Why not Open the Black Box of Journal Editing in Philosophy?
Make Peer Reviews of Published Papers Available

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Abstract:

Despite general agreement within philosophy that peer review is indispensable, its fairness and reliability is often questioned. We suggest that such worries to a large extend can be met by adopting the practice that reviews as well as earlier versions of papers are made publicly available when the final version of a paper is published. This suggestion combines the advantages of transparency with the merits of anonymity of reviewers. While there are obstacles to our suggestion, we argue that it would be worthwhile to implement it because it can help map patterns of conduct and secure confidence in the fairness and reliability of review procedures and journal editing within philosophy.

Key words:

Anonymous peer review; journal editing; patterns in publishing; fairness; reliability

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1. Introduction: Open the black box of journal editing

The system of anonymous reviewing is long established and widespread within philosophy. It plays a key role in assessing quality of academic work and deeply influences processes such as publishing, hiring, funding, and granting academic titles. It is also, however, a topic for vivid discussion both in scientific litterature, on blogs, and in staff rooms (see e.g. Weller 2001, Shatz 2004, Lee et al. 2013 for overviews). Many of these discussions concern whether anonymous reviews are really as fair and epistemically reliable as one could hope, and both principled objections and anecdotal evidence suggests that they may not be.

In this paper we consider peer review processes in the selection of papers for philosophy journals. Our suggestion is that peer reviews should be made publicly available once a paper is published. This new procedure should include not only reviews from the journal accepting the paper, but also previous reviews which resulted in rejections from other journals. The idea is not to breach anonymity of the reviewers, but to put forward their arguments for public scrutiny.

Accordingly, when a paper is accepted, it should be published along with anonymous reviews from the journal it has been accepted by as well as reviews from previous submissions to other journals. Previous versions of the published paper should also be made public to ensure access to the specific material assessed in the earlier reviews.

We argue that this transparency would further attempts to secure fairness and reliability of the reviewing process, or—if it is already fair and reliable—document that it is so and help put an end to misplaced suspicions. In addition, our suggestion can provide information useful for both authors, editors, reviewers, and the academic community as such. We also argue that making peer reviews publicly available is a natural extension of ordinary research practices, and although there may be concerns about the proposal, they appear to be outweighed by its significant benefits.

The paper proceeds as follows: The next section outlines the reasons for making peer reviews publicly available. Section three considers possible objections.

2. Reasons to make peer reviews publicly available

The key problem our suggestion addresses is that of suspicion of unreasonable bias, unreliability of peer reviews, and unfair editorial desk decisions (see Resnik et al. 2008 for a survey among researchers at NIH). So long as, for instance, "desk acceptance" is at all possible—even when in conflict with official guidelines of a journal—it nourishes the suspicion that some authors have unfair privileges in the form of easier access to publishing papers (because they are, e.g., famous, from a prestigious institution, or acquaintances of editors). Our suggestion counteracts such breaches of procedure by making them more difficult to cover up, and the transparency will in general help prevent stories fuelled by suspicion (and perhaps self-serving biases) to gain traction.

The suggested procedure may also help improve quality in assessments because it makes scrutiny of decisions and reasons for these possible. First, transparency can counter reckless and abusive reviews. For instance, editors will have an interest in securing that reviews meet ethical and academic standards when they know that reviews will be made publicly available and so reflect on the journal's reputation. A more subtle challenge concerns countering the many ways in which unsound bias can occur: In a normal double anonymous review process, the editor knows the identity of the author and this means that the choice of reviewers can be influenced by positive or negative biases concerning authors (and reviewers).

In addition, a review can be subject to bias if it involves a breach of anonymity such as when, for instance, a reviewer discovers the identity of the author by accident. Because anonymity is seen as essential in avoiding decisions based on unfair and unreliable bias (see e.g., Weller 2001, 207-240, Shatz 2004, 35-73, Suàrez et al. 2012), such cases clearly pose a problem and reviewers are often urged not to seek information about authors (e.g., De Cruz 2013).

