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Peer Review May Not Be Such a Bad Idea:

Response to Heesen and Bright

Darrell P. Rowbottom

darrellrowbottom@ln.edu.hk

Abstract

In a recent paper in BJPS, Heesen and Bright argue that prepublication peer review should be abolished and replaced with postpublication peer review (provided the matter is judged purely on epistemic grounds). In this paper, I show that there are three problems with their argument. First, it fails to consider the epistemic cost of implementing the change to postpublication peer review. Second, it fails to consider some potential epistemic benefits of prepublication peer review, which involve avoiding bias. Third, it fails to consider some potential epistemic disadvantages of postpublication peer review, which stem from the greater number of papers that would be published under that system.

1 Introduction

Heesen and Bright ([2019], p. 1) state that 'prepublication peer review should be abolished' if epistemic consequentialism is assumed and non-epistemic issues are put to one side.[1] This provocative claim will seem highly implausible to many academics, judging by survey results such as those reported by Ware ([2008]). However, Heesen and Bright ([2019]) argue for it on the basis of a thorough empirically-minded survey of the literature on peer review, across several disciplines, while paying close attention to several ways in which the abolition of prepublication peer review (and its replacement with postpublication peer review) would change science and external engagement therewith. Their argument is therefore worthy of serious examination, given the considerable time and effort presently devoted to prepublication peer review.

In this response, I argue three things. First, even assuming the truth of its premises, Heesen and Bright's argument does not provide support for the conclusion that 'prepublication peer review should be abolished' ([2019], p. 1) because it fails to consider the epistemic costs involved in implementation. Their argument does, however, provide support for the more moderate conclusion that additional empirical research on peer review should be conducted. Second, there are potential benefits of prepublication peer review—significant with respect to avoiding the effects of bias—that Heesen and Bright do not consider. Third, and finally, there are potential problems with postpublication review taking the place of prepublication review that Heesen and Bright don't consider. In particular, there is a significant possibility that abolishing prepublication review would lead to a sharp increase in the rate of publication. This would likely have several negative consequences. For example, it would probably result in postpublication review processes becoming at least as costly, in time and effort, as their forerunners (if they were performed thoroughly).

[1] Further page number references concern the 'final draft' version of Heesen and Bright's paper published online, unless otherwise specified.

Note that I write of 'potential benefits' and 'potential problems' above, in presenting the last two points that I shall argue for. They are only potential because more empirical work is needed on several of the pertinent issues, such as the extent to which non-blind review processes introduce bias; indeed, Heesen and Bright ([2019]) emphasize the need for more such work. Thus, my intent is to identify pertinent empirical hypotheses that haven't been adequately explored (or considered by Heesen and Bright) and provide reasons for thinking that these are true. In short, that's to say, I present relevant empirical hypotheses that have reasonably high priors on the reasons that I adduce and on the current evidence (which I present, where possible, even when it is relatively weak).

2 Heesen and Bright's Argument and Implementation Cost

Heesen and Bright's argument is rather complex and subtle. If I were forced to summarize it, I would do so as follows. It compares two alternatives—prepublication and postpublication peer review—with respect to merit on a number of epistemic desiderata. It concludes that the latter would be preferable to the former, from the point of view of epistemic consequentialism, on the evidence we currently have. However, this summary is useful, at best, to give an orientation point for a more detailed exposition of their argument. I will start with such an exposition and include some initial critical analysis where appropriate.

The first aspect of the argument to appreciate is its comparative character: 'our primary aim is to evaluate the current system. However, we believe that is only really possible by comparing it to an alternative' (Heesen and Bright [2019], p. 5). The 'current system' referred to is the familiar one in which papers—such as journal articles or discussion notes, as opposed to proposals for grants or conferences—cannot be published without undergoing peer review and being judged to have 'passed' (Heesen and Bright [2019], p. 3) by at least one editor. (Given the relative familiarity of this system, I will not say anything more about it at this juncture.) The alternative—their counterproposal to prepublication peer review—is postpublication peer review, which they outline as follows:

> Scientists themselves will decide when their work is ready for sharing. When this happens, they [will] publish their work online on something that looks like a preprint archive… Authors can subsequently publish updated versions that reply to questions and comments from other scientists, which may have been provided publicly or privately. The business of journals will be to create curated collections of previously published articles. Their process for creating these collections will involve (postpublication) peer review, insofar as they currently use prepublication review. (Heesen and Bright [2019], p. 6)

The basic idea behind this proposal is clear. First, however, it is somewhat vague as it stands. Postpublication peer review could take many different forms, some of which have probably not even been conceived of, and it is therefore difficult to reach any firm conclusions about the epistemic desirability of implementing it (rather than implementing a specific, well-articulated, form thereof).[2] Second, the 'current system' involves a variety of practices, some of which seem highly similar to postpublication peer review, as Heesen and Bright define it, while still not counting as such.[3] So care should be taken to ensure that these current but

[2] For example, Heesen and Bright do not specify a mechanism by which published articles will be considered for inclusion in journals. But different mechanisms will require different amounts of academic labour, and will result in different biases, *inter alia*. The importance of this will become evident in the subsequent discussion.

[3] Heesen and Bright ([2019], p. 6) acknowledge the variety of existing practices and claim of the status quo only that: 'The *vast majority* of scientific work is shared through journal publications, and

atypical practices are carefully considered. One particularly interesting example is the interactive public peer review system employed by *Atmospheric Chemistry and Physics*. This is described, on the journal's website, as follows:

> In the first stage, papers that pass a rapid access peer review are immediately posted on the Atmospheric Chemistry and Physics Discussions (ACPD) website. They are then subject to interactive public discussion, during which the referees' comments (anonymous or attributed), additional short comments by other members of the scientific community (attributed), and the authors' replies are also posted in ACPD. In the second stage, the peer-review process is completed and, if accepted, the final revised papers are published in ACP. To ensure publication precedence for authors, and to provide a lasting record of scientific discussion, ACPD and ACP are both ISSN-registered, permanently archived, and fully citable.[4]

This is an interesting approach because it is similar to Heesen and Bright's proposal in significant respects (albeit on a smaller scale). Plausibly, due to its ISSN-registration (and 'permanent' citability), publication in ACPD is easy to achieve. (In the first 'access' stage, the responsible editor only evaluates whether the manuscript is 'within the scope of the journal and whether it meets a basic scientific quality'. Only minor 'technical corrections', such as typographical alterations, may be suggested at this point. Anonymous peer reviews

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the *vast majority* of journals uses some form of prepublication peer review. *Ordinarily* this means that an editor assign one to three peers … who provide a report … The peer reviews feed into the final judgement…' [emphasis mine].

[4] Quotation from: https://www.atmospheric-chemistry-and-physics.net/about/aims_and_scope.html

are not solicited, although independent opinions might be.[5]) Only publication in ACP, the journal, requires passing peer review in an editor's view.

However, Heesen and Bright's ([2019]) postpublication peer review proposal explicitly rules out access review: 'Scientists themselves will … publish their work online' (p. 6). Yet access review has a potential advantage that they do not consider. Specifically, it may be desirable to ensure that repositories do not become dumping grounds for self-styled scientists, which they might, at least in the short-term, given the prestige that is currently attached to publication. If scientists have to trawl through irrelevant material (and spend time personally assessing flawed material) when searching an archive for work on a specific topic, their efficiency will be impaired.[6] (And as I will discuss later, in section three, using search

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[5] See the description of the 'access stage' at: https://www.atmospheric-chemistry-and-physics.net/peer_review/interactive_review_process.html

