Commentary

Facilitating Well-Founded Criticism of Established Scientific Paradigms

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This is a preliminary editorial of a publication platform "*Well-Founded Extraordinary Science*". It describes the problem this medium is meant to solve and presents the attempted solution. 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 ad hoc modifications. 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, this open access medium with published reviews promotes epistemic progress by challenging established wisdom. It 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. 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.

1. Introduction

This editorial contributes to "metascience" or the "science of science". It describes a serious problem in traditional peer reviewing and presents the solution offered by the publication platform "*Well-Founded Extraordinary Science*". The circumstances under which the problem arises were already described by Thomas Kuhn[1] in the book *The Structure of Scientific Revolutions*, which is familiar to

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researchers in various fields. Kuhn analyzed the evolution and progress of science in its sociological context. In his description, periods of "normal science", characterized by conceptual continuity of scientific paradigms and cumulative progress, can be interrupted by scientific revolutions or periods of "extraordinary science", during which new paradigms appear. Imre Lakatos[2] 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. This is however problematic.

It occurs that researchers come up with a criticism or an innovation that is incompatible with Kuhn's "normal science" and that goes against the "hard core" of a Lakatosian research program. In the present article, "innovation" refers to any process of and result from innovative thinking. This includes new concepts and theories as well as any inventions and revolutions. Although it is widely accepted that, in its theoretical aspects, science progresses by criticism and falsifications[3] (Popper 1935), it is a fact that experts only allow the *completeness* of an established theoretical framework (of a trusted paradigm or a followed research program) to be put into question in traditional peer review. This attitude prevents objective evaluations of critical studies and blocks fundamental advances in areas of science where a single but not fully well-founded paradigm or research program dominates. 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 call a degenerative development, characterized by the introduction of ad hoc modifications or "fudge factors" in order to protect the core theory from falsification. The subsequent occurrence of innovation-blocking is a threat to the epistemic progress (progress in knowledge and well-founded understanding) of science, which 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 approved and well-founded innovations promoted.

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 although some

researchers see it less favorably, it is mostly believed to work well enough 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 sections 2 and 3, 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.

Authors' academic rank and the prestige of their institution affect how much attention is paid to their publications. However, reviewers are often aware that such prejudices should be suppressed when evaluating an article. Some traditional journals also tell this in their guidelines for reviewers or try to avoid the bias by a double-blind procedure. If a lack of prestige is the reason for rejection, authors have a chance to get their article published by another journal. Review reports are inescapably biased also by various additional factors whose strength varies between reviewers. Here, we are only concerned with the bias that most consistently blocks Kuhnian revolutions and so the progress of science.

After a short review of previous studies of gatekeeping and of the fate of scientific innovations in peer review (section 2), a rather drastic case from cosmology will be mentioned (section 3). 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 in section 2, 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.

2. 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[4]. 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[4] 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.[5] 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[6]: "*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[7] stressed that peer review requires both quality control and the encouragement of innovation, while Benda & Engels[8] confirmed that "*tension exists between peer review and innovative ideas, even though the latter underlie scientific advance.*" In a wider perspective, Alvesson & Sandberg[9] 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[10] 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[11] 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 perceived 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[12] 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).

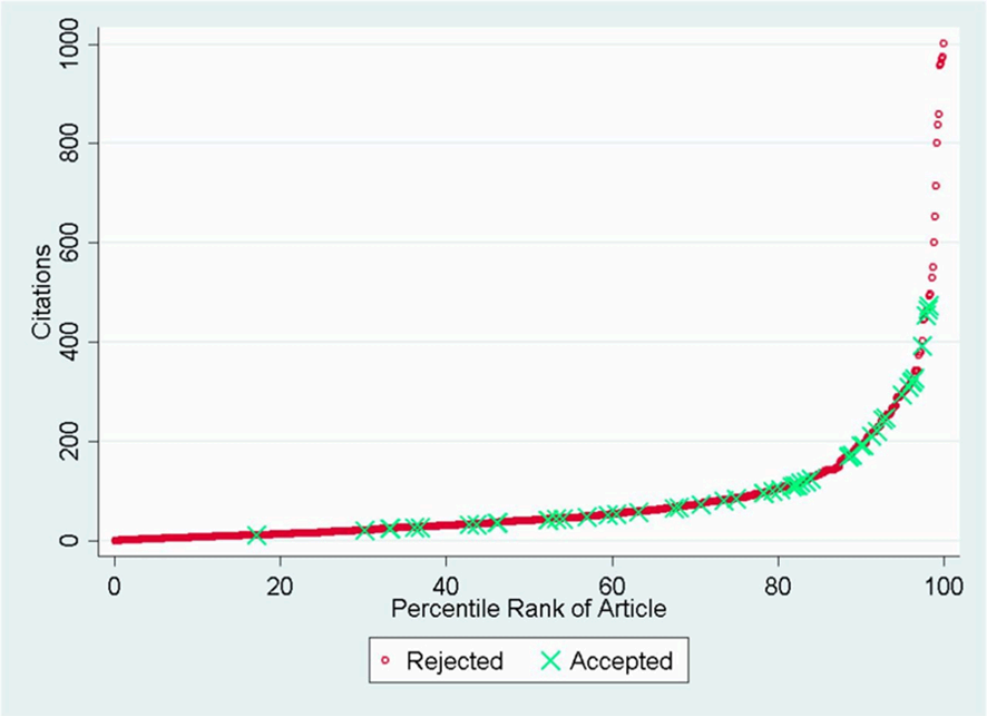

Figure 1. Citation distribution of accepted and rejected articles originally submitted to three elite medical journals, from Siler, Lee & Bero[12], with permission.

Concerning the tendency visible in Figure 1, one can consider two issues. First, the process of an article being reviewed and rejected can actually increase its quality or impact. However, this effect is likely to have been minimal here, since 12 of the 14 eventually most cited papers had been directly rejected by editors, and such rejections are rarely motivated in a way that would be helpful to the authors. Second, papers receive more citations if published in a journal with higher prestige, but since the most cited papers in Figure 1 had eventually been published in journals with lower prestige, this lends further emphasis to the visible tendency.

Honest applications for research grants end up similarly: the system eliminates the best together with the worst, and this may be more inescapable there[13] because future accomplishments can, strictly speaking, only be gauged on the basis of prejudices.

Siler & Strang[14] 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 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[15] showed that canonical progress is slowed in fields of science with many publications. Peer review is certainly a factor in this. Watve[16] independently expressed the view that the practice of peer review has hindered Kuhnian revolutions. 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”[17] like ‘string theory’ and ‘strangelets’. The hope that, e.g., dark matter might be a reality has led to very expensive and unsuccessful experimental searches.

3. A case in point

Although I am a professor emeritus in auditory phonetics, my current research interests have shifted towards the methodology of science in the field of physical cosmology. In my most recent study, I critically examine certain assumptions that are widely accepted within the Big Bang paradigm. These assumptions involve contradictions that deserve closer scrutiny. One can see deficiencies that are worse than ad hoc assumptions. However, challenging well-established paradigms in scientific publications has, during the last 60 years, been a distinctly unrewarding endeavor.

In my manuscript, I argue that standard cosmology is not a unified theory and contains conflicting models and an erroneous assumption. Specifically, I highlight two issues: (a) the "relic radiation blunder", where free radiation is treated as expanding with the universe, overlooking the fact that free radiation escapes 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.

Initially, I submitted my manuscript to several mainstream journals, where it, with one exception, was immediately rejected by editors. The explicit motivation was a different one from each journal, mostly not appearing sincere. The exception was *Foundations of Physics*, which sought feedback from two anonymous reviewers. The first reviewer criticized the manuscript for omitting a type of scattering of the cosmic microwave background radiation, though my criticism begins at a stage in cosmic evolution when no further scattering of this radiation is said to occur. The second reviewer accurately summarized my arguments but, in effect, advised the editor not to trust an outsider in the field.

After considering other options and encountering delays for various reasons, I eventually posted the article on *Qeios*, a platform that publishes articles together with their reviews. The first version of my article was rejected by four reviewers, who in different ways restated the prevailing paradigm addressing neither the relic radiation blunder nor the cosmometric contradiction. These referees may have been blinded by the established theory, felt hurt by the disclosure of the flaws or felt obliged to reject the reasoning because it exceeds questioning the completeness of the paradigm. However, each of these alternatives clashes with the requirements of science as such.

The first version of my manuscript was not reviewed by any of the ten experts I had suggested. These preferred not to engage publicly, possibly due to the manuscript's controversial nature. Some provided private feedback, for which I am grateful, but overall, the readiness to serve as a reviewer can be expected to covary with the reputation of journals (and platforms) and to be much reduced for manuscripts that go outside normal science.

A second version of the article was reviewed and rejected by another referee, who showed some open-mindedness but nevertheless aligned with the more traditional perspective. None of the reviewers addressed or refuted the specific issues I highlighted. In the third version, I made these more explicit by naming them already in the title. However, this version remained unreviewed. This may in part have been due to the platform presenting it as already rejected by multiple referees. When I raised concerns about this policy with *Qeios*, they suggested a new title and abstract and encouraged me to clarify what the reviewers had misunderstood. I followed this advice, but the platform maintained its prior rating policy, which can also infect reviewers (concluded from a review that was secretly

retracted). The last version of the manuscript[18] received, in addition to one review that opposed mere criticism, also two shorter supporting ones (by Chonghua Fang and Peter Chen).

My article in *Qeios* was intended as a more systematic presentation of flaws that emerged in a prior open peer review publication, in *F1000Research*[19], in which I asked whether standard cosmology really predicts the cosmic microwave background and answered in the negative. 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. This is a particularly valuable trait of publications with accessible reviews, provided that even unjustly rejected articles and their reviews are accessible.

4. Peer review for extraordinary science

The terms "revolutionary science" and "extraordinary science" were both used by Kuhn as opposites of "normal science". Popper[20] saw dangers in Kuhn's "normal science". In his mind, falsifications of hypotheses had a more central role than any "normal science". In a recent publication, Robergs, O'Malley & Torrens[21] treat a biochemical issue from sports medicine and answer questions about the human flaws that compromise science. They adopt Popper's view and detail strategies for avoiding errors. The proposal presented here constitutes an important complement to this, and Kuhn's attribute "extraordinary" most appropriately describes the first impression pertinent studies evoke in those who have some knowledge in the respective field.

While Tennant et al.[5] compared a range of different models for peer review of articles, none of these circumvents the problem that is our topic here. These models may be adequate for normal science, but they all fail for well-founded extraordinary science.

It would help if there was a publication platform or medium intended specifically for studies that go against established assumptions. These are the studies that elsewhere face unjust but concordant resistance and rejection. Manuscripts on discoveries and tool-driven methodological innovations may

rarely be submitted because these face less resistance as compared with conceptual criticism and concept-driven innovations. However, the studies need to be checked for crucial flaws.

If deficiencies are discovered in a doctrine or practice, this calls for innovations, but it is acceptable to present criticism alone. Deficiencies should not be kept secret for the reason that no convincing improvement is known. It is also acceptable to present an innovative approach with just a summary comparison with the established one.

If an innovation is elaborated, it must be more well-founded or 'well grounded' and/or more carefully thought out in order to advance science. Since the publication platform requests this, the title "***Well-Founded Extraordinary Science***" suggests itself for it. While the requirements will keep the number of acceptable articles within any field small, those that pass could have a good chance of finding attention and thereby raising the prestige of the medium.

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. Ad hoc assumptions and merely postulate-based studies, common in theoretical and cosmological physics, both mainstream and otherwise, do not meet this standard. In this respect, the acceptance threshold here is higher or more realistic than that in theoretical journals. However, in order to fit all sciences, problems and perspectives, some room for variation in the definition of 'well-founded' must be allowed. Theories can not only be improved by fixing internal flaws and making better assumptions but also by reducing the number of assumptions, following Ockham's razor. A theory requiring fewer assumptions has higher epistemological value.

In addition to the articles, it is planned to make all review reports and the authors' responses openly accessible. Since there can be reasons for not serving openly as a referee for a non-conforming article, referees shall be given the choice to remain anonymous with only a slight reduction in the weight of their reviews. Bravo et al.[22] investigated the effect of publishing peer review reports on referee behavior. They found that open reviews do not compromise participation if referees can protect their anonymity. (In their study, those who preferred anonymity were mostly the ones proposing rejection, but the conditions are modified if non-conformity prevails in the medium.)

In normal peer review, experts appear mostly to defend their trusted paradigm or to *remain silent*. Several systematic studies cited in section 2 as well as case studies in section 3 suggest the former, and at least the experience with *Qeios* in section 3 the latter. 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 ***Well-Founded Extraordinary Science***, in which reviewers are requested to follow the checklist in section 5, which hopefully brings them to proceed despite nonconformity.

In order to facilitate an objective review, authors are requested 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 should in some sense be scientifically more valuable than the criticized approach.

5. Checklist

Reviewers are asked for a short summary of the article and to check the six questions on the following list, beginning with the first one. Authors can also consider this checklist and structure their article in a way that facilitates for reviewers. They are welcome to provide their own answers to question 1 and the applicable ones of 2, 3 and 5 in a concluding section.

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 or is it critical of a previous article in this medium? (yes / no)

   Here, the answer "no" shall normally keep the manuscript out from this medium, which is intended for well-founded critical studies. If the answer is "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 your answer is "yes".
2. If criticism of established detail is in focus (preferably evaluate all crucial points), 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? (not applicable / convincing / minor revision / major revision / reject)

   By having to evaluate the authors' points of criticism (or innovations), reviewers are brought to focus on the claims in the manuscript, which may restrain preoccupation with their own prior perspective.
3. If theoretical innovations are proposed (preferably evaluate all), 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? (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 / minor revision / major revision / reject)

   Suggestions for improvements to various aspects of the argumentation are welcome - specify section and passage. For each proposal, each in a separate passage, reviewers expect authors to react by accepting it as suggested (authors may respond: “ϑ”), by modifications the authors find appropriate, or by a rebuttal. Reviewers who require major revisions here or elsewhere should be ready to check these in a new version.

5. Is the claimed practical impact reasonable? (not crucial / yes / minor revision / major revision / reject)

   This is only crucial for methodological innovations. It is the purpose of advancing research that shall be decisive for your rating here. However, your comments and suggestions are welcome concerning impact outside research as well.

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

   If “no”, your name will not be made public.

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