

The Importance of Resubmitting Rejected Papers

by Stephen K. Donovan

If your research paper is rejected for publication by a peer reviewed journal, what do you do next? Some editors would suggest that it should not be sent to another journal, yet there are contributors who would just put a 'failed' paper straight back in the mail to another journal and trust to luck. I argue that rejection should be a time for reassessment, where the input of the editor(s) and reviewers(s), all experts in their field, should be considered and acted upon as necessary. The revitalized and improved contribution can then be sent to a new journal with confidence.

There are two kinds of academic research journals, broadly, those with a policy of peer review of submissions and those without. The greatest kudos is attached to publication of research papers in the former. Although we all publish in a range of academic forms and fora, such as conference abstracts, book reviews, papers in conference transactions, invited chapters, and books and monographs (whether written or edited), it is the peer reviewed journal papers that receive most notice from promotion panels and search committees in many areas of enquiry. Although there are various metrics used by administrators in research institutions to determine the performance of their staff, the focus in many areas of study is on publication of research papers in the top journals in the given field, whatever the imperfections may be of assessing the merit of such publications by merely counting them.¹ This means that the editors of these journals wield considerable power and it is not too much to say that an editorial decision made during a crucial review period may make or break a career. So, any decision to reject a research paper needs to be made on the basis of the best available advice, that is, the input provided by one or, commonly, more than one expert external peer reviewer. The journal peer review system is imperfect and has been rightly criticized,² but many suggestions for 'improvement' only succeed in outlining more cumbersome systems that would increase the workload of editors and their reviewers, all of whom are already overworked.³

Peer review is imperfect, but hopefully it is as fair as it can be. Indeed, we all have tales of editors and reviewers who have bent over backwards to ensure the publication of good research that was, at first, poorly presented. But what happens when this spirit of assistance fails? Consider the following, reported to me by a colleague. My correspondent was attending a meeting to discuss, among other topics, 'best practice' guidelines for peer reviewed journals. A member of an advisory board said that a journal should automatically reject a paper that had obviously been rejected previously by another journal.⁴ I was delighted to hear that the suggestion was not supported, but appalled that such a perversion of the system should be worthy of serious comment.

An obvious flaw of this argument is just how can an editor be sure if a paper has already been rejected by another journal unless the author tells you or it is common knowledge 'on the grapevine'? There are, of course, ways to detect whether a paper, submitted for review to *Contributions to Bee*, had

previously been offered to *Journal of Aye*. The most obvious is poor reformatting, which retains tell-tale features that would only be included in a typescript destined for *Journal of Aye*⁵. More subtly, an editor might recognize a paper as being an unlikely (but not improbable) candidate for their own journal. A telephone call to the editor of *Journal of Aye* might confirm that it had already failed the reviewing mill at that journal. But there are contributors who will openly inform the editor of *Contributions to Bee* in their covering letter that their paper was rejected previously by *Journal of Aye*, even including copies of editorial correspondence and reviewers' reports, and explaining how the typescript has been improved and why it is worthy of publication now. Surely such transparency deserves serious consideration?

Rejection doesn't mean that a paper is wrong (although, of course, it may do); for example, it could be outside the narrow subject area of the target journal. Some years ago a colleague had a paper rejected from the *Scottish Journal of Geography* because it was too geological. The paper was reformatted and resubmitted to the *Scottish Journal of Geology*, where it was rejected because—you guessed it—it was too geographical.

Peer review isn't just a 'yes' or 'no' system. Review comments help improve any paper, whether it is eventually published in the target journal or not. Indeed, perhaps the worst reviews are those that say 'publish as is'; they fail to contribute to the paper and, indeed, are potentially harmful if the reviewer has not done his or her job.⁶

There are two common reasons why even well written, publishable typescripts are rejected by journals. Perhaps most commonly, papers are submitted to the wrong journal. I am not suggesting that anyone would send, say, a paper on the psychology of the criminal mind to a journal on nuclear chemistry. Rather, even though the subject of the paper lies within the broad area of study covered by the journal, it lies within a sub-discipline that is outside the normal remit of the target publication. This is easiest to illustrate using an example from my own experience. My own journal states in its instructions to authors that 'The majority of publications in *Scripta Geologica* are the result of research projects of the Nationaal Natuurhistorisch Museum, Leiden, or are based mainly or entirely on specimens in the collection of the Museum. Other papers are accepted, very rarely, at the Managing Editor's discretion.' In truth, 'very rarely' only really applies to thematic special issues which include invited papers or contributions arising from conferences organized within the museum. Any other paper that is not connected to my museum's collections is automatically rejected without review.