[6] One referee raised the issue of whether current archives could be characterized as 'dumping grounds'. This is a matter on which much more research is required. However, I did take a look at academia.edu in order to form an impression. First, I searched for material contributed by 'independent researchers'. In the philosophy section, there were 320 documents; many were of poor quality. The physics section contained a lot of material from only a small number of cranks. Second, I browsed through the philosophy section and selected work from people I hadn't heard of. I found several instances of individuals claiming affiliations when they were undergraduate students or ex-students; I also found several undergraduate essays, or pieces published in undergraduate journals (which were typically not of a high enough standard to be published in good professional journals). My brief exploration was no substitute for a systematic investigation (although this would likely be very time consuming, as there's no obvious heuristic that can be used to isolate poor quality material). Even granting that there's not much irrelevant material on such archives at present, however, there's also not presently much prestige involved in depositing material there (since it doesn't count as published).

heuristics or 'following' functions may have other disadvantages.) This is a key reason why an access review process is useful. Moreover, the obvious ways to replace it—say, by restricting publication to verified academic staff of universities or to verified possessors of PhDs—may be too restrictive. Not all independent scholars are cranks, and occasionally an undergraduate student has something of worth to contribute. Thus it is not clear how something fulfilling the function of access review could be profitably obviated, and whether any satisfactory alternative would prove any less costly, in terms of time and effort. Indeed, the amount of work involved in ACP's access review is minimal in comparison with that involved in standard peer review (or the rest of their review process). It's also limited to a small fraction of scientists, who voluntarily perform editorial roles, for the most part. The opportunity cost involved, at the level of science rather than a small number of scientists, is of little significance.

So far, to summarize, we have seen that Heesen and Bright's argument involves a comparison between the typical way in which peer review is conducted and a proposed ('postpublication') way in which it might instead be conducted. We have also seen that a key difficulty in making an informed judgement about the relative merits of these approaches is that the proposed ('postpublication') way has a significant element of vagueness. Different ways of putting flesh on the bones of the proposal—say, of ensuring that scientists can publish on their own behalf—will result in different advantages and disadvantages, which could potentially affect its relative desirability (for example, with respect to the time and effort required). We have also seen that there are other tried and tested atypical ways of approaching publication—such as the approach adopted by ACP—that might be worth considering as alternatives.

I shall now return to the exposition of Heesen and Bright's argument. How does it compare the two types of review process it concerns? On a variety of epistemically significant factors, which I won't enumerate, such as: how easily new scientific results can be shared, the degree to which scientists are free to choose how to spend their own work time, the amount of vetting power given to 'gatekeepers' such as editors, and the probability of malpractice being detected. They argue not only that the postpublication review approach would be superior to the prepublication one when judged on several of those factors in isolation, but also that the former would be no worse than the latter when judged on the remainder of those factors. But they only argue that this is so relative to the evidence we have. In their own words:

> we admit to a number of cases where the evidence is ambiguous or simply lacking … we claim that the present state of the evidence suggests abolishing prepublication review would lead to a peculiar sort of Pareto improvement: each factor considered is either neutral or favours our proposal. (Heesen and Bright [2019], p. 4)

Therefore there is allegedly no need to weigh the significance of each of the factors in order to see that the postpublication review proposal comes out 'on top'; it could be no worse than, although it may very well be better than, the dominant prepublication review system. Heesen and Bright ([2019], p. 5) continue as follows:

> where the evidence is sufficient to make definite conclusions, our claim is that depending on how one weights the different factors one will never be able to say that the status quo is preferable to our proposal, and will sometimes be able to say that our proposal is preferable to the status quo. However, for some criteria of evaluation present evidence does not warrant definite conclusions. Hence there is considerable inductive uncertainty about our conclusion.

Unfortunately, Heesen and Bright ([2019]) equivocate, in a subtle but significant way, on what the conclusion of their argument is. In fact, this is suggested by a careful reading of the paper's abstract. It begins, as I earlier noted, with 'Prepublication peer review should be abolished' (p. 1). And that seems, at first glance, like the conclusion presented upfront for stylistic reasons. But the abstract ends as follows: 'We conclude that on present evidence abolishing peer review weakly dominates the status quo' (p. 1). Thinking of the issue semi-formally will help to clarify the difference. Let h be a practical rather than theoretical hypothesis, such as 'Prepublication peer review should be replaced by postpublication peer review, from a purely epistemic point of view', and let e be a subset of our available evidence. To argue that $P(h|e)$ is high—on a logical or objective Bayesian variant of probability, if desired—is not to argue that h. For example, the weight of the evidence $W(e)$ may be low. And one may have little 'inductive uncertainty' about $P(h|e)$ while having a great deal of 'inductive uncertainty' about h, or vice versa.

