

Review of: "Facilitating Constructive Criticism of Established Scientific Paradigms"

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Potential competing interests: No potential competing interests to declare.

Overall, I think there are some good points in the paper, but I think the argumentation and presentation need work. See my specific comments below.

As I read this paper, I am struck by the question, who is this paper for? As it's written, it speaks to a philosophical audience rather than a scientific one. That may be a deliberate decision on behalf of the author, which I have no objection to. If, however, there is a desire to speak to practicing scientists across a range of fields, I think the manuscript needs to be re-styled so that it can be digested by that audience. I make some specific suggestions that could assist with that below:

- Remove or explain philosophical terms or jargon. Examples: specific terms used by philosophers (normal science, extraordinary science, hard core, research programme, paradigm), epistemic, fudge factors, Cartesian doubt, hypothetico-deductive
- The way the manuscript is written assumes that readers appreciate the significance of people like Kuhn, Lakatos, and Popper, and they are quoted as authorities. For a scientific audience, I think you need to assume that your readers don't know who Kuhn and Lakatos are and that they have a misconception of what Popper was saying. I think it would be better to interpret what they've said yourself and cite them – rather than trying to quote from them directly and assuming that your audience can integrate that information.
- Stylistically, I would also recommend including fewer quotes – it's just not how scientific papers typically present information, in my opinion

Separately, I am concerned that the author states that peer review works well for normal science. I would very heartily refute this. As, I believe, would the editors of Qeios. In my opinion, peer review performs a gatekeeping role that has almost no relevance to the quality of research being conducted (see literature on questionable research practices, publication bias, etc.). I believe that the author needs to temper the assumption that peer review works for normal science – and just focus on evidence and reasoning about how it is worse for extraordinary science and how this can be addressed.

When presenting evidence by Siler, Lee, and Bro, I would like the author to discuss the limitations of using citation count of rejected papers as evidence that journals are excluding impactful studies. Two issues that readily come to mind are that papers receive more citations based on where they are published (e.g., if you published the same paper in Science and

PLoS One, the Science paper would be cited more), and the process of an article being reviewed and rejected might actually increase the quality or impact of the article – so the final published paper may not be meaningfully the same as the rejected paper.

The author touches on the fact that it might be reasonable to be skeptical of divergent thinking – because often it has faulty underpinnings. But I would like to see more discussion of what the ideal would be here. I would contend that implausible findings warrant a higher degree of scepticism and scrutiny than plausible findings. However, I agree that this is problematic in an unstructured peer review context where reviewers usually just provide one global judgement plus feedback – this conceals the complexities of the review circumstance. If, for example, there were separate judgements assigned to methodological adequacy and calibration of conclusions to evidence, it might provide a more reasonable view of the article's merit.

The case study of the author's experience trying to publish an article and being rejected on unfair grounds feels like it has too much emotion in it for me. I feel that the author's obvious frustration makes it hard to give full merit to his arguments. I suggest asking a colleague to rewrite this section to take the heat out of it. As it stands, I find it hard to shake the feeling that this piece is more a reaction to a personal slight than the discovery of a shortcoming of the peer review process.

Lastly, I think that the peer review for the extraordinary science section needs some reworking. This is what the whole article has been leading up to, but the order and emphasis of the writing seem confused. I think the first 4 paragraphs could basically be removed, and the article could move straight into a proposal for the journal, highlighting specific elements that link back to the author's arguments about shortcomings in the peer review system.

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