

Scientific journals should encourage, not hinder, debates about their published papers

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LETTER TO THE EDITOR

The revolution in electronic publishing now allows for papers to be continuously critiqued through letters to the editor, online comments, tweets and other means. However, established top-ranked journals still pose serious barriers regarding cultivation, documentation and dissemination of post publication critiques (1). To improve on this situation, Hardwicke et al. published a set of rules, one being for journals to actively encourage and highlight post publication critique to their readership. In this commentary, I present a case whereby the editors of a top ranked journal hindered the discussion/debate between authors and expert readers. Highlighting and publishing such cases will likely put pressure on journals to modify their current policies and actively encourage post publication review. Like Hardwicke et al., we believe

that post publication review is a major vehicle for advancing and accelerating science, by encouraging debates, resolving disagreements and revealing flaws in already published (and in many cases seemingly high-impact) papers.



INTRODUCTION

A recent paper (1) documented that only a small fraction of the 330 top-ranked scientific journals published critiques of their published papers. Undoubtedly, the minuscule number of post-publication critiques slows the advancement of science, by not actively engaging the readers and by not promoting healthy scientific debates. Those who want to raise concerns have very limited fora to express their opinions. Many journals publish “letters to the editor” to address reader’s comments and to give the opportunity for authors to respond. But not all journals encourage such policies, many have numerous restrictions and a few of them are unwilling to publish critiques, presumably for non-scientific reasons (see below). In general (1), published critiques are a rare, with only 2% of published papers being linked to a comment, but admittedly, this percentage differs between disciplines.

I here describe a case, with the hope of helping to catalyze changes, and put some pressure on journals to follow the Hardwicke et.al. recommendations (1), which I wholeheartedly sponsor. My vision is that debates can contribute decisively to scientific progress and should be encouraged.

CASE REPORT

In ’t Veld et al (2) presented in a top-ranked journal a method for cancer detection by using transcripts isolated from platelets exposed to cancer tissues. Since this paper was societally

consequential, public media invited me, as an expert, to comment on the validity and applicability of these findings in clinical practice. In parallel, I prepared a critique, indicating that it is unlikely for the described method to have value for early cancer detection (3). I used the sensitivity and specificity of the test, as mentioned by the authors, to calculate the positive predictive value (PPV) (the positive predictive value represents the chances of someone having cancer if the test is positive). The pretest probability of somebody having cancer was about 1% (which is equal to the prevalence of cancer in the screened population). I calculated that if the test was positive, the PPV was only modestly increased to about 3%, making the test unsuitable for practical applications

The finding of low PPV in cancer and other disease screening is a common deficiency (3). I also remind that 7 years ago, I drew attention that the same technology, may not be promising for cancer detection, for similar reasons (4). The mere fact that this technology did not as yet advance to the clinic, after an almost a decade, confirms that likely, it has important limitations.

I submitted my critique as a “letter to the editor”, carefully avoiding offensive language. The Editor-in-Chief (EIC) indicated that they discussed my letter and decided not to publish it. The Editor did not mention any specific deficiencies of my letter and did not question my PPV calculations, which, as mentioned, were based on the author’s data. I protested the decision and asked her to reconsider, or provide specifics as to why the letter was not acceptable (such as if it had calculation or other errors, offensive statements etc.). I also invited the EIC to review my letter externally, so that she formulates a better opinion. I did indicate that journals have an obligation to publish critiques of papers published, for the purpose of advancing science, finding the actual truth and informing the lay audience/non-experts, about

questionable “medical breakthroughs” (a major point made also by Hardwicke et. al.et al.) (1).

The EIC replied back with a negative answer. I was not surprised since in my 30-year career in publishing, I have never seen an editor changing a decision regarding rejection (although others may have different experiences).

In short, the editor asked me to take up the issues with the authors, in private, or in public, at scientific meetings that I may or may not attend. In essence, the editor shut the door for a debate. I believe that the action was inappropriate and hindered the advancement of science through a civilized scientific dialog.

In my deliberations with the Editor, I indicated that I have 30 years experience in cancer biomarkers and during my long career, like Ioannidis, I revealed many deficiencies of numerous technologies that have been touted as “revolutionary” in the past (5-8). These include the recent Theranos scandal (7). I thought that the EIC should have an excellent chance to initiate a debate as to the validity of the proposed test, between other experts, the authors and our group. Instead, the editor decided to shut down the discussion.

What other avenues do authors like us have, in order to challenge seemingly flawed papers and protect the integrity and avoid contamination of the scientific literature? One avenue would be to publish our critique elsewhere. In such occasions, where I tried to submit critiques related to papers published in other journals, I was justifiably told by the editors of these journals, that the best forum to publish our critiques are the journals that originally published the papers. In some cases, we did manage to get our opinions published in other journals including this incidence (3), with the hope that our opposition will be documented and be visible to interested audiences in the future, through PubMed searching.

Last but not least, it is worth examining why some top-ranked journals decide to block certain scientific debates related to papers that they publish. While some letters may be inappropriate for legitimate reasons, such as conflict of interest, this is an easily addressable concern since the editors have the opportunity to review the critiques externally and then decide. However, our belief is that editors of top-ranked journals, do not like to publish debates and possible flaws in papers that they thoroughly reviewed and finally decided to publish. They are likely concerned that their journals may lose some prestige if they are proven to occasionally publish flawed science that leads to misleading press releases or to retractions. But flawed papers, sooner or later, will prove to be wrong, even if some of them reach citation classic status. One paper we challenged in the past, received more than 3,000 citations (9) before it was shown by an independent validation by the Early Detection Research Network (EDRN) investigators to be flawed by bias (10) and after the authors received (undeserved) prestigious awards and lots of related grants.

We are well aware of many papers (maybe too many!) in the biomarker field which became citation classics and were considered valid for many years, before confirmatory experiments showed that they were flawed (6). Similar experience is shared by Ioannidis (8). In another communication we suggested, like Hardwicke et al., (1) that the outcomes of scientific debates should be published, in an effort to clean the literature from misleading information (5).

I congratulate the authors (1) for an insightful study on publication practices and debates in the scientific literature. I hope that my own commentary will help convince editors to encourage comments for their papers published, even if the comments are not congratulatory for their journals. In such case, the journals may seemingly lose some prestige but in essence,

they contribute to the advancement of science by finding the truth, in the long run.

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