfortunately, often seen as criticizing) the work of more senior scientists may harm their career prospects. F1000Research[35] and many of the medical journals of BioMed Central, among others, operate open peer review. BMC Anesthesiology is a good example of how it works.[36] This system allows reviewers to take credit for their reviews.

- **Post-publication peer review.** The post-publication peer review is a relatively new approach adopted by some open access publications in an attempt to overcome the limitations of traditional forms of peer review. As the name suggest, in this type of review, reviewers evaluate a manuscript after it has been published on an open access platform. Here, the identities of author and reviewer are known (published) and so is the peer review evaluation of the manuscript. This system also allows reviewers to take credit for their reviews. See also the paragraph about **Preprints**, below.

- **Technical, also known as 'non-selective' or 'impact neutral' peer review.**[37] This is a form of peer review sometimes dismissively — but unfairly — referred to as 'peer-review light'; the description 'technical peer review' is more accurate. It has been pioneered by PLOS One and aims to remove a judgment of 'significance' or 'relevance' — often an important criterion for journals — from the peer review criteria, on the reasoning that significance and relevance are relative and can almost never be truly determined at the point of publishing. Instead, it can take years, even decades, before true significance is known. Subsequently highly cited articles are known to have been initially rejected, and even the most prestigious journals regularly fail to recognise significance. Peer review reverts to the mean: "research that is categorizable into existing research frames is more likely to appeal to risk-averse gatekeepers [reviewers] with time and resource constraints, because people generally find uncertainty to be an aversive state. This may serve as an explanation for why exceptional and uncommon work may be particularly vulnerable to

rejection." (Siler et al.)[38] To be accepted for publication in PLOS-One, research articles must satisfy the following criteria (PLOS One)[39]:

- The study presents the results of primary scientific research.
- Results reported have not been published elsewhere.
- Experiments, statistics, and other analyses are performed to a high technical standard and are described in sufficient detail.
- Conclusions are presented in an appropriate fashion and are supported by the data.
- The article is presented in an intelligible fashion and is written in standard English.
- The research meets all applicable standards for the ethics of experimentation and research integrity.
- The article adheres to appropriate reporting guidelines and community standards for data availability.
- Technical soundness of the work
- Rigor of the analysis
- Adherence to our [PLOS] data availability policy

The requirement for "clear use of the English language" and "standard English" can sometimes be problematic for non-native speakers of English, but unambiguous clarity should not be confused with perfect grammatical correctness. As this form of 'technical' peer review attracts also papers reporting 'less interesting' results (confirmatory, negative or null-results), it may also be more difficult to find reviewers. Negative results are, by the way, extremely important to publish. Not doing so, and in effect withholding such results from other researchers, can result in literally lethal consequences, where researchers embark on trials without being aware of risks. This article, in The Guardian[40], describes a trial in which babies died. Although it is not mentioned as such in the article, in Dutch radio and television interviews blame was laid at unawareness of the researchers of major risks, even though it appeared those risks were known to others, but just not published.

- **Peer Review by Endorsement.** This is a very new form of peer review, although endorsement systems by e.g. the Proceedings of the National Academy of Science (PNAS) are not dissimilar, whereby authors themselves invite [at least two] peers to review their paper (according to some rules to avoid nepotism and friend-bias). ScienceOpen[41] and, to a somewhat more limited degree, the BMC journal Biology Direct[42] are examples of this method.

The STM Report of March 2015[43] describes two further – related – forms of peer review:

- **Cascade peer review.** This seeks to avoid the necessity of repeated peer reviewing each time a paper is rejected and resubmitted to another journal, by forwarding (with the author's consent) the article and its accompanying review reports to the new journal. This approach was pioneered by BioMed Central and later became seen as characteristic of the PLOS One-type 'megajournal'.

- **Portable peer review.** In cascade review, peer review is carried out by a journal in the usual way, and if the paper is rejected the review may accompany the paper to a new journal submission. A more radical idea is for authors to commission their own peer reviews prior to journal submission. This might be as a pre-submission process intended to improve the paper before submission in the conventional way, or even a fully 'portable' review that participating journals could agree to accept (if not necessarily to be bound by). There were two organisations offering portable peer review services, each with quite different business models. Rubriq[44] used to provide authors with peer review in return for a fee, but seems to have discontinued that srvice. Peerage of Science[45] offers a platform for journal-independent review which publishers can similarly scan for potential submissions. It does not charge authors but seeks to cover its costs by charging organisations such as publishers, funders and universitees a fee per paper.

Irrespective of the method adopted, these peer review practices are all primarily intended as screening mechanisms to weed out bad science and to help authors improve the quality of their research.

Should peer review be open?