The most charitable way to address this is to understand Heesen and Bright's argument as consisting of two parts. First, $P(h|e)$ is high, where e is (a large and putatively adequate tranche of) the current evidence.[7] Second, from the fact that $P(h|e)$ is high, we should conclude that h although *this* move is 'inductively uncertain' (p. 5) to a considerable extent.

This brings me to my objection, which is as follows. Heesen and Bright do not consider the significant epistemic costs that would accrue from changing our current peer review processes. Instead, they think as follows: if we were to click our fingers and magically make all peer review postpublication, wouldn't that be at least as good, and probably even

[7] Heesen and Bright ([2019], p. 7) state that they 'focus on a large number (hopefully all) aspects of the social structure of science that will be affected'.

better, for science (in terms of the epistemic consequences)? An affirmative answer, while interesting if true, doesn't help us much in our actual situation because there is a significant (epistemic) opportunity cost involved in changing the system, in the short-term and medium-term. Existing journals would have to be abolished or repurposed. New archives would have to be set up. Policies for governing those archives would have to be agreed on. New data would have to be gathered. (Recall the earlier discussion about how to determine who should be allowed to post on such archives, and how to make them secure in this regard.) Internal means for universities to assess staff members, in personnel actions, would have to be revised. (Universities in many areas of the world have internal journal lists, which they consult to inform decisions about how well faculty members are performing, for instance.) Universities might also need to develop a hybrid system for internal assessment during the period in which change is underway.[8] And scientists would need to spend time and effort learning to navigate the new system(s) effectively, even if they weren't involved in their implementation. (For those who aren't technologically savvy, such changes could be especially distracting.) Moreover, to touch upon an issue I will discuss towards the end of this paper, a lot of effort would need to be devoted to considering how future (postpublication) journals might legitimately determine which pieces to consider, since they won't be able to consider everything possibly relevant. Will searches of voluminous archives be conducted? If so, will

[8] In so far as scientists would be involved in such processes, there is an epistemic opportunity cost involved. It is plausible that they would be to a considerable extent (at the level of the whole community). For example, journal lists are normally generated in consultation with academics working in the relevant fields. Universities also sometimes require those academics to gather and present evidence in order to justify their rankings. This is a time-consuming process.

computers or scientists do the searches? Even if it's only the former, programming and extensive testing, involving the time and effort of scientists, will likely be required.[9]

In summary, Heesen and Bright do not consider implementation cost (in the epistemic dimension), although it is necessary to do so in order to make a good decision about whether prepublication peer review should be replaced by postpublication peer review. What that cost would be, or could be, is up for debate.[10] But it seems considerable enough that seeking further (much less costly to obtain) evidence about the relative merits of pre- and post-publication peer review, before trying to decide whether to make a change, would be strongly advisable.

As I noted in the introduction, Heesen and Bright ([2019]) agree that more empirical research on peer review is needed. They: 'take the calls for further empirical research we make… to be just as important a part of the upshot of our article as our positive proposal' (p. 5). I disagree in so far as I think the aforementioned calls are a much more important upshot of the article than the positive proposal (especially with a view to implementation in the near future), given the status quo.

3 Unconsidered Benefits of Prepublication Peer Review: Preventing Biases

[9] One might try to address this concern by suggesting that authors could 'nominate' their published pieces for consideration by one journal at a time. But then at least as much time and energy will be spent on postpublication peer review as it is on prepublication peer review, on the reasonable assumption that scientists will still want their papers to appear in journals as much as they do now (for prestige reasons, *inter alia*).

[10] One might also strive to argue that these initial costs would be worthwhile for the benefits later on. But knowing roughly how long it would take for those benefits to accrue might be worthwhile before making any decision.

