

Editorial: A Response to Criticisms of the OpenPsych Journals

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OpenPsych

Abstract

The OpenPsych journals were set up in 2014 by Emil Kirkegaard and Davide Piffer due to dissatisfaction with existing journals in differential psychology and behavioural genetics. To date, 51 papers have been published in total, encompassing a range of topics from differential psychology and behavioural genetics to socio-political science. However, the journals have come under criticism in both online articles and unpublished offline discussions. This editorial responds to the main criticisms that have been levelled at them, namely that it is unethical or illegitimate to: (1) publish research on politically controversial topics; (2) publish papers in journals of which one is an editor; (3) have papers be reviewed by individuals who do not possess satisfactory academic credentials; (4) have papers be reviewed by individuals with controversial political views; and (5) have papers be reviewed by individuals who are personally acquainted with the authors. Since the first of these criticisms has been answered extensively elsewhere, here we focus our attention on the other four.

Keywords: OpenPsych, differential psychology, behavioral genetics, research ethics

1 Introduction

In 2014, Emil Kirkegaard and Davide Piffer set up the OpenPsych journals due to dissatisfaction¹ with existing journals in differential psychology and behavioural genetics. At the present time, there are three such journals: *Open Differential Psychology (ODP)*, *Open Behavioural Genetics (OBG)*, and *Open Quantitative Sociology & Political Science (OQSPS)*. As noted on the OpenPsych website, these journals share the following core features: open access, authors retain rights, no author fees, open forum peer review,

and mandatory data sharing. The publication process at OpenPsych is as follows: first, an author submits a paper by creating a thread in the submissions subforum; second, others discuss it openly with the author while he submits revisions; third, a set number of reviewers agree that the paper is ready; fourth, the editor asks for a final version; and fifth, when the final version has been uploaded, the editor uploads it to the journal and moves the submission thread to the post-publication subforum; the paper is then published. Once a paper has been published, additional commentary can be submitted in the post-review discussions subforum. To date, 51 papers have been published in total, encompassing a range of topics from differential psychology and behavioural genetics to socio-political science.

The journals were founded on three key principles: accessibility, transparency, and insensitivity to controversy. Accessibility means that anyone can read the journals without going through a cumbersome paywall, and that researchers are not charged exorbitant (or indeed any) fees for publishing. Transparency means that the entire review process can be viewed by anyone both during and after publication, and that all empirical papers are uploaded along with accompanying data² and code for replication. Insens-

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¹ The major sources of their dissatisfaction with existing journals were: prohibitive costs of open access publishing; excessively long wait times; lack of data sharing; and politically motivated article rejections.

² In a small number of cases, papers were uploaded without ac-

sitivity to controversy means that the journals do not shy away from discussing topics that are politically controversial (e.g., the genetics of race, population differences in IQ, criminality among immigrants)³. We will return to each of these principles in the discussion below.

Since the OpenPsych journals were set up in 2014, they have come under criticism in both online articles and unpublished offline discussions (e.g., [Resnick 2016](#); [SciForums 2016](#); [RationalWiki 2018](#)). As far as we can tell, the main criticisms to which the journals have been subjected are that it is unethical or illegitimate to:

1. publish research on politically controversial topics;
2. publish papers in journals of which one is an editor;
3. have papers be reviewed by individuals who do not possess satisfactory academic credentials;
4. have papers be reviewed by individuals with controversial political views; and
5. have papers be reviewed by individuals who are personally acquainted with the authors.

In light of the preceding criticisms, it has been asserted (e.g., in various offline discussions) that papers published in OpenPsych are not up to the standards of proper, peer reviewed scientific research.

2 Response to Criticisms

Since the first of the five criticisms we mentioned above has been answered extensively elsewhere ([Flynn, 2017](#); [Carl, 2018a](#); [Woodley of Menie et al., 2018](#)), here we focus our attention on the other four. We begin by briefly responding to each of the criticisms individually. We then proceed to argue that the traditional model of peer review suffers from a number of important limitations. Our overall conclusion will be that, insofar as the traditional model of peer review suffers from these limitations, the scientific community should be willing to consider alternative models for publishing, including (among others) the model on which the OpenPsych journals are based.

comparing data because the data came from a publicly accessible source or one that requires registration prior to use.

