Chapter 7
What an Editor Looks For

Everyone who submits a manuscript for peer review dreads one thing above all else: getting rejected (though the gentlefolk among us journal editors prefer the phrase “decline to publish”). There are many reasons why a manuscript might be rejected, and a good understanding of the reasons can help you make sure your manuscript has the best chance possible of acceptance.

To be publishable in a scientific journal, a paper must meet four important criteria:

- The content of the paper must match the scope of the journal,
- The quality of the paper (method and execution of the research, as well as the writing) must be sufficiently high,
- It must present novel results (with the exception of review papers and the like), and
- The results must be significant enough to be worth reading about (and thus worth publishing).

Most of this book is dedicated to the quality of the writing, with the goal of improving the writing sufficiently so that a manuscript would not be rejected on that basis. This chapter will focus on the other things a journal editor will look for.

7.1 Scope

The easiest way for your manuscript to be rejected is to submit it to the wrong journal. A perfectly good manuscript will be rejected if the topic of the manuscript does not match the scope of the journal. Thus, you should carefully research the scope of any journal you wish to submit to and make sure there is scope match. See Chapter 8 for more advice on picking the right journal.

7.2 Quality

There are two aspects of quality relevant to journal publications: the quality of the work being reported, and the quality of the reporting (that is, the written manuscript). The quality of the work is essentially a judgment of the science involved, including the care taken in planning and executing experiments, as well as in analyzing the resulting data and fitting these results into the larger framework of the scientific field. Fully defining what is meant by the quality of the science is a rather large undertaking and is outside the scope of this book.

The quality of the written presentation of the work is the overall subject of this book. Here, I will only add that the quality of the *presentation* can and should be judged separately from the quality of the work itself. The reason for this is simple: it is often much easier to fix a faulty presentation than to fix faulty science. Still, if the initial quality of the writing is not sufficiently high, it may be nearly impossible to judge the quality of the work itself, and we are sometimes forced to reject a paper due to poor writing without any real judgment of the science involved.

Put another way, you want the editors and reviewers to focus on the quality of your scientific work. The writing should make it easy for readers (and the first readers will be the editors and reviewers of the journal) to understand and evaluate the science you report.

7.3 Novelty

With the exception of review papers and tutorials (see Chapter 11), a manuscript must contain something new to be worthy of publication in a scientific journal. The explicit mission of the science journal is to add to the body of knowledge in the field. Thus, a journal paper must add something new to that body of knowledge (new theory, new designs, new models, new methods, new data, or new analysis). As a consequence, an effective literature search and comprehensive citations are a requirement to establish what about the submitted work is novel (see Chapter 5).

Of course, not everything in the paper must be new. Often, publications are akin to progress reports, produced upon the achievement of a milestone in a longer-term research project. In such a case, it is appropriate that some parts of the paper review prior published work from the same effort. This reality sets up an expected tension between a desire to publish the latest results, even if incomplete, and a desire to ensure that there is sufficient new information in this latest paper to make reading it worthwhile in light of past publications and acknowledged need for future work.

A good rule of thumb is that at least 50% of the results being presented must be new. If you find that more than half of the results you present have been published before, chances are you have not done enough new work to warrant a new paper. Of course, fully explaining what is new is required.

7.4 Significance

The final publication requirement is perhaps the most nebulous: the work must be sufficiently significant. Significance should be judged based on the viewpoint of the readers: how many people will read the paper and put the conveyed knowledge to use.

There were about 28,000 peer-reviewed journals in 2012, and they now publish about 2 million articles a year (with these numbers growing by about 3.0–3.5% each year).[1] This rate represents a doubling of the number of scientific papers every 20 years or so, and has been relatively constant for over 300 years.[2,3] If you are like me, your inbox overflows with invitations to publish in new journals you have never heard of. An uncomfortable reality is that a fair number of the papers published in these journals are rarely, if ever, read by anybody. Publishing a paper that has little or no impact on our scientific community does not serve the interests of science, and yet many of these “peer-reviewed” journals will pretty much publish anything (for a fee), gratifying the ego and the “publish or perish” needs of the researcher. Thus, the more reputable journals are anxious to ensure that the papers they publish are significant, adding signal rather than noise to our communal collection of knowledge.

It is very hard for editors and reviewers to prospectively judge the significance of a submitted manuscript. Generally, editors and reviewers take a two-step approach to making such an evaluation: How important is the problem being addressed by the work, and how big of an advance over the prior literature does this work represent? For example, even a small advance in a topic that hundreds or thousands of readers care about can be considered significant. Alternatively, a big improvement in a technology that few care about may not be as significant. As one can imagine, these judgments are not easy to make.

