

# Journals, repositories, peer review, non-peer review, and the future of scholarly communication

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## **Abstract**

Peer reviewed journals are a key part of the system by which academic knowledge is developed and communicated. Problems have often been noted, and alternatives proposed, but the journal system still survives. In this article I focus on problems relating to reliance on *subject-specific journals* and *peer review*. Contrary to what is often assumed, there are alternatives to the current system, some of which have only becoming viable since the rise of the world wide web. The market for academic ideas should be opened up by separating the publication service from the review service: the former would ideally be served by an open access, web-based repository system encompassing all disciplines, whereas the latter should be opened up to encourage non-peer reviews from different perspectives, user reviews, statistics reviews, reviews from the perspective of different disciplines, and so on. The possibility of multiple reviews of the same artefact should encourage competition between reviewing organizations and should make the system more responsive to the requirements of the differing audience groups. These possibilities offer the potential to make the academic system far more productive.

**Keywords:** Academic journals, Open access, Peer review, Scholarly communication, Science communication.

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## Introduction

Papers published in peer reviewed journals are a key part of the process by which academic knowledge is developed. Articles are checked through by the authors' peers to try to "ensure that the valid article is accepted, the messy article cleaned up, and the invalid article rejected" (Weller, 2001: p. xii). The goal of this process is to ensure that the papers published are of as high quality as possible, so that fellow researchers and other readers can have confidence in what they read.

According to Larsen and von Ins (2010: <http://tinyurl.com/m853lps><sup>1</sup>) the number of peer reviewed journals "most likely is about 24,000". Authors of research articles wishing to publish in one of these journals, must first select their journal (usually not easy, given the choice), make sure their article conforms to the requirements of the chosen journal, send a copy to the editor who will decide if it is worth serious consideration, in which case he or she will send it out to two or three peer reviewers, and then make a decision based on the recommendations of these reviewers. This decision might be to publish the article as it is, to reject it, or to ask the authors to improve the article in line with the reviewers' recommendations and then reconsider it. The reviewers are "peers" in the sense that they are typically working in the same area, and may have submitted work to the journal in question. They are usually unpaid, anonymous volunteers who cannot be expected to respond immediately, so the whole process may take some time - in the worst cases, several years. The details of this process vary from journal to journal, and conventions in different disciplines differ, but the above outline is typical. However, this is a relatively recent development: "Peer review in its modern present form is only about 40 years old and is not standardized. ... a systematic peer review for Nature was only introduced in 1966 ... Proceedings of the National Academy of Sciences introduced peer review only a few years ago" (Larsen and von Ins, 2010).

There are differences between disciplines, but in general far more credence is placed on research published in peer reviewed journals than on other sources: researchers typically only take work published in peer reviewed journals seriously, students are routinely told that the only references that really count are to peer reviewed journals, and when research results are given in news bulletins the name of the journal is often given as evidence that it is worth taking seriously. There is a sense in which research *is* the contents of peer reviewed journals, although this may obviously be recycled for a wider audience in books, TV programmes, educational courses and so on.

The background to the business of research is changing, at a rate often said to be unprecedented, in a number of different ways. These changes include the availability of web-based search and communication in all its guises, the growing number of journals, most of which are now available on the web, increasingly rigid incentives for academics (e.g. in the UK, for the past decade or so, university academics have been under pressure to publish regularly in "good" journals with their "best" four papers used to evaluate their performance), and so on. These factors seem likely to lead to authors of research papers reading more widely but less thoroughly (because there are more references and they are easier to find, obtain and scan for keywords, but reading itself, of course, has not become any faster). This changing background means that sensible academic communication systems will need to

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<sup>1</sup> For reasons explained [below](#) I have included a short url in the text to open access copies of citations.

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evolve to avoid the problems caused by this changing background, and to take advantage of the opportunities it presents.

The peer review journal system is still the gold standard in most disciplines. It has, however, attracted considerable criticism. The most commonly made points are:

- 1 Most journals are expensive for readers to access which, in practice, means that the readership is often restricted to those with university subscriptions to the journals. This has led to calls for open access journals, which are being increasingly heeded. The system does seem to be progressing in the direction of open access so that the products of research should be accessible to all. (In the UK, only papers in freely available institutional repositories are eligible to be counted in the formal research assessment from 1 December 2013.)
- 2 The fact that there are typically only two or three volunteer reviewers chosen by the editor, may lead to the suspicion that "bad" papers may sometimes be published and "good" papers rejected. Empirical studies often find little correspondence between the verdicts of different reviewers (see references below), so which papers get to be published may be little more than a lottery. For these reasons, peer reviewed journals may not be an efficient quality monitoring system.
- 3 The demand for quality assessment of research has led to quality ranking of journals so that the journal a paper appears in can be used to assess the quality of the research reported in a paper. This has led to the feeling that the top ranking journals are unlikely to accept risky or innovative papers for fear that this may damage their status. This may have a detrimental effect on the growth of knowledge, and encourage researchers to write papers according a formula approved by the top journals.
- 4 It may take a long time to get a paper published, particularly if it fails to be accepted by the first choice journal and is submitted to a sequence of journals, each of which will take months, or even years, to make a decision (parallel submissions are not allowed). In a study of one author's submissions over a given time period, the average time between submission and appearance in print for the 39 articles which had been published was 677 days (nearly two years); he had a further 14 articles he was still trying to find an outlet for - on the date he did the analysis these articles had been waiting an average of 3.5 years since their initial submission and had been submitted to between 1 and 5 journals (Nosek and Bar-Anan, 2012). My experience<sup>2</sup> suggests that this is not untypical. This has obvious implications in terms of slowing down the growth of knowledge, as well as being frustrating for researchers and their audience.