³ Note that, while a number of the papers published in OpenPsych journals deal with highly controversial topics, many others are concerned with more mundane topics (e.g., statistical methods, geographical inequality, political attitudes).

2.1 Responses to individual criticisms

In response to the criticism that it is unethical or illegitimate to publish papers in journals of which one is an editor, we would make two points. First, all the papers published by OpenPsych editors ([Kirkegaard and Piffer](#)) were subject to the same protocols as those published by other contributors, including both open forum peer review and mandatory data sharing. And second, it is not unknown for editors of traditional journals to publish papers in their own journals, so it can hardly be said that the practice is unprecedented. For example, the highly esteemed former editors of *Intelligence* and *Personality and Individual Differences* both published a number of papers in their own respective journals. The specific reason that [Kirkegaard](#) chose to publish so many papers in the OpenPsych journals, rather than in other outlets, is that at one point he objected on principle to publishing in non-open access journals. In other words, since he believes that all science should be in the public domain, he was unwilling to publish in journals that require a subscription (see, e.g., [Buranyi 2017](#)). Given that traditional journals typically charge exorbitant fees for publishing open access, he was left with few options other than OpenPsych. Note that he has since published papers in *Intelligence*, *Journal of Individual Differences*, and *Journal of Methods and Measurement in the Social Sciences*⁴. He has also had presentations accepted at the *International Society for Intelligence Research* conference three times.

In response to the criticism that it is unethical or illegitimate to have papers be reviewed by individuals who do not possess satisfactory academic credentials, we would point out that possessing advanced degrees is by no means a necessary condition for having expertise in a relevant subject area (even though individuals who possess advanced degrees, on average, have greater expertise than those who do not possess such degrees). In other words, there is clearly some overlap in the distributions of expertise for advanced degree-holders versus non-advanced degree holders. As a matter of fact, the OpenPsych review team includes 9 PhD-holders, as well as 9 faculty members/postdocs, out of 16 members in total. Some of these individuals prefer to remain pseudonymous so as to avoid reputational attacks. Note that, according to a recent survey of experts who had published in journals related to intelligence, 13 % did not have a PhD, 16 % had never formally studied psychology, and 33 % were not tenured faculty members ([Becker et al., 2018](#)).

In response to the criticism that it is unethical or illegitimate to have papers be reviewed by individuals

⁴ [Kirkegaard](#) decided to abandon the principle of only publishing in open-access journals (OpenPsych, *Mankind Quarterly*, etc.) because he felt that it had engendered too many reputational attacks.

with controversial political views, we would point out that it is a fallacy to presume that someone is incorrect or unqualified just because he or she happens to have political views that differ from one's own. Moreover, we would argue that being willing to converse, debate and potentially collaborate with someone whose views one finds objectionable (or even abhorrent) is an ethically defensible position. Indeed, it is consistent with the principle that intellectual disagreements are best resolved by actively engaging with one's opponents, rather than by trying to stifle their speech (Pinker, 2015; Reeves & Haidt, 2018). After all, conversing, debating or collaborating with an individual does not imply endorsement of his or her views. In this regard, one of the chief recommendations put forward by Duarte et al. (2015) at the end of their long review article on political diversity in social psychology⁵ was for researchers to conduct more adversarial collaborations. Interestingly, Shi et al. (2017) recently documented that politically polarised teams created higher quality Wikipedia pages than politically homogeneous teams.

In response to the criticism that it is unethical or illegitimate to have papers be reviewed by individuals who are personally acquainted with the authors, we would make two points. First, given that the entire review process can be viewed by anyone both during and after publication, readers can judge for themselves whether a particular paper has been reviewed properly⁶. Second, it is taken for granted within the wider research community that reviewers often know exactly whose paper they are reviewing, and that authors often have a good idea about who is reviewing their paper (see Yankauer 1991). An obvious explanation is that, since researchers tend to publish in relatively narrow areas, they often know most or all of the other scholars who are working within that area. Indeed, many journals ask authors to recommend reviewers when they submit their manuscripts. Moreover, there is widespread anecdotal evidence of favouritism on the part of journal editors, especially at top-ranked journals where only a small percentage of papers are sent out for review⁷. In fact, Colussi (2018) analysed data on ties between authors and editors of top-ranked economics journals, and found that the graduate students and faculty colleagues of a journal editor will see the number of their publications increase in that journal by 14 % and 8 %, respectively, during that person's term as editor. Our point here is

⁵ Most academic social science departments exhibit a strong left-liberal skew, and hence little political diversity (Carl, 2018b; Langbert, 2018).