But an assessment may also be biased even when anonymity is still intact. Most texts have in-text markers (such as voice, topic, number of references, choice of examples etc.) which automatically shape a reader's picture of an author. For this reason, the reviewer may form opinions of, for instance, the author's gender, seniority, age, skin color, nationality, native language, and institutional affiliation (cf., e.g., Argamon et al. 2003, Matsuda and Tardy 2007, Davila 2012, Lee et al. 2013, 7). Such guesses may again give rise to unsound bias related to these perceived features of the author, or this is at least what many scientists have feared (cf., e.g., Gillespie et al. 1985, 49). Similarly, content itself may give rise to e.g. confirmation bias due to preferences for/against a topic, interdisciplinarity, or similar. Even the mere style of writing may hamper accurate assessment (cf. Amabile 1983). Our suggestion may help combat biases by mapping the problem in order to inform possible counterstrategies.

Desk rejections present another important issue. It is important because desk rejections comprise a significant portion of rejections, but do not receive reviews and perhaps not even comments from the editors. This procedure is equally open to suspicion of unfair or epistemically inadequate assessments, such as decisions based solely on the author's institutional affiliation, nationality, or lack of renown. These suspicions may be entirely unreasonable, but as with reviews, the procedure does not have mechanisms to reassure authors and others that it is so. It would put an extreme burden on editors to require something like reviews of desk rejections. Instead, our suggestion could provide relevant information by revealing patterns. For instance, if it turned out that a journal often desk rejected papers that were subsequently published unaltered by other journals, this would call for an explanation. And, similarly, if an editor tended to desk reject papers with views opposing her own this could be uncovered. So tracking desk rejections could also play a meaningful part in the endeavor to open up the black box of journal editing.

In addition to addressing possible breaches of procedure and lack of transparency in the process, our suggestion can also offer new opportunities for evaluation of the review system itself (see e.g. Weller 2001, 307-322, Calgano et al. 2012 for similar interests). First, it will make is easier to track correlations between, for instance, initial acceptance and later impact of revised papers. This could be useful in various discussions such as how reviews influence the quality of journal papers, whether, for instance, what later turns out to be seminal papers are likely to be subject to conservative biases, or what the main differences are between earlier and later versions of (successful) papers. Second, it will be easier to track journal behavior and make reviewing processes and self-evaluation of these more transparent. Third, the prospect of having unqualified and abusive reviews made public along with information relating them to specific journals may serve to deter editors from allowing these untoward reviewing practices. And, similarly, may serve to hinder inappropriate editorial practices from remaining unchecked. Fourth, opening the process by making reviews publicly available may aid discussions about best practice in reviewing which

could help secure a high quality in this most important part of academic work. Finally, it would serve to provide information on the standards of academic evaluation of quality within the profession. This is something that would be tremendously helpful to both established authors and students in philosophy.

On the whole, the suggestion that peer reviews should be made publicly available is a natural extension of academic standards. Publishing papers is an essential part of research practices and the feedback obtained through peer reviews is important in shaping arguments and ideas. So, just as log book keeping, defining roles of authors, access to raw data, etc. is important in making research processes transparent within the natural sciences, opening up the review process could further epistemic transparency in academic work.

Who will benefit from publicly available reviews?

The main advantage of this system is the transparency it offers authors. It will help reduce suspicion of unfair treatment, unreliable assessments, and breaches of procedure. For journal editors the proposal offers several advantages as well. First of all, it is likely that authors will be less likely to submit work they know is unfinished just to get qualified comments on papers in progress. Second, it is a tool that can help secure quality of reviews and to sharpen a journals academic status. And third, and most importantly, the suggestion offers a transparent process to help prevent suspicion of unfairness or epistemic unreliability.

Reviewers can benefit from the open review process in at least three ways. They will be able to compare (and perhaps reevaluate) their own assessment of a paper if they can see other reviews of the same paper. They can also compare their own way of writing reviews to that of other reviewers and through this refine their own methods. Some philosophy journals (e.g., *Erkenntnis*) already share reviews among reviewers, probably for these reasons. On our suggestion reviewers are also more likely to be credited (although not named) for suggestions made in a review (because the open process would make it difficult for author to lift an argument from a review without acknowledging the reviewer). And if a reviewer judges that an editor has paid less attention to an objection than this objection merited, the reviewer can be satisfied that the objection will figure when the paper is published.

Another impotant advantage falls on readers and the academic community. Opening the black box of peer review and making reviews and prior versions of papers publicly available, can reassure audiences of the quality of the peer review process and so of the quality of papers published. Because the idea is to offer checks against unfairness, breaches of process, and unreliable assessments, it can also be used to reduce the impact of arguments fuelled by self-serving bias. Unfounded conspiracy theories thrive less well when a system is transparent. Moreover, our suggestion offers valuable means to academic development of reviews and review

processes. It can, for instance, offer data material for studies directed at articulating best practice within the field, and it can provide material helpful in teaching philosophical standards to students.