Not surprisingly there is an extensive literature on these problems and how they might be remedied: e.g. Adler (2012: <http://tinyurl.com/kcqbvr9>), Nosek and Bar-Anan (2012: <http://tinyurl.com/mdvtwtc>), Priem and Hemminger (2012: <http://tinyurl.com/oaj7jgy>), Bornmann (2011), Weller (2001), and *Nature's* peer review debate at <http://www.nature.com/nature/peerreview/debate/index.html> has links to a

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<sup>2</sup> My record for the time delay between submission and publication is 4 years and 4 months.

number of articles. [Appendix 1](#) below contains sketches of a few stories which illustrate some of the problems of the current system.

This literature is, however, widely ignored because of the assumptions that there is little alternative to the current system, and that this system does work adequately. There are alternatives (see links in previous paragraph), and it is also worth challenging the assumption that there is little point in improving the system. Einstein's papers on special and general relativity were accepted by the journals (although without peer review according to Adler, 2012), but many such revolutionary ideas are likely to be rejected. If this had happened, physics may have progressed more slowly or along a different route. Or perhaps, someone else came up with ideas similar to Einstein's years before him? There is no way of knowing. Gregor Mendel's now famous work on genetics was published in 1866 but was rarely cited and Darwin appears not to have known about it. Eventually the ideas were rediscovered in 1900. The publication system had failed to adequately distribute a theory which subsequently turned out to be very useful. Many other ideas have almost certainly never seen the light of day; although we obviously cannot point to these ideas, it is possible to estimate the impact of the inadequacies of the publication system using some plausible assumptions (see [Appendix 2](#) below).

The perfect system would deliver to every relevant person just those artefacts that are useful and of a sufficiently high quality, and do so immediately with no publication delay. Merely stating this makes it clear that this ideal is both impractical, and not clearly defined (how are usefulness and quality defined from the perspective of each person?). However, imperfections are easier to spot (see [Appendix 1](#)): it seems plausible that even a small improvement could make a large difference. Perhaps, for example, medical advances which have not yet happened, might already be common practice with a more efficient system for distributing knowledge?

If there were a universally accepted checklist against which a new research article could be evaluated in an objective way, so that all reasonable people would reach the same conclusion, then the peer review system might make sense. In practice, in most disciplines, this is far from the case: the process of reviewing a research paper requires subjective judgments and differences of opinion are almost inevitable. It is impossible to predict which research efforts the future will judge favourably: we can only guess. Given this, it seems, to me, almost inevitable that the present system will be arbitrary and, given the high stakes involved for the careers of academics, on occasions, possibly corrupt.

The current system depends on two key components: *journals* - which are *subject-specific*, and *peer-review*. In the next two sections I discuss these two concepts and argue that they are both, to some extent, problematic. I then make two simple suggestions which could lead to a very different system with many advantages over the current one. The later sections of the article explore some of these advantages, and also at some of the barriers to change.

It is important to mention two preliminary issues. The first is the unit of research output. Traditionally this is the "paper" but there are strong arguments that this should be broadened. In the UK the term "research output" is used in documentation relating to research assessment - and I will use this rather awkward phrase in this article. Some of the issues are outlined in [Appendix 3](#).

Second, academia encompasses many different disciplines and often different "tribes" within a discipline. This means that there is a great variety of types of research including, for example, randomized control trials in medicine, case studies in management, conceptual analysis in philosophy, and symbolic arguments in mathematics, to mention just a few. Words like "research", "scholarly", "scientific" and "rigorous" may seem natural in some contexts but not others, and may have different implications in different contexts. The use of the word "scholarly" in the title is intended to signal that my intention in this article is to provide a general analysis, not tied to a specific discipline.

## **Subject-specific journals**

A few decades ago journals were printed on paper, often by learned societies, and distributed to members of the society and libraries. Researchers in a particular discipline would browse through the appropriate journals to find material relevant to their research. They would probably have the most relevant ones in their office, and the university library could supply the rest. Gradually the number of journals increased, indexing services arose to help researchers cope with a much larger search domain, and online availability means that most articles are now instantly available. Google Scholar is now probably the most used indexing service aiming to include all scholarly outputs, and the typical researcher's tactics have changed from browsing key journals to searching on Google Scholar or other databases. The journal a paper is published in matters little now to researchers or their audience, except in so far as a prestigious journal legitimises the research and persuades people to take it seriously.

However, subject specific journals persist. If I want to publish an academic article which wins the respect of the academic community I have to do so in a journal. This seemingly innocuous fact has many unfortunate consequences. I am only allowed to submit to one journal<sup>3</sup> at a time so if the first rejects it I may resubmit to a second, and so on - this can result in delays of years. (In any commercial domain, this would be outlawed as a serious barrier to competition.) My paper may be interdisciplinary and it may not be obvious which journal is appropriate. If my paper presents, say, statistical evidence on a medical matter, I may want my work checked by both the statisticians and the medics. There are a few journals which cater for a broad range of subjects - e.g. *Sage Open* spans "the full spectrum of the social and behavioral sciences and the humanities" - but these are the exception not the rule.

There is now little point in journals apart from their legitimising function which could easily be taken over by organizations with a more flexible remit, as explained [below](#). There is a detailed review of some of the problems of journals, attempts that have been made to remedy them, and some further suggestions in Priem and Hemminger (2012).

## **Peer review**

Most of the published research on peer review is about its efficiency – does the current system provide valid and reliable assessments of research papers? The evidence here varies from discipline to discipline,

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<sup>3</sup> For obvious reasons it is difficult for one journal to relax this restriction unilaterally, but if it were possible for authors to submit their paper to a range of journals, and then accept the best publication offer, this would obviously transform the market for academic papers.

but is generally negative (see the citations at the end of the Introduction above). Peer review tends to be unreliable in the sense that different reviewers are likely to give different opinions, so taking just two or three reviewers makes the review process into a lottery - e.g. Kravitz et al (2010: <http://tinyurl.com/2uukbc7>), Wood et al (2004: <http://tinyurl.com/nx8ygbd>), Peters and Ceci (1982). This is not surprising - two or three anonymous, unpaid and unaccountable reviewers, who are probably just experts in some of the areas relevant to a thorough critique of the paper, would not be expected, on a priori grounds, to produce definitive reviews. Even when reviewers agree there is, of course, no easy way of deciding whether they are right, or even of defining what "right" means.

