A Student's Guide to Peer Review

by

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Publishing is an essential part of research. Publications provide a lasting record of scientific accomplishment that other researchers can refer to and build on. Before a manuscript is accepted for publication, however, it must undergo review by other researchers. These reviewers evaluate the manuscript to judge its suitability for publication. This process is called *peer review*.

Peer review is extremely important for maintaining the quality of publications. In view of this importance you might be surprised to learn that there are virtually no published guidelines that govern peer review. To be sure, there is some traditional etiquette that most researchers agree on and general guidelines that journals send to reviewers along with manuscripts. However, there are many gray areas and ethical dilemmas that can arise. My goal is to discuss a few of these and provide some guidance to new researchers.

This article is primarily directed at students who are becoming involved in research. My intention is to help you understand your obligations and responsibilities concerning reviewing. However, several of the issues I discuss are relevant to reviewing practices in general and thus may be of interest to more experienced researchers as well.

Why should you be a reviewer?

Reviewing takes time and effort, and you may wonder why you should bother. While no one can force you to be a reviewer, there are extremely good reasons to do so. First, when you submit manuscripts for publication, it is your obligation to review manuscripts as well. To be sure, this is a moral obligation only, and no one can force you to be a reviewer. However, journal editors will appreciate your effort and you will gain a reputation for contributing to the good of the research community. Most importantly, it may be naive to expect that your own manuscripts will be reviewed in a timely manner if you yourself do not fulfill your obligation.

Furthermore, when you review manuscripts, you have the opportunity to see research results well before publication. In this way, you will become aware of research trends ahead of the community at large, which can be beneficial to your research program. This does NOT mean that the content of the manuscripts you review is available for you to use in your own research, however, and I will discuss the confidentiality issue in more detail below. Nevertheless, what you should realize is that there are benefits to the time and effort that it takes to be a reviewer.

Are you an appropriate reviewer?

When you receive a manuscript to review, you should decide immediately whether or not you're an appropriate reviewer. If you're reasonably familiar with the subject matter of the manuscript, especially the references that provide the background for the manuscript, then you're probably an appropriate reviewer. If you're not sufficiently knowledgeable about all aspects of the manuscript, then you can limit your review to those aspects that you know well and so inform the editor in a note accompanying your review. If you're not familiar the with topic of the manuscript, then you're probably not an appropriate reviewer, and it is important to return the manuscript to the editor without delay. While journals often allow you to pass the manuscript on to a suitable colleague, I recommend that you return the manuscript to the editor with suggestions for potential reviewers thereby removing yourself from the "loop" to avoid later confusion.

What are the objectives of your review?

Essentially, your review serves three purposes, namely, 1) to determine whether the content of the manuscript is *novel* (that is, new), 2) to determine whether the results of the manuscript are *correct*, and 3) to determine whether the results are *significant*. These issues may seem straightforward, but in many cases they're not. I will address each of these issues separately.

How is novelty determined?

Novelty is extremely important in research, and a manuscript is not publishable if its results are already known (except if it is a review or tutorial paper). While research is novel if it has not been published before, it may be difficult in practice to determine this. First, the research world is vast and multifaceted. For starters, there are numerous journals, not only the well-known, large-circulation journals, but also many small-circulation ones. In addition, there are foreign-language journals, which may not be easily accessible. That's the easy part. To make matters worse, there are conferences on virtually every academic and technological discipline taking place throughout the year. Some of these conferences have reviewed proceedings, others have unreviewed proceedings, and others publish only abstracts. Proceedings of these conferences are often not accessible to researchers who have not attended the conference. And what about Ph.D. dissertations? Those are available from archives, but they're not distributed to the research community. An even grayer case concerns masters' theses which are not publicly accessible.

A more serious problem is the fact that not all publications undergo the same level of scrutiny. A typical dilemma is the following. A reviewer points out that overlapping material can be found in a dissertation that was never published. Since the manuscript lacks novelty, the reviewer may recommend that it be rejected. But the dissertation, because it was not submitted to a journal or conference, had never been peer reviewed. Thus one could argue that the "results" of the dissertation have the status of a conjecture or unproven claim. (Moral: Publish your dissertation.) Thus the correctness of the prior publication has not been established with a high level of scrutiny, and this makes it difficult to determine novelty. As a researcher, it is important that your own research is subject to scrutiny and is made accessible to a wide audience.

