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Before the law: open access, quality control and the future of peer review

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- OA is not about abandoning peer review but it does provide the opportunity to rethink its role and our methods.
- 67% of existing OA journals do not charge APCs and yet academics have tended to steer clear of them.
- People opt for recognised outlets because of the (erroneously) perceived emphasis on publication venue by accreditation structures such as RAE/REF/tenure.
- In the print world peer review was historically linked to page limits; these do not apply in the electronic realm.
- Double blind review is a misnomer and even then preserved anonymity can be problematic.
- The alternative is to publish everything that meets a certain threshold of academic soundness and to let readers decide what should last; in effect a kind of post-publication, or peer-to-peer, review.
- This modification of peer review could lead to more collaboration and less insistence on an individual finished product.

Introduction

As Peter Suber notes in his commendable book, open access is not about circumventing peer review.¹ In fact, the *only* aims of the Open Access movement that can be stated with conviction are the removal of price barriers and the lowering of permission barriers.² These elements are to be achieved through an adaptation of scholarly publishing practice to the mutations in technology that allow for non-rivalrous commodity exchange of works upon which the authors do not rely for income; the ability to disseminate perfect copies of academic material to anybody who can get access to the Internet at an extremely reduced, even if nominally non-zero, cost.

Given that this is the case – and you will note that there is specifically no mention of peer review practice in the above definition – why is it necessary to debate peer review at all? I would suggest that there are two

reasons why peer review must not remain the unmentioned elephant in the room when we talk about open access. Firstly, it is a frequent accusation of detractors that the changes introduced by open access models will lead to a slippage of standards in one way or another, an aspect that must be dispelled. Secondly, these shifts in practice at the reader-side allow us the space to rethink peer review and to ask whether there are analogous changes, facilitated either socially or technologically, that could be worth exploring at this time of transition. In fact, just as one of the arguments for open access is that it is culturally elitist and untenable to presume that a broader audience can neither understand nor appreciate scholarship, there are, I would argue, parallels in peer review practice that could reflect this same principle on an intra-academy basis.

The primary reason behind this assertion is that, at a fundamental level, the gatekeeper model – that is, the system of deciding on permissibility before publication through both publisher policies and peer-review practice – also works on a series of unspoken ideological assumptions that are never wholly objective and apolitical, but rather based on a series of exclusions and marginalisations in exactly the same way as the elitism argument pre-defines its audience. Indeed, while the argument-by-elitism, as it could be called, contends that it requires tuition and process to navigate the labyrinth of scholarship and disregards John Willinsky’s comprehensive arguments for technical solutions to this, exemplified in the ‘reading tools’ component of his OJS software,³ what of the students who graduate from this process and who are then still financially excluded from it? Does not the argument-by-elitism purport to teach critical thinking but then deprives those taught of the material with which to critically engage once they leave the university? What about those institutions who cannot afford subscriptions but whose staff are perfectly capable of understanding scholarly research and its production contexts?

Likewise, however, within our own academic circles, a gatekeeper model pre-defines its audience and disregards a series of important questions. For example, how can we wholly know the value of the material that we are pre-excluding given that we exist within ideologies that are not always explicitly clear from our immanent positions? How do we know what *will*

be of value in the future? What do we make of the exclusions and other spaces that, under the gatekeeper model, we cannot even know at present? Without wanting to seem Panglossian, for moves to open access should not be naively utopian, if we mark some aspects of the move to open access as challenges, the arguments for open access also present new opportunities in the realm of peer review as in the counterbalanced increased access to readers.

Quality, prestige and labour

In order to understand the potential routes of the future, it is important to understand the future's roots in the present and the past. It is also important to note, up front, that I am dealing primarily here with the state of the Humanities as this is the area with which I am most familiar. Turning then to the current state of peer review, it is worth noting that, as it stands, in many disciplinary spheres, academic publishers believe themselves to be responsible for the quality of the academic material that appears within their titles. This has been the case traditionally and continues to be the norm in the open access sphere with SAGE Open declaring their journal an ideal venue for 'Authors who want their articles to receive quality reviews and efficient production'.⁴ What is worth exploring, as a preliminary rebuff to those who simplistically equate open access with a decline in standards, is the way in which prestige is actually formulated.

