



LETTERS TO THE *BULLETIN*

Rejecting Editorial Rejections: a Critique to Avoid Real Revisions in Submitted Papers

The divulgation of ecological studies is essential to improve our knowledge of the natural systems. The adequate spread of this information to the scientific community minimizes the duplication of research efforts, gives reason to sustain or discard hypotheses, and stimulates the generation of new ideas. To do this, researchers often try to publish their studies in specialized journals. However, competition for space in high-ranking journals is severe, and these journals require an objective way to accept the best works.

Traditionally, the quality of a manuscript is evaluated through a “peer-review” procedure. A submitted manuscript is sent to several specialists in the same field, and the editor makes a decision based on the reviewers’ comments and his/her own opinion. The review of a manuscript by several colleagues reduces subjectivity and improves the quality of suggestions. Overall, in a good peer-review process everybody wins: authors, referees, and the journal. Authors and reviewers learn something about the topic of the work, and if the paper is accepted for publication, the published manuscript is a better version than the previously submitted one. However, this fruitful practice is now decaying.

More and more ecological journals are shortening this process, and rejecting some papers based only on the opinion of one person: the subject-editor. The main justification is that this saves authors from wasting time waiting for the revisions that will surely be negative, allowing them to quickly send the rejected paper to another, obviously lower-ranked journal. Ironically, the argument is given in terms of benefits for authors. I am convinced that this explanation is irrelevant and unconstructive for science progress.

First, saving time by not waiting for colleagues’ revisions to re-send the same manuscript elsewhere *as soon as possible* is not a genuine advantage for anybody. As an author, I am not in such a hurry. I want to gain knowledge from nature and not to collect published papers *as quickly as possible*. Hence I prefer to wait, learn from the reviewer’s comments, and if eventually the manuscript is rejected, to send a better version to another journal. As a reader, I wish to read the better—not the worst—version of a paper. It is clear that a real revision of a manuscript implies a profit for the author *and* the readers: the author improves in knowledge, and the scientific community reads a better paper. Moreover, the reason why specialized journals reject papers without real revisions (i.e., saving time for authors) is now slight. The use of the Internet currently reduces the review process to 4–6 weeks, surely a sufficiently short period for all but perhaps the fastest-moving fields of biology (certainly fast enough for ecology). Moreover, this wait could be even shorter if reviewers and editors did a more efficient job.

Second, a decision made by only one person is inevitably biased by the strengths and weaknesses of this person, and will often work against the spirit of discussion essential for the progress of science. It is difficult to understand how the subject-editor can prejudge the opinion of other experts about the quality of a paper. Although the subject-editor is often an authority on the general topic of the study, he/she will seldom be a specialist on the specific subject of the submitted manuscript. Normally, this fact generates a bias against publishing scientific novelty (Nature Publishing Group 2003). Furthermore, the opinion of one person, independently of his/her expertise, is always subjective. This is the reason why in the traditional peer review process of submission, papers are sent to 2–3 reviewers and not to only one. A revision performed only by the subject editor is like a study designed without real replications (e.g., $n = 1$); it weakens the inference that can be made from it (e.g., about the quality of the manuscript).

In sum, the practice of rejecting submitted papers without multiple reviews weakens the spirit of forum that is crucial for progress in science. Neither authors nor reviewers (nor journals) learn from evading reviews, and the only advantage, saving time for the author, is trivial and unhelpful. It is unquestionable that journals must establish quality standards and need to have acceptance criteria. Perhaps it is time to rethink which type of manuscript evaluation is more constructive: a monarchical criterion supported in the opinion of only one person, or a parliamentary criterion supported in several contrasting opinions.

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Literature cited

Nature Publishing Group. 2003. Coping with peer-rejection. *Nature* 425: 645.

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