# Review of: "The Contribution of the Rejection Mechanism to Scientific Knowledge Production: A View from Granular Interaction Thinking and Information Theories"

Lev Klebanov[1]

1 Charles University Prague

Potential competing interests: No potential competing interests to declare.

The paper under review discusses the significance of rejections of articles proposed for publication. I decided to write my review of this paper precisely because it will neither lead to its acceptance nor its rejection. Towards the end of the review, I will explain what I have said now.

The impetus for writing the review was the ambivalent impression the proposed publication made on me. More precisely, the issue of rejection of articles seems to me to be very important, but the principles for accepting or rejecting scientific work are not formulated enough. Therefore, an attempt to "bring order" to this area is undoubtedly of interest. However, I completely disagree with some of the author's conclusions, and the approach he proposes seems questionable to me. I will begin by nitpicking the author's words. The Abstract states: "Rejection is an integral part of the scientific publication process, acting as a filter for distinguishing reliable from less reliable scientific work." But for such a "filter" to work, the scientific level of the reviewers must be higher than the scientific level of the author of the rejected publication. Indeed, if we assume that the author is a conscientious scientist (and why should we assume the opposite?), then, when submitting a paper for publication, he is confident in the reliability and validity of the results he presents. This means that the doubts of less experienced reviewers are most likely erroneous. Of course, in extremely complex studies, the author may be mistaken, and reviewers may find an error. But in such cases, with rare exceptions, the qualifications of reviewers must again be very high. Unfortunately, many scientific journals use young scientists as reviewers who do not have a sufficiently broad outlook but are guided only by the "mainstream" in the relevant field. Thus, they cannot create the specified filter for the reliability and validity of the proposed results. The history of mathematics (and, I believe, other sciences) has many examples of erroneous reviews written by eminent scientists. Unfortunately, the author of the publication does not focus on these circumstances, although he notes "subjectivity can lead to the rejection of scientific papers that are useful and reliable, but do not correspond to the knowledge pool or the worldview of the editors and reviewers."
The matter is aggravated by the fact that, for the most part, reviewers are anonymous and therefore do not bear real responsibility for their reviews (i.e., for their reliability, credibility, and objectivity).

Now a few words about the use of Shannon's information theory in the context of the work. In my opinion, the process of its use is described incorrectly here. Namely, if the work with number k is rejected, then $p_k=0$. This simply means that the number of results n decreases by 1. This, of course, leads to a decrease in the value of H, and, of course, to maximize H, it is necessary to leave no more than one work (but H is not defined for $n=0$). The point is not that H is an entropy that needs to be reduced, but that science and scientists need not a smaller, but a larger spectrum of approaches to the problems being studied. If we offer only one point of view (one work), then we will refuse to study all the alternatives, and they may turn out to be better than the one approved for publication. It is very important to understand and remember that the published work will be studied by a large number of specialists and not a couple of reviewers. These specialists will undoubtedly understand everything themselves no worse than the reviewers, since no one obliges them to blindly believe the published results. If superficial results are accidentally published, they will simply remain unnoticed. I am not discussing the issue of payment for scientific results here, but it also cannot be entirely based on the opinion of anonymous reviewers. Thus, the use of the "informational" approach can lead us to a decrease in the possible number of research approaches. Thus, this approach is rather anti-scientific. It seems to me that such an approach has no real prospects.

I would not want my review to play any kind of prohibitive role. Let the readers themselves evaluate both the work on which this review is written and the review itself.
Lev B. Klebanov