The first and most important aspect to grasp (and one that seems incredibly obvious once articulated) is that the gatekeeper model, in which material is pre-screened for worthiness, relies upon (almost always uncompensated) academic, not publisher, labour. Validation is performed through a hidden but nonetheless presumed process whereby academics confer acceptability upon the piece in question. In many of the Humanities and Social Science systems, this process is undertaken on a double blind basis, meaning that, in theory at least, neither the author nor the reviewer(s) are aware of each other's identities. In any case, though, the system that is erected here is one wherein academics cyclically confer prestige upon a journal twofold by submitting their

pieces to the venue that they believe to be the most prestigious and by reviewing with strict (even if unformulated) standards for those same destinations. Furthermore, reviewer selection is often the task of an academic editor who knows the field as, unsurprisingly, they are nearly always better placed to know the most appropriate reviewer than commercial publishers. In short: the gatekeeper process, from reviewer selection through to submission and review itself, all of which are the only parts of publishing that confer authority upon academic worthiness, are voluntarily undertaken by academics.

It should be clear, from the above, that there is, therefore, theoretically no reason why a Gold open access venue (which, remember, does *not* necessarily mean an 'author pays' article processing charge (APC), no matter how frequently less informed commentators uncritically repeat this assertion) could not accumulate substantial academic credibility, should it attract the prerequisite submissions and reviewers. The one caveat that I will add, however, is that it is imperative, if a journal does follow an APC model that review and ability to pay are strictly separated. This is not always guaranteed to be the case and aspects of an author pays model could lean towards unethical practices, as Jeffrey Beall's list of 'predatory open access publishers' demonstrates.⁵ Although I readily see the danger of review corruption in the APC model (of which I am not a fan in any case), two objections can be raised, however, to the restriction of this predatory mode to open access: 1) does not the 300% above inflation increase in journal subscription costs since 1986 smack of an overarchingly predatory field in the first place?⁶ 2) it is easy to spot these publishers through either Beall's criteria,⁷ or simply their lack of academic credentials, non-membership of publishing ethics organisations such as COPE or lack of explicit policies for separation of finance from review, just as it is in non-open access venues.

Given the freedom, to date, of academics to act as the king-makers of their publication venues, it is surprising, for an optimist, that open access venues have not fared better. After all, at present, 66.7% of open access journals in the Directory of Open Access Journals (DOAJ) do not implement APCs, so they carry no financial cost either to publish in or to

obtain.⁸ Indeed, despite the inability of university library budgets to keep pace with subscriptions, academics have, for the most part, continued to invest their academic capital in traditional (and expensive) journals, thus perpetuating these venues' prestige (and cost). There are, however, other mechanisms that have stifled the uptake of open access that relate to quality control, the most notable of which for the UK is the 'peer review of peer reviews' that is the Research Excellence Framework (REF) and its Research Assessment Exercise (RAE) precursor. The repeated assertion of REF panel members that publication destination will *not* be used as a criterion has often been treated with scepticism by universities and researchers. This has led, in conjunction with hiring and firing procedures, to a strongly conservative disciplinary mechanism that, itself, in part, restricts academic freedom; researchers publish where they feel will do the most for their REF return (regardless of the truth of such statements) or employability and, in many cases, fledgling open access journals are not believed to fulfil these (imaginary) criteria.⁹ While the current consultation by the Higher Education Funding Council for England looks set to mandate Gold/Green open access for any post-2014 REF, the fact that this is being considered after the Finch *fait accompli* of making Gold open access synonymous with APCs means that the moment for radical (and beneficial) economic transformation through academic agency in conferring validation through peer review has, once more, almost passed.

The current problems of peer review

Leaving aside now the issues of whether open access must intrinsically disregard, or experiment with, peer review (there is no reason why it should, in either case), I want now to turn to an analysis of the current workings of peer review within Humanities and Social Sciences disciplines, the potential pitfalls of the extant systems and to give some examples and suggestions for ways in which the system might be re-worked. It is worth stressing that I believe that there is no necessary causal need for open access to tinker with peer review but I wish to also state that the born-digital medium of open access publications may lend itself to new modes that were impossible under the model of its print predecessor.

In order to think through peer review in the present moment, it is necessary to briefly lay out the mechanism that propelled the gatekeeper model to dominance: print economics. Historically, one of the key functions of the gatekeeper has been to reduce the quantity of permissible material. This was not only an effort to avert what we now call 'information overload' and what are perceived as low standards, but also because each issue of a print journal had a specified page budget. In the world of print and physical commodities, there is a need to restrict the quantity of output because there is a material cost for each page that is printed and distributed. This is, clearly, no longer the case but persists through a culture that Gary Hall calls our 'paper-centrism'.¹⁰

While, therefore, we most often like to think of peer review via the gatekeeper as an issue that pertains strictly to academic standards, there is also an economic history of which it is easy to lose sight. This, though, is not the sole problem of the gatekeeper system, especially as it applies to the double blind review system in many Humanities disciplines. Indeed, the first question that springs to mind is whether it is right that a mere two academics, in most instances, although sometimes only one, have the private, unaccountable, final word on an article's acceptability. For Early Career Researchers (ECRs) this private decision can be the difference between a lifetime of employment in academia or a lengthy period of re-training. Furthermore, to repeat, looking outside the academy briefly, one of the arguments made against open access is that there may be no need for public access to scholarship; perhaps, it is claimed, the public won't understand or value our contributions. The problem with this argument, again citing Peter Suber, is the question of how anyone can 'know in advance the level of demand for peer-reviewed work among lay readers'.¹¹ The same argument can be made for an 'informed' audience, though. How can one accurately pre-judge, within one's own temporal, geographical and disciplinary immanence, what may be of worth to scholars free of these constraints? This lack of accountability and, as will be explored below, logic in the admissibility of papers is a problem that is exacerbated by the traditional double-blind system.

Taking a slightly different tack first, however, it is worth querying the exact extent to which the double blind method might actually be a misnomer. Theoretically, the author should be unaware of the identity of his or her reviewers and *vice versa*. The benefits of this are easy to articulate: it is designed to encourage an impartial assessment of the work, rather than the author. Furthermore, reviewers are supposed to be protected from professional repercussions in cases where, for instance, the author is a prominent figure in their field. Often, however, this is utopian. In many small fields where work may have been presented in early versions at conferences, where authors are known for adopting a specific stance, or simply through flawed meta-data erasure and/or slips of self-citation, the identity of the author can be ascertained. While it is less often that slips occur the other way around, it is often possible to guess the most likely reviewer of one's work simply by dint of their expertise.

Furthermore, anonymity can be problematic. The lack of accountability of reviewers can lead to harsh, penalising reviews, rather than feedback that, while rigorous, intends to work in community to elevate a work to a publishable standard. Additionally, there is also something strange about the perseverance of anonymity after publication. Universities and academia function, as Martin McQuillan put it to me, on genealogies of validation; that is, on hierarchies of prestige that trace the flow of academic 'capital' and authority through publications. As explored above, journals are only as valuable as the genealogies that validate their work as high quality, through submission quantity/quality and rejection rate, underpinned by the labour of peer review. However, in the current way of working, what remains is a situation where, instead of the process of review being *visible* in order to *validate* the work, the quality of the review process and the prestige of the people doing the review must be inferred from the perceived post-publication quality of the publication.

To rephrase this: there are, under current practice, only two ways, both flawed, in which the quality of the review can be ascertained. The first of these is through trust in nominal journal brand. While there are some good practical arguments for this (i.e. when a journal continually publishes good material, then it's probable that their review process is solid) there are also

some problems, most notably that a 'journal' seems too wide a measure of quality. As aspects of the journal change (for instance, editor resignation, editorial board changes, financial problems), the quality of output could decline but awareness of this will always be outdated as it takes a long while for a drop in quality to register in the general perception of scholars. During this period of unawareness, the journal (based on historical prestige precedent) would continue to attract high quality submissions and would, therefore, find it easier to recover; just one further instance that demonstrates the way in which academic prestige is a top-loaded, non-trickle-down economy.

The second way in which journal quality is crudely measured and the one that surely most affects scholars' perceptions lies in the duplication of labour when reading a paper; a type of second review in which academics bring their own evaluative skills to bear on already published work. Clearly, this is inherent in the act of reading an academic paper but the blame for poor quality is put down to either the author or to the journal brand. This is interesting; what seems to have failed is actually the peer review, gatekeeping function, but this is not, in a mode of journal brand, the way in which it is perceived. While in some ways this is a fair appraisal, there could be ways in which the journal could signal the degree of delegation and trust that has been relied upon and to which I will now turn my attention.

What is to be done?

The most obvious way in which we might begin to address these problems at the moment of transition to open access is to rethink anonymity in the review process, as has already happened in many scientific disciplines. However, it is worth saying up front that each of the various combinations of the review anonymity matrix comes with its own problems and it may be the case that none are, in the end, as satisfactory as blind review, except, perhaps, for at least being more honest about the potential flaws. The first of these potential changes would be to remove the author's anonymity while maintaining the anonymity of the reviewers, which seems to

add very little. Reviewers could judge solely on the past reputation of the author, rather than the merit of the piece alone while remaining unaccountable for their actions.

Conversely, we could take the opposite stance and remove reviewer anonymity (at various stages in the process, but primarily after the review and regardless of outcome) while retaining the author's veil. This mode brings absolute accountability upon reviewers while protecting the author from pre-judgements. It also gives a clear genealogy of validation and militates against corruption to some degree as any conflicts of interest would be immediately clear. The disadvantages of this approach are also obvious, though. Any system that brings unbalanced extreme accountability will result in a conservative situation of strict, normative appraisals, thereby potentially ruling out a whole body of useful work that may be barred by the gatekeeper. While some may see this as an advantage – a tightening of review standards – given the historical parallel to page budgets and evolutions in social and technological filtering processes (see below), the argument for this may be less solid than might be thought. Finally, although this approach in some ways helps spot corruption through transparency, the extreme burden to 'make the right call' could encourage reviewers to seek the author's identity. This tactic exposes reviewers and makes a thankless task perhaps even more risky.

What, then, about completely removing all anonymity from the process? There are some advantages in this case (as outlined above) but there still remains no counterbalance to the elements of conservatism that could arise as a result of exposing reviewers. Conversely, reviewers would surely also be prone to appraise the authors' identity in this case.

Evidently, in each of the cases where anonymity is removed, during the review process itself, there are problems that seem, to some degree, worse than the flaws in a double blind setup. However, this only applies when we assume that we are dealing with a gatekeeper model in which a paper only sees the light of day so that the journal may be associated with the most exclusive papers in order to protect its brand. Other, more radical,

experiments in the sciences have worked to change this. For instance, the review criteria of PLOS ONE reads as follows:

Too often a journal's decision to publish a paper is dominated by what the Editor/s think is interesting and will gain greater readership – both of which are subjective judgments and led to decisions which are frustrating and delay the publication of your work. PLOS ONE will rigorously peer review your submissions and publish all papers that are judged to be technically sound. Judgments about the importance of any particular paper are then made after publication by the readership (who are the most qualified to determine what is of interest to them).¹²

At first, in a knee-jerk reaction, this standard of publishing all papers that are 'technically sound' appears to have no analogue in the Humanities. As a hypothesis, though, a 'technically sound' paper in the Humanities could evince an argument, make reference to the appropriate range of extant scholarly literature, it could be written in good, standard prose of an appropriate register that demonstrates a coherence of form and content, it could show a good awareness of the field within which it was situated, it could pre-empt criticisms of its own methodology or argument and it would be logically consistent. While this is just a cursory stab at a definition and not meant to be finalised, implemented criteria, many of the problems of the review system as it stands could certainly be addressed through the formation of explicit consensus as to what constitutes an acceptable barrier to entry in the Humanities, so as to remove the Kafka-esque situation from which this paper takes its name: at present it can seem as though we each have our own personal gatekeeper with impenetrable logic.

Secondly, though, the inversion that PLOS ONE effects upon the original goal of the *Philosophical Transactions of the Royal Society* to assess on the 'importance or singularity of their subjects' could leave it open, as was the *Transactions*, to John Hill's 1751 critique of the inclusion of 'trivial and downright foolish articles'.¹³ The difference in situation to the contemporary, however, lies in the economic situation and technological filters at our disposal. In 2013, we have sophisticated full-text and social

search mechanisms that can bury unpopular material on the furthest pages of results but without removing such items from the economy altogether. The advantage of this, as with the arguments for open access more generally predicated upon an anti-elitism, is that we dismiss our own arrogance of knowing what will be important for all time and replace it twofold with the mechanism to ensure that what is relevant now is found and valued while also allowing those papers in niche fields or in areas that have yet to gain any prominence to be found, *if and only if* the seeker desires. In this mode of post-publication review, everything will be assessed, but it will be done after the fact and the exclusion of material will not be a permanent pre-silencing, but rather a process of continuous community consensus. Of course, there is no guarantee that the peer-review criterion of ‘technical soundness’, however translated, will be free of abuse in itself, but this could be a step in the right direction.

This raises an aspect that I’ve left until the close of this piece to explicitly articulate under the bipartite logic both that it is Kathleen Fitzpatrick who deserves the most honourable and prestigious place on the topic, but also because it closes the loop of necessity of reform alongside technological innovation with which I began. In her seminal book on the subject, *Planned Obsolescence*, Fitzpatrick systematically interrogates Humanities’ peer review practices in the age of the digital and concludes that we require a mode that is less certain of the merits of ‘the stability that we’ve long assumed in the print universe’ and one that is more adaptive to generative possibilities.¹⁴ What Fitzpatrick addresses, in essence, is the problem of the fundamentally anti-collaborative nature of Humanities research in most cases. At present, review is not usually a community endeavour but rather an activity that expects to see a final artefact in which no traces of the construction remain visible. Experiments such as McKenzie Wark’s collaboration with the Institute for the Future of the Book on his 2007 *Gamer Theory* suggest, however, that while an online collaborative model currently solicits sub-optimal levels of participation, there can be merit in the process.¹⁵ Most importantly, though, I want to use my final words to reiterate, but modify, my opening gambit. Fitzpatrick astutely notes that, in this case (and others), ‘the system that needs the most careful engineering is less technical than it is social’.¹⁶ Bearing this in mind, we must be careful

never to succumb at any point to a techno-fetishism but always consider whether the technological facilitates desirable social changes. We have built, over many years, systems for appraising the individual rather than acknowledging the way in which knowledge is collaboratively produced and, for the first time in many years, we may have an opening through which to address this. Open access does not *require* us to change our peer-review practices any more than the codex meant that readers *had* to abandon their palaeographic antecedents. There might, however, be practical ways in which a moment of technological change could enable us to see, with apologies for inverting Churchill's well-known aphorism, that perhaps our review practices are not so wholly democratic, not so entirely objective, fair, or community-based; that they may not be the best that have been tried, apart from all the others.

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Notes

- 1 Peter Suber, *Open Access*, MIT Press Essential Knowledge Series (Cambridge, Mass: MIT Press, 2012), pp. 20–21.
- 2 Throughout this piece I am using the term 'open access' to denote the principles and reserving the capitalised 'Open Access' for the movement calling for/demanding/advocating on behalf of those precepts.

- 3 John Willinsky, *The Access Principle: The Case for Open Access to Research and Scholarship*, Digital Libraries and Electronic Publishing (Cambridge, Mass: MIT Press, 2006), pp. 160–168.
- 4 Sage Publishing, 'SAGE Open: About the Title', SAGE, www.sagepub.com/journals/Journal202037?prodId=Journal202037&ct_p=manuscriptSubmission (accessed 6 May 2013).
- 5 Jeffrey Beall, 'List of Publishers', *Scholarly Open Access*, <http://scholarlyoa.com/publishers> (accessed 6 May 2013).
- 6 This figure is a trans-disciplinary breakdown from the ARL and may not reflect the reality solely within a Humanities environment. However, while most Humanities journals have a far lower absolute cost than many of their STEM counterparts, Library Journal's 2013 'Periodicals Price Survey' seems to show an analogous proportional increase. Stephen Bosch and Kittie Henderson, 'Periodicals Price Survey 2013', *Library Journal*, 2013, lj.libraryjournal.com/2013/04/publishing/the-winds-of-change-periodicals-price-survey-2013 (accessed 6 May 2013).
- 7 scholarlyoa.com/2012/11/30/criteria-for-determining-predatory-open-access-publishers-2nd-edition
- 8 'Journals by Publication Charges', *DOAJ: Directory of Open Access Journals*, www.doaj.org/doaj?func=byPublicationFee&uiLanguage=en (accessed 6 May 2013).
- 9 An interesting further aspect, which sadly I did not have space to consider here, would have been the methodological practice of meta-review frameworks, such as the REF; could, for instance, publication venue be shielded from REF panel members so as to allay researcher fears?
- 10 Gary Hall, *Digitize This Book!: The Politics of New Media, or Why We Need Open Access Now*, (Minneapolis: University of Minnesota Press, 2008), pp. 59–61.
- 11 Suber, p. 117.
- 12 PLOS, 'PLOS ONE Journal Information', www.plosone.org/static/information (accessed 6 May 2013).
- 13 Willinsky, p. 203.
- 14 Kathleen Fitzpatrick, *Planned Obsolescence: Publishing, Technology, and the Future of the Academy*, (New York: New York University Press, 2011), p. 196.
- 15 Fitzpatrick, pp. 110–112, 192.
- 16 Fitzpatrick, p. 192.