There is another, more fundamental, problem with peer review: it is restricted to peers in the same discipline or sub-discipline. It is no longer considered acceptable that doctors, teachers and so on only review themselves: review by customers, and possibly other stakeholders, is now the norm. And, of course, producers of cars, haircuts, computers and meals have always been subject to market forces: the ultimate test is whether people are prepared to spend their money. The academics' argument in defence of their exception from the general rule of external review is that only peers are in a position to understand their work, and there is some justice in this. However a discipline which has no response mechanisms to the views of potential customers or audience, including researchers in other areas, may fail to generate the most relevant and interesting research. Small cliques of academics may review each other's work positively despite its lack of interest or relevance to anyone else. Occasionally this may work, and the argument that short term responsiveness to customer feedback may inhibit creativity may be valid, but as a general principle it seems suspect. There is a strong case for at least a limited amount of *non-peer review*.

## **Suggestions**

Systems like the journal publication system evolve as circumstances change. Many of the changes discussed here will happen inevitably, and in some cases have happened already, as the agents involved see and exploit obvious possibilities. Experimental journals incorporating new ideas include Cureus (<http://www.cureus.com/>), InterJournal (<http://www.interjournal.org/>) and World Science - "the revolution in writing, reviewing, accessing, sharing, searching and using credible knowledge" (<http://www.world-sci.com/>). On the other hand some things are too deeply embedded to change easily - e.g. a movement away from the journal system is inhibited by the feeling that researchers need to publish in top journals to achieve the prestige necessary for career advancement, which means that alternative publication avenues are not taken seriously.

My intention here is to sketch an alternative system based on two simple suggested changes to the overall publication system. Some aspects of these suggestions are outlined and discussed in Nosek and Bar-Anan (2012), Priem and Hemminger (2012) and doubtless other works too. Nosek and Bar-Anan (2012) use the term "utopia" in their title to indicate that their proposals are a goal to strive for, but that there are substantial barriers to change which they spell out in some detail.

## **1. All research outputs should be freely available on the web - which would effectively incorporate an open-access journal of everything**

The obvious way of achieving this is to have a single global repository covering all disciplines (Wood, 2010: <http://tinyurl.com/nmggt6g>), but this is unduly restrictive. Such a repository might, for example, be unable to accept certain formats - e.g. video. All that matters is that they should be freely available - to everyone, without charge - on the web in a way that does not tie them to any particular discipline. The importance of this covering all disciplines is neatly illustrated by the fact that two of the most interesting articles I found when research this topic were published in journals specialising in areas which are largely irrelevant to the topic (Priem and Henninger, 2012, in a neuroscience journal; Nosek and Bar-Annan, 2012, in a psychology journal). At present there are repositories for papers which are freely available but these are either restricted to particular ranges of subjects (e.g. [arXiv.org](http://arXiv.org) or [www.ssrn.com](http://www.ssrn.com)) or for a single institution.

There is an argument that subject specific repositories would be useful, because then people interested in, say, social enterprises, can go to the social enterprise repository. However, the difficulty is that there might be relevant material in general business repositories, or in repositories focusing on charities, and so on. Separate repositories for separate areas will lead to the same problem as that created by the 24,000 journals. The problem of finding relevant material can be solved by using web search facilities aided by labels or keywords (as they are called in the journal world).

In an ideal world, then, there would be repositories which would take research outputs from anyone on any topic. However, in practice, if authors deposit their articles, or other outputs, on a personal website, or an institutional or public repository of some kind, readers can then find them using web search engines like Google or Google Scholar, so the lack of general repositories may make little difference in practice (although it might have an impact on how easy they are to find).

One thing which might enhance the current, or future, system would be a service which lists the url of each article submitted to it, certifies it as conforming to some minimum standard of presentation, and guarantees the stability of the output. If, for example, a pharmaceutical research article recommends Drug A, and the quality of this research is certified by a reviewing organization (as explained [below](#)), readers would want to be reassured that the article which obtained the quality stamp has not been changed in any way. It is possible to envisage an article being awarded a quality stamp certifying the rigour of the research on which the recommendation is made, and then the article being amended so that another, perhaps cheaper, drug is substituted in the article. The DOI system does not do this. If an object is changed there is no ruling on whether the DOI identifier needs to change: FAQ 7 at <http://www.doi.org/faq.html>.

There are more potential advantages here than may be obvious at first sight. Free availability, or open access, means that work is available to those who are unwilling or unable to pay and who have not got institutional connections. Besides this, objects on the web are far easier to obtain than old-style hard copies, and the page limit constraints of traditional paper journals should be a thing of the past - articles should no longer be turned down because there is no space in the journal and longer, more detailed articles would be feasible (Nosek and Bar-Anan, 2012). It is also far easier to publish updates and

corrections where they are likely to be noticed, thus avoiding the problem that a printed journal article may continue to be accepted despite being discredited - see [Appendix 1](#) for an example. I discuss these advantages more detail below.

## **2. Reviews of research outputs should be carried out by a range of organizations**

The equivalent of peer review would be carried out by organizations which would award some type of quality stamp to outputs. This review could go beyond peer review and might include reviews from various user perspectives, reviews of features like the readability of an article or the prerequisite knowledge necessary to understand it, as well as statistical reviews, research design reviews, and so on. Many aspects of this idea are discussed by Nosek and Bar-Anan (2012), and Priem and Henninger (2012).

