Facilitating or Allowing well-founded criticism of established scientific paradigms

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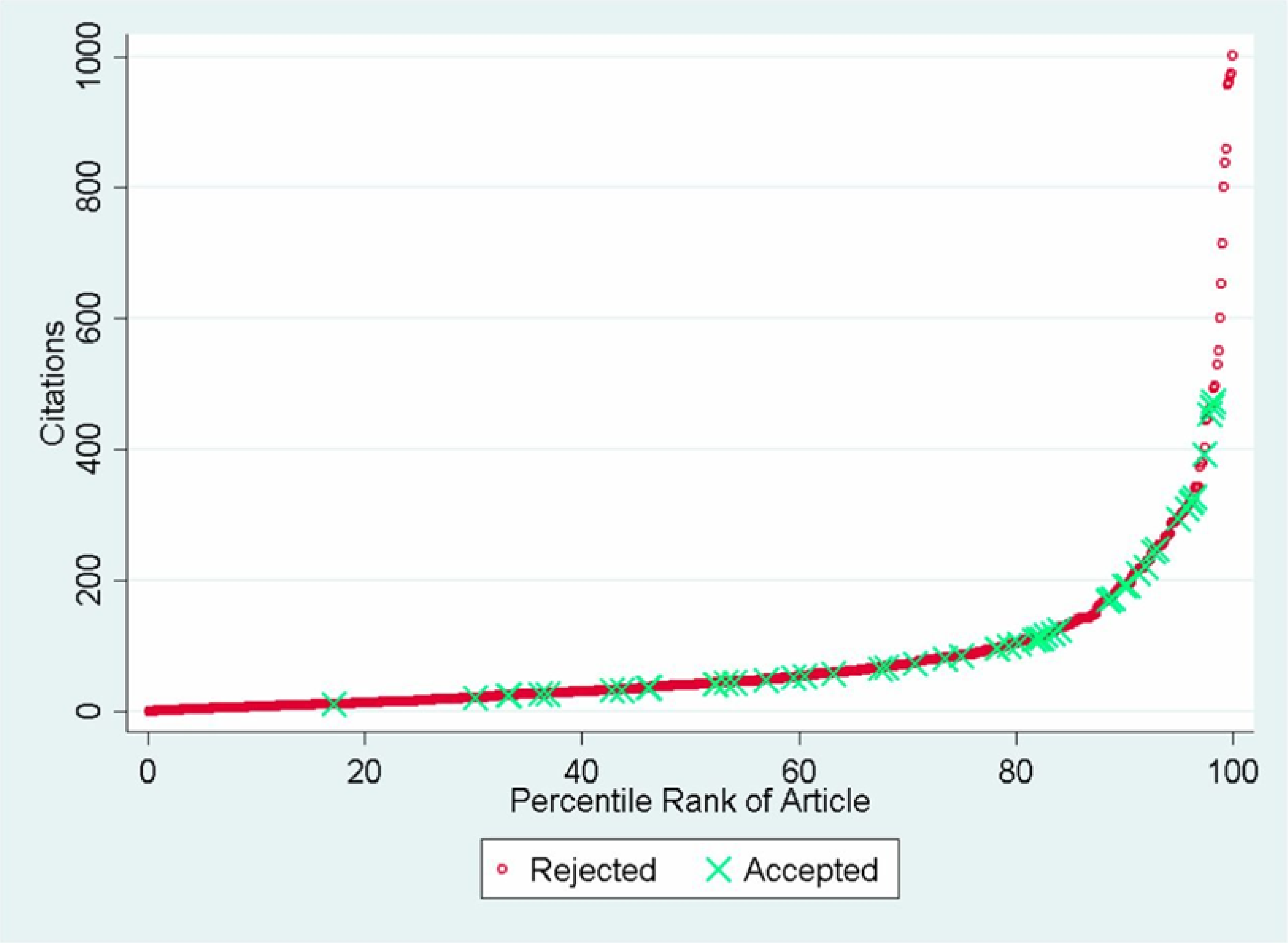
Allowing well-founded criticism of established scientific paradigms

I plan to start a publication platform with the title “*Well-Founded Extraordinary Science*”. It is to publish articles from different fields of science together with the review reports and the authors’ responses.

It is meant to solve a serious problem. Scientific innovations often struggle to pass through peer review and regularly fail under certain circumstances. By “innovation” I mean any process and result of innovative thinking.

Figure 1. Citation distribution of accepted and rejected articles originally submitted to three elite medical journals, from Siler, Lee & Bero (2015).



The effectiveness of *scientific gatekeeping* has been investigated by considering manuscripts originally submitted to three elite medical journals. Some were accepted, others rejected. Here, the number of citations eventually acquired by each article is plotted against the rank order of this number expressed as a percentage. The rejected manuscripts (in red) were later published in a more common journal. 200 of 1008 articles remained unpublished.

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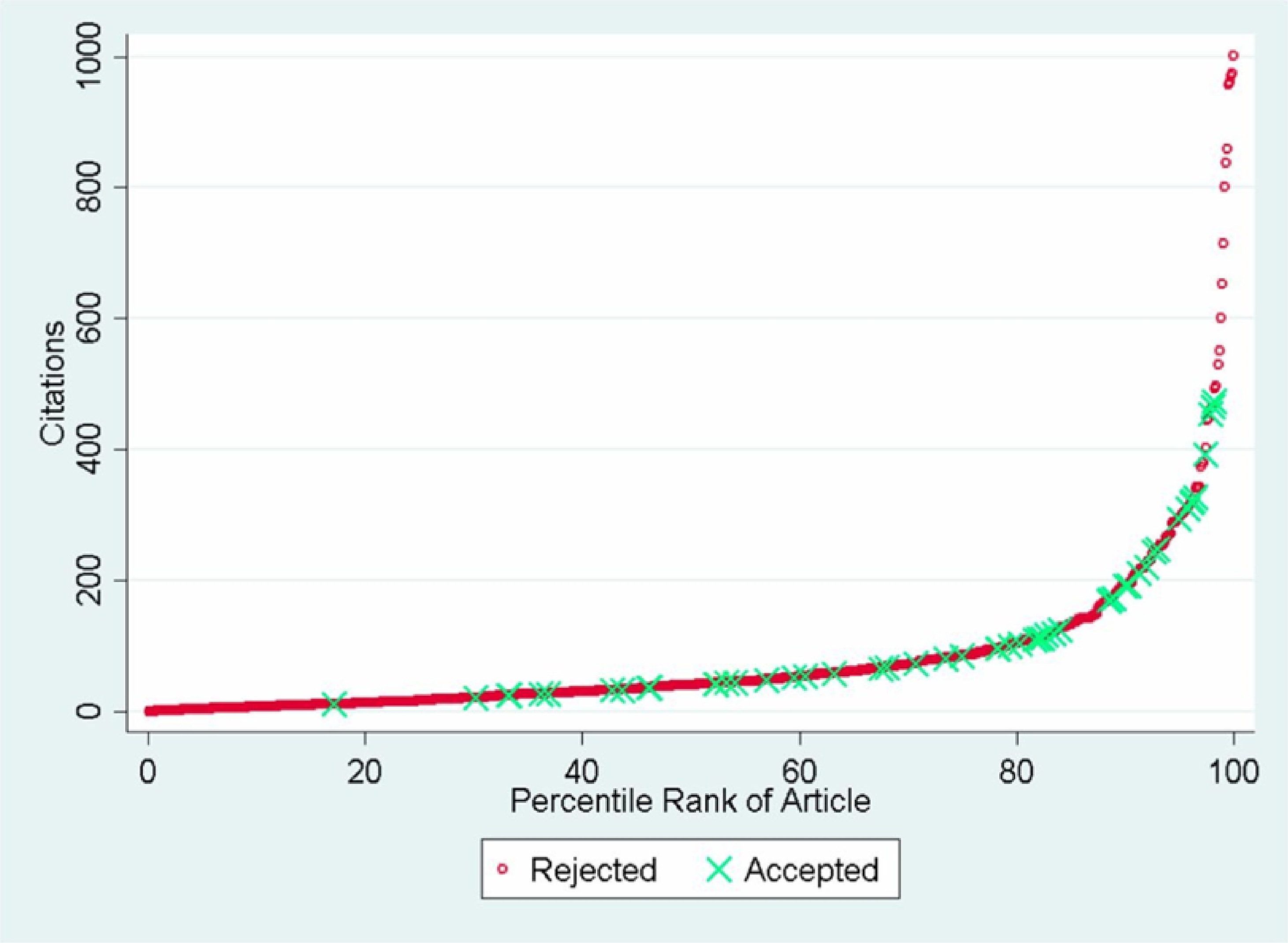

Here, traditional gatekeeping appears, on the one hand, to work well by excluding (with only one exception) the least cited 30% from publication, but it works, on the other hand, alarmingly poorly by also excluding all the 14 eventually most cited articles. Of these, 12 had been deemed as unworthy of peer review by editors. The most impactful ideas and research are often too innovative in the eyes of editors and reviewers.

The most serious problem is this: In its theoretical aspects, science is believed to progress through criticism and falsification, but where a dominant theoretical framework is established, expert reviewers practically only allow its *completeness* to be questioned. If an innovative manuscript is rejected by all experts although its reasoning is well-founded and correct, peer reviewing fails totally.

The preconditions for this were already described by Thomas Kuhn (1962) in *The Structure of Scientific Revolutions*. Imre Lakatos (1970) went a little deeper in his analysis, in which he considered “research programmes”, each of which had a “hard core”. This core, like Kuhn’s “paradigm”, consists of those principles and assumptions that are taken for granted by the members of the respective research community and are normally considered inviolable.

Although periods of normal science can be followed by revolutionary stages, the present sociological conditions work against a successful, progressive development of this kind. They work for what Lakatos would have called a degenerative development, characterized by the introduction of ad hoc modifications or “fudge factors” in order to protect the core theory from falsification.

An example of a fudge factor in standard cosmology is the supposed presence of “dark matter”. This has created a large number of speculations and expensive long time experiments, which all failed.

Criticism, even if fatal, can be turned a blind eye. This blocks fundamental scientific progress. An article about cosmology with published reviews highlights this issue. There, a critical evaluation is suppressed despite evident contradictions in the standard approach.

I argued that standard cosmology is not a unified theory. It contains conflicting models and assumptions. I highlighted two issues:
(a) the "relic radiation blunder", where free radiation is treated as expanding with the universe without considering that it escapes from its source at the speed of light, and
(b) a "cosmometric contradiction", which consists in accepting that the universe was already as large as it is today during an earlier and much smaller phase - that it was much larger when it was much smaller.

Four (of seven) cosmologists who rejected subsequent versions of my article just restated certain features of the standard model without even mentioning the issues (a) and (b). There were only two reviewers who accepted my reasoning, but these were not typical cosmologists. Another one rejected my article because I did not offer an alternative.

As a countermeasure against similar cases, *Well-Founded Extraordinary Science* accepts only well-founded studies that go against established assumptions, theories or methods. Besides criticism of established paradigms, it promotes innovations or ‘revolutions’ that are well-founded and parsimonious.

It deserves to be said that nearly all proposals of alternatives in cosmology that do get published fail to be well-founded. They represent merely imaginable physics.

In *Well-Founded Extraordinary Science*, reviewers are guided to check each claimed deficiency and innovation, whereby their confirmation bias is bridled. It is also acceptable to present criticism without elaborating innovations and innovations without elaborate criticism of the established framework.

Checklist

Write a short summary of the article and check the following questions. Authors can also consider this checklist and structure their article in a way that facilitates for reviewers.

1. Is the manuscript essentially critical of or incompatible with a research program, paradigm, theory, model, assumption or method that is established in the mainstream or in a considerable sidestream of science (yes **/** no)

   Here, a “no” keeps the manuscript out from this medium. If “yes”, reviewers can only proceed if they step out of their default mode, in which they might quickly reject any manuscript of that kind. Please proceed if “yes”.

2. If criticism of established detail is in focus, is this criticism convincing, does it need minor or major revision, or does it need to be rejected on grounds that are not criticized by the author?

   By having to evaluate the authors’ points of criticism (or innovations), reviewers are brought to focus on the claims in the manuscript, rather than on their own prior perspective.

3. If theoretical innovations are proposed, are these sufficiently well-founded and parsimonious for advancing science, and free from logical fallacies, or do they need to be thought over or rejected on grounds that are not criticized by the author?

4. Are clarity, novelty and academic quality of the manuscript sufficient as is or expectable after proposed minor revisions or are major revisions required?

5. Is the claimed practical impact reasonable?

   This is only crucial for methodological innovations. It is the purpose of advancing research that shall be decisive for the rating here.

6. Do you agree to reveal your identity? (yes **/** no)