Some research has suggested that reviewers often exhibit conscious or unconscious ('implicit') biases, based on factors such as an author's gender, race, or institutional affiliation. For example, Wennerås and Wold ([1997]) studied applications for postdoctoral fellowships to the Swedish Medical Research Council, which they gained access to via Freedom of the Press Acts. They took their findings to strongly suggest 'that peer reviewers … over-estimated male achievements and/or underestimated female performance' (Wennerås and Wold [1997], p. 341), and other studies, such as (Budden *et al*. [2008]), purport to show that double-blind review processes (as opposed to single-blind processes) can increase the representation of work by women in journals. Further studies have suggested that institutional affiliation can affect results of review processes; Peters and Ceci ([1982]), for example, found that eight out of nine papers produced by psychologists in prestigious institutions, and published in top psychology journals, were rejected—often on the basis of putative 'serious methodological flaws'—when they were resubmitted to the same journals using fictitious names for authors and institutions.[11]

[11] There were several flaws in this study, which were identified by the commentators on it. The sample size was small, and the findings were compatible with the process being highly arbitrary rather than biased, when taken in isolation, as noted by Colman ([1982]) and Glenn ([1982]). Nonetheless, several commentators on the piece, many of whom were journal editors, supported the underlying claim that bias was a significant factor in reviewing processes (and the fact that the study detected arbitrariness doesn't mean that it failed to detect bias, since both may be present at the same time). Some commentators viewed such bias as unproblematic (and would therefore presumably be happy to exhibit it in their own practice). Here are three examples. First, Geen ([1982], p. 211) wrote: 'Individuals reporting a study from Stanford, for instance, hold their appointment at that school because in all probability they have demonstrable ability and a record of good research. A reviewer may be justified in assuming at the outset that such people know what they are doing'. Second,

However, findings on the extent to which discrimination occurs, and the positive effects of blinding, often conflict. For example, some recent meta-analyses, such as Marsh *et al*. ([2009]), find no gender discrimination, whereas others, such as Bornmann *et al*. ([2007]), do find such discrimination.[12] The evidence is mixed, and there are methodological concerns about some of the studies, as indicated by Lee *et al*. ([2013]). But I agree with Heesen and Bright ([2019], p. 12) that overall 'there is some evidence of gender bias in peer review' and will continue to focus on gender bias for present purposes. Ultimately, nevertheless, my subsequent argument will go through provided there is at least one form of epistemically disadvantageous bias that can legitimately be substituted for gender bias.[13]

Perlman ([1982], p. 232) opined that: 'an institution's prestige is a valid predictor, and editors may be justified in using this as a factor in their decision making'. Third, Yalow ([1982], p. 244) stated: 'I am in full sympathy with rejecting papers from unknown authors working in unknown institutions. How does one know that the data are not fabricated? Those of us who publish establish some kind of track record. If our papers stand the test of time and are shown to be valid through confirmation by other investigators, it can be expected that we have acquired expertise in scientific methodology'. Moreover, Tomkins *et al*. ([2017]), in a methodologically sounder study, conclude that: 'single-blind reviewing confers a significant advantage to papers with famous authors and authors from high-prestige institutions'. See also Blank ([1991]).

[12] One can similarly compare the findings of McNutt *et al*. ([1990]) with those of Van Rooyen *et al*. ([1998]); the former find that blinding improves the quality of peer review, whereas the latter find that it has no effect on quality.

[13] I hold that there is evidence for the other kinds of bias mentioned too, such as that mentioned in footnote eleven, but will put this aside in the interests of expediency. The points made in this section about gender go equally for race and institutional affiliation, *mutatis mutandis*. The issue of topic or approach bias, covered ably by Katzav and Vaesen ([2017]) for the case of philosophy, is significantly different.