⁶ The fact that all reviews of OpenPsych papers are public arguably gives reviewers an additional incentive to be impartial.

⁷ If only a small percentage of papers are sent out for review, editorial sway can make a big difference, since the editor can personally select which papers to send out for review and which to unilaterally reject (i.e., 'desk reject'). Note that editors at OpenPsych are unable to desk-reject papers.

not that favouritism is okay, but rather that the traditional model of peer review is no bulwark against it.

We appreciate that some readers may not find the preceding arguments convincing, and will therefore presumably continue to believe that papers published in OpenPsych are not up to the standards of proper, peer reviewed scientific research. Before going on to point out the limitations of the traditional model of peer review, we would suggest that it is somewhat eccentric to dismiss papers published in OpenPsych purely based on one's dissatisfaction with the specific features of the publication process. In many disciplines, it is common to cite working papers, non-peer reviewed reports (e.g., by thinktanks or government agencies) and even academic blog posts⁸. In fact, in the discipline of economics, working papers often remain online for several years before being published in peer reviewed journals, and sometimes rack up tens or even hundreds of citations during that time. Occasionally, they are never published at all, presumably because of that discipline's particularly pronounced winner-take-all system, whereby papers published in lower-ranked journals can actually detract from the perceived quality of a researcher's CV (Powdthavee et al., 2018). Because working papers are so commonplace in economics, and hence can easily be tracked down online by inquisitive reviewers, some journals have purposely moved from double-blind to single-blind peer review (Fischman, 2011). Likewise, the famous arXiv preprint repository, which was set up as an archive for physics papers in the early 1990s, now receives >11,000 submissions per month (arXiv, 2018); see also (Nature Neuroscience, 2003). Our point here is that scientific writings can be highly informative even if they have not gone through 'proper peer review'⁹. The fact that citations to working papers, pre-prints and other non-peer reviewed works are commonplace in many disciplines indicates that, in practice, few researchers really disagree with this point.

⁸ There are a number of highly influential academic blogs (e.g., Marginal Revolution; Statistical Modeling, Causal Inference, and Social Science; Slate Star Codex; among others). To take just one example, following the publication of Case & Deaton (2015) widely discussed paper on rising mortality among white Americans, the statistician Andrew Gelman (Gelman, 2015) published a critique on his personal blog. This critique did not go through 'proper peer review', but it was arguably just as rigorous and far more influential than a great many academic papers.

⁹ One prominent academic psychologist recently tweeted that he "only reads preprints nowadays" (Lakens, 2018). Moreover, the fact that arXiv primarily caters to physicists, mathematicians and computer scientists shows that even scholars from the 'hardest-science' disciplines recognize that non-peer reviewed work can be highly instructive.

2.2 *The limitations of the traditional model of peer review*

It is now a well-known fact within the scientific community that many fields (especially medicine, neuroscience and social psychology) are afflicted by a so-called replication crisis (Washburn et al., 2018). In one provocatively titled paper ('Why most published research findings are false'), Ioannidis (2005) used simulations to show that it is plausible that more than 50 % of published findings in some fields are false positives. In a later, highly publicised study, the Open Science Collaboration (2015) attempted to replicate 100 experimental and correlational studies that had been published in three psychology journals. They found that the replication effects were, on average, half the magnitude of the original effects, and that while 97 % of the original effects had reached statistical significance, only 36 % of the replication effects did so. In an even more recent study, Cramer et al. (2018) attempted to replicate 21 experimental psychology studies that had been published in Nature or Science between 2010-2015. They again found that the replication effects were half the magnitude of the original effects, and that only 62 % of them reached statistical significance, despite sample sizes being five times larger than in the original studies. The alarmingly low replication rate in some fields has been attributed to research practices such as publication bias ('the file drawer problem'), where significant results end up being published but null results do not (e.g., Franco et al. 2014; Kühberger et al. 2014), as well as *p*-hacking ('researcher degrees of freedom'), where scientists collect or analyse data in such a way as to ensure that they uncover a significant result (e.g., Head et al. 2015; Wicherts et al. 2016).