More subtly, the emphasis of a journal may change with the arrival of a new editor(s). An example of this sits on my desk right now; a journal in which I have been publishing regularly since the mid 1980s and which has never rejected one of my papers did so this morning, without review, a decision by the new editor. I submitted this paper, confident in the knowledge of the sort of research that the target journal would publish, but the goalposts have moved. My next task will be to reformat it and resubmit elsewhere.

Rejection may also come from what I would call geographic prejudice. Most of the leading international journals in any field are published in North America or Europe, have mainly North American or European editors and reviewers, and publish a lot of papers on North America and Europe. One of the strongest cards played by reviewers and editors of these journals in rejecting a paper based on data from outside the North Atlantic region is that a paper may be considered as 'parochial.' I make this charge with feeling, based on 12½ years at the University of the West Indies, Mona, Jamaica. Having an exciting discovery on, say, Jamaican paleontology, that you think deserves international exposure, labeled as 'parochial' by a reviewer is, to be polite, galling. The reviewer may be a leading expert on a subject area, but perhaps not on its implications outside North America or Europe. Yet it is very difficult to argue with an editor once the 'parochial' card has been played.

Rejection is surely part of the structure of academic publishing, as much as publishing itself. But rejection should lead to a re-evaluation and strengthening of a paper by the author. Are the comments of the editor(s) and reviewer(s) valid? Do they justify extensive revision, perhaps new data analysis or even renewed sampling? Then do so. A bad, rejected paper, submitted elsewhere and without any improvement, is still bad and is always likely to be returned. An improved, formerly rejected paper—

better written, new data, improved analysis or whatever—should not be prejudged on its earlier failure, but should enter the review process as a new contribution, worthy of critical examination. What editors and authors need to appreciate is that the rejection of a paper should lead to it being strengthened, which in turn makes it more likely to be accepted for publication.⁷

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Notes

1. Such as: David Adam, 'The counting house,' *Nature* 415 (14 February 2002), 726-9; Frank-Thorsten Krell, 'Why impact factors don't work for taxonomy,' *Nature* 415 (28 February 2002), 957; N. Haeffner-Cavaillon, C. Graillot-Gak and C. Bréchet, 'Automated grading of research performance clearly fails to measure up,' *Nature* 438 (1 December 2005), 559; Shu-Dong Zhang, 'Judge a paper on its own merits, not its journal's,' *Nature* 442 (6 July 2006): 26.
2. Such as: Michael Gordon, 'Evaluating the evaluators,' *New Scientist* 73 (10 February 1977), 342-3; Declan Butler, 'Peer review 'still essential', say researchers,' *Nature* 379 (29 February 1996), 758; Rex Dalton, 'Peers under pressure,' *Nature* 413 (13 September 2001), 102-4; Rustum Roy and James R. Ashburn, 'The perils of peer review,' *Nature* 414 (22 November 2002), 393-4; Stephen K. Donovan, 'The joys of peer review or do you get the willies when you submit a paper?' *Palaios* 20 (April 2005), 99-100.
3. Such as: Alexandra List, 'Reviewers' reports should in turn be peer reviewed,' *Nature* 442 (6 July 2006): 26; but see Ron Kitchin, 'The editor's role in refereeing: a response to Johnston and Pattie,' *Area* 37 (2005), 455-6.
4. I thank my correspondent, who prefers to remain anonymous, for sharing this editorial experience with me.
5. Stephen K. Donovan, 'How to alienate your editor: A practical guide for established authors', *Journal of Scholarly Publishing* 36 (July 2005): 238-42.
6. Donovan, 'The joys of peer review or do you get the willies when you submit a paper?'
7. Stephen K. Donovan, 'Rejected? Only temporarily,' *European Geologist* 16 (December 2003), 41.