Now as suggested above, Heesen and Bright do discuss the issue of gender bias to some extent; their focus is on how to reduce the gender skew in publications, which they take to be to science's epistemic detriment. They claim that: 'Insofar as there is gender bias—in the sense of women's work being judged more negatively by peer reviewers—abolishing peer review will remove this and help level the playing field for men and women' (Heesen and Bright [2019], p. 12). Yet while this appears plausible on the surface, it is far less so when one recalls that 'abolishing peer review' is Heesen and Bright's elliptical way of saying 'replacing prepublication peer review with postpublication peer review'. Why? Because inclusion of one's work in prestigious journals after publication—in some disciplines, such as philosophy, at least—may become important for the purposes of achieving academic tenure or promotion, or indeed having one's work become highly influential, *inter alia*.[14] And postpublication peer review will tend to be inferior to prepublication peer review in one clear respect. Reviewers and editors will know the identities and probable genders (and affiliations, etc.) of the authors, or at any rate have easy access to this information by conducting archive searches. Women scientists won't have the protection, so to speak, of (probable) anonymity.[15] So we might reasonably expect the gender skew in authorship of journal papers

[14] As emphasized by Niles *et al*. ([2019], p. 13), in a survey of over 1500 academics from 334 units in 60 universities, 'faculty believe that quantity and prestige of publications still dominate RPT [Review, Performance, and Tenure] decisions'. Respondents also, on average, took 'overall prestige of the journal/publisher/venue' to be the second most important factor when they considered where to submit their work for publication. Over 65% of respondents rated the importance of this as 5 or 6, on a scale of 1 to 6.

[15] I use 'probable' because I grant that anonymous peer review of some papers is impossible (or extremely difficult) despite best efforts; the author's identity might be known to all the reviewers with the correct expertise, or to the editor (despite a triple-blinding procedure being involved). However, this is the exception rather than the norm. See, for example, Le Goues *et al*. ([2018]), who solicited

to persist or even worsen. Heesen and Bright might respond by suggesting a novel selection process for inclusion in journals that doesn't involve 'peer review' in the current sense; perhaps the 'peers' involved could be the scientific community, and perhaps papers with sufficiently many citations (over some time period or range of periods) would be included in journals, for instance. However, there is no reason to think that members of the community as a whole wouldn't exhibit bias (perhaps especially if the relevant community were already male-dominated) that would be suppressed by anonymous review. Such a system might also lead to well-networked scientists having an unmerited additional advantage in influencing scientists outside their networks, and this might lead to more sidelining of higher-quality, or even merely relevant, work.

There is also another interesting way that the absence of pre-publication peer review might result in less exposure for, or uptake of work by, women scientists. Peer review, especially of a blind variety, is often—rightly or wrongly—taken to give imprimatur to work. For example, one may say of an accepted paper in a leading philosophy journal using triple-blind peer review that, in all probability, it passed where the vast majority of papers fail when considered largely on its own merits. And that is a reason to take it seriously. Besides, even if one disagrees, there is evidence to support the view that the majority of scientists, in several fields, think that the typical peer review system has considerable merit. In the survey conducted by Ware ([2008]), there was 'strong support for the idea that it [prepublication peer review] determines the importance of the findings and the originality of the

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guesses of author's identities from reviewers of over 2,500 anonymized papers, submitted to three different conferences, and found that 74%–90% of the reviews did not contain a correct guess. I should also add that I here assume that publication won't be anonymous. I take it that Heesen and Bright would have specified if they were proposing such a radical change. I also think such a change would be problematic for a number of pragmatic, if not epistemic, reasons.

manuscript'.[16] It follows that many of the surveyed scientists would take passing such review to be an indication of significance. Furthermore, 49% of respondents to Ware's ([2008]) survey thought peer review would pick the best manuscripts for a journal, whereas only 22% of respondents disagreed. In the words of Ware ([2008], p. 14): '[a potential] way in which peer review can provide a filter for readers … important for working academics: it provides the basis for the stratification of journals… the system that routes the better papers to the better journals … Respondents to our survey have a lot of confidence in the peer review system to support these filtering functions'. It therefore seems reasonable to conclude that many scientists take a publication appearing in a top (or prestigious) journal not only to be significant and original, but also probably to be more so than those appearing elsewhere. It is irrelevant if they're wrong about this, as Heesen and Bright ([2019], p. 19) argue they are, for current purposes.[17] The point is simply that they will take a publication in such a journal by a woman to have a stamp not only of approval, but also of excellence. This makes it much harder for them to justify failing to engage with or to cite such a paper in their own work (provided it's relevant). In summary, that's to say, I am here suggesting that the absence of such a stamp—which might merely be a perceived stamp—may negatively affect the prospects of papers authored by women. This effect will be cumulative with the other identified above, due to the loss of anonymity at the postpublication stage of review.