In an era when openness and transparency are increasingly valued for their benefits to science, economic development particularly of SMEs (Small and Medium-sized Enterprises) and scientific awareness and literacy of the general public, it seems that not having open peer review is fast becoming anathema. Open peer review can – at a minimum – mean that it is made known who the reviewers have been. It could also mean that the whole peer review reports or substantial portions of those, signed by the reviewers, are published with the article that has been reviewed. This is seen by some as dangerous to the interests of especially young, early career scientists reviewing work of more established colleagues on whom their career progression or funding might one day depend. Though this is possibly based more on FUD (Fear, Uncertainty and Doubt)[46] than reality, the dangers to (early career) scientists who do the reviewing should be taken seriously. The trouble is probably more associated with the reviewers' judgment of relevance and significance, than with observations and critique of factual assertions and interpretations. As said above in the paragraph about fairness, judgments of relevance and significance should not be made by reviewers and ideally, journal editors and publishers instruct reviewers accordingly. Of course, double-blind peer review can be a defense against these effects. But openness of peer review has important advantages. Among those are first of all that reviewers would likely be better motivated to be tactful and constructive. Secondly, reviewers with a vested interest in suppressing the publication of a manuscript could be more easily unmasked by

authors. (Although manuscripts are rarely reviewed by a single reviewer, anonymous review does offer unscrupulous reviewers more opportunities for blocking publication without repercussion.) Thirdly, a completely open review system would have reviewers' names published and encourage reviewers to do a thorough job. “When bad science is published, the negligence of reviewers can be as aggravating as the incompetence of authors.” (Nature Perspective)[47]

The future of peer review

Pre-publication peer review may have been necessary when scientific experiments were principally done by a single researcher and articles had a single author, which was more prevalent in the past than it is at present. After all, peer reviewers provide a second (third, etc.) pair of eyes. With the advent in the last few decades of larger teams of researchers collaborating and multi-authorship of the resulting articles, there are ‘internal’ pairs of eyes, and peer review by (random) others in order to provide the extra pair of eyes, is somewhat redundant. Given the difficulty of finding appropriate peer reviewers, the time peer review takes, and its lack of clear and unambiguous benefits, the necessity of systematic universal pre-publication peer review (see relevant paragraph above) should perhaps be reconsidered. Web technology permits systems of post-publication peer review to emerge, and so make the fact that “the entire scientific discourse is a form of peer review” an everyday practical reality. Preprint services and platforms will play a crucial role in this development (see paragraph on Preprints, below). On the Frontiers Blog it is phrased as follows:

“What we see is a process of rapid innovation and diversification, in which a monolithic, ‘one size fits all’ system of peer review is gradually replaced by a range of different review mechanisms, many of which are only used within specific disciplines. Nearly all these changes (e.g. non-selective review, open review) have been pioneered by Open Access publishers, while publishers with traditional business models have been slower to adopt the new practices.” (Frontiers Blog 2016)[48]

Should peer reviewers be rewarded?

Proper peer review takes time and effort on the part of the reviewer, so it stands to reason that they are rewarded in some way.

But reward does not equate financial compensation. The current ‘cultural’ convention in science is that having peer-reviewed, particularly for journals high up in the prestige ranks, is an asset to any researcher’s curriculum vitae.

Actual payment for reviews may at first seem reasonable, but the resulting cost of journals, already very high in general, would then increase to unfeasible levels, particularly if reviewers were paid amounts that would reflect the time and effort invested. Token amounts

– which are sometimes paid, albeit by a very small number of journals[49] – are not helpful for the general quality of reviews and risk being seen as derogatory.

That said, the difficulty of finding appropriate peer reviewers is felt by even the largest publishers. In an effort "to encourage efficient and timely reviewing and to recognize the appreciation for the important work of reviewers", Elsevier has recently sent emails[50] to reviewers, which include the following paragraph:

> *"To encourage efficient and timely reviewing and to recognize the appreciation for the important work of reviewers, Elsevier will publish on the journal's website a list of reviewers with their full names and their relative ranking and percentile in how quickly they submitted their report (computed as days between the invitation to review and the submission of a referee report). Referee anonymity will be preserved because authors are not aware of the dates in which a reviewer was invited and submitted his report. Moreover, Elsevier will not publish the number of days taken for the referee to complete the report, but only the relative ranking and percentile (e.g., a ranking of 120 among 300 reviewers and the 40th percentile). The reviewers' names, ranking, and percentile will be published only for the top 80% of reviewers in terms of days taken to review. The 20% of reviewers with the longest review times will not appear in the list."*

This is not likely to be of much help, as it may be seen as undue 'peer pressure' on reviewers and serve as a disincentive to review thoroughly, since doing so requires time.