The evidence adduced in the previous paragraph illustrates that the current state of scientific research does little to flatter the traditional model of peer review. This is not to say that the traditional model is uniquely flawed (or that it directly caused the replication crisis), but just that it has quite evidently failed to prevent a huge quantity of unreplicable studies from being published. However, the replication crisis is by no means the only piece of evidence indicating that the traditional model does not guarantee research quality. In a Cochrane Review published only 11 years ago, Jefferson et al. (2007) concluded that "little empirical evidence is available to support the use of editorial peer review as a mechanism to ensure quality of biomedical research". And while studies in specific journals comparing articles before and after peer review have reported positive effects on measures of article quality (e.g., Goodman et al. 1994), firm evidence in favour of the traditional model is surprisingly scant (see Smith 2010; Klein et al. 2018).

Particularly concerning is the fact that agreement among reviewers as to whether a paper should be

published is typically very low, and sometimes no better than chance (e.g., Rothwell & Martyn 2000; Wood et al. 2003; Kravitz et al. 2010)¹⁰. As Bornmann et al. (2010) note in their meta-analysis of inter-rater reliability estimates for peer reviewers, "All overviews of the literature on the reliability of peer reviews published so far come to the same conclusion: There is a low level of IRR". The traditional model of peer review suffers from a number of other major problems, which are reviewed in detail by Smith (2010) and Walker & da Silva (2015).

These include: weak error detection (Schroter et al., 2008), opportunities for plagiarism (Rennie & Gunsalus, 2001), bias against iconoclastic papers (Horrobin, 1990), bias against radical new ideas (Steinhauser et al., 2012), bias against papers from less prestigious institutions (Peters & Ceci, 1982), bias against expertise from female reviewers (Ross, 2017), and bias against papers by conservative scholars (Inbar & Lammers, 2012)¹¹.

Further evidence that the traditional model of peer review is no guarantee of research quality can be found in analyses of the relationship between methodological quality and journal rank. If the traditional model worked effectively, one would expect the highest-quality research papers to be sorted into the best-ranked journals. However, as Brembs et al. (2013) and Brembs (2018) demonstrate, what one typically observes is that measures of methodological quality are unrelated or sometimes even inversely related to journal rank. For example: top-ranked medical journals are no more likely to meet the criteria for evidence-based medicine than low-ranked journals; there is no association between statistical power and impact factor in neuroscience; the quality of computational crystallography is lower in higher ranked journals; randomisation of in vivo animal experimentation is weaker in higher ranked journals; spreadsheet errors are more common in higher ranked genetics journals; bias in GWAS effect sizes is greater in higher ranked journals; errors when calculating *p*-values are more common in higher ranked psychology journals; and the retraction rate is positively correlated with journal impact factor. Furthermore, Wang et al. (2017) recently constructed a novelty metric for scientific papers, and found that highly novel papers, which are more likely to end up in the top 1 % of cited research, tend to be published in journals with lower impact factors.

In light of the sorts of problems we have discussed above, a number of scholars have called on re-

¹⁰ Similarly, in a recent study, Pier et al. (2018) found essentially no agreement among reviewers of grant applications at the US National Institutes of Health.

¹¹ Campanario (2009) documents numerous cases in which future Nobel Laureates encountered resistance on the part of journal editors or referees to papers reporting discoveries that would later earn them the Nobel Prize.

searchers to abandon the traditional model of peer review altogether, or at the very least to consider alternative models (e.g., [Brembs et al. 2013](#); [Moylan 2014](#); [Smith 2016](#); [Amaral 2018](#); [Wagenknecht 2018](#); see also [Walker & da Silva 2015](#)). For example, [Smith \(2016\)](#) goes as far as to say:

Peer review is faith not evidence based, but most scientists believe in it as some people believe in the Loch Ness monster. Research into peer review has mostly failed to show benefit but has shown a substantial downside (slow, expensive, largely a lottery, wasteful of scientific time, fails to detect most errors, rejects the truly original, and doesn't guard against fraud).

