Problems, Pitfalls, and Promise in the Peer-Review Process: Commentary on Trafimow & Rice (2009)

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Issues in Publishing, Editing, and Reviewing

Problems, Pitfalls, and Promise in the Peer-Review Process

Commentary on Trafimow & Rice (2009)

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ABSTRACT—*In their critique of the peer-review process, Trafimow and Rice (2009, this issue) dramatize a number of ways in which the review process can go awry. On the whole, I agree that the issues highlighted by the authors are indeed problems worthy of careful consideration. However, I fear that their treatment of these issues could send mixed and misleading messages to would-be reviewers about what constitutes proper reviewing behavior, in part because the authors had little to say on this topic. In the present commentary, I attempt to address this omission by discussing qualities of a good review and by distinguishing several responsibilities in the peer-review process that lie uniquely with the editor and the author. Finally, I identify three general strategies for improving the peer-review process that center on providing better, more formalized training of reviewers; increasing accountability of both editors and reviewers; and reducing burden on the peer-review system.*

Trafimow and Rice (2009, this issue) have written a clever and instructive article on some of the flaws in the peer-review process. Their central argument seems to be that " . . . a reviewer who wishes to find fault is always able to do so. Therefore, the mere fact that a manuscript can be criticized provides insufficient reason to evaluate it negatively. Rather, one must consider the possible gains against the possible losses involved, and if the former outweighs the latter, the work should be evaluated positively in spite of the potential criticisms" (p. 65). Although one could quibble with the exact wording, I would be hard pressed to disagree with the basic principle. Any evaluation worth its salt must take into account the balance of strengths and weaknesses, weighing each according to its severity to arrive at an overall assessment of merit. Failure to do so, by definition, leads to a biased and unbalanced assessment.

In fact, the authors raise many valid points with which I, and I suspect most readers, would strongly agree. Who could argue, for example, with the idea that judgments about the scope of a paper's contribution should not be based solely, or even primarily, on whether the reviewer happens to agree with the author's position (p. 77)? Or that a reviewer should not recommend rejection of a paper simply because an alternative explanation, no matter how unlikely, could be dreamed up (p. 77)? Or that a reviewer should be careful to evaluate all parts of a paper's contribution in arriving at an overall assessment of its worth, not just the part that is closest to his or her research interest (p. 77)? And who among us approves of an editor basing his or her editorial decision solely on a vote count of the number of recommendations for and against publishing a manuscript?

In short, I agree with Trafimow and Rice on many of the issues they raise. Bad things do sometimes happen in the peer-review process—it isn't a perfect system and never will be. Reviewers and editors are after all people and are subject to the same limitations when reviewing a paper as they are in all other human activities.

Indeed, human imperfection taints the review process in a number of predictable and well-documented ways. One important problem dramatized in several of Trafimow and Rice's simulated editorial letters is the tendency for reviewers to assimilate new information into existing knowledge structures, meaning that they are inclined to accept without question data that fits with a priori beliefs and more closely scrutinize data inconsistent with these beliefs. This tendency, known as *confirmation bias* (Nickerson, 1998), has been shown to operate in a variety of ways in both personal and academic arenas (see Greenwald, Pratkanis, Leippe, & Baumgardner, 1986). That this bias also operates in the peer-review process (see, e.g., Mahoney, 1977) is worrisome, but not surprising.

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Reviewers are also motivated by self-interest, and this can come into play under various guises. For example, a reviewer may find it especially difficult to remain open and render an unbiased evaluation of a paper when that paper challenges the reviewer's own work or core tenets of that work, as several of Trafimow and Rice's vignettes illustrate. Reviewers are understandably invested in their own work and care deeply about how it is viewed by their peers, as many rewards—both tangible (tenure, raises) and intangible (respect, recognition)—depend on these valuations. Reviewers can also be motivated by a desire to "look good" to the editor. To be sure, this dynamic can benefit the review process by motivating reviewers to be thoughtful, conscientious, and timely in their reviews. But it can also work to its detriment by promoting overly critical reviews, as research shows that people look smarter to onlookers when they criticize, rather than praise, a work (Amable, 1983).