3. Objections to making peer reviews be publicly available

Nevertheless, some may have concerns about making peer reviews public. An objection to our suggestion might be that current anonymous reviewing processes are sufficiently fair and epistemically reliable. But this argument is more difficult to defend than it may seem offhand. First, although the case has been made that reviewing processes are less biased than is sometimes assumed, others have argued otherwise (see Lee et al. 2013 for a review of research). Some empirical findings suggest that unsound bias is in fact an important concern in reviewing despite anonymity because it can make the selection process unfair and not sufficiently reliable. Another concern is whether reviewing procedures advertised by journals are always met in practice. Authors and others have come forward with firsthand knowledge of instances of breach of procedure (see Ichikawa 2015 for a recent example) and although these cases do not establish the existence of inherent or widespread problems, they cause discouragement, frustration, and distrust. Even if the system is in fact perfect, there is still a serious problem if this not how it is perceived by the academic society. Publication opportunities (and with them academic positions, grants, recognition, influence etc.) are a scarce goods, and if they are suspected to be unfairly distributed this itself can undermine academic society.

Alternative options in comparison

Another concern about our suggestion might be that other (already existing) reviewing processes offer sufficient measures for solving the problem. These other options include triple anonymous review, open post-publication commentary, open prepublication review, open prepublication history, or no review. Clearly, opting for no review (see, e.g., Fish 2004) will not address the concerns raised about the risk of unfair and unreliable processes. In contrast, the other practices offer interesting alternatives, but none of them combine the undeniable merits of anonymous reviewing with the benefits of transparency of procedure the way our suggestion does. A brief survey will show why.

Triple anonymous review is a procedure in which the identity of an author is hidden not only from reviewers, but also from the editors who decide whether a paper is suitable to go out for review and in that case assign reviewers. The triple anonymous review reduces biases in decision and selection to those which stem from in-texts markers and so it can prevent unfair and unreliable steps in this part of the procedure. This measure will not, however, address the other concerns raised such as risk of poor quality in reviews, suspicions of breach of procedure, poor editorial decisions, or arbitrariness in how recommendations of reviewers are weighed. Our suggestion

contributes to the latter by making it possible to track relation between reviewer recommendations and editorial decisions.

Open post-publication commentary is a procedure in which a paper is published as usual after an anonymous reviewing process, but then subjected to further discussion (see, e.g., Lee et al. 2013, 12-13). This has several advantages, but it cannot solve the problem of potential distrust in the original selection procedure because it still leaves open the possibility that there is not equal access to publish in the first place.

An open prepublication review is a procedure according to which papers are only published after open (non-anonymous) online discussions of the paper (see e.g. Lee et al. 2013, 11-12). This procedure has the advantage of making the selection process transparent, but it also has the disadvantage of not offering anonymity to author and reviewers. Anonymity is important because it lessens risks of unsound bias, of exchanging favors, or of harming rivals, and so the open prepublication review cannot protect, for instance, authors who are junior or from less prestigious institutions from unreasonable treatment. Our suggestion combines anonymity of authors and reviewers with the transparency of arguments made in the process.

Open prepublication history procedure is adopted by BMC journals. It involves that normally when a paper is accepted and published, the history prior to publication (initial submission, reviews, and revisions) is posted at the same time (see BioMedCentral 2015). However, this procedure does not include reviews from earlier, unsuccessful submissions to other journals and the identity of the reviewer is revealed. This procedure addresses the concerns raised here that some may suspect that papers are sometimes accepted without reviews or after less severe scrutiny. But it does not address problems concerning suspicion of unfair or inaccurate assessment in reviews, because rejection assessments will not be revealed. In addition, unveiling the identity of the reviewer may provide an unhealthy motivation to reviewers who will benefit from making friends in the field.