Preprints

Preprints are publicly available versions of articles that have as yet not been [peer-]reviewed. They are often seen as 'informal' publications, with only articles that have been peer-reviewed and published in a journal regarded as 'formal'. This situation is changing. "Peer-reviewed journals are often called 'primary literature', but this is increasingly becoming a misnomer. Theoretical computer scientists do not use the *Journal of the ACM* as primary material. They rely on papers that are posted on web sites or discussed at conferences for their current work. The slow and deliberate process of peer review means that papers in the published journal are a historic record, not the active literature of the field." (Arms)[51] The situation in some other disciplines, outside computer science, has been similar. Physics, in particular high-energy physics, has a long tradition of so-called 'preprints', which were already part of the way physicists communicated long before the electronic publishing possibilities of the web came into being. Preprints are articles that have not yet been formally peer reviewed, but published nonetheless, in the interest of rapid scientific communication, while awaiting formal peer review. Publishers used to print researchers' submissions and distribute them among members of the community well before they were published in their peer-reviewed journals. In August 1991, Paul Ginsparg, then at the Los Alamos National Laboratory, launched a repository for electronic preprints (or 'e-prints'), making use of the much faster, electronic, distribution that the Web enabled. It was originally called the 'LANL preprint archive' (xxx.lanl.gov),

but later, when moved to Cornell University, re-named arXiv.[52] Initially focussed on physics, it now hosts preprints in areas such as astronomy, mathematics, computer science, quantitative biology, quantitative finance, and statistics as well. ArXiv's monthly submission rates still show a steady increase.[53] Submissions to arXiv are moderated or endorsed, but not peer reviewed before being posted. Moderation and endorsement are merely mechanisms to prevent inappropriate or unscientific articles from polluting the repository. The total number of preprints available on arXiv is currently (August 2018) approaching 1.45 million.

The attractions of preprints go well beyond the speed of scientific communication. Preprints are expected to be openly and freely accessible by anyone (open access). The combined attraction of speedy communication and open access have led to disciplines other than the ones mentioned above to warm up to preprints, in spite of the fact that there are still some journals that do not wish to consider manuscripts for publication if they have already been made available as a preprint. This is known as the 'Ingelfinger rule'.[54] Support for the Ingelfinger rule is dwindling, and even the New England Journal of Medicine seems to soften its stance in that regard.[55]

A relatively recent initiative has been ASAPbio[56], an effort aimed at discussing and considering the desirability of widespread use of preprints in the life sciences with stakeholders in those fields, ranging from junior and senior scientists, academic chairs/ administrators, scientific societies, publishers, and private and public funders, with a view to ultimately implementing "a sensible preprint system". In his article 'Accelerating Scientific Publication in Biology', Ronald Vale presents a comprehensive analysis of the potential of preprints.[57] He considers the pros ("fast, free, and feasible"), the cons ("lack of peer review and information overload"), and the uncertainties ("culture, priority, and government and journal support") of a communication system based on preprints. The pros are beyond doubt; the cons merely speculative, but it is the uncertainties that need attention in order for preprints to really take off. Once preprints are accepted as evidence of priority of a discovery, as it is in physics (an issue for the culture of various other scientific disciplines), once they are accepted as evidence for productivity (an issue for funders), and once the majority of journals accept submissions that have already been posted as preprints (an issue for publishers), scientific communication can truly accelerate.

That being said, some preprint activity is already taking place in the life sciences, principally via the preprint service bioRxiv,[58] "the preprint server for biology". BioRxiv currently (August 2018) receives in the order of 1500 preprints every month. Preprint services have also been launched or announced in other areas, such as chemistry.[59]

Preprints are not limited to central, discipline-oriented services, but many institutional repositories[60] host preprints as well. The issue, and challenge, for many institutional repositories is to ensure sufficient discoverability. "Simply placing research outputs in an institutional repository is not enough." (JISC)[61] Some journals and journal publishers have been getting into the act, too, for example PeerJ[62] and F1000.[63]

Preprint services that, upon articles being posted, accommodate subsequent peer review, or even enable submission to journals for formal peer review (such as 'direct transfer to journals' by bioRxiv[64]) seem to offer the best of both worlds (though "the best of both worlds" is contested, see Eisen and Voshall below), in that by separating informal preprint publication and peer-reviewed formal publication they satisfy the demands of open and speedy scientific communication with the career and reputation demands of having formally peer-reviewed publications. (Velterop)[65]

Eisen and Vosshall argue that pre-prints and journal-based peer review cannot coexist.[66] I am of the opinion that maintaining an – expensive – journal system is merely undesirable. Some do serve as a focal point for specific sub-disciplines. However, the socio- cultural compulsion to formally publish in a journal should be abolished, and publishing in a journal subsequent to a preprint posting should be voluntary only. The option should remain in place, though, until the primacy of preprints is generally accepted for career progress and researchers' reputations, and journal publications are not needed any longer. As Eisen and Vosshall also make clear, they consider proper peer review important, albeit in their preference done in the form of post-publication peer review carried out on preprints, without the involvement of journals.

Various preprint services have recently sprung up, including those owned and operated by traditional publishing outfits. Care should be taken that if those facilitate subsequent submission to journals, they do not only do that to journals they own themselves, although that may be difficult to prevent. Authors should be made aware of these issues, by clearly stating their freedom to subsequently submit to any journal, from any publisher, or choosing not to submit for subsequent journal publication at all.

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