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[16] 60% of respondents agreed, and only 16% disagreed, that importance was determined by the process. 58% of respondents agreed, and only 17% of respondents disagreed, that originality and importance were determined.

[17] Citing Brembs *et al*. ([2013]) and Brembs ([2018]) in support, Heesen and Bright ([2019], p. 19) state that 'the available evidence does not support the idea that the prestige hierarchy of journals tracks some underlying quality of articles'. Presumably they therefore think that the opinions of scientists referred to do not reflect the reality.

Perhaps there are ways to perform postpublication peer review such that these problems are averted or minimized. But it is far from obvious what these might be. Thus it seems, for the time being, that there are advantages to prepublication peer review that Heesen and Bright haven't allowed for.

4 Unconsidered Disadvantages of Postpublication Peer Review: The Consequences of a Publication Explosion

Making all peer review occur postpublication would neither reduce the desire of scientists to publish nor lessen the existing professional pressures to publish. It would not diminish the extent to which academia is a 'publish or perish' game, especially for younger scholars.[18] The more publications a scientist has the better, *ceteris paribus*, for several reasons. Raw number of publications is relevant in determining who gets hired, promoted, and tenured. One's scores on various standard metrics used in such processes are also constrained by how much one has published; for example, one's h-index can be no higher than the number of publications one has. Recall also that publication would be a prerequisite for papers to be selected for inclusion in journals, on Heesen and Bright's proposal. So if getting papers into prestigious journals remained important in the academic credit economy, as it likely would, this would also drive publication.

[18] Niles *et al*. ([2019]) show that 'total number of publications' and 'number of publications per year' are perceived to be the two most important factors in performance review (and tenure decision) processes by academics, by a considerable margin. (For example, 64% of respondents rated the former, and 57% rated the latter, as being of maximal significance on the 6-point scale employed in the study.) Even if such perceptions are inaccurate, they are significant in driving behaviour.

Given the above, making publication much easier—making it a matter of individual choice—would result in far more publications. Consider, for example, how graduate students and those on the tenure-track would publish papers that they would instead have submitted to journals under the prepublication review system. Consider also how many publications would result from those with PhDs on the job market who weren't fortunate enough to have tenure-track jobs or even any kind of academic employment. (For reasons mentioned in section two, it wouldn't be profitable to exclude these groups from publishing, or indeed prevent their work from ever appearing in journals.) Wouldn't the increase in the number of publications be dramatic?

I grant that some authors would exhibit more caution in self-publishing than they do (or would) in submitting to journals. For example, graduate students would be concerned about publishing material that might not paint them in a good light to hiring committees (which they would be emboldened to submit to journals given the opportunity for useful feedback). However, this would only serve as a partial brake on publication. That's to say, it remains probable that graduate students would, on average, publish more. Furthermore, graduate students would likely expect their supervisors, other mentors, and other graduate students to help more often with informal review. (The same might be true for junior academics.) This would result in less time doing science for those individuals (who would often feel some obligation to assist). And this would be in addition to the efforts required to conduct postpublication review for journals (at the level of the group).

It's also likely, moreover, that some authors would self-publish papers that they wouldn't even have written, for pragmatic reasons, under the current system. Consider replies to other papers, in the case of philosophy. Writing these is often a risky strategy because they are typically eligible for publication in only one journal (in which the paper to which they respond appeared). If rejected, they normally have to be discarded or extensively repurposed.