The procedure that most closely resembles our suggestion appears to be a procedure adopted in an experiment to the purpose of exploring the benefits of open peer review (Bingham et al. 1998). The experiment concerned a different matter than the present (namely the effectiveness of post publication open review), but as part of the experiment, reviewers were given the option of letting their reviews become public in case the paper they were about to review should later be accepted and published. 92 % of the reviewers accepted this and 62 % also agreed to sign their reports. This procedure resembles our suggestion, only we argue—for the reasons just stated— that it should be seen as a valuable procedure in itself (not only a means to test a hypothesis), that reviewers should not be given the opportunity to sign reports, and that the practice should not be confined to papers accepted by the journal one is reviewing for. Our suggestion is aimed at

eliminating suspicion of unfair and inaccurate assessment more broadly and countering the risk of unfair or poorly argued rejections.

Risk of shaming

Another kind of concern related to our suggestion could be that making reviews and earlier versions of papers publicly available will involve sharing unpleasant and potentially shameful information (a concern which can also be discerned in other suggestions of making reviews available for scrutiny, cf., e.g., Buriak 2014). We acknowledge that authors as well as editors may have legitimate reasons for preferring that reviews are kept confidential and that these reasons should be taken into consideration, but we nevertheless maintain that they do not outweigh the potential benefits.

First, authors may be reluctant to make reviews and earlier version of papers available for public scrutiny if they have received negative, but fair reviews pointing to, for instance, embarrassing mistakes such as basic misunderstanding, logical flaws, or neglect of key literature. It is a common experience that papers improve after every revision, and many would probably prefer not to have their final, published version stained by long forgotten imperfections. This may be especially true for papers that have met perhaps 10 or 20 rejections before the successful submission. In these cases, the amount of critical reviews and the number of changes prompted by these could appear quite overwhelming and so reflect badly on the final, official version of the paper. Also, the number of resubmissions itself may make the author look like a poor professional who needs extensive help from the reviewers in order to publish his or her work.

Regarding this concern, it should be noted that reviews and prior versions will only be available when the paper is published and this will put the paper in a somewhat shielded position. This will not prevent that the author feels uncomfortable with the situation, but if our suggestion is widely implemented, it will be a routine matter for everyone (and so diminish feelings of being singled out as imperfect). And perhaps it would even benefit the profession to embrace the idea that good papers may have had a rugged history.

Another risk of shaming relates to journal editors. Editors may find it tiresome if it is revealed that their journal is always third, fourth, or seventeenth choice among authors in the field. In addition, the editors may suspect that authors care less about fairness and such than they care about publishing in prestigious journals. If this is the case, the information provided by our suggestion, will be irrelevant for journals if prestige does not track fairness and epistemic reliability. How much weight should be assigned to this concern regarding reputation depends on whether our suggestion will in fact serve the purpose we hope it will. If authors and readers are concerned with quality and fairness of peer review assessment, such problems will disappear in the long run. If not, the purpose of peer reviews becomes fundamentally unclear.

But editors also have an interest in attracting efficient reviewers and making the reviewing process smooth in order to diminish response time. So it is a potential worry that reviewers might feel pressured to spend more effort on their reviews if the procedure is implemented. If, because of this, the procedure turns out to become more time consuming, it will be harder to make appointments, and perhaps the extra trouble may even be to no avail because it will not result in better basis for their editorial decisions. This concern may also be shared by prospective reviewers who will hesitate to undertake a more time-consuming task. Whether increased time consumption will be justified, is something that must the object of later empirical evaluation. It is possible that open access to reviews will increase the amount of work put into writing them, but we predict that it will turn out that quality increases correspondingly to warrant it. In addition, the pressure to spend more efford on reviews will be countered by the fact that reviewers remain anonymous.

Another risk of shaming comes from the possibility that perhaps making reviews public could produce unwanted consequences such as inviting hatchet job top tens (and reviewers aspiring to be part of them) as well as similar kinds of shaming. Concerning this last worry one should, as before, remember that reviews will only be available once the papers are published, thus giving the paper this extra weight and (perhaps) the final word. In addition, it may be expected to reflect badly on journal editors if they repeatedly assign reviewers whom they know to have a record of abusive reviews.

Note, however, that public exposure is not only a problem. It is also a social mechanism that makes us evaluate our behavior. This could be the case for, for instance, authors who perhaps presently benefit from unfair privileges (such as getting papers accepted without reviews), authors who abuse the review system (to get easy feedback or make simultaneous submissions of the same paper, for instance), or reviewers who engage in blameworthy conduct (such as writing reviews that are abusive or of poor quality). Potential embarrassment may encourage people to reconsider their actions and responsibilities. If they would rather not reconsider, then of course our suggestion will disadvantage them. Journal editing presents a case of complex power relations (see, e.g., Lipworth and Kerridge 2011) and lots of opportunities for misconduct on all sides (see, e.g., Hames 2007, 171-197). Opening the black box of journal editing in philosophy would provide much needed transparency.