7.4.1 Measuring significance

Journals generally use two useful though imperfect measures of significance when retrospectively evaluating published articles. The number of downloads is becoming the dominant measure of readership for a paper, although this metric measures interest in the topic and quality of the title, abstract, and keywords rather than the significance of the work as a whole. The number of citations that a paper garners is, over the long run, a measure of its significance, but only to one segment of the readership: those who go on to publish other papers. A paper that significantly influences the practice of scientists and engineers, especially as it relates to commercial application, may not find its importance reflected in its citation numbers. Still, the combination of downloads and citations over a long period of time is a reasonable measure of the significance of a paper.

How well have we done at picking significant papers for the *Journal of Micro/Nanolithography, MEMS, and MOEMS* (JM^3)? As of the end of 2013, I analyzed all of the papers published in JM^3 between 2003 and 2008. Within five years of publication, the average number of citations for a JM^3 paper was 4.4. The distribution of five-year citations is highly skewed (about an exponential, see Fig. 7.1), with a maximum of 42 citations, and with 10% of papers having twelve or more citations (as of the end of 2013). But about 22% of JM^3 papers were not cited over the first five years after publication. While this number is certainly higher than anyone would like, it is not out of line with the more engineering-related disciplines. According to the Web of Science, 18% of the approximately 38,000 articles published in 2008 in journals related to electrical engineering were not cited by the end of 2013. The citation rate is also a function of how broad or narrow the scope of the journal is, with broad-based publications (think *Nature* or *Science*) having both higher readership and citation rates.

Because many of the papers published in JM^3 appeal to semiconductor and MEMS/MOEMS manufacturing, readership is also an important measure of a paper's success, independent of citations. Today, most reading is done after downloading an article (libraries being the primary destination of the printed JM^3 journals), and download rates have steadily increased each year. Up through the end of 2013, the average JM^3 article has been downloaded over 300 times (with an average of about 55 downloads a year). The median number of downloads per paper per year was about 35, indicating a highly skewed distribution. From 2009 to 2012, the top papers received about 700 downloads in a year, but since then the feedback loop of promoting the top downloads on the JM^3 digital-library homepage has resulted in papers with up to 7,000 downloads in a year. Obviously, some JM^3 papers are very well read. For papers published in 2008, the five-year total of downloads averaged 253 per paper (median of 231), and the paper with the least number of downloads received 87 over that five-year period (the second-least downloaded had 107).

It is interesting to note that only four of the top-ten most-cited articles (using the five-year citation total) are also in the top ten of the most downloaded articles. Clearly, citations and downloads are different measures of impact. Another interesting metric is citations in patents. A quick search in 2014 on the US Patent Office website found over 750 US patents that cite JM^3 papers—quite a significant number.

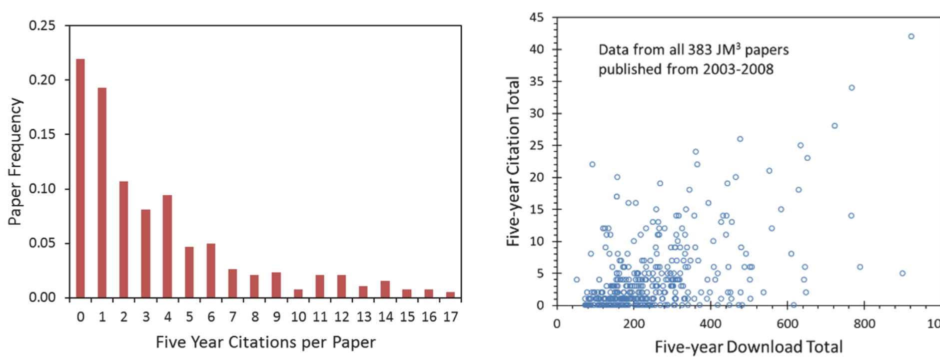

Figure 7.1 Five-year citations and downloads for all JM^3 papers published between 2003 and 2008.

7.4.2 In praise of the null result

One unfortunate side effect of the search for significance is a bias against the null result. Almost all scientific studies look for effects: does input A affect output B?

The null result (also called the negative result) is simply a “no” in answer to that question. Theoretically, science should be neutral to the answer: no is just as good an answer as yes. But human nature does not usually work that way. In most cases, we study the effect of A on B because we *want* to see an effect. We want our new drug to have a positive impact on patient outcomes. We want our new process to result in better properties for the device being fabricated. There is almost always a preferred answer to the question “Does input A affect output B?”

In science, the only failed experiment is one that does not lead to a conclusion. Yet it is easy to think that an undesired conclusion is also a failure. One consequence of this very human tendency is a publication bias against the null result: journals are much more likely to publish papers that provide a positive result than ones that present a null or negative result.

