

Editorial rejects? Novelty, schnovelty!

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Because many journals are currently increasing the rate of pre-peer-review editorial rejects, the editorial criteria upon which such decisions are based are very important. Here, I spotlight 'novelty' as a criterion and argue that it is a very problematic decisive factor at this stage of the editorial process.

Highly ranked primary research journals in ecology and evolution are increasingly burdened: the submission rates in many journals have doubled in the past 10 years or so. Not only does this mean that acceptance rates have dropped dramatically, it has also increased the workload placed upon reviewers to a point where it becomes more and more problematic to find those competent to carry out the task. Most journals in our field have met this dilemma with the same recipe: an increased rate of pre-peer-review editorial rejections of submitted manuscripts [1]. Although this most reasonable measure solves some of the problems that journals are facing, as it decreases the number of manuscripts sent out for in-depth peer-review, it also elevates the requirements on perspective, insight, and professionalism of editors and associate editors. A well-functioning editorial machinery that is able to reach wise and informed decisions is absolutely key for any journal that receives many more manuscripts than it can publish. The job of the gatekeepers of science is both necessary and important, but it is made more difficult as the gate narrows. Here, most primary research journals seem to have left associate editors with the two favorite editorial criteria [2,3] to guide their decision to either reject or send a submitted manuscript out for peer-review: (i) whether the topic of the manuscript is appropriate for the journal and (ii) whether the work is novel or not. Although the first is reasonable and often unproblematic, I suggest here that using novelty as an important criterion in pre-peer-review editorial screening of submitted manuscripts is very problematic for three related reasons.

Firstly, because an assessment of novelty critically depends upon a reader's knowledge and perspective, the degree of novelty is arguably more demanding than many other criteria that can be used in editorial assessments. Because single associate editors cannot be experts in every subdomain, which is required for an informed and fair assessment of novelty, this increases the rate of poorly informed and mistaken editorial decisions. It has been argued that the degree of arbitrariness in editorial decisions increases as submission rates go up [4,5], and I suggest that using novelty as a key criterion in editorial pre-screening aggravates this problem [1,6]. Secondly, because editorial machineries and authors 'co-evolve' [5],

the novelty criterion has led to many authors desperately striving to delineate their own unique corner of the scientific territory, sometimes reminiscent of a second rate circus act proudly declaring that it is the first time ever that someone is juggling three clubs while standing on one leg in a bucket of water, simultaneously yodeling Auld Lang Syne. This is distracting, at best.

Thirdly, and most alarmingly, the novelty criterion tends to reward poor scientific practice. Very little in science is fundamentally novel. Progress is instead built upon the scientific foundation formed by previous work in an area and relating research findings to the extant body of work conducted is therefore a cornerstone of sound scientific practice. This involves giving reference to and explicitly discussing previous work and how the current findings relate to this in a balanced manner. However, this can make the current findings seem more or less incremental (which most research findings actually are) rather than entirely novel. The effect is that authors are tempted, deliberately or unconsciously, to inflate the impression of novelty in their own work by deflating, ignoring, or disregarding earlier work. No doubt all of us have our favorite examples of this phenomenon within our own domain. With this in mind, it is perhaps not surprising that ignorance of previous work, which is counter to sound scientific practice, seems to be an increasing problem despite ubiquitous access to literature databases and search engines.

I suggest that initial editorial pre-peer-review decisions should not be based on assessments of novelty of submitted manuscripts but on criteria that are less problematic at this stage, such as those related to topical relevance and the scientific or technical quality of the work. Novelty and the degree to which a given research contribution advances a particular field should form an important part of the final editorial decision in highly ranked primary research journals, but assessments of these qualities are generally much better made by within-field expert peer-reviewers [6]. I argue that the current pre-peer-review focus on novelty among editors should be abandoned as it is destructive to the scientific endeavor: it increases arbitrariness in the decision-making process and promotes poor scientific practice.

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