Difficult cases and practical concerns
It is impossible within this paper to present a detailed design of how to implement our suggestion, but obviously it will be necessary to think through the possibilities. Should papers and reviews be stored locally at individual journals (e.g. at that of the journal accepting the paper in the end), or would a common database be preferable? How should it be financed? How can papers be tracked?

By something similar to the DOI system? And what measures can be taken if authors and editors do not comply with the procedure? But while these questions need to be addressed, the do not appear to pose insurmountable obstacles to our proposal and we are optimistic that experiences from comparable tasks in journal editing and handling of databases can be employed to solve the practical issues involved.

It is clearly difficult to estimate the actual costs and inconveniences of our suggestion. It is clear, however, that it is likely to result in additional administrative work and more organizing from both journals and authors. This may be seen as an undue burden, but it should be remembered that in the short run fairness and quality in assessments are always more costly and rather inconvenient compared to doing nothing. This is already the case in double anonymous review, and it is even more obvious in juridical systems or democratic processes. But if this can help secure the benefits, the difficulties and troubles will be worthwhile. And this appears to be the case concerning the black box of peer review. Something along the lines of our proposal seems needed to dismantle suspicions against peer review processes. In addition, it is worth noticing that scientists in other fields go to extreme measures to ensure fairness and quality in recognition of its importance. For instance, the co-author statement information which is required for papers in *Science* must be provided by all authors separately in a detailed form with a total of five pages.

However, organizing, costs and inconveniences are not the only practical concerns one needs to attend to. There are also difficult questions relating to the identity of a paper and to papers that are never published after (perhaps multiple) rejections. What constitutes the identity of a paper becomes important in cases where a longer paper is split into two or more new papers. Will an earlier review then need to be made public at the time of publication of the first of the two new papers? If it does, it will include material which could otherwise have been kept confidential until later publication. If it does not need to be made public, this offers the opportunity for an author to resist disclosing earlier versions and reviews without further argument by claiming that some material is being reworked for review (whether this is the case or not).

One way of accommodating this objection could be to argue that it should be left to the author to decide whether or not reviews or previous versions should be published alongside a paper. This would, however, undermine our suggestion to a significant extent because it would then still be possible to suspect that some papers (i.e. those were no reviews were available) were accepted with no or with less severe scrutiny.

Other cases present equally difficult problems. For instance, if an author insists that a paper is reworked to the extent that it should be regarded as a new paper with no review history even though it has in fact been submitted several times already. In such cases, only earlier editors and reviewers will be in a positon to judge whether the author's argument is fair. So authors willing to make this move would be in considerable risk of being found out when reviewers of previous

versions of the paper come across the published paper. In addition, if patterns of authors are tracked, it will be revealed if some appear to always get their papers accepted in first try.

Another kind of worry concerns papers that unfairly never get accepted. The author may so discouraged as to shelve the paper and the unfair assessment will never be disclosed. This is not a problem which is caused or even aggravated by our suggestion; it is one that remains despite opening up the review process. But perhaps our suggestion may mean that authors are less likely to be discouraged and so still help by addressing this problem more indirectly. It will also help provide an estimation of the problem if patterns turn out to reveal that many papers are rejected multiple times before acceptance.

4. Conclusion

The anonymous peer review system plays a key role in regulating access to publication opportunities within academic philosophy. Yet, worries about reliability and fairness of review processes continue to surface within the profession. We propose that these problems could be met by adopting the practice that publication of a paper is accompanied by the reviews the paper received prior to its publication. By opening the peer review system to public and scientific scrutiny this practice would serve either to affirm the system's reliability, and put an end to doubts about its fairness, or reveal where the system needs improvement. Access to reviews would also contribute to protecting the virtues of the peer review system by making the prospect of public exposure a deterrent to inappropriate editorial practices and unqualified or abusive reviewing. Therefore, while the proposal has costs in terms of added administrative burdens and possible personal embarrassment, these concerns are outweighed by its benefits with respect to fairness, reliability, and transparency.

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