This does happen currently, but only to a limited extent. Journals sometimes include comments on articles in earlier issues, there are various opportunities for post publication review either on an ad hoc basis, or via a post-publication review service (e.g. Mathematical Reviews) or an "overlay journal" for a repository such as the arXiv (<http://www.ucl.ac.uk/lis/rioja/>), some web-based journals do encourage reviews from readers, and various styles of social media can be are being used in a variety of ways (see Priem and Hemminger, 2012).

How might this work in practice? The author would deposit his or her research output (an article or something else) on the web at a place of his or her choosing. He or she would then apply to one or more reviewing organizations to obtain a quality stamp. This might certify that the statistical analysis is correct, or that it reports rigorously conducted research according to some specified standard. They may insist on changes, in which case the changed document would also be posted on the web (and readers could see the changes made if they were interested). Then potential readers might search the web, or might search articles listed by specific reviewing organizations. Then, as well as having access to the research output, they would also have information about its quality on various dimensions from these reviewing organizations.

Some of these reviewing services might publish details of individual reviews; others might not. Conventionally peer reviewers are anonymous, but there are strong arguments in favour of publishing authors and details of reviews (Nosek and Bar-Anan, 2012: 236-8). In a more open system, if readers find open reviews helpful, there is likely to be competitive pressure to provide them. Some reviewing services might stick with the current system of soliciting reviews from carefully selected experts, others might apply the principle of crowd sourcing and invite reviews from anyone with anything to offer - a principle very widely applied on the web (e.g. <http://www.amazon.com/>, <http://www.tripadvisor.com/>).

These reviewing services might be provided by the learned societies which formerly published journals; their reputations would help to convince readers that the output is sound. If a problem is subsequently found with a paper, this might lead to the reviewing organisations amending their reviews. Like any information providing service in the public domain, these reviewing organisations would depend on their reputations with readers, and competition and market forces should make the system evolve in

ways to suit all parties. The reviews themselves may be subject to review, which may also help readers assess the value of reviewing organizations.

## **Pros and con of implementing these suggestions**

There is, of course, no one body with the power to force change. Change will come because various stakeholders see an opportunity and take it, but the current system has considerable inertia: I discuss the barriers to change in the next section. In this section I look at some of the likely advantages if the suggestions above were implemented. These all contain the word "would", but they are really hypotheses which seem likely. The evolution of the system is likely to be different in different fields: in an established scientific field where a cumulative model of knowledge may be reasonable the key issue may be confirming whether research conforms to the standard procedures of the field (which seems to be what Nosek and Bar-Anan, 2012 have in mind), whereas in other fields knowledge may not be cumulative and the concerns about research may be more varied.

The metaphor of a market<sup>4</sup> is helpful here. Transactions in typical economic markets are mediated by money, which is true to only a very limited extent in the market for academic knowledge, but the basic principle of a free market enabling more productive transactions carries over. The effect of the suggestions above would be to make the market for scholarly research freer, and to create a market for reviews. Currently, the fact that authors of research articles can only submit to one journal at a time, and the fact that the journals themselves are the only real players in the review game, are restrictive, uncompetitive practices that would be outlawed in most markets in the formal economic system. Separating the review function from the distribution function is applying the principle that too much vertical integration hinders competition. Van de Sompel (2006: <http://tinyurl.com/l3mb5nj>) cites a 1997 article by Roosendaal and Geurts who takes this principle further by distinguishing five separable functions of scholarly communication. Free markets are said to have numerous advantages in terms of the efficiency with which the needs and desires of customers are satisfied by the outputs from producers. In the remainder of this section I list what these advantages are likely to be. The final one is two edged: it could be construed as either an advantage or a disadvantage.

## **Distribution and review of research outputs would be quicker**

Currently readers have to wait until a paper has been accepted and published in a journal. This can take between a month or so and several years. Under the new regime, the first draft would be available immediately, and the reviews are also likely to be available quicker than publication under the old regime for two reasons. First, reviews could be carried out in parallel, so the necessity to submit failed papers to another journal would be avoided. Second competition between reviewing organizations is likely to reduce delays.

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<sup>4</sup> An alternative metaphor would be the evolution of the organisms in an ecosystem by natural selection. I think the market metaphor is more helpful because markets operate by man-made rules, and the criteria they operate by can be more varied than simple survival.

## **More information would be available on the quality of research outputs from the reviews**

At the moment the information available to readers is typically that the article has been passed by the peer review process of the journal. What this means depends on the rigour of the review process, about which readers can only guess. Some journals have a good reputation, but it is difficult for readers to know what this reputation is based on and whether it is deserved.

In the new order, when reviewing organizations do nothing except review, it is unlikely that they would content themselves to a pass-fail rating, particularly when other reviewing organizations may be providing more information. As well as specifying the precise criteria on which a paper has been passed, they may also make further comments on strengths, weaknesses and possible extensions.

## **There would be a richer diversity of research outputs and types of review**

Because submission for review no longer excludes other options, it is likely that it would be easier for new entrants to the review market: this should lead to a richer diversity of review possibilities than currently. Different quality stamps for different criteria would be the likely result. And competition between different measurements, or even definitions, of academic quality should be useful.

A more nuanced category of evaluations would be feasible with the new system - perhaps including an "interesting but not proven" category, or a "good idea but needs work" category - so that papers which would have been rejected if the journal was publishing, and so legitimizing, the papers may be made available to readers. It should encourage a more flexible and informative reviewing system, and encourage competition between reviewing organizations on the quality of their judgments, and indeed on the definition of quality in a research area. Authors might still choose the safe, high ranking option, but they would also be able to submit to other possibilities which may thus be encouraged to flourish. And, of course, a more varied set of review possibilities would encourage a more varied ecology of research outputs.