Reviewers also make routine cognitive errors of judgment and have limited capacity to process information. Research shows, for example, that reviewers can be influenced by an author's reputation or by the prestige of his or her academic affiliation (e.g., Peters & Ceci, 1982; see also Petty, Fleming, & Fabrigar, 1999). As Miller (2006) explains, "Such criteria provide clues, albeit imperfect ones, as to the competency of a manuscript's author(s), and these clues can be used . . . as unconscious or conscious shortcuts around any uncertainty about the value of a submission" (p. 425).

In addition to motivational and information processing biases, more pragmatic issues can also undermine the fairness of the review process. Reviewers, like most of us, are busy people, and they may not always be able to give a paper the time it deserves. In addition, some papers are just harder to review than others due, for example, to the abstractness of the ideas, the complexity of the methods, or their length.

With all these dynamics in play, it is hardly surprising that reviews vary in quality, even among highly conscientious reviewers. So what can be done to minimize some of the problems dramatized by Trafimow and Rice? Unfortunately, Trafimow and Rice have little to offer in the way of concrete advice. In essence, they counsel awareness of the possibility that a wacky-seeming idea might really be brilliant, that a ridiculous-seeming prediction might be a sign of creativity, that difficulty wrapping one's mind around a concept might be a sign that the idea is truly revolutionary, and so on.

Although all of these suggestions are certainly worth bearing in mind, my guess is that many would-be reviewers will finish reading the Trafimow and Rice article with a slightly sinking feeling, not knowing how they should behave the next time they sit down to write a review. In the remainder of this commentary, I therefore focus on three primary issues. First, I briefly outline qualities of a good review. Although several previously published articles provide guidance on this topic (e.g., Lee, 2008; Roberts, Coverdale, Edenharder, & Louie, 2004; Tesser & Martin, 2006), these ideas bear repeating in light of the Trafimow and Rice article, which seems to contradict some of the conventional wisdom on appropriate criteria for scientific review and the role of the reviewer. Second, I distinguish several responsibilities that I believe lie uniquely with the editor and with the author, whose specific roles in the peer-review process were largely ignored in the Trafimow and Rice article. And finally, I end by offering several suggestions for improving the peer-review process.

QUALITIES OF A GOOD REVIEW

Reviews are intended to serve two primary purposes: They provide input to the editor for use in making a decision regarding the disposition of the manuscript, and they play a constructive role in helping authors to improve their manuscript and, more generally, their scholarship. Thus, reviews play both an evaluative or gatekeeping role and a developmental, collaborative, or generative role. Fortunately there is a good deal of overlap such that the qualities of a review that tend to serve one set of goals also tend to serve the other.

First, a good review is evaluative, but balanced. Although many of the concerns raised by Trafimow and Rice could be interpreted to mean that reviewers should be not be critical or evaluative (or at the least, that they should be less so), good reviews are necessarily evaluative. Editors rely on reviewers to tell them what is wrong with a manuscript from the particular vantage point that their expertise provides. Editors need to know if the reviewer sees problems with the theory or guiding ideas, the methods, the data and its interpretation, or with the fit between and among these elements.

Just as important but sometimes overlooked, editors also want to know what the reviewer thinks is right or good about a paper, especially which aspects are potentially important, innovative, and informative. And finally, editors want reviewers to flag and focus on the most important issues and to deemphasize or clearly label those problems that are minor and/or easily fixed. Thus, achieving balance and maintaining focus on the big issues are features of a good review that, as Trafimow and Rice point out, reviewers sometimes lose sight of.

However, I take issue with Trafimow and Rice's largely one-sided criticism of two evaluative tasks that reviewers typically, and properly in my view, take on in their reviews. First, they imply that methodological criticisms are often "cheap shots." Because no study is perfect, they argue, every study can be criticized on methodological grounds. Consequently, the existence of a methodological flaw should not in and of itself be used to justify rejection of a manuscript. Again I agree in principle, but I worry that these comments may cause reviewers to feel constrained from carefully scrutinizing a study's methods. Yet I would argue that providing a thorough and thoughtful methodological critique is the cornerstone of a good review, just as solid methods are the cornerstone of any empirical science. Indeed, scientific psychology is not united by any particular content

expertise or theoretical viewpoint, but rather by its reliance on a set of methods and procedures by which we evaluate the truth value of our claims.