But full articles can typically just be sent somewhere else. It might be added that discussion notes typically require less effort to write than articles, which might also encourage production of rather more of them.[19]

It therefore appears likely that the increase in the number of publications would be dramatic. And it's plausible that this would have some negative epistemic consequences, even granting that it would have some positive ones too. First, it might make it considerably more difficult (and costly) for scientists to find high-quality published work that's relevant to their own (from new sources). Scientists would need to conduct searches on large archives to learn about the status quo in some area, and to go through the process of reading, and deciding for themselves about the quality of, a large number of publications. Avoiding obviously flawed work may not be easy. (They won't be helped, at the level of looking through publications at least, by other scientists performing peer review.) One might rejoin that wily scientists would develop good heuristics for searching or rely instead on 'following' functions linked to specific individuals or departments. That's true. But a key concern is that such heuristics or links would be satisfactory for picking out some good quality research, while marginalizing other good quality research. For instance, a scientist might search only for papers written by authors from particular institutions—those renowned for having strong research profiles in the relevant area—or produced by known experts. (Think, by way of analogy, about how selection committees are liable to assess applications for academic jobs when these are very high in number. At the first pass, they might look only to the publications of the applicant. They might judge the quality of applicants' work, in the first

[19] Naturally one can think a little deeper: if more time is spent on writing discussion notes, then less might be spent on writing full articles, and so on. More discussion could also be argued to be beneficial for some disciplines, but not others, relative to the status quo. For present purposes, however, I need not pursue these issues.

instance, by looking only at where they have published and how much they have published relative to their career stage.) Hence, work by talented graduate students or new workers in an area (without the right social networks) might be overlooked in a way it would not be if it were published in a journal (under the current system). This might not be so bad if such work had a fair opportunity to be selected for inclusion in a journal under the postpublication review system. However, those performing editorial or peer review roles for journals would be likely to want to use similar search heuristics—or even more crude measures, such as number of citations—to sort through the large number of publications. I might add that this also bears on whether the sharing of scientific results would really become easier if scientists were able to self-publish. Granted, more papers would be available to find. But if the likelihood of those papers being engaged with or taken up were not significantly higher than it is now—if, as seems plausible, the probability of any given publication being engaged with would drop significantly—then this wouldn't be of significant benefit to the scientific community.

This brings me to a second probable consequence of an increase in publication numbers, under the postpublication peer review process that Heesen and Bright advocate, which I discussed somewhat in the second section. Because there will be so many published papers, postpublication peer review will be a costly process, in terms of time and effort, if it is to be done properly. Each published paper could not be subjected to peer review by all the relevant journals, as obviously that would result in far more reviewing work for scientists than under the current (standard) system. And even allowing each published paper to be reviewed by one journal at a time—say, by a random assignment procedure implemented by a computer programme—would be more costly than the current system. So by what means, precisely, would or should journals select papers for inclusion? I have already explained, above, why it would be problematic to select only highly cited papers or to employ some kinds of search heuristic. But having scientists manually go through each and every published paper would likely have no less epistemic opportunity cost than the present system does (at the level of the group). And then one of the putative advantages of postpublication peer review would evaporate (at the bare minimum).

Perhaps there is a satisfactory alternative to the approaches I've considered here, but that remains to be seen. Either there are problems with postpublication peer review that Heesen and Bright haven't allowed for, or more work is needed to specify exactly how postpublication peer review could be set up so as to avoid those problems.

5 Conclusion

Let PRE represent pre-publication peer review and POST represent post-publication peer review of the form specified by Heesen and Bright. In summary, speaking only in terms of epistemic consequentialism, I have argued three things: (1) the (considerable) opportunity costs associated with changing from PRE to POST are relevant in assessing the wisdom of making that change; (2) there are merits of PRE that are absent in POST; and (3) there are problems with POST that there aren't with PRE.

Whether the advocates of replacing PRE want to alter POST as a result—either by fleshing it out or by abandoning some components thereof—or instead to argue that POST is still superior to PRE, all told, these findings are each of significance for the future debate on, and future empirical investigations of, peer review.

Darrell P. Rowbottom

Department of Philosophy

Lingnan University

Tuen Mun, Hong Kong

darrellrowbottom@ln.edu.hk

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