There is a sense in which the current system is consistent with the idea of a paradigm and normal science (Kuhn, 1970). Editors of subject specific journals, and their panel of peer reviewers, are only likely to accept papers which conform to their taken-for-granted assumptions about how their subject should be done and what is worthwhile. Papers from outside this framework, or which challenge one of its cornerstones, are likely to be rejected out of hand. Horrobin (1990: <http://tinyurl.com/kzggqgu>) gives many examples in the biomedical sciences where peer review has resulted in the rejection of important articles and the "suppression of innovation". From the point of view of the efficiency with which the discipline is developed this may, in some circumstances, be sensible. However, it does make it difficult to publish work which challenges a framework because there may be no suitable journal for this work<sup>5</sup>, and even if there is, researchers in the area are unlikely to read it. This conformist tendency is likely to be reinforced by journal ranking systems which may make journals fearful of risking their status by

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<sup>5</sup> Although one possibility for paradigm-changing work in science or technology may be the new journal *Disruptive Science and Technology* (Russell, 2012: <http://tinyurl.com/ntm6tbc>).

accepting risky work. This conformist tendency would be loosened in the new regime if the greater diversity of possibilities allows some heretical views to flourish.

The current system is rigid because researchers who want to promote their careers or their research have little choice but to submit their work to high ranking journals. This ties them in to the expectations of these journals, and prevents them experimenting with other options. This rigidity could be loosened by a more flexible system which allows authors to submit outputs to a number of review organizations simultaneously, which can be facilitated by separating the publication and review processes.

### **Increased status and variety of reviews would improve the quality of research outputs**

Currently reviewing papers for journals is voluntary, unpaid and anonymous; in a new, freer market there might be opportunities for reviewing to have more status than currently and the quality and extent of the reviews might be enhanced. Arguably, too much effort now goes into producing research, and not enough into evaluating and improving it. The proposed market in reviewing might help to correct this imbalance: some reviewing organizations might experiment with different approaches to the reviewing process, which, hopefully, should encourage the development of better approaches than the current, often desperate, scramble for reviewers of dubious and unknown quality.

This does not mean, of course, that disagreement between reviewers would never happen. In the absence of unambiguous criteria for evaluating research outputs, disagreements are inevitable. Indeed, the interaction between different viewpoints is likely to be essential for the healthy growth of knowledge, and a good dissemination system should facilitate such interactions.

This is in line with the views of the philosopher Karl Popper (1972) who stressed the importance of criticism for the growth of knowledge. How it might work in practice is sketched out in Nosek and Bar-Anan (2012). It should reduce the possibility of mistakes - clearly erroneous papers being legitimized by publication in prestigious journals, and useful work failing to find an outlet (see [Appendix 1](#)).

### **It would be less obvious where the acknowledged good research outputs are to be found**

The downside of the proposed more anarchic regime is that the equivalent of the "top journals" may not be obvious, which means that it may not be clear where readers should go for the latest research in their discipline. Against this, of course, the current system arguably excludes good work for essentially arbitrary reasons. Anarchy may be harder to cope with, but in the long term it should be more productive.

### **Barriers to change and the likely evolution of the system**

Technically, everything proposed here could happen tomorrow. There are two big barriers to change: funding, and the inertia of the present system, particularly concerning reputational factors.

## Funding

Old style journals are funded by selling the journals to readers and libraries, often at exorbitant prices. This regime is already starting to crumble with the advent of open access journals, so it is unlikely that the reviewing organizations would be able to charge their audience anything other than a nominal fee without something concrete like a journal to sell. The funding models for the new system could easily evolve from the various existing models for funding open access journals. One obvious possibility is that authors, or their institutions, would pay the reviewing organizations. However, it is likely that other possibilities would evolve. Nosek and Bar-Anan (2012) review some of the possibilities. It is worth bearing mind that the web does seem to foster large scale developments whose funding mechanism is not planned in advance: fortunately the absence of a clear business model is unlikely to discourage innovation in this area.

## Inertia, especially to do with reputation and research assessment

The main barrier to change in the market for academic knowledge is probably that researchers feel that their reputations, and salaries, depend on publishing in the top journals. This gives the top journals a monopoly on prestigious research. In time, papers published in repositories on the web (like [arXiv.org](http://arxiv.org) or <http://www.ssrn.com/>) and reviewed elsewhere may achieve the necessary status to make the journals unnecessary, but at the moment in most disciplines the stranglehold of the top journals is still strong.

There is a tendency for crude and rigid grading systems to be imposed by key players on markets where customers may have difficulties in choosing between competing products. So schools and hospitals are graded with some being labelled failures, consumer reports by organizations such as Which? (<http://www.which.co.uk/>) give scores to products and label some best buys. Similarly the pressure to grade academic departments in order to decide which are deserving of funding has led to pressures to grade research papers for which journal grading (four star, three star etc) is the common proxy used. This is unfortunate from many points of view - including the healthy growth of knowledge. Inevitably the grades will ignore some important criteria. In the sciences there is even evidence that retraction rates, due to fraud or errors in the research, are *higher* in the higher ranked journals (Brembs & Munafo, 2013: <http://tinyurl.com/k9gzc7z>). Academic research deserves a more flexible and nuanced system for quality evaluation.

Despite this, the increasing pressure to make access to research open access may provide an escape route. Research published in the so-called top journals is increasingly likely to be made freely available (because of pressure from various sources) - either in the journals themselves, or by depositing copies in institutional or subject-based repositories. And, of course, anyone is free to publish reviews of the papers in these journals. At the moment this is not done to any great extent because of the assumption that publication in a good journal is the only type of review necessary. However, in the future, particularly if links to freely available copies of the papers in question are available, such reviews could easily be published to provide more information than the mere fact of publication in a prestigious journal.

## **A few specific possibilities which may be facilitated by these suggestions**

The detail of how this system might evolve is, of course, very difficult to predict and there is little point in trying. I would, however, like to explore a few interesting possibilities.