The existence of a publication bias against null or negative results was first described in 1959,[4] and this bias has stayed the same[5] or gotten worse since then.[6] Many studies have shown that the vast majority of published scientific papers show positive results, that input A does in fact affect output B in the desired way. Negative results suffer from the “file-drawer” effect: a study that finds no impact or an undesired impact of A on B will likely be filed away in the researcher’s desk drawer rather than published in a peer-reviewed journal.[7] This leads to an incorrect impression that such experiments have never been tried.

There are three potential reasons for the existence of such a publication bias: editorial policy, reviewer bias, and author-submission bias. Although there may be some journals that actively discourage the publication of negative results through their editorial policy, such journals are probably the exception, and certainly JM^3 is not among them. Reviewer bias is probably more common because reviewers are tasked with evaluating the significance of a manuscript, and there is often an unstated assumption that positive results are more significant than negative results.

Still, I think submission bias accounts for a majority of the publication bias. Authors, either anticipating a reviewer bias or having a bias for positive results themselves, are much more likely to submit a manuscript that contains positive results than negative results. A journal cannot publish a paper that demonstrates a null result if that paper is never submitted. The reasons for these biases are probably rational: positive results generally attract more readers and citations. The undesirable consequences of a bias against the null result, however, can be significant.

There are two major consequences of the publication bias against the null result, both unpleasant in their own way. The first is wasted effort. As I mentioned, most researchers are looking for positive results: they are trying to reduce the leakage current of a CMOS transistor, increase the Q-factor of a MEMS device, or reduce the roughness of a lithographically patterned feature. They try many different approaches, testing the effectiveness of many different variables. Most of the approaches do not work, but a few yield positive results. If the publication bias is at work, only the positive results are published, and the fact that certain experiments led to null or negative results remains unmentioned.

If readers remain unaware of these negative results, they are more likely to repeat these experiments in their own efforts to find positive results. The consequence is unnecessary waste. A completely valid and potentially important scientific outcome, that input A does not impact output B, is not published and so does not join the collective knowledge of the community. And the search for positive outcomes proceeds more slowly as a result.

The second consequence of the publication bias against null results is more insidious: it increases the likelihood that published results are wrong. In some cases, entire fields of study (such as extra sensory perception, ESP) publish only spurious positive results[8] (a negative result, showing no evidence for ESP, would be unlikely to be published). But leaving aside such extreme cases, there is evidence that the publication bias against the null result leads to the frequent publication of spurious positive results in most or all fields, as John Ioannidis has persuasively claimed in his provocatively titled essay "Why Most Published Research Findings Are False".[9]

Consider twenty researchers all independently trying to see if input A affects output B. If A really has no impact on B, then one out of the twenty researchers will likely produce a spurious positive result to a 5% significance level ($\alpha = 0.05$) by pure chance. This will not cause any problems if all twenty researchers publish their results. But if the nineteen null findings remain unpublished (the file-drawer effect) and the one spurious positive result is published, readers will very reasonably assume that the results in the one published paper are representative of all studies and are likely to be true. The publication bias against the null result naturally leads to a degradation of the overall quality of published research as a whole.[10]

However, science is supposed to be self-correcting, imbued with a "trust, but verify" mentality. Replication of results by other researchers should identify these spurious positive findings, eventually leading to sound conclusions. But "eventually" can be a long time. Further, there is some evidence that most scientific studies are never replicated, so bad results can linger in the collective consciousness of the scientific community for a very long time.[4] The "publish or perish" mentality in academia, coupled with a publication bias against the null result, means that the scientific community often rewards impact and quantity over reproducibility and quality. Few scientists seem willing to devote significant time and resources towards replication of others' results.

Here is my modest proposal to help mitigate the negative impacts of a publication bias against the null result: when you write a paper and emphasize the positive results that you think are most important, please do not forget the negative or null results that you found along the way. Include a few sentences about the variables you tried that did not produce the desired effect. Show a graph of the data that demonstrates no significant effect, if for nothing else than to compare to the graph of data that does demonstrate the desired effect. Think about all the dead-ends and blind alleys that you went down in your search for a solution to your problem, then warn the rest of us about them. Consider the null result as a valid and important scientific discovery, and add it to your paper of positive results.

Reviewers and editors, do not recommend that null results be deleted from a paper just because they are null results. Although you may always consider the positive result to be more significant, do not automatically think that a null result is not important. Consider all of the wasted effort that can be avoided if just a few paragraphs of a paper are devoted to those null results that are almost always lurking around every scientific study.

7.5 Conclusions

Journal editors are always looking for four things in every manuscript submitted to their journal: scope, quality, novelty, and significance. Before submitting your manuscript for publication, try evaluating it yourself using these four categories.

Because this book is about writing your paper, my advice here is to make it easy for a reader (and reviewer) to evaluate your work when reading your paper. Write so that it is clear what is the scope of your work, what is new and how it fits with prior published work, and why it is significant. And make the quality of your writing sufficiently high so that the reader can properly judge the quality of the science.

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