The review process usually takes months, and sometimes even years. In addition, once a manuscript is accepted for publication, it is often many more months before it is published. These delays present timing problems in determining novelty. For example, suppose you're reviewing a manuscript and it turns out that overlapping results appear in another manuscript that is either under review or awaiting publication. While there are no set guidelines to govern this situation, I follow the rule that, until a manuscript has been published, its results cannot preclude those of another manuscript under review. In other words, a manuscript should only be judged on the basis of published material. But even more subtle problems can arise. Specifically, during the time that a manuscript is being reviewed, it may happen that a paper with substantial overlap is published. This may affect the review of the manuscript in the midst of its review process. In such cases, editors usually allow the submitted manuscript to complete its review cycle.

How is correctness determined?

Determining correctness can be difficult for a variety of reasons. First, each field of research has its own standards for determining correctness. These standards are not absolute (in spite of what mathematicians often claim). What is most important is that you apply these standards in a manner that is appropriate to the field. For example, there are control theorists who consider a "result" to be nothing less than a proven theorem. With this standard, a control theory researcher would be inclined to reject engineering manuscripts that lack a theorem-proof format. (Actually, very few engineering journals use this style. Control theory is an exception.) Conversely, a researcher in an applied area of engineering would be inclined to reject control theory manuscripts which are often based on idealized mathematical assumptions without reference to physical reality.

Nevertheless, although standards of mathematical rigor vary from field to field, a manuscript must conform to basic standards of clarity and logic. If not, you may judge the manuscript as "not even wrong," which is to say that it is not sufficiently well written for you to determine whether or not its results are correct. Some reviewers may recommend rejection of a manuscript based solely on the lack of clarity without investing the effort to determine novelty and significance. This kind of recommendation is valid only to the extent that it is both objective and specific, aspects that are discussed below.

All of this assumes that the manuscript possesses sufficient detail for you to determine correctness. However, few manuscripts are self contained. For example, most manuscripts depend on results in books and papers, which is not a problem unless you're skeptical about the correctness of the cited material. You may have valid grounds to worry if the crucial references have undergone less scrutiny, which may be the case if they're conference papers and the manuscript you're reviewing was submitted to a journal, which demands higher scrutiny. The point here is that publications, however defined, form a kind of *pyramid* with different levels of scrutiny being applied at different levels of the pyramid. Discrepancies in scrutiny of relevant publications may impede your ability to determine correctness.

There are other cases in which you can't check the correctness of a manuscript. For example, you usually can't verify the computer programs that the authors developed. Furthermore, you can't check their experimental data, much less their experimental apparatus. In these cases, it is obviously impossible to verify the correctness of the manuscript. While there is no simple solution to this problem, at the very least you might require that the authors provide sufficient detail and diagnostics to minimize the possibility of hidden flaws. (When you write your own manuscripts, you should reverse this process and think about how you should report your results to demonstrate that your methods and results are correct.) Some disciplines require "reproducibility" as a requirement for publication, but this is more of an ideal than a practical reality.

Some reviewers believe that the standards of correctness need to be higher for beginning researchers than for more established authors. While closer scrutiny of the work of less established researchers can be viewed as beneficial to novices, this distinction implies less rigorous review of more established authors. Consequently, this practice violates fairness and objectivity while undermining the ultimate goal of reviewing. Like the emperor and his clothes, all manuscripts deserve equal treatment regardless of the identity of the authors, however renowned. Blind reviewing, that is, reviewing manuscripts with the authors' names removed, can help to prevent this problem; however, it is rarely practiced by engineering journals.

How is significance determined?

A manuscript may be both correct and novel, but its results may not be significant enough to warrant publication. In some cases, it is a simple matter to decide whether this is the case. For example, the results may be only a minor extension of known results, perhaps more in the vein of an exercise than a true research contribution. But even here there are gray areas. For example, suppose that the results of a manuscript are actually a special case of a more general, known result. Some reviewers would recommend that the manuscript be rejected. However, special cases are frequently nonobvious and may be of great value. To make the point concrete, consider the consequences of rejecting every manuscript that presents a Lyapunov function on the grounds that all such functions are merely special cases of an established framework.