### **Competition in formats of research outputs and reviews**

I recently wanted to read the comments posted on an article in a journal on the web (PLOS Medicine). There were 30 of them and I had to click on each separately: as this would have taken too long, and would have resulted in a confusing mixture of points, I looked at two or three, and then posted my comment without reading the rest. The comments facility could be made much more useful if they could all be viewed on one screen, or even if they were edited into separate themes.

On another tack, I have recently found videos posted on YouTube helpful in understanding mathematical material. The reason is that the spoken word helps to relate the symbols to intuitions in a way which is difficult to achieve in writing. This prompted the idea that written articles could be posted with video or audio "footnotes".

And, on a more obvious level, articles need careful proof-reading, and editing for readability, aesthetic factors and conformity with web search facilities like Google and Google Scholar.

In an ideal world, authors would have benefit of services to edit their work, post it in an attractive and accessible form, and to include useful facilities for commenting, linking to data sets and so on. At the moment authors are at the mercy of their journals, but in the freer world envisaged here, there are likely to be organizations offering services like these.

### **Non-peer review and cross-fertilization between disciplines**

Like non-fiction and non-vegetarian food in India, non-peer review might turn out to be a broader and more important category of review than it may initially seem. The basic case for non-peer review is outlined [above](#): peer review may degenerate into cliques of academics reviewing their own work by criteria that are of no relevance to anyone else. (It may be that these disciplines are not interested in being of wider relevance: in this case there is no problem!) A bit of competition between different measures of academic quality might be useful.

There are different categories of non-peers. Review from the perspective of lay readers is one possibility. Other possibilities are review from the perspective of neighbouring or competing disciplines, review from the perspective of relevant techniques such as statistical techniques, and so on. There are many possibilities and it is not helpful to categorise them rigidly. Reviews from the perspective of another discipline should help to encourage the transfer of useful ideas across the boundaries of disciplines and sub-disciplines - something which the present system does not encourage. Some non-peer reviews might legitimately be regarded as less rigorous than peer reviews, but others might apply more rigorous criteria.

Statistics itself provides an interesting example. Firstly, statistical review is probably relevant to the vast majority of research claiming empirical results. Secondly, statistics itself may benefit from an external perspective. The subject is increasingly mathematical and riven by subtle philosophical distinctions (e.g.

the distinction between confidence and probability), and contributions which are likely to be accepted have to use the language of mathematics and take note of these philosophical distinctions. Both of these are unfortunate. Computer simulation methods (e.g. resampling and bootstrapping) provide an alternative to mathematics for most statistical tasks, but the stranglehold of the mathematical view is so powerful that these opportunities are very rarely taken (see for example Simon, undated, Chapter VI-2: <http://tinyurl.com/k9nh7yr>). Similarly the complexity of the philosophical basis leads to the widespread use of null hypothesis testing in contexts where it makes little sense (e.g. Cohen, 1994: <http://tinyurl.com/d2vqhqk>, and the blog at <http://tinyurl.com/dyyzawc>). What is needed is a simpler philosophical basis. However, articles proposing heretical views are unlikely to be reviewed favourably by statistical peers: what is needed is non-peer review.

### **Reviewing the literature and designing articles for a wider readership**

There are two basic problems for people searching and using the academic literature. First, there's too much of it and no foolproof way of finding what is relevant. Second, when suitable articles have been found, readers may lack the necessary academic background, or simply not have time, to absorb them adequately. Both of these deserve to be taken very seriously.

There are many ways of tracking down relevant articles and other types of scholarly output, and the suggestions above are unlikely to change these in any fundamental way. However, it does seem worth acknowledging that, for articles like the one I am writing now there is simply too much literature out there for any one person to absorb and review, particularly when one remembers tangential fields like epistemology, human computer interaction and history and philosophy of science, to name but a few. Any review which seems complete only does so by citing the conventional sources cited by similar articles. I cannot possibly review and cite all relevant works in this article. I cannot even read more than a small fraction of them. So what should I read, and what should I review in this article?

There is no easy answer to the what should I read problem. Focussing on articles which review some of the literature is one possibility but these are also likely to be incomplete, and I may not want to follow the authors' biases. Another interesting possibility is to read articles at random so that my work will be based on a different subset of the literature to other work and may add something for this reason. This is however, difficult to do with Google Scholar which ranks the results of searches using an unpublished algorithm which weights citations heavily (Beel and Gipp, 2009: <http://tinyurl.com/lcrx49v>), and so the articles I find are likely to be those other workers have already found and cited. (This manipulation of the information available may be a serious impediment to the free working of the knowledge market.)

The implications for what I should write in this article, and, more generally, for useful types of article, are rather clearer. I would propose the following principles for scholarly articles (and similar principles may apply to other types of research output):

- 1 The content and presentation of the article should be as simple, short and accessible as is possible without sacrificing the message (Wood, 2002: <http://tinyurl.com/pq4ya5h>) . If one type of article takes twice as long to read and understand as a second type, then readers will be able to read twice as many of the second type. This principle is very important: readers

- are likely to take matters into their own hands with long articles and just read, perhaps, the abstract and conclusions.
- 2 As far as possible, readers should be helped with technical concepts, or background knowledge, by means of footnotes, appendices or links to other sources. These avoid the problem of the article becoming too cumbersome for those without the necessary background.
  - 3 Articles should not normally seek to review the literature except in very specific fields and on issues very closely related to the topic of the research, but should contain links and citations to other sources which would be useful to interested readers. Unnecessary citations (perhaps to things which are essentially obvious or widely known) should be avoided. In many cases, a complete literature review is impossible and will probably just repeat what has been written elsewhere.
  - 4 Where there is an open access version of a cited source, a short link should be inserted in the text so that readers can access it with a single click. Saving the reader time should be a priority.