Similarly, [Brembs et al. \(2013\)](#) suggest that:

Abandoning journals altogether, in favor of a library-based scholarly communication system, will ultimately be necessary. This new system will use modern information technology to vastly improve the filter, sort and discovery functions of the current journal system.

Some of the alternative models reviewed by [Moylan \(2014\)](#) include open peer review (which OpenPsych employs; see also [Pöschl 2012](#); [Tattersall 2016](#)), collaborative peer review, and post-publication peer review (possibly in combination with the sort of online archival system recommended by [Brembs et al. \(2013\)](#); see F1000Research).

Notwithstanding these different alternatives, one practice that essentially all of those concerned about the current state of academic publishing espouse is data sharing. In a well-known study, [Wicherts et al. \(2011\)](#) analysed the results of 141 papers in four top-ranked psychology journals, and found that reporting errors were more common in papers whose authors were unwilling to share their data. In a more recent study using slightly different methods, [Nuijten et al. \(2018\)](#) did not find any relationship between willingness to share data and the prevalence of reporting errors. Yet in spite of their null finding, these authors were at pains to emphasise the importance of data sharing:

Some of the greatest advantages of sharing data include, but are not limited to, the possibility to run secondary analyses to answer new questions, verify analyses of published work or examine the robustness of the original analyses, and compute specific effect

sizes for meta-analyses... On top of that, sharing data upon request is not robust to time: how likely is it that the data are actually still available after ten years? Or fifty? Or even longer?

Recall that mandatory data sharing is one of the core features of the OpenPsych journals. To the extent that data sharing represents the single most reliable way to discourage selective reporting, ensure replicability and facilitate additional research ([Smith & Roberts, 2017](#)), the OpenPsych journals are arguably more in keeping with current open science best practices than many traditional journals. Along with a number of other researchers ([Brembs et al., 2013](#); [Moylan, 2014](#); [Smith, 2016](#); [Amaral, 2018](#); [Nuijten et al., 2018](#)), we believe it is high time for the scientific community to consider alternative models for publishing, including (among others) the model on which the OpenPsych journals are based.

3 Conclusion

The OpenPsych journals were founded in 2014 by Emil Kirkegaard and Davide Piffer due to their frustration with existing journals in differential psychology and behavioural genetics. Since then, they have been subjected to criticism in both online articles and unpublished offline discussions. This editorial has responded to five criticisms that have been levelled at the journals. We began by briefly responding to each of these criticisms individually. We proceeded to argue that there are a number of important limitations to the traditional model of peer review. First and foremost, it has failed to prevent the so-called replication crisis. Indeed, the traditional model seems to be afflicted by low inter-rater reliability among reviewers, weak error detection, opportunities for plagiarism, and various forms of bias. In addition, studies typically find that measures of methodological quality are unrelated or even inversely related to journal rank. Our overall conclusion was that, insofar as the traditional model of peer review does not guarantee research quality, the scientific community should be willing to consider alternative models for publishing, including (among others) the model on which the OpenPsych journals are based.

Of course, the traditional model of peer review is just one way of communicating new theories and findings to other researchers. Insofar as it serves this function, it affords certain benefits, and comes with certain costs. Crucially, it should not be conflated with what science is. A positive claim about the nature of the world does not go from being 'non-science' to 'science' the moment an article is accepted for publication. In fact, science proceeded successfully for

several hundred years prior to the introduction of the modern peer review system (Annesley et al., 2017). Moreover, the full landscape of scientific communication is much broader than what is published in traditional journals. It comprises activities as diverse as tweets, blog posts, reports, working papers, preprints, conference presentations, and even informal conversations with colleagues. We would argue that the OpenPsych journals occupy a useful place within this landscape. Going forward, we hope that more researchers interested in differential psychology, behaviour genetics and quantitative sociology will consider reviewing for or submitting to the OpenPsych journals.

Author contributions and acknowledgements

Carl and Kirkegaard wrote the first draft of the paper. Other authors provided constructive criticism, and approved the manuscript prior to publication. We would like to thank Jonatan Pallesen for comments that improved the manuscript.

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