Reviewers often say that a manuscript is “not interesting” to imply that it is not sufficiently significant to warrant publication. This judgment entails a serious problem, namely, that significance can often be difficult to judge. Even famous researchers have made horrendous errors in judging significance, and history is filled with examples of such errors. (See the References below.) In some cases, a researcher’s work was published only after protracted, strenuous conflict against the established “experts” whose views were later found to be woefully misguided. If you insist on judging a manuscript to be “not interesting,” be absolutely sure that your judgment is based on thoughtful, rational arguments rather than personal prejudices.

The issue of significance is the least objective and therefore the most contentious. If the manuscript has marginal novelty in a dense field of research, significance may be in doubt. On the other hand, if the manuscript presents ideas or concepts that are original, creative, speculative, or unusual, then it may be appropriate to give the authors the benefit of the doubt. In such cases, the possible harm to the journal may be outweighed by the stimulus to the research community, not to mention harm to the authors. Remember that new ideas often require the development of an intellectual framework that can takes years for acceptance by the research community as a whole.

Be specific and helpful.

It is important that your evaluation of a manuscript be based on specific examples. For example, if the manuscript is not clearly written, give an example to demonstrate. You need not point out every such instance, but you might make it clear that you’re merely providing a few specific cases as evidence of the lack of rigor or clarity. The use of such examples will strengthen your evaluation by convincing the editor of the objectivity of your review. In addition, the examples you include in your review will be of great help to the authors when they revise their manuscript.

You can be helpful to the authors in other ways as well. For example, you can suggest technical improvements to the manuscript, and you can provide additional, relevant references, whether or not these references preclude novelty. By helping the authors improve their manuscript, you’re improving the literature to the benefit of all researchers.

Be timely.

When an editor asks you to review a manuscript, he or she will usually give you a deadline. If you know you cannot meet the deadline, tell the editor immediately and you will usually be granted an extension. If an editor notifies you that the deadline has passed, you should respond immediately with an apology and an estimate of when your review will be complete. Remember that if your review is excessively late, the editor may make a decision without your input, and you will lose your opportunity to judge the manuscript.

Respect confidentiality.

The manuscript that you're reviewing has been sent to you in confidence. Although there is no legal requirement for secrecy, it is understood in the research community that you're bound to respect the confidentiality of the manuscript. This means that you cannot use the results of the manuscript in your own research (which would be fraud), and you cannot divulge the results of the manuscript to other researchers. If you wish to cite or make use of the results of a manuscript, you can reveal your identity to the authors and request their permission. However, the authors need not grant such permission.

Be objective and fair.

As you determine the novelty, correctness, and significance of a manuscript, it is important that you be objective. Objectivity is extremely important because, as already mentioned, the review process entails a conflict of interest. The editor, of course, is aware of this conflict, and thus is obligated to place high weight on objective comments and little or no weight on subjective opinion. In fact, any comments that you include in your review and that lack objectivity should be ignored by the editor. If your review is excessively subjective, then the editor may completely ignore your evaluation and thus you will lose the opportunity to judge the manuscript.

Objectivity is essential for ensuring fairness. It is an unfortunate, and sometimes ugly, aspect of the profession that research can be extremely competitive and some reviewers will take advantage of the review process. Unfortunately, it is not uncommon for a reviewer to dismiss a manuscript with a few subjective comments. It is the editor's duty to ignore such reviews regardless of the stature of the reviewer. At the very least, fairness means that you do not impose standards on the authors that you yourself do not abide by in your own work.

Unfortunately, a review is essentially a critique of a manuscript, and thus most reviews are largely filled with negative comments. However, there is nothing to prevent you from being complementary if there are aspects of the manuscript that you feel are deserving of praise. Supportive comments and encouragement can generate good will in an often competitive environment.

Beware of conflicts of interest.

The institution of peer review is complicated by conflicts of interest. Only in peer review is the value of one's work judged by one's competitors. Our society recognizes the inherent dangers of conflicts of interest, and numerous laws and social customs have been developed to avoid if not banish it (think of nepotism, for starters). Unfortunately, there is no alternative to peer review to determine the worthiness of a manuscript for publication. However, in cases of severe conflict of interest and obvious bias, editors have been known to ignore the reviews and publish manuscripts sight unseen.

When you receive a competitor's manuscript to review, you may be placed in a conflict of interest. To make matters worse, this conflict of interest is imposed upon you involuntarily when the manuscript is sent to you unsolicited. In extreme cases, you may wish to return the manuscript to the editor to avoid being placed in a difficult situation. Unfortunately, the very fact that you have opened the envelope may force you to demonstrate that you have respected the manuscript's confidentiality. While such situations illustrate flaws in the peer review system, fortunately they are rarely problematic.

Fulfill your obligations and review with integrity.

As a researcher seeking to publish your own work, it is your obligation to review manuscripts by your fellow researchers. The goal of your review is to determine novelty, correctness, and significance. Your review should strive to be specific, helpful, timely, objective, and fair. You must also respect the confidentiality of the contents of the manuscript.

As a beginning researcher your reviewing efforts may entail difficult issues. You should deal with these problems with utmost integrity, always treating your fellow researchers in a manner that you yourself would want to be treated. Peer review is a practice where simple application of the golden rule should prevail. Just as you would not want your research judged in an unfair manner, it is inappropriate to treat other researchers' work unfairly. The ethical standards that you uphold and the consideration that you show to your fellow researchers reflect your personal integrity.

References

A thoughtful critique of some aspects of the peer review process is given in

R.C. Thompson, "Author vs. referee: A case history for middle level mathematicians," *Amer. Math. Monthly*, vol. 90, pp. 661-668, 1983.

An argument in favor of blind reviewing is given in

L.B. Bourbaki, "On blindness," *Math. Intell.*, vol. 21, no. 1, pp. 4-5, 1999.

See also the exchange in

L.B. Bourbaki, “Response,” *Math. Intell.*, vol. 21, no. 3, pp. 3-4, 1999.

Chapter 5 of

M.C. LaFollette, *Stealing into Print: Fraud, Plagiarism, and Misconduct in Scientific Publishing*, University of California Press, Los Angeles, 1992.

presents a detailed discussion of the peer review process and many of the issues that I have raised.

Every field of research has its horror stories about great ideas that were initially rejected by the research community. In mathematics, Fourier was ridiculed, Galois’ work was rejected, the Dirac delta function was laughed at, and Cantor’s work in set theory was highly controversial. All of these ideas are standard today. A classic case in control engineering is the development of the negative feedback amplifier, a spectacular technological breakthrough recounted in

H.S. Black, “Inventing the negative feedback amplifier*,” IEEE Spectrum*, vol. 14, pp. 55-60, Dec. 1977

B. Friedland, “Introduction to ‘Stabilized feed-back amplifiers,’” *Proc. IEEE*, vol. 87, no. 2, 1999.

Unfortunately, most of the credit went to Nyquist, who expanded on Black’s insights in his stability theory.

Further examples of initial rejection in technology development are the invention of the jet engine and FM radio. The jet engine was patented in 1930 by Frank Whittle (1907-1996), but was ignored by the British aerospace industry until Whittle formed a company and demonstrated a promising engine in 1939. At that time the British aerospace industry convinced the British Government to order Whittle to hand over his life’s work, which he was forced to do without just compensation. Likewise, Edwin Armstrong (1890-1954) invented wideband FM radio in 1933, but RCA, which had a considerable investment in AM radio, used its Government influence to emasculate Armstrong’s work while infringing on his patents.

What can go wrong with peer review is discussed in the LaFollette book above as well as

W. Broad, *Betrayers of the Truth*, Simon and Schuster, New York, 1982.

D.J. Kevles, *The Baltimore Case: A Trial of Politics, Science, and Character*, W.W. Norton, New York, 1998.

R. Bell, *Impure Science: Fraud, Compromise, and Political Influence in Scientific Research*, Wiley, New York, 1992.

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