Trafimow and Rice are similarly critical of reviewers' tendencies to require data in support of theoretical propositions, and they raise several important points in this regard. I agree, for example, that judgments about the amount of data needed to justify publication are necessarily subjective and therefore open to abuse. I also agree that the evidentiary threshold required of any given paper should take count of the novelty and potential importance of its ideas. I also agree that psychology has been moving in the direction of requiring ever increasing amounts of data as a condition for publication and that this can have a chilling effect on creativity and forward movement in the field. These issues notwithstanding, psychology is an empirical science, and it is our insistence on evaluating the quality of ideas by their correspondence with empirical evidence that distinguishes us from other disciplines such as philosophy and literature. In short, reviewers should not be made to feel guilty about criticizing a study on methodological grounds or about evaluating its theoretical propositions on evidentiary grounds. Both are essential to any data-based science.

Second, a good review evaluates the connection between a paper and the literature. Trafimow and Rice argue that truly novel ideas by definition have only weak links to the existing literature and that reviewers should not insist on a strong connection between the submitted work and the existing literature (p. 38). Although I agree that strong ties need not always be established, it is the rare work that has no roots in the literature (particularly if one broadens the scope of that literature to include allied disciplines) and that would not benefit from tracing those roots, at least briefly, in the introduction to the paper. Moreover, one can only determine whether a paper covers new ground or brings fresh insight to an existing area by knowing the existing literature. Thus, assessing ties to the literature is a necessary and crucial part of a good review, in part because it provides the background for assessing the scope of a paper's contribution.

Third, a good review is specific and factually accurate. Many of the most important concerns raised by Trafimow and Rice could, I believe, be addressed if reviewers routinely backed up their main points with reference to specifics, be they specific examples from the paper or specific elaborations on their own criticisms and arguments. Providing detail requires that reviewers process their arguments more deeply than they otherwise might. For example, a reviewer might form an impression based on an initial reading of a manuscript that a disjunction exists between the hypotheses and the data. However, if the reviewer were to search the paper for specific examples of this problem and attempt to elaborate his or her point using these examples, any weaknesses in the criticism would likely be revealed. Thus, the very process of instantiating one's criticisms requires deeper processing of those criticisms and thus can serve to both weed out faulty or irrelevant criticisms and clarify and strengthen valid ones. As a result, reviews that are well documented will be more accurate on average and more helpful to both the editor and the reviewer, as their message will be easier to understand. Moreover, well-documented reviews are also likely to be perceived as both fairer and more credible, to the extent that unsubstantiated statements contribute to an impression (perhaps rightly so) that the reviewer has not thought deeply or fully about the issues at hand.

Fourth, a good review is fair and unbiased. Many of the issues raised by Trafimow and Rice concern conflicts of interest that occur when reviewers review papers that bear directly on their own research. In such cases, reviewers may be unfairly predisposed to review the work either positively or negatively, depending on the implications of the submitted work for the reviewer's own work. As Trafimow and Rice point out, this is a difficult situation because the most knowledgeable reviewers are typically those working in a given area, and these are the same reviewers most likely to have a conflict of interest. Thus, one can trade off expertise for impartiality, but it is hard to have both in equal measure. The best advice I can give, and the advice most commentators give, is to decline reviewing a paper if you question your ability to provide an impartial review. It is interesting that in 6 years serving as an editor, I recall only one time that a reviewer explicitly turned down a request to review because of a conflict of interest. This suggests that such conflicts are extremely rare, that reviewers are reluctant to acknowledge their existence, that reviewers have a great deal of confidence in their ability to render fair evaluations of works despite potential conflicts, or that reviewers are not as sensitive to these issues as perhaps they should be. It is also possible, though rare in my opinion, that reviewers knowingly accept reviews of manuscripts they cannot evaluate in an unbiased fashion because they wish to influence the outcome of the review. Perhaps all these possibilities are true to varying degrees across reviewers and within reviewers across different reviews. Regardless this is an area where efforts to heighten awareness of the issues might prove especially important.

Fifth, a good review is tactful. Although Trafimow and Rice did not explicitly complain about tactless, rude, or nasty reviewer comments, the fact that their simulated decision letters were littered with such comments certainly suggests that they view this as a problem. Fortunately, truly rude or nasty comments are rare in my experience, though tactless ones unfortunately are not. Carelessly worded statements that may be offensive or hurtful to an author are most likely to occur, in my view, when reviewers are single-mindedly focused on communicating their concerns in a compelling and authoritative manner. Tactless comments can often be avoided, however, by setting the review aside for a day or two and then rereading it for emotional tone. Reading it aloud also helps to make the emotional impact of one's words salient. Both strategies facilitate perspective-taking, making it easier not only to identify tactless

and potentially hurtful statements, but also passages that are confusing or unclear. The net result is a review that is more clearly written, less injurious to the author's ego, and therefore more effective in getting its message across.

Finally, a good review treats the author as an equal. Good reviewers do not talk down to authors or insist on having things done their way. As Trafimow and Rice point out, an author may choose to do something differently than the reviewer might have done it, but this does not necessarily make it wrong. Thus, in cases where the difference ultimately boils down to a matter of choice or style, reviewers must respect the author's right to make the call. In my experience, one area where differences often arise concerns the analysis and interpretation of data. Though, on the surface, this seems like an area where relatively clear-cut standards ought to exist, disagreements among experts over data analytic issues are surprisingly common. Indeed, data issues often involve competing considerations and multiple trade-offs, and these can be weighted differently by reviewer and author to arrive at reasonable yet different decisions. In such situations, reviewers are right to draw attention to the alternatives and their relative merits and even to argue that the data should be analyzed and reported both ways. But reviewers who insist that data be analyzed or interpreted their way and their way only are on shaky ground, in my view.

In short, reviews are most helpful to both the editor and the author when they are thoughtful, balanced, fair, sensitively worded, and collaborative in nature and when the main points are sufficiently detailed and developed that both audiences can grasp the issues being raised and understand what, if anything, can be done to address them.

THE EDITOR'S RESPONSIBILITY

Trafimow and Rice say little about the specific responsibilities of the editor in addressing the issues raised in their article. Yet, I was struck by how often the editor was, at least in my view, the real culprit in the failures and shortcomings highlighted in their editorial letters. In one of the only comments specifically directed to editors, Trafimow and Rice suggest that editors should solicit reviews from researchers who do not have a stake in the issue or that they should be more willing to overrule reviewers who do, and this is a point with which I heartily agree. But what other steps should editors take to safeguard the integrity of the review process? I believe there are two areas of the review process in which editors can play a particularly important role in constructively shaping the outcome.

Reviewer Selection

The seeds of many of the problems highlighted by Trafimow and Rice are sown at the outset when reviewers are selected. Accordingly, the careful selection of reviewers is the first point in the process where editors must exercise due diligence. The task of selecting reviewers is surprisingly challenging, however, as editors seek to satisfy multiple criteria, including relevant expertise, fair mindedness, conscientiousness, and willingness to review. Within these sometimes formidable constraints, however, two strategies can be used that, in my experience, lead to an overall increment in the quality of the reviews.

First, standardized procedures should be established in which editors rate the quality of submitted reviews. The resulting data can then be used as one criterion for selecting reviewers. Having a systematic rating procedure in place allows each editor to draw on the experiences of the entire editorial team in identifying good reviewers and in weeding out those who consistently submit poor quality reviews. Fortunately the advent of centralized, online resources for manuscript processing makes the tasks of obtaining, compiling, and disseminating this information to the editorial team much easier than in the past.

Second, editors should purposefully select reviewers to represent different areas of relevant expertise. For example, one reviewer might be chosen for theoretical expertise, a second for expertise on the particular behavior under study, and a third for methodological expertise. Such an approach helps to ensure that the contributions of a paper will be considered from different perspectives and levels, as Trafimow and Rice recommend, and also that any bias present in the reviews will at least represent different types of bias. Editors who follow this strategy can direct reviewers to evaluate the paper primarily through the lens provided by their particular expertise, while at the same time asking them to comment on the manuscript as a whole. Each reviewer is thus in a unique position to comment on the accessibility of the paper and its interest to a broader audience and to provide an unbiased (though admittedly less expert) evaluation of those parts of the paper that lie outside his or her primary area of research.

Of course this practice can lead to another oft-cited criticism of peer review: a lack of consensus (i.e., dissensus) among reviewers about the strengths, weaknesses, and overall merit of a paper (e.g., Hargens & Herting, 1990; Whitehurst, 1984). Disagreement among reviewers need not be interpreted as an indictment of the review process; it can instead be seen as a reflection of the unique perspectives from which each reviewer approaches the paper. Nevertheless, reviewer dissensus poses serious problems for an author if the editor is not willing to prioritize among or in some way reconcile these differences, which leads us to the second part of the review process where an editor must step up to the plate.

Crafting the Action Letter

Street and colleagues found that authors rated the failure to intervene when reviewers provide conflicting advice the single most egregious behavior, and the second most common behavior, among a list of 10 negative editor behaviors (Street, Bozeman, & Whitfield, 1998). So why do editors commonly avoid weighing in

on something so obviously important? One reason is that editors rely on the good will of reviewers and don't want to risk losing that good will by siding with one reviewer over another. Another reason is that grappling with conflicting comments to come up with a resolution or synthesis is hard intellectual work. It requires a careful reading of both the manuscript and reviewer comments, as well as deep processing of these materials. Even with that, a single best solution may not emerge, thus precluding a clear directive to the author about what to do next. Authors will be much more accepting of this, however, if it is clear to them that the editor exercised due diligence—that is, if the editor read and thought carefully about the manuscript and the issues that were raised and provided at least some guidance on how the issues might be clarified and a resolution pursued.

The Author's Responsibility

Trafimow and Rice largely portray authors as innocent victims in the review process. Although it is clear that authors can indeed be victimized through no fault of their own, authors sometimes contribute to the problems highlighted by Trafimow and Rice in their critique. Indeed an alternative explanation for reviewer comments that are factually inaccurate, incomplete, or superficial can sometimes be found in the quality of the author's writing or thinking. When papers are poorly written, organized, or reasoned, reviewers are more likely to get it wrong in one way or another. Reviewers are also, understandably I think, less motivated to write a carefully crafted and detailed review in such circumstances. As a reviewer, there may be nothing more annoying than the perception that you are thinking harder about the author's work in preparing the review than the author did in writing the paper! Thus, suffice to say that sometimes the blame for reviews that are off the mark lies with the author more than with the reviewer.

SOME SUGGESTIONS FOR IMPROVING THE PEER-REVIEW PROCESS

I want to conclude by offering three suggestions that I believe hold particular promise for addressing many of the concerns raised by Trafimow and Rice.

Increase Formal Training Opportunities

Reviewers are not taught how to review. Learning occurs largely by trial and error or by emulating the style and substance of reviews one receives, which, as the Trafimow and Rice critique illustrates, could be one source of the problem. Formal training could be usefully implemented at a variety of levels (see Raelin, 2008, for a more in-depth discussion). Lectures and activities could be incorporated at the graduate level, for example, as part of a professional socialization seminar. Bringing a graduate student on as a "coreviewer" is another possibility, one that can provide a highly effective form of supervised experiential learning. Although this should be cleared in advance with the action editor, most editors are comfortable with this arrangement so long as confidentiality of the manuscript is protected and the primary reviewer retains ultimate responsibility for the review. Indeed, some journals even have a formal provision for adding a coreviewer. Professional associations also have a stake and could sponsor workshops or talks during their annual conferences or meetings on the craft of reviewing. Lastly, editors are in a special position to educate reviewers on their role in the peer-review process through a variety of both and informal communications, and they themselves can set an example through the crafting of thoughtful, balanced, constructive, and respectful action letters.

Increase Accountability

A lack of accountability, owing in part to reviewer anonymity, has been identified as an important cause of problems in the peer-review system. Although requiring reviewers to reveal their identity has been suggested as one obvious remedy (e.g., Epstein, 1995), the costs of doing so would appear to outweigh the benefits (Suls & Martin, 2009, this issue; see also Fine, 1996). However, two suggestions have been put forth that, in my view, have particular merit.

First, Epstein (1995) proposed that authors should be provided a structured opportunity to give reviewers feedback at the close of the review process. Providing such an opportunity would have several benefits. As Epstein (1995) points out, knowledge that one's reviews will be explicitly evaluated should encourage more conscientious, fair, and constructive reviews. Such feedback would also serve an important educational function, helping reviewers to spot deficiencies in their reviews and improve their overall competence as reviewers. In addition, the data from author evaluations would provide another important source of information for vetting and selecting reviewers.

Although Epstein did not propose allowing authors to rate editors, I see no reason why they shouldn't. Fine (1996) noted that the lack of anonymity and a fear of retaliation by editors would prevent authors from providing honest feedback. However, these problems seem easily surmounted with the advent of centralized, online procedures for tracking manuscripts through the review process.

Second, Epstein (1995) also suggests that an appeal process, which offers the possibility of a new, independent review, should be established for authors who believe that their manuscript was improperly handled. Although my belief is that only a small percentage of editorial decisions would be appealed (and an even smaller percentage would be successful), the mere existence of an appeals procedure would have important benefits. It would provide recourse for authors whose papers were in fact mishandled, and would minimize the types of errors highlighted by Trafimow and Rice that lead to meritorious papers being wrongfully rejected. It would also enhance the perceived fair-

ness of the process and provide authors with some sense of control. Finally, knowledge of the existence of an appeal process should encourage more balanced and unbiased reviews, as well as more thoughtful, deliberate, and well-documented editorial decision letters. In short, although implementing both ideas would carry some increased administrative burden, the potential benefits appear substantial.

Reduce Reviewer Burden

Finally, many of the problems identified by Trafimow and Rice occur in part because reviewers are overburdened, making it difficult to find the time to write thorough and thoughtful reviews. Thus, taking steps to reduce reviewer burden could also lead to higher quality reviews. In this regard, several possibilities should be explored.

First, editors could triage (i.e., return without review) a larger proportion of submitted manuscripts. The typical triage process works something like this. A senior editor reviews all papers within a week or 10 days of submission to determine which should be sent out for review. Papers judged to be inappropriate for the journal or uncompetitive (i.e., to have little or no chance of being accepted) are flagged and returned to the author with a decision letter explaining why the paper was rejected without review. When I write such letters, I try to identify the three or four most important problems I see with the paper and provide enough detail about these issues so that the author can see that the decision, though reached without the benefit of input from reviewers, was nevertheless based on a careful reading of the paper. I also try to offer suggestions for possible next steps, including alternative publication outlets, ways to refocus the paper, additional data that might be collected, and so on. Triage is more widely used outside of psychology and has been well-received in those fields (Suls & Martin, 2009, this issue). Its benefits to the author, who receives quick turnaround on a paper that was unlikely to be accepted, and to the field, in terms of reduced burden on the peer-review system as a whole, are well recognized. Although the practice of triage raises legitimate concerns about the potential for abuse, it will be important in the coming years to carefully consider how triage can be more widely implemented, while still safeguarding the rights of authors (e.g., through appeals processes).

Second, editors should also consider soliciting fewer reviews per paper. Although many journals routinely obtain three or even four reviews, two is often sufficient to render an informed decision. If reaching a confident editorial decision based on two reviews proves difficult, an editor can always solicit an additional review. Although this can lead to delays for those whose manuscript requires a third review, the majority of papers will in fact be handled more quickly. Editors should likewise try to reach decisions on revised manuscripts without the benefit of additional review whenever possible. Moreover, in situations where reviewer input on a revised manuscript is required, the editor can minimize additional burden by focusing the reviewer's attention on specific aspects of the manuscript where questions remain.

Finally, psychology should experiment with methods of sharing reviews across journals, as some fields are already doing. In January 2008, a consortium of nearly 40 neuroscience journals, called the Neuroscience Peer Review Consortium (http://nprc.incf.org/), was established for the sole purpose of sharing reviews across participating journals. The system works like this. If a manuscript is rejected by one journal in the consortium, the author can resubmit that manuscript to any other journal in the consortium and request that the reviews be forwarded from the rejecting journal to the new journal. Authors are instructed to write a cover letter detailing the changes that were made to the manuscript in response to the reviews, and the editor of the new journal then renders an editorial decision based on the author's letter and the existing reviews. Authors have choice in the matter as do editors. If an author is unhappy with the reviews, he or she is not obliged to have the reviews forwarded. Likewise if the editor finds the reviews lacking for any reason, additional reviews can be requested. A similar but less coordinated policy, called streamline review, has been in place at the *Journal of Research in Personality* since 2004 (Cooper, 2004) and has been very well-received. Review sharing represents, in my view, one of the most promising developments in the peer-review system in many years. It reduces reviewer burden, saves editors time by eliminating the sometimes arduous step of securing qualified reviewers, and saves authors time because editorial decisions can be rendered more quickly.

THE FUTURE OF PEER REVIEW

Trafimow and Rice are to be commended for raising a number of important issues about peer review. Although more sweeping reform may ultimately be required to address these issues (see, e.g., Suls & Martin, 2009), such reforms are unlikely to be fully implemented any time soon and, moreover, many of the proposed reforms rely on some form of peer review. Thus ongoing efforts to scrutinize and improve peer review are warranted, as it is likely to remain an indispensable ingredient in the publication enterprise.

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