I have tried to adhere to these principles in this article. On a similar theme the Open Journal Project in Australia (<http://www.openjournalproject.org/>) is seeking to make the latest scientific research accessible to those without advanced scientific expertise.

(It is worth pointing out that as citation counts are a standard measure of the impact of an article, citations are often inserted for reasons other than helping the reader. If (3) were taken seriously, the number of citations may be fewer than other papers which may not help the author's citation count, either because of an implicit or explicit "you cite me if I cite you" deal, or because authors base some of their reading on the "cited by" button on Google Scholar.)

## **Conclusions**

I have discussed some suggestions to make the academic knowledge market more efficient. At the moment, if I write an article on, say, the effectiveness of a new diet, I need to decide which peer reviewed journal to submit it to, make sure it conforms to the requirements of the chosen journal, send it off, wait for and react to the comments of the editor and reviewers, and then if it is rejected send it to another journal, and so on. This may take a long time, and the paper may get reviewed only from the perspective of the one journal - and it may be far from clear what sort of checks have been carried out

Under the alternative system proposed here, I would post the article, or other research output, on a public website. Then I might register it with an organisation which lists urls, and then I might get it reviewed from the statistical point of view by one certification service, from the medical point of view by another, and from the social science point of view by a third. Another organization might produce a one paragraph summary in layman's terms of the status and credibility of my paper, assuming of course that it met their minimum standards - they might insist on, and draw on, reviews from the previous three bodies. Some quality stamps might depend on others: for example a medical reviewing service might insist on certification by a statistical reviewing service.

This system would have many advantages in terms of distributing articles and other artefacts quickly and efficiently, and responding to comments and requirements from a range of stakeholders - not just the "peers" in each discipline who control the present peer reviewed journals. It would also enhance the visibility and status of critiques of research. Instead of just knowing that a paper has been "accepted" by a journal, we would also know what comments and criticisms have been made and by whom.

In practice, at present, many research outputs are freely available on the web. However, many are not so available, and even if an open access copy is posted, the primary publication outlet is the journal. The suggestion in this paper is that authors should consider an open access repository as being their primary outlet. Such repositories do exist, but they are subject specific, and many like [arXiv.org](http://arxiv.org) or [www.ssrn.com](http://www.ssrn.com) insist on putting articles into specific categories which is unhelpful for articles which straddle disciplines. Using labels, rather than hierarchical categories (like gmail, which uses labels rather than folders), would make such repositories more flexible.

The second suggestion, the development of reviewing organizations, has happened to only a very limited extent. This is a pity. I would like to encourage readers of this article to post reviews of research outputs on the web (e.g. <http://tinyurl.com/p3owodv> and <http://tinyurl.com/qfsh6df>), and to consider forming organizations to do this in a systematic way.

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## **Appendix 1: A few stories to illustrate problems with the current system**

### **The unreliability of peer review**

If different reviewers from the pool of expert peers give different recommendations, then accepting the verdict of two or three of them makes the process into little more than a lottery. The two stories below illustrate this problem.

Peters and Ceci (1982) resubmitted 12 articles to the psychology journals which had published them 18 to 32 months previously, after changing the names of the authors and institutions and a few other minor details. Only three (25%) of the articles were recognised, and eight of the remaining nine were rejected by the same journals that had originally published them!

I first got into research on peer review when I asked a colleague who was organising a conference if I could have a look at the results of the peer reviews of the submitted papers (Wood et al, 2004). Each of the 58 papers was reviewed by two reviewers. If we divide the reviews into two categories -- good and bad - 50% (29) of the papers received one good and one bad review. This percentage represents the proportion of paper where the reviewers did not agree; ideally the conference organizer would have preferred this percentage to be much lower. Overall 69 of the 116 reviews were good, and the rest bad. If these good or bad evaluations had been awarded to the papers at random the disagreement rate would have been 48%. In other words, the disagreement rate between individual reviewers of the same paper was slightly greater than it would have been if the process were entirely random. The reviewing process was, almost literally, a lottery. According to the conference organiser, one of the participants in the conference was decidedly unhappy because his paper had been rejected following two bad reviews. If the conference organizer had done this analysis at the time he would surely have had less confidence in rejecting this paper!

The obvious response to this problem is to make the process more open (so that readers can see the basis and source of reviewers' judgments), and to publish, not reject, papers if there is any doubt (which is facilitated by the lack space constraints on web-based systems).

### **The consequences of erroneous decisions**

Sometimes articles are published when they probably shouldn't have been. These range from accidental errors, to systematic bias and fraud. One high profile example concerns an article published in the medical journal *The Lancet* in 1998 that

"lent support to the subsequently discredited theory that colitis and autism spectrum disorders could be caused by the combined measles, mumps and rubella (MMR) vaccine. ... Investigations by Sunday Times journalist Brian Deer revealed that Wakefield [the author] had multiple undeclared conflicts of interest,

had manipulated evidence, and had broken other ethical codes. The Lancet paper was partially retracted in 2004 and fully retracted in 2010 .... In 2011, Deer provided further information on Wakefield's improper research practices to the British medical journal, BMJ, which in a signed editorial described the original paper as fraudulent. The scientific consensus is that no evidence links the vaccine to the development of autism, and that the vaccine's benefits greatly outweigh its risks" ([http://en.wikipedia.org/wiki/MMR\\_vaccine\\_controversy](http://en.wikipedia.org/wiki/MMR_vaccine_controversy) accessed on 3 September 2013).

The problem of articles like this is exacerbated by the fact that corrections may take a long time to be published (more than 10 years in this case), and that readers may not notice them when they are published because the original article, and the paper version of the journal in which was published, cannot be changed. With less serious errors, corrections may not be published and the original error, legitimized by publication in a prestigious journal, may persist and even be cited as an example of how things should be done. Nosek and Bar-Anan (2012: 237) give an example of two published articles in a high profile journal which, they say, had critical errors in their analysis. One of these articles was cited by an editor of another journal as an example of how the analysis should be done. They comment that "it is very difficult to get such an error out of the present system."

There is obviously no foolproof way of avoiding error in research, but if reviews are available on the web, and articles are updated as problems are found, readers would be much more likely to be aware of criticisms. If these reviews were crowd sourced, instead of relying on two or three reviewers selected by an editor, there is a much greater likelihood that errors like this would be found.

The opposite problem, of course, occurs when an article is rejected when it perhaps should have been published. Horrobin (1990: <http://tinyurl.com/kzggqgu>) outlines several such examples from the biomedical sciences.

## **Appendix 2: A very crude model of the speed of intellectual evolution**

To give some idea of the potential impact of some of the ideas in this paper, I have constructed a very crude probabilistic model dealing with the speed of intellectual evolution. Let's suppose that groundbreaking research in a particular field is produced, on average, every  $n$  years, and that the probability of the "movers and shakers" (M&Ss) of the domain noticing it is  $p$ . This means, for example, that if groundbreaking research is produced every five years ( $n = 5$ ) but only one in ten of these innovations are noticed ( $p = 0.1$ ), then the mean time to the next innovation which is noticed is 50 years and the 95th percentile is about 150 years (the first result is fairly obvious and the second is derived from the simulation at <http://woodm.myweb.port.ac.uk/jrpnpxlsx>). If half of innovations are noticed ( $p=0.5$ ) the corresponding figures are 10 and about 30: the latter figure indicates that there is 5% chance that no innovation will be noticed within 30 years.

The model is obviously very crude<sup>6</sup> but it does illustrate the key importance of the two quantities involved. It is possible for example, that another key innovation made at the time of Einstein's 1905

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<sup>6</sup> What counts as an innovation? Innovations might not be a random process as we are assuming. The M&Ss might notice only some aspects of innovations. The whole context will have changed after 150 years. And so on - the model ignores all such fuzzy areas.

paper was missed and the subsequent development of physics was very different from how it might have been.

How can we improve the values of two key figures in the model? The probability of a piece groundbreaking research being noticed ( $p$ ) can be decomposed into the probability of publication ( $p_p$ ) multiplied by the probability of it being noticed ( $p_n$ ) once it's been published. The first probability ( $p_p$ ) should be as close to one as possible: the problem here is that of ensuring that the review system works as efficiently as possible and does not exclude useful innovations. The traditional way of improving the second probability ( $p_n$ ) is to reduce the size of the pool of published papers; the difficulty here is that will very probably lead to the exclusion of useful innovations and so we need to think about how scanning the literature can be made more efficient (see [above](#)).

The other variable is the frequency with which groundbreaking research is produced. I can envisage no appropriate, simple model to understand this, but one reasonable assumption is that innovations are fostered by exposure to a variety of types of research which in turn is more likely with the open, anarchic system of dissemination I am proposing here. The censorship which is likely with the traditional system is likely to have the effect of inhibiting the development of a "long tail" (Anderson, 2006) of infrequently read, but potentially important, research articles.

### **Appendix 3: The unit of research output**

The conventional unit of research output is the "paper" – a written report on research carried out. Papers are typically about 2-15,000 words, and include citations to other relevant work. There are different types of paper – review papers, short papers sometimes called letters, comments on other papers, etc – and the detailed conventions are different in different fields. However, they are all *written* documents, designed to be read by "*peers*", with a clear *publication date* after which the documents are not changed, and a named list of *authors*. There may be good reasons to reconsider all these features.

With the advent of the web other formats besides the written word are possible - videos, audio and pictures being the obvious examples, but there are other possibilities such as computer programs. Conventionally these are not used for presenting research but this may change. There is also the possibility of providing access to data files of various kinds.

Conventional scholarly papers are intended for peers in the discipline. In practice the readership may be wider than this - researchers in other disciplines may be interested, as might members of the general public. This has implications for the prior knowledge assumed by the authors of the paper. Many conventional papers may give more background than is necessary for true peers, but insufficient for outsiders (I discuss tactics for dealing with this [above](#)).

Conventional papers have a publication date after which the paper is not changed. Updates and corrections and comments may appear in later issues of the journal, but readers may not see them. Web based media, however, can incorporate updates, different versions, corrections and so on. This is obviously particularly welcome when important flaws are found in the research - readers of the paper can then be alerted to this (see [Appendix 1](#) for an example).

### **M. Wood: Journals, repositories, peer review, non-peer review ...**

It is also worth pointing out the importance of named authors in academia. If you buy a car, or have a meal in a restaurant, you usually have no idea about the identities of the creative team behind the product. If you read some research, however, the author is an important and prominent piece of information. Academic pride and careers depend on these authorships. This may lead to disputes, and to a failure to share data and ideas. If, in another parallel universe, authors of scholarly papers were anonymous figures in the background, would this make a difference to the progress of knowledge? It is impossible to know, but the success of Wikipedia suggests that something similar may be worth considering in academia (Rice, 2012: <http://tinyurl.com/ktnwaog>). Obviously there are important issues concerning intellectual property, copyright and patents, and the motivation of researchers here.

For these reasons, we should avoid the word paper with its implication that written papers are the only valid form of output: Priem and Hemminger (2012) suggest the term "scholarly object ... which can be anything from a dataset or annotation to an article or monograph—anything scholars produce that they want to share". This sounds a bit clumsy, but it can refer to any type of object -- including those with ongoing revisions, and those without a list of named authors. The term "research output" is another, similarly non-committal term. Obviously, when we are talking about an article in the old sense the term "paper" is acceptable, but when we are discussing things in general terms, then a general word is important to avoid restricting the discussion unnecessarily.