The Air We Breathe: A Critical Look at Practices and Alternatives in the Peer-Review Process

Jerry Suls and René Martin

Perspectives on Psychological Science 2009 4: 40
DOI: 10.1111/j.1745-6924.2009.01105.x

A JOURNAL OF THE ASSOCIATION FOR PSYCHOLOGICAL SCIENCE

Issues in Publishing, Editing, and Reviewing

The Air We Breathe

A Critical Look at Practices and Alternatives in the Peer-Review Process

Jerry Suls[1] and René Martin[1,2]

[1]University of Iowa and [2]Center for Research in the Implementation of Innovative Strategies in Practice, Veterans Affairs Medical Center, Iowa City, IA

ABSTRACT—*Anonymous peer review has served as the bedrock of research dissemination in scientific psychology for decades and has only sporadically been questioned. However, other disciplines, such as biomedicine and physics, have found the traditional peer-review system to be wanting and have begun to test and try alternative practices. In this article, we survey criticisms of the traditional peer-review system and describe several alternatives in the interests of facilitating discussion and debate. We also consider why the natural sciences tend to employ fewer reviewers and have lower rejection rates than do the social sciences. Our two recommendations are that a serious discussion of problems and alternatives to peer review should be started at all levels of psychology and that a science of research communication should be a priority, with psychologists as part of its advance guard because of their relevant substantive and methodological knowledge.*

> *Reviewing is essential to all sciences. It is a shame we have not taken it more seriously.*
>
> —Sternberg, 2006, p. x

In psychology, as in other sciences, the peer-review system is as ubiquitous as the air we breathe—so pervasive, so ever-present, and so taken for granted that it is difficult to cast a critical eye in its direction. If explained to a novice, an overview of the peer-review system might go something like the following. New empirical findings are submitted for consideration, and, if accepted, they are disseminated via publication in scientific journals. The journal editor plays the role of gatekeeper and ultimately is responsible for making the decision whether to publish the manuscript as submitted, request revisions subject to further review and evaluation, or outright reject the submission from further consideration. The editor's decision generally is made only after consulting with other experts—that is, the peers—in a process dating from the 18th century. The peers' role is to provide input as independent, impartial, and unbiased third parties able to ascertain the scientific worth of the submission as a function of their own expertise. The peers provide their reviews under conditions of anonymity and typically are drawn from the journal's editorial board and from the field at large. The underlying assumption is that guaranteeing reviewers' anonymity will allow them to be frank and honest with impunity. The utilization of independent peer evaluations led one historian of science (Ziman, 1969) to call the peer-review process "... the key event in the history of modern science" (p. 318).

Although the introduction of peer review has been very significant, the inequities associated with this convention are frequently discussed informally. The casual eavesdropper strolling through any poster session or social hour at a professional conference will overhear an abundance of editorial complaints. However, in the behavioral sciences, formal study and discussion of peer review and its potential alternatives has been sporadic (e.g., Latané, 1978; Mahoney, 1985; Peters & Ceci, 1982). In contrast, colleagues in the biomedical and physical sciences (e.g., Ginsparg, 1994; Godlee & Jefferson, 2003; Smith, 2006) have been far more active in their discussions of peer review and related publication issues, leading to empirical investigations of the review process and the adoption of alternative review strategies. Peer-review defenders remain in all fields, however, and some surveys suggest that authors generally are satisfied with the system as it is (e.g., Nickerson, 2005). Both Alpert (2007) and Triggle and Triggle (2007) likened peer review to Winston Churchill's assessment of democracy—that is, "... the worst form of government except all the others that have been tried." There is a critical difference, however, between peer review in psychology and forms of governance. In psychology,

Address correspondence to Jerry Suls, Department of Psychology, E-11 SSH, Spence Laboratories, University of Iowa, Iowa City, IA 52242; e-mail: Jerry-suls@uiowa.edu.

PERSPECTIVES ON Psychological Science

alternative review strategies have not been tried—in fact, they scarcely have been considered.

Our purpose is to describe the evolution of current editorial practices, detail criticisms and concerns about peer review and then consider alternative strategies, some of which currently have been adopted or are being tested by other fields. Although we do not champion any particular editorial option, we argue strenuously for serious discussions of alternative models and for a psychological science of research communication. This area has been markedly understudied in psychology, yet it is one for which psychologists are eminently suited to serve as the advance guard, rather than the rear guard (see Mahoney, 1985).

A SHORT HISTORY OF PEER REVIEW

From the 18th through the middle of the 20th century, most scientific and medical journals functioned rather like newspapers or magazines. Journal editors did not perceive the need for outside or specialized expertise and made their own decisions. Moreover, editors typically had more page space than submissions; thus, they frequently had to solicit material or write it themselves. On those rare occasions when the editor felt unable to judge a paper, local colleagues provided advice. Peer review originated with the Royal Society of Edinburgh in 1732 and subsequently was adopted by the Royal Society of London in 1752. Even in its earliest iteration, peer review resembled contemporary practices in that experts provided anonymous comments to the editor and authors (Kronick, 1990). However, the adoption of peer review proceeded slowly. In 1873, the editor of the *British Medical Journal*, Ernest Hart, instituted peer review by special experts. Hart proposed peer review to a large assembly of North American physicians as a means of improving their published science. But there is no evidence that North American medical editors adopted this innovation or that peer review spread from editor to editor by emulation (Burnham, 1990).

Several factors contributed to the widespread adoption of peer review across the physical, biomedical, and behavioral sciences after World War II, including increased federal support for research, dramatic growth in the research community, increased specialization among researchers with a concomitant demand for reviewer expertise, and intense competition for journal page space (Burnham, 1990). However, editorial practices appear to have changed piecemeal, with each editor and journal rediscovering the process anew (Burnham, 1990). Perhaps this is because various scientific fields and journals experienced the need for increasing editorial expertise and page space competition at different points in time. However, a definitive answer to this question is unlikely because, in contrast to the elegant records kept for laboratory experiments and clinical practice, ". . . good records of the editorial process . . . from any period before World War II either do not exist or have yet to come into the hands of historians" (Burnham, 1990, p. 1323).

PEER-REVIEW PRACTICES IN PSYCHOLOGY

The technology associated with the editorial process has improved (e.g., electronic submission, Web-based editorial management systems, and e-mail). These advancements were initiated to reduce the managerial burden of assigning and tracking reviewers, facilitate the processing of editorial action letters, and (potentially) provide prompt decisions to authors. Despite these technological changes, the philosophy and practice of peer review in psychology has remained unchanged since the 1940s.

Number of Reviewers

The most prestigious journals in psychology experience intense competition for pages and have rejection rates ranging from 80% to 90%. A majority of rejected papers eventually find homes in less competitive journals, usually after being refurbished with additional data and conceptual reframing (Lock, 1985). The remainder—the orphans—languish in authors' file drawers or hard drives. In psychology and other behavioral sciences, it is rare for a manuscript to be accepted for publication on its first submission. In the case of manuscripts "accepted with minor revisions," the editor usually evaluates the revised paper from his or her desk, without seeking further council from reviewers. Editorial practices are more variable for those manuscripts rejected with an option to resubmit. Such manuscripts may require data reanalysis and/or the collection of new data, so most editors will solicit another round of reviews for the revision from either the original reviewers or from new reviewers. An editorial minority will read and make the final decision regarding a revised manuscript without a second round of reviews (see Suls, 2001; Zanna, 1992, for exceptions).

The number of reviews sought by editors has tended to increase over time, especially for journals published by the American Psychological Association (APA). In the 1950s, editors (even of competitive journals) commonly solicited no more than two reviews. By the 1990s, many APA journal editors were encouraged to obtain three to four reviews (Finke, 1990). Journals produced by other publishers and more specialized journals generally require fewer reviewers (Zanna, 1992), although making an editorial decision on the basis of a single review always is strongly discouraged.

Triaging

Editors, of course, always have had the option of returning an off-topic, inappropriately formatted, or clearly uncompetitive manuscript to its author without having sent it out for review (a practice called *triaging*). *Psychological Science*, the general empirical journal of the Association for Psychological Science (APS), triages submissions. Upon receipt, the editor and an associate editor read each new submission and make a decision regarding its likely competitiveness for publication. Within 2 weeks of submission, authors are notified via e-mail that their manuscript either has been declined without review (i.e., triaged) or sent to outside referees for review. APA journals also use a triaging policy, with the senior editor typically returning less than 10% of submissions without external review. Triaging is used sparingly in psychology and has elicited few scholarly complaints. However, triaging is likely to become a more frequent outcome if imbalances between page space and submissions continue or intensify.

Reviewer and Author Anonymity

Although there are some variations in the operational details, anonymous peer review is used by the vast majority of journals in psychology and related social and behavioral sciences. In fact, APA policy prohibits reviewers from identifying themselves to authors. Author identity is also sometimes concealed from reviewers (see below). For some journals, this policy has been instituted for all submissions. However, the more common scenario is that blind review is available to authors only when explicitly requested.

PEER REVIEW AND PUBLICATION IN CROSS-DISCIPLINARY CONTEXT

Some comparative statistics are presented to provide a context for the discussion of frequent criticisms of peer review to follow. In psychology, approximately 2% of manuscripts are accepted for publication upon initial submission, with 20% to 40% of revised manuscripts eventually being accepted (Eichorn & VandenBos, 1985). In a wider survey, Zuckerman and Merton (1971) found acceptance rates of 10% to 30% in the social sciences and humanities and 60% to 80% in the physical sciences. Manuscript triage is fairly rare in psychology. However, it is used extensively in the biomedical sciences, especially in high-profile journals that receive thousands of submissions every year such as the *Journal of the American Medical Association* (*JAMA*) and the *New England Journal of Medicine* (*NEJM*). Editors in the physical sciences rely on fewer reviewers than their social and behavioral science counterparts, typically soliciting one or, at the most, two reviews (Hargens, 1988); the number of reviewers sought in the biomedical sciences appears to be more variable. Articles in physics, biology, and biomedicine tend to be brief, with publications of less than 5 pages being normative, and the physical sciences enjoy the briefest time lags between submission and publication (Garvey, Lin, & Nelson, 1970). Finally, in the physical sciences, a small number of highly prestigious journals publish a majority of the literature, whereas in the social sciences, the most prestigious journals publish small proportions of the literature (Garvey et al., 1970).

Several explanations have been advanced for the differential acceptance rates for the social versus physical sciences. One is that federal and institutional funding for physical (or so-called "hard") science research has been far greater than it has been for psychology and other social and behavioral sciences. In fact, authors publishing in hard science journals typically pay all or part of the publication costs for their article. Grants and funding requests for hard science research nearly always include substantial provisions for publication costs in addition to funds to cover the costs of the research itself (Bornstein, 1990). Publication charges presumably allow for more pages and, therefore, less competition that, in turn, allows for higher acceptance rates. In psychology journals, the page costs are born by professional societies (via dues and subscriptions) or commercial publishers (via subscriptions). Thus, pages are scarcer; competition encourages the solicitation of more reviews and perhaps increases the likelihood of negative comments. However, additional factors are likely to drive the preponderance of negative reviews obtained in the social and behavioral versus physical sciences.

Some scientists, sociologists, and philosophers of science argue that social and behavioral science paradigms are less developed than their physical science counterparts (Kuhn, 1970), contributing to relatively low consensus regarding manuscripts worthy of publication (Hargens, 1988). Hargens (1988) observes, "When scholars do not share conceptions of appropriate research problems, theoretical approaches, or research techniques, they tend to view each other's work as deficient and unworthy of publication," (p. 147). This is a popular idea; in fact, one of the present authors fell prey to this idea in an earlier publication (Suls & Fletcher, 1983). However, as we will see, it is incorrect.

CRITICISMS OF PEER REVIEW

> *Although we regard manuscript reviews as a 'test' or measure of scientific worth . . . even a cursory reading of the APA's Standards for Educational and Psychological Testing reveals that this 'test' fails . . . with respect to every technical criterion for establishing the reliability and validity of an assessment instrument.*
>
> —Bornstein, 1991, pp. 444–445

Rennie (2003), alleges that the present approach to peer review is ". . . unreliable, unfair and fails to validate or authenticate" (p. 8). In fact, criticisms of peer review abound. Speaking tongue-in-cheek—knowing full well that there are many hardworking and discerning reviewers—critics of the peer-review system caricature reviewers as mean-spirited, lead-footed, capricious toadies and hacks who hide behind the cloak of anonymity. Even worse is the suggestion that the current peer-review system fails to produce superior science. In this section, we evaluate the evidence for such claims.

Reviewers as Hacks?

Authors often complain that reviewers' comments reveal their lack of familiarity with the subtleties of the research reported. Who constitutes a peer in peer review? Someone in the same discipline? A methodologist? Someone who conducts the same kind of research? Although an active researcher in the same domain will possess the greatest expertise, that person also is likely to be a direct competitor (Smith, 2006). This raises the possibility that limiting the dissemination of an author's findings could be in a reviewer's best interests. Thus, editors are challenged to balance the benefits of reviewer expertise with the potential for bias, whether unintentional or intentional.

Wrapped in the Cloak of Anonymity?

Peer-review practices are seen as unfair because reviewers make their comments and recommendations anonymously, blinding authors to the source of their criticisms. Some critics argue that anonymity undermines accountability and promotes both irresponsibility and malice. Consistent with this perspective, the results of a survey of review accountability revealed that a majority of academic psychologists reported receiving reviews with obvious errors in fact (73%), subjective reviewer judgments that were treated as objective truths (76%), and other unconstructive and unscientific qualities (Bradley, 1981). Critics propose that identifiable or open reviewers would be accountable and thus motivated to produce high quality reviews.

Those who defend reviewer anonymity note that masking allows reviewers to be forthcoming without fear of future retribution. This issue might be especially important for scientists early in their careers who are asked to review a senior scientist's research. One defense of the present system is that reviewers might "pull their punches" if their identities were made public. Another defense is that editors already find it difficult to recruit reviewers from among the ranks of overburdened academic psychologists; taking away reviewer anonymity might further discourage potential reviewers from taking on a largely thankless task. However, this issue has received limited empirical exploration. Identifiability in the peer-review system has the potential to reduce well-known social psychological phenomena in which group members who feel anonymous eschew their social responsibilities (e.g., social loafing, free-riding, the commons dilemma). In addition, ". . . as a side benefit, referees would be recognized for the work they had done (at least for those papers that were published)" (Armstrong, 1982, p. 198).

Five trials have assessed review quality (as rated by editors and authors) as a function of reviewer identifiability. Results did not suggest notable difference in review quality (Godlee, 2002), but there were downsides. Under identifiable conditions, potential referees were more likely to decline invitations to review, and those who did review were more likely to recommend manuscript acceptance. In a particularly noteworthy trial, Godlee, Gale, and Martyn (1998) altered a manuscript (with authors' permission) already accepted for publication in *British Medical Journal* (*BMJ*) by intentionally introducing eight new weaknesses in design, analysis, or interpretation; they then manipulated both reviewer and author identifiability and sent the altered manuscript out for review ($N = 221$). On average, reviewers commented on only two (of eight) errors, and error detection did not vary as a function of either reviewer or author identifiability.

Mean-Spirited, Lead-Footed Reviewers?

There is some evidence that controversial work is more likely to receive harsh reviews (Smith, 2006). Merton (1968) observed that Mendel's genetic discoveries were "neglected for years," (p. 62). Horrobin (1990) documented 18 cases in the biomedical sciences where major innovations were initially blocked by the peer-review system (see also Garcia, 1981). There may be an intrinsically conservative bias in the review process, but this may also be characteristic of both psychology as a field and science in general. An alternative perspective would argue that truly innovative research deserves the benefit of the doubt, even if it possesses some deficiencies in its earliest stages. This approach assumes that novel ideas will attract attention and generate enthusiasm among other researchers who then will work out the "bugs" in their subsequent research.

At minimum, reviewers in the social scientists seem to take their gatekeeper role (finding weaknesses in the manuscript) more seriously than their generative role (finding the positive contributions of the paper; Tesser & Martin, 2006). This may be because appearing to be too lenient seems worse than appearing to be too harsh. In a relevant experiment, Amabile (1983) asked subjects to form impressions of stimulus persons who made substantive positive or negative assessments (reviews) of a single entity (books). The negative reviewers were perceived to be more intelligent (but less kind) than the positive reviewers. It is probably not too much of a leap to conjecture that peer reviewers have this metaknowledge. If reviewers want to be seen as competent, they are likely to perceive that negative comments will be more likely to garner the editor's esteem (Glenn, 1982). To paraphrase and replace a few of Amabile's (1983) words, "[reviewers] who are particularly concerned with an [editor's] perceptions of their intelligence will tend toward negative criticism as a strategy of impression management" (p. 153). But scientists, editors, and authors have been slow to recognize the social psychological aspects of peer review.

Reviewers as (Conscious or Unconscious) Toadies?

A frequent criticism is that peer review rewards the prominent (i.e., senior scientists and research from prestigious institutions) while holding junior authors and those from less elite institutions to a higher standard. Some of the evidence for this perspective is anecdotal. For example, Merton (1968) relates that a very famous physicist's submission was rejected because his name had been left off the manuscript; once his identity was revealed, the editorial decision was reversed.

Peters and Ceci (1982) provided empirical evidence for prestige bias by selecting 12 papers authored by researchers from elite institutions that already had been published in top-tier psychology journals (with rejections rates $\geq$80%). These articles were retyped with slight changes to the title, abstracts, and introductions; most notably, the authors' names and affiliations were changed to invent low-profile institutions (e.g., "Tri-Valley Center for Human Potential"). The papers then were resubmitted to the same journals in which they had already been published. The editors or reviewers recognized that the paper already had been published in only 3 of the 12 instances. More significantly, eight of the remaining nine papers were rejected by the reviewers because of poor quality (e.g., "serious methodological flaws"). Peters and Ceci suggested that the reason for the rejection of previously published papers was bias with respect to authors' status and institutional affiliation. Their idea was that the original reviewers had approached the manuscripts with salient stereotypes associating elite institutions and superior empirical work and that their evaluations had thus been subject to a self-fulfilling prophecy (Rosenthal, 1966). But when virtually identical manuscripts were labeled with unknown authors and institutions, lower quality was both expected and perceived, leading to negative reviews and recommendations to reject. Peters and Ceci acknowledged counterexplanations for their findings (e.g., perhaps, by chance, the subsequent reviewers were less competent), and their report engendered much attention and controversy (see Open Peer commentary, Peters & Ceci, 1982, pp. 196–246). Although Peters and Ceci's study remains controversial, a wealth of experimental social psychological evidence supports the pervasive influence of halo and prestige effects. There is no obvious reason why peer reviewers would be immune to such social perceptual biases.

The obvious—and seemingly straightforward—way to minimize prestige bias is for editors to blind reviewer to the identities and institutional affiliations of authors. Unfortunately, this strategy is surprisingly difficult to implement. Four randomized trials in biomedicine found that reviewers were able to successfully identify the authors of a blinded manuscript in 23%–42% of cases (Godlee et al., 1998; Justice et al., 1998; McNutt, Evans, Fletcher, & Fletcher, 1990; van Rooyen, Godlee, Evans, Smith, & Black, 1998). A survey by an editor of *Physics Review Letters* found that referees could correctly identify 80% of the submitting authors, despite efforts to mask author identity (Adair, 1982). It is possible that physics provides a narrower range of potential reviewers, but anecdotal evidence suggests that psychology reviewers often are able to correctly guess authