



# What if Peer-Review Process Is Killing Thinking-Out-of-the-Box Science?

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Scientific ideas and hypotheses may be accepted, refuted, criticized, or subject to rebuttal by researchers (e.g., Mayo and Spanos, 2008). Dogmas do not belong to the territory of science, and ending up in uncharted territories should be the most encouraging and hopeful achievement for a researcher (Lampel, 2016). However, modern science is quite different than the ideal image of a scientist in a lab coat looking for new results or staring to the horizon. The scientific world, instead of being supported by inspiration in search of new ideas or deep thinking, is now dominated by other aspects (Byers, 2014; King et al., 2018). Scientists are meticulously evaluated by their work, *i.e.*, scientific contributions, funded projects, student tuition, and outreach activities, among others (Bornmann and Marx, 2014). To be promoted or to get a permanent position, several “tick boxes” need to be filled, especially those concerning paper production and funding securing (e.g., Moher et al., 2018; Schimanski and Alperin, 2018). These are often disconnected from the genuine goals of science, such as producing out-of-the-box ideas that are generally difficult to spread and accept (Wang et al., 2017; Mairesse and Pezzoni, 2021) or subjecting previous proposals to empirical testing to explore their reliability, a type of task that Scheiner (2013) considered as deficient in ecology. Thus, most researchers spend their time trying to get the ball rolling in this environment (Milem et al., 2000; Link et al., 2008).

We are living in a zeitgeist where the number of scientific contributions is steadily increasing year after year, with hundreds of thousands of available journals to showcase our work (Larsen and Von Ins, 2010; To and Yu, 2020). However, this spectacular increase of production is not correlated to the number of new ideas, ways of thinking, or, at least, “discoveries” (Fried, 2020; Bhattacharya and Packalen, 2020). Slicing papers, multi-author contributions, revisions of reviews, meta-analyses, and empirically untested modeling approaches solely based on pure mathematics (Vora, 2018; Tolsgaard et al., 2019; Lee-Yaw et al., 2021) are on the spot, dominating the issues in elite journals (Ketcham and Crawford, 2007; Fontelo and Liu, 2018). So, the point is—why are we not developing new ideas in science as fast as we are publishing papers? Particularly, why are unconventional theories lacking despite the enormous scientific production?

Let us go to the current model of peer review and see a common situation of a flawed process (Smith, 2006) and how it affects the development of new ideas or theories—for example—a review or a paper with meta-analysis that compiles information from previous papers is a “shining diamond” for journals if the topic is sound and timely. This piece of science would receive loads of citations, which is so good for the journal and for the authors, even better if as contributors there is a

bunch of researchers that have been working on this topic. It is a win-win scenario for everybody. On the other hand, try to figure out the situation with a small group of researchers that have been brainstorming for a quite long time on an unconventional topic that might shock the pillars of orthodox hypotheses, principles, or theories that have been largely established on the targeted topic. These researchers may be entrapped into the Draconian peer-review filters to glory (Aarssen, 2012). The first filter might be the chief or associate editor, who firstly receives the submitted manuscript. If the manuscript is considered to have potential, it will be sent to the managing editor, and he/she will send it to several referees. They immediately notice that the manuscript is unconventional. They revise the piece several times, raising more questions than certainties. In their reports, these doubts would be pointed out, and the editor would have to take a decision after evaluating these reports and his own opinion. Guess what would be the final decision? Probably a rejection. Why? As Jubb (2016) pointed out, the peer-review procedure tends to be conservative and suspicious of unorthodox research or unexpected results. The reason is that the peer-review system is satisfactory during quiescent times, but not during a revolution in a discipline, when the establishment seeks to preserve the *status quo* (Peratt, 2016). Extraordinary claims conventionally require extraordinary evidence (ECREE or Sagan standard; Tressoldi, 2011), but even with the right mix of novelty and strong evidence to back them up, extraordinary claims also require receptive ears ready to pick up new proposals.

Let us contextualize this situation. Short-term metrics, *i.e.*, social impact, potential for citations, *etc.*, clearly support scientific productions that are “on the wave”. Authors in tune with the mainstream and publishing in high-impact-factor journals receive citations and lots of attention in social media that impact on the journal for it to start rising in the rank of the subject area. It is a win-win scenario: authors get recognition, increase their CVs, *etc.*, and the journal also wins. This produces a self-reinforced positive feedback loop, focusing our vision in an already known direction while hindering lateral thinking. This is in favor of dominant schools of thought, greater universities, as well as classic ideas and their well-established followers that suffocate other emerging proposals with their shadow, a persistent problem in ecology [see Simberloff (1980)].

However, what about a manuscript regarding a weird argument, idea, or theory? The short-term benefits for the journal publishing this manuscript may be limited and lower compared with those conventional and “successful” pieces of science. The urgency is overshadowing the important, and it is clearly shown in the field of scientific editorials where long-term important papers are increasingly sparse in top-tier journals. There are still meritorious exceptions of journals that are good candidates in terms of where to publish this kind of thinking-out-of-the-box contributions. Most of these journals are “far-fetched” when citations, journal impact, and other measures of “success” are pointed out. Fortunately, publishing in these journals is still prestigious in the targeted scientific fields where editors and reviewers are not aligned to the aforementioned metrics. However, these journals constitute an exception and

mostly are relegated to a “second-class wagon” of cutting-edge journals in terms of impact factor and, hence, citations.

Surprisingly, most scientists would tend to think that this issue belongs to the 20th century, yet it has been accentuated by the steady increase of published literature and the hard competency between research proposals (Gross and Bergstrom, 2019). The act of “going against the grain” leads directly toward a confrontation between paradigms whose spontaneous solution goes through generational change (Planck, 1950). As a result, many journal editors worry that reviewers will have more difficulty with new ideas than with run-of-the-mill ones, and it has been recorded that at least eight Nobel laureates were initially rejected by reviewers and editors (Weller, 2002).

The key problem is that the solution commented by Planck (1950) is currently too slow compared to the growing urgency of the problems facing civilization. One of the feasible short-term options to solve this could be to create a section within journals regarding “thoughts or testable hypotheses or theories” or, somewhat similarly, by promoting a framework to share unorthodox proposals. Non-conventional science needs to be considered in the top-tier journals, though not in the front page, but it might be helpful to give some visibility in order to promote critical thinking, rebuttal, or support of untested or non-conventionally tested ideas. The paradigm of the scientific method seems to be a bit straightforward and not flexible enough to incorporate out-of-topic ideas that may constitute scientific knowledge. Most journals offer a wide range of articles, though mostly are focused on current news or debates or responses to papers that receive so much attention. Hence, it seems not an issue for them to incorporate these pieces of science. Editorial boards have demonstrated flexibility in other aspects, *i.e.*, reduction of gender differences (Fox and Paine, 2018), birthplace (non-English natives), or nationality biases (Fox et al., 2016), mainly in urgent circumstances (Berenbaum, 2020). We sincerely believe that including non-conventional papers that may promote scientific discussion would be a step further to science.

A first step is to track the way of unconventional scientific contributions that may have the potential to get long-term citations after several years or even decades. It is well known that new ideas need time to thrive and to be assimilated by most of the scientific community. These contributions are not candidates to trigger substantial changes in the peer-review process and journal policies since they constitute a clear minority relative to the conventional bunch of papers that are published in scientific journals. Other issues that need to be considered are those regarding conventional editorial “filters”, such as “chaperone effect” (Sekara et al., 2018) or including top researchers since they could affect these new ideas/theories where not competitive scientists are involved. This aspect is potentially important since the number of internationally unrecognized researchers is overwhelmingly dominant compared to those that are known by the scientific field.

Strict compliance with the suggestions of the San Francisco Declaration (DORA, Declaration on Research Assessment, ASCB, 2012), the establishment in academic, scientific, and

publishing institutions of “originality assessment boards” made up of researchers, and mentoring new peer reviewers by experienced reviewers are other measures that could promote greater freedom of thought, not merely potential, in the research environment.

The aforementioned ideas would constitute an asset to start considering “those undiscovered gems” that need to be polished by the scientific scrutiny. However, the long-term goal is to systematically monitor the extent to which the current peer-review process is stuck (Aarssen and Lortie, 2009; Wardle, 2012) in order to facilitate and shed light on these sparse genuinely ground-breaking scientific contributions. There is an urgent need to promote more collaborative, transparent, and interdisciplinary communication regarding the peer review process (Hoffman, 2022).

From our perspective, additional editorial mechanisms need to be developed or, at least, improved to counteract the current trend. We are aware that a significant number of conventional and prestigious journals are struggling to manage the current number of submissions that they have, but these filters need to particularly evaluate the “potential” of these contributions to advance our whole understanding of medicine, environment, cosmos, *etc.* Slight changes in the peer-review process and editorial policies would facilitate spreading out the message of

something particularly novel or new. This is important, since other areas of knowledge, such as art, politics or history, have been shown to evolve by non-gradual steps that become significant tipping points, instead of by means of continuous shifts that are unavoidable in the long-term.

## AUTHOR CONTRIBUTIONS

RR (1st author) wrote the first draft and RRv (2nd author) revised and refined the revised version of the manuscript. Both authors developed the idea of this piece. All authors contributed to the article and approved the submitted version.

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