COMMENTARY

Facilitating Constructive Criticism of Established Scientific Paradigms

Hartmut Traunmüller[1]

1 Stockholm University

Funding: No specific funding was received for this work.

Potential competing interests: No potential competing interests to declare.

Abstract

Scientific innovations often struggle to pass through peer review and regularly fail under certain circumstances: 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. Falsifications are frequently bypassed by fudge factors. 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. As a countermeasure against similar cases, a new open access medium "*Well-Founded Extraordinary Science*", with published reviews, is proposed. It aims to promote epistemic progress by challenging established wisdom. It accepts only studies that go against established assumptions, theories or methods and advances innovations or 'revolutions' that are well-founded and parsimonious. Reviewers are guided to check each claimed deficiency and innovation, whereby their confirmation bias is bridled. It is acceptable to present criticism without elaborating innovations and innovations without elaborate criticism of the established framework.

Hartmut Traunmüller

Department of Linguistics, Stockholm University

(Preprint of a research-based editorial of a planned journal, 2024-08-23)

Keywords: peer review, extraordinary science, scientific innovations, innovation-blocking, epistemic progress, methodological skepticism.

Introduction

It occurs that researchers come up with an innovation that is incompatible with what Thomas Kuhn (1962) called “normal science”, as opposed to “extraordinary science”, which goes against what Imre Lakatos (1970) called the “hard core” of a “research programme”. 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 it is widely accepted that, in its theoretical aspects, science progresses by criticism and falsifications (Popper 1935), it is a fact that experts only allow the *completeness* of an established theoretical framework to be put into question in traditional peer review. This attitude prevents objective evaluations and blocks fundamental advances in areas of science where a single but not fully well-founded paradigm or research program dominates. Although Kuhn noted that 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 call a degenerative development, characterized by the introduction of ad hoc modifications in order to protect the core theory from falsification. The subsequent occurrence of innovation-blocking is a threat to the epistemic progress of science that has become increasingly evident during the past century. Something still needs to be done about this. In order to advance epistemic progress, objective evaluations need to be facilitated and well-founded innovations encouraged.

While speculative modifications of established doctrines abound in scientific journals, manuscripts that do not respect the hard core of the current research program in their field use to be immediately rejected by the editors of reputable and trusted journals, and if exceptionally not by the editors, then by reviewers established in the field. These are experts who can easily see deviations from orthodoxy and research practice, and it is considered their task to review manuscripts in the light of the relevant teaching and prior research in the field. The quality of peer review varies widely, but it works mostly quite well for normal science, to which the vast majority of all scientific studies belong. In cases in which authors think that their manuscript has been rejected for no good reason, they have the option of submitting it to another journal, where they may still have a good chance to get it accepted. However, as substantiated in the next two sections, ordinary peer review is normally *devastating* for studies that are critical of the established approach or that are downright revolutionary. All studies of this kind are normally rejected even by most journals with a below-average threshold for acceptance.

After a short review of previous studies of gatekeeping and the fate of scientific innovations in peer review, a fairly drastic case from cosmology will be mentioned. In this case, an objective evaluation of the standard approach is turned a blind eye to, even though it is easy to see that the approach is inherently contradictory. Based on this case and the systematic studies that will be mentioned, a proposal of how to advance epistemically progressive science despite peer review will then be presented. If the standard approach is not only criticized, but an alternative is considered, it is essential to keep in mind that epistemic progress requires an increase in the well-foundedness of crucial assumptions or, equivalently, a decrease in their number. The latter is parsimony.

Peer review and scientific innovations

In contrast to financial and social ‘conflicts’ or ‘competing interests’ that are often attempted to be avoided or at least

mentioned in peer review, it is normally neglected that experts also react in a biased way when the foundations of their trusted paradigm are put into question. Experts are subject to a natural confirmatory bias (confirmation bias, myside bias) in the peer review process (Mahoney, 1977). This holds not only in the reviewing of manuscripts but also in the evaluation of applications for funding. The extra willpower required to objectively evaluate aberrant 'heretic', 'revolutionary', or likewise 'counter-revolutionary' reasoning is rarely summoned. The circumstance that nearly all such approaches come from outsiders supports a negative prejudice. This arises because faulty reasoning is understandably overrepresented in studies by outsiders. For a reviewer, it would also take courage to ignore the conformity expectations that exist within the profession. This is why fundamental deficiencies likely to exist in all sciences that are still developing tend to be overlooked and perpetuated.

Since the expertise of experts is limited to the knowledge they already have and to procedures they are familiar with, innovative ideas have in general a hard time in peer review, typically already in the first step, which consists in having a manuscript deemed worthy of peer-review by an editor. Mahoney (1977) summarized his experimental study of confirmatory bias, in which all manuscripts were sent to reviewers, like this: "*In the present study, 75 journal reviewers were asked to referee manuscripts which described identical experimental procedures but which reported positive, negative, mixed, or no results. In addition to showing poor interrater agreement, reviewers were strongly biased against manuscripts which reported results contrary to their theoretical perspective.*" Mahoney considered confirmatory bias as an ironic feature because, following Popper, successful experiments have no necessary bearing on the truth status of a tested theory or hypothesis – only negative results have.

In a multi-disciplinary investigation of various forms of peer review, Tennant et al. (2017) conclude that peer review *"protects the status quo and suppresses research viewed as radical, innovative, or contrary to the theoretical or established perspectives of referees, even though it is precisely these factors that underpin and advance research."* This is in accord with Armstrong (1997): "*Current procedures to assure quality and fairness seem to discourage scientific advancement, especially important innovations, because findings that conflict with current beliefs are often judged to have defects.*". Horrobin (1990) stressed that peer review requires both quality control and the encouragement of innovation, while Benda & Engels (2011) confirmed that "*tension exists between peer review and innovative ideas, even though the latter underlie scientific advance.*" In a wider perspective, Alvesson & Sandberg (2014) distinguish between boxed-in and less common box-breaking research, among which the first mentioned tends to generate a shortage of novel and influential ideas.

Campanario (2009) reviewed and discussed instances in which 19 future Nobel Laureates in Physics, Chemistry, and Physiology or Medicine experienced resistance towards their discoveries from the scientific community, in particular cases in which manuscripts that dealt with discoveries that later would earn them the Nobel Prize encountered resistance in the refereeing process or outright rejection by referees and/or editors. There is a prior study by Campanario & Acedo (2007) about the views of authors who published highly cited articles and encountered resistance to their discoveries from other scientists. Although the arguments given against problematic articles varied widely, the underlying obstacle was most commonly a clash with established wisdom or, rarely, a lack of practical import. Most other arguments appeared specific

to particular reviewers and so less problematic because these can mostly be circumvented by submission to a different journal.

Siler, Lee & Bero (2015) studied the effectiveness of *scientific gatekeeping* by considering the numbers of citations of articles that were published in a different journal after having been rejected by the elite medical journals *Annals of Internal Medicine, British Medical Journal*, and *The Lancet*. Among the initially rejected articles, there were many that were not often cited but, surprisingly, all the 14 subsequently most often cited articles (808 of 1008 were eventually published) had also first been rejected by one (in one case by two) of these journals. Of these 14 articles, 12 had been deemed as unworthy of peer review by editors. This makes it clear that editors of elite journals tend to fail in recognizing the most impactful ideas and research, at least within the medical field. However, the investigation does not suggest that the result would have been much different if expert referees had been asked in each case: of the 5 most cited articles, 3 had in fact been rejected after peer review. If the number of citations garnered is taken as the criterion, traditional gatekeeping appears, on the one hand, to work well by, in this example, excluding (with one exception) the eventually least cited 30% from publication, but it works, on the other hand, alarmingly poorly by consistently excluding all the 14 eventually most cited articles (see Figure 1). Honest applications for research grants end up similarly: the system eliminates the best together with the worst, and this may be more inescapable there (Horrobin, 1996).

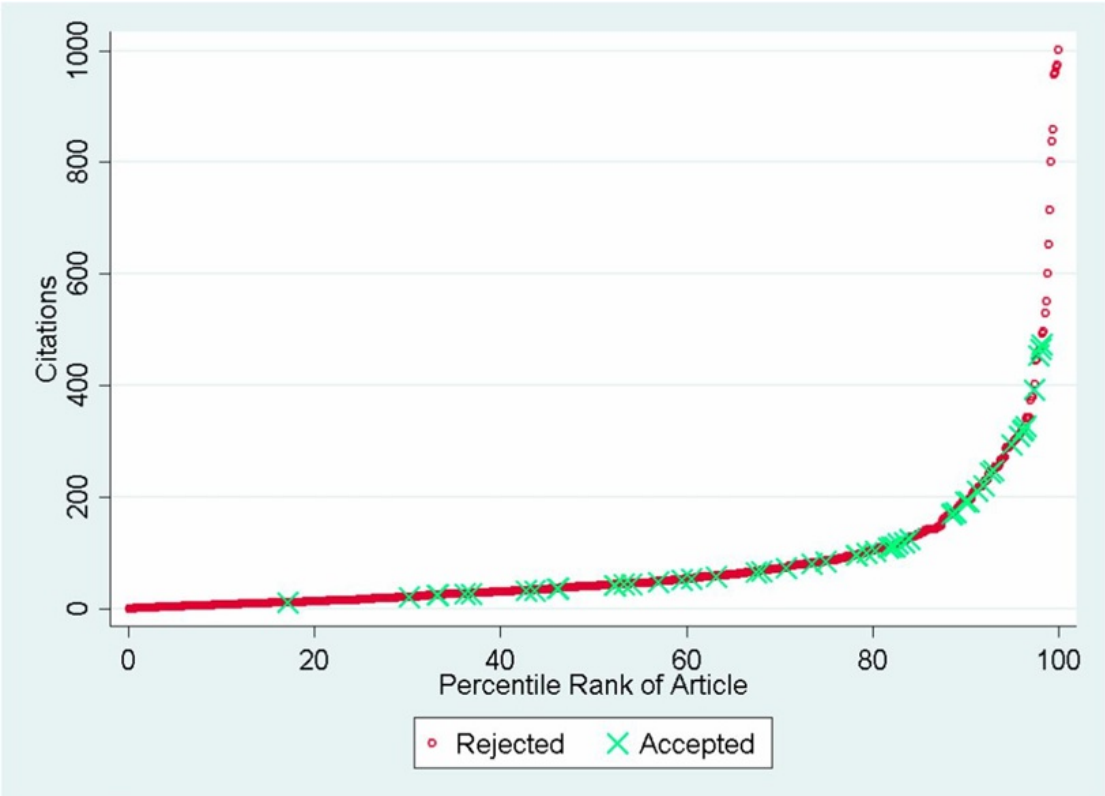

Figure 1. Citation distribution of accepted and rejected articles originally submitted to three elite medical journals, from Siler, Lee & Bero (2015), with permission.

Siler & Strang (2017) surveyed 52 scholars who had recently published articles in another field, in *Administrative Science*

Quarterly. They observed that papers that challenged established perspectives faced distinctly more criticism than those that offered a new perspective and, not unexpectedly, that also papers that extended or combined established perspectives were less criticized and changed. Challenges to established wisdom evoke most opposition.

The risk for becoming confronted with important innovations that one fails to recognize as such is likely to vary between different fields of science. It can be lower in narrow, mature and well-founded fields, where traditional peer review may serve its purpose well. However, like other wider fields of science, also physical cosmology and modern theoretical physics as a whole are still far from completely well-founded. Otherwise we would know by now why dimensionless physical constants, such as the fine structure constant or the proton-electron mass ratio, have the numerical values they have.

It is thought provoking that, in contrast with the increased number of publications, fundamental progress has become rare in the mentioned fields since peer review became general in the middle of the last century. Chu & Evans (2021) show that canonical progress is slowed in fields of science with many publications. Peer review is certainly a factor in this. Progress is now more often tool-driven than concept-driven. Where corrective innovations appear required, fudge factors have found acceptance. These are ad hoc excuses for observations that do not fit the model, like ‘exotic dark matter’, ‘dark energy’ and ‘cosmic inflation’. These belong to “fairy tale physics” (Baggott, 2013) like ‘string theory’ and ‘strangelets’. For a history of peer review and its alledged crisis, see Horta & Jung (2024).

A case in point

Although I am professor emeritus in auditory phonetics, I have now turned my interest mainly towards how science is pursued in physical cosmology. In my latest study in this field, I am critical of certain assumptions that are taken for granted by the vast majority of cosmologists, adherents of the Big Bang paradigm, although it is not difficult to see that the assumptions involve blatant contradictions. These are deficiencies that are clearly worse than fudge factors. However, attempting to reveal the deficiencies of a firmly established paradigm in a scientific article has, up to now, been among the most unrewarding tasks in science.

In my manuscript, I point out that standard cosmology is not a unitary approach but involves an obviously false assumption and conflicting models side by side. This shows itself in what I call (a) “the relic radiation blunder” and (b) a “cosmometric contradiction”. In (a), free radiation is treated as if it expanded with the universe *while disregarding that it escapes from its source at the velocity of light* (!) and in (b), it is accepted and taught that the universe must already have been as large as it is now *when it was still much younger and smaller than now*(!).

First, I made several attempts to get my manuscript published in a mainstream journal. It was each time, with one exception, immediately rejected by editors. The explicit motivation was a different one from each journal, mostly not appearing sincere. The exceptional journal, *Foundations of Physics*, acquired comments from two anonymous reviewers. Each of them recommended rejection. The first one lacked an account of a type of scattering of the cosmic microwave background radiation, although my critical reasoning begins only when no further scattering of this radiation is said to

occur, about 380 000 years after the zero point of cosmological time. The other one correctly summarized my claims without objection but, in effect, advised the editor not to trust an outsider like me. This looks rather like a response by somebody who *lacks* expert knowledge.

After considering and rejecting other media and after delays for less relevant reasons, I posted the article (Traunmüller, 2023) in *Qeios*, where articles appear together with their reviews. The first *Qeios* version of my article was rejected by four referees who in different ways just highlighted the established paradigm and its inviolability. No one argued specifically against the existence of the relic radiation blunder or the cosmometric contradictions I focus on. Most referees nevertheless must have been dissatisfied with the disclosure of these flaws or felt obliged to reject it because it is at worst allowed to question the *completeness* of the theory. However, this restriction clashes with the requirements of science, where discourse about realities and phenomena needs to be kept rational and preferably well-founded (or 'well-grounded'), and parsimony is desirable.

The first version of my article has not, in fact, been reviewed by any of the ten experts I had suggested first and judged capable of objective review. They seem to prefer not to express themselves openly. Some might not do it anonymously either. I gratefully acknowledge having received personal messages from some, but the readiness to serve as a reviewer can be expected to covary with the reputation of journals and to be much reduced for manuscripts that go outside normal science.

A second version has been reviewed and rejected by one invited referee who followed me a few steps before joining the orthodox referees. None of the referees said anything that would make the conflicts (a) and (b) vanish. In the third version, I named these already in the title, in order to give them more prominence. However, this version remained unreviewed, perhaps because *Qeios* presents it as if it had already been rejected by five. When I had complained about this rating policy, the *Qeios* team sent me a proposal for a new title and abstract as well as encouraging me to explain what the reviewers had misunderstood. I did this, but *Qeios* stuck to its prejudiced rating policy. The last version earned in any case a brief supportive review and a rejection motivated by the rejections of previous versions by other reviewers, but which nevertheless recommends "that the ideas mentioned are looked at again and understood". However, I agree with a reader in that the article itself is already excessively repetitive.

My article in *Qeios* was intended as a more systematic presentation of flaws that emerged in a prior open peer review publication, in *F1000Research* (Traunmüller, 2021), in which I asked whether standard cosmology really predicts the cosmic microwave background and answered with "not really". Each of the five reviews published there, obtained from invited reviewers, reflects primarily the reviewer's own prior perspective, in a more or less striking fashion, whether or not it is in line with the mainstream. This may have been caused by the structure of the article and the absence of sufficiently elaborate guidelines for reviewers. The guidelines are even less elaborate in *Qeios* and the quality of the reviews varies accordingly. Open peer review reports make the barriers against objective evaluations of established theories and against even well-founded innovations in science visible. The reviews of my *Qeios* article disclose the selective blindness of adherents that follows with doctrines and established practices. While such disclosure is a valuable trait of publications with accessible reviews, the prejudiced rating policy is a drawback of *Qeios*, which can also infect reviewers.

Peer review for extraordinary science

In a recent publication, Robergs, O'Malley & Torrens (2024) answer questions about the human flaws that compromise science. They favor Popper's view of Kuhn's distinction between 'normal' and 'extraordinary' science (Popper, 1970) and detail strategies for minimizing errors. The proposal presented here constitutes an important complement to this.

While Tennant et al. (2017) compared a range of different models for peer review of articles, none of these circumvents the innovation-blocking problem. Traditional peer review severely hampers epistemic progress in many fields of science. In order to avoid accumulating degenerative speculations and to return from defense of orthodoxy by fudge factors and even self-contradictions to epistemically progressive science, it would help if there was a journal, publication series or repository that focused on studies that go *against* established assumptions and approaches that are not well-founded. These are the studies that elsewhere face unjust but concordant resistance and rejection. In other cases of unjust resistance against innovations, such as in most of those mentioned by Campanario & Acedo (2007) and Campanario (2009), submission to another traditional journal may solve the problem. This is also what happened in all the cases shown in Figure 1. Innovations that are not in conflict with established teaching and practice would remain outside the medium proposed here, and tool-driven methodological innovations may rarely appear because these face less resistance as compared with concept-driven. Since this leaves very few potential contributions within each field of science, there will be room for contributions from all fields of natural science, but in this case an artificial intelligence will be required in order to find potential reviewers, who should preferably be experts in the criticized approach.

If it is shown that an established approach is deficient, it will be evident that an alternative is desirable, but authors should not be required to deal with this necessarily. Criticism alone must also be allowed. Any elaborated alternative needs to be more well-founded and/or more carefully thought out in order to contribute to the epistemic progress of science: Procedures and assumptions need to be contradiction-free, and all assumptions, at least all new and modified ones, should be rooted in proven experience and non-circularly in outside knowledge, so that they withstand methodological skepticism to a reasonable degree. Most articles in theoretical and cosmological physics inside as well as outside the mainstream do not withstand methodological skepticism. Such skepticism is similar to but not strictly equivalent with 'Cartesian doubt' because there must be room for inductive approaches. The pursuit of a postulate-based or hypothetico-deductive approach, which is prevalent in theoretical physics, is not enough for being advanced in this medium. Here, any assumption needs to be rooted in something deeper or more general in order to deserve advancement. Not having been falsified is not enough unless it is at least supported by an inductive argument. In this respect, the threshold for acceptance is higher here than in most traditional journals.

A sub-optimal theoretical framework can in principle not only be improved by eliminated internal flaws and more well-founded assumptions but also by a more parsimonious use of assumptions. This is using Ockham's razor. The epistemological value of an approach that requires fewer assumptions is higher and makes it worthier of advancement. The most comprehensive assumption is the universality principle, which says that *the same physical laws are valid everywhere in space, direction, and time*. This metaphysical principle expresses a precondition for physics. It is indispensable for there to be any 'law of nature' and any explanatory physics at all. Any less universal principle is less

parsimonious.

In order to fit all sciences, problems and perspectives, some room for variation in the definition of 'well-founded' must be allowed. While the requirement of well-foundedness will keep the number of acceptable articles small, these could have a good chance of finding attention and acceptance. In normal peer review, experts appear mostly to defend their trusted paradigm or to *remain silent*. The former is suggested by several systematic studies cited above and the latter at least by the mentioned experience with *Qeios*. In peer review adapted to extraordinary science, reviewers need to be guided so that their preoccupation with their prior view and, most importantly, their confirmation bias is bridled. This is done in the new medium "***Well-Founded Extraordinary Science***", in which articles are published together with their reviews and the authors' responses. It gives the hard cores of established research programs and any manuscripts critical of them a chance to be objectively reviewed and also offers room for well-founded innovations in science. It comes with an adequately ordered list of questions primarily intended for its reviewers but also for its authors.

In this medium, it is obligatory to be critical of an established conception, of one or several aspects of an established paradigm, theory or model. In order to facilitate an objective review, authors are required to be explicit about any essential assumptions, reasoning and procedure that intentionally deviates from the mainstream or from the specific criticized approach. Any deviations and innovative proposals need to be scientifically more well-founded or somehow more valuable than the criticized approach. Authors should attempt to show this convincingly. Disclosed deficiencies call for innovations, but it is acceptable to present criticism alone. It is also acceptable to present an innovative or revolutionary framework with just a summary comparison with the established one.

Bravo et al. (2019) investigated the effect of publishing peer review reports on referee behavior in five scholarly journals. They found that open reviews do not compromise participation if referees can protect their anonymity. They showed that it is mainly those who propose rejection who want to remain anonymous. However, this may be different if non-conformity prevails in the medium. While it is desirable to make all reviews and the authors' responses openly accessible, there can be reasons for not serving openly as a referee for a non-conforming article. In order not to miss potential referees, they are given the choice to remain anonymous with a slight reduction in the weight of their review.

Reviewers are asked to check the following questions, beginning with the first one:

1. Is the manuscript critical of or incompatible with a paradigm, theory, model, crucial assumption or approach that is established in the mainstream or in a considerable sidestream of science? (yes / no)
   Here, the answer "no" shall normally keep the manuscript out from this medium. In order to proceed, reviewers have to step out from the usual mode, in which they might quickly reject any manuscript that here requires the answer "yes". This medium is intended for well-founded studies of this kind. It shall also be open for criticism of articles that have been published in it. These can be treated as if representing considerable sidestreams. Positive commentaries and follow-ups can be acceptable in traditional media.
2. If criticism of established detail is in focus (preferably evaluate all points), can this criticism be considered as convincing, does it need minor or major revision, or does it need to be rejected on grounds that are not criticized by the

author? (not applicable / convincing / revise / revise / reject)

By having to evaluate the authors' points of criticism, reviewers are brought to focus on the claims in the manuscript, which may prevent preoccupation with their own prior perspective.

3. If theoretical innovations are proposed (preferably evaluate all), can these be considered as 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? (not applicable / yes / revise / revise / reject)
4. Are clarity, novelty and academic quality of the manuscript sufficient as is or expectable after proposed minor revisions or are major revisions required? (yes / revise / revise / reject)

   Suggestions for improvements to various aspects of the argumentation are welcome - specify section and passage. For each proposal, reviewers expect authors to react by accepting it as suggested, by modifications the authors find appropriate, or by a rebuttal. Reviewers who require major revisions or amendments here or elsewhere should be ready to check these in a new version.
5. Is the claimed practical impact reasonable? (not crucial / yes / revise / revise / reject)

   This is only crucial for methodological innovations, since it is the purpose of advancing science that is decisive for acceptance here, but your evaluation and suggestions are welcome in any case.
6. Do you agree to reveal your identity? (yes/ no) If not, it will only be shown to the editor(s).

Authors may consider this checklist when structuring their article and feel free to mention their own answers to question 1 and the applicable and crucial ones (mentioning all points) of 2, 3 and 5 in a concluding section (which also facilitates reviewing) or at least for themselves before submitting a manuscript to ***Well-Founded Extraordinary Science***.

References

- Alvesson M, Sandberg J (2014) Habitat and habitus: Boxed-in versus box-breaking research.*Organ Stud.* 35(7) 967–987.
- Armstrong JS (1997) Peer review for journals: Evidence on quality control, fairness, and innovation.*Sci Eng Ethics* 3(1) 63–84.
- Baggott J (2013) *Farewell to Reality: How Modern Physics Has Betrayed the Search for Scientific Truth*, New York: Pegasus Books.
- Benda WG, Engels TC (2011) The predictive validity of peer review: A selective review of the judgmental forecasting qualities of peers, and implications for innovation in science. *Int J Forecasting* 27(1) 166–182.
- Bravo G, Grimaldo F, López-Iñesta E. Mehmani B, Squazzoni F (2019) The effect of publishing peer review reports on referee behavior in five scholarly journals. *Nat Commun.* 10: 322.
- Campanario JM (2009) Rejecting and resisting Nobel class discoveries: accounts by Nobel Laureates.*Scientometrics* 81(2) 549-565.
- Campanario JM, Acedo E (2007) Rejecting highly cited papers: the views of scientists who encounter resistance to their discoveries from other scientists. *J Am Soc Inf Sci Tec.* 58(5) 734–743.

- Chu JSG, Evans JA (2021) Slowed canonical progress in large fields of science.*Proc Natl Acad Sci U S A.*118 (41) e2021636118.
- Horrobin DF (1990) The philosophical basis of peer review and the suppression of innovation*J Amer Med Ass.* 263(10) 1438–1441.
- Horrobin DF (1996) Peer review of grant applications: a harbinger for mediocrity in clinical research?*The Lancet* 348, 1293–1295.
- Horta H, Jung J (2024) The crisis of peer review: Part of the evolution of science*Higher Educ Q.* 00:e12511.
- Kuhn T (1962) *The Structure of Scientific Revolutions.* Chicago: Univ. Chicago Press.
- Lakatos I (1970) Falsification and the methodology of scientific research programmes. In:*Criticism and the Growth of Knowledge*. Lakatos I, Musgrave A (Eds.); London: Cambridge University Press, 91–196.
- Mahoney MJ (1977) Publication prejudices: An experimental study of confirmatory bias in the peer review system. *Cognit Ther Res.* 1(2) 161–175.
- Popper KR (1935) *Logik der Forschung: Zur Erkenntnistheorie der modernen Naturwissenschaft.*Wien: Julius Springer. English version (1959) *The Logic of Scientific Discovery.*
- Popper K (1970) Normal science and its dangers. In: *Criticism and the Growth of Knowledge*. Lakatos I, Musgrave A (Eds.); London: Cambridge University Press, 51–58.
- Robergs R, O'Malley B, Torrens S (2024) The missing hydrogen ion, Part-3: Science and the human flaws that compromise it. *Sports Medicine and Health Science* 6, 193-199.
- Siler K, Lee K, Bero L (2015) Measuring the effectiveness of scientific gatekeeping.*Proc Natl Acad Sci U S A.*112(2) 360–365.
- Siler K, Strang D (2017) Peer review and scholarly originality: Let 1,000 flowers bloom, but don't step on any.*Sci Technol Hum Val.* 42(1) 29–61.
- Tennant JP, Dugan JM, Graziotin D, Jacques DC, Waldner F, Mietchen D, Elkhatib Y, Collister LB, Pikas CK, Crick T, Masuzzo P, Caravaggi A, Berg DR, Niemeyer KE, Ross-Hellauer T, Mannheimer S, Rigling L, Katz DS, Tzovaras BG, Pacheco-Mendoza J, Fatima N, Poblet M, Isaakidis M, Irawan DE, Renaut S, Madan CR, Matthias L, Kjær JN, O'Donnell DP, Neylon C, Kearns S, Selvaraju M, Colomb J. (2017) A multi-disciplinary perspective on emergent and future innovations in peer review. *F1000Research 6*: 1151.
- Traunmüller H (2021) Does standard cosmology really predict the cosmic microwave background?*F1000Research 9*: 261.
- Traunmüller H (2023) Reevaluating cosmic origins: a critical analysis of relic radiation and cosmometric assumptions in Big Bang cosmology. *Qeios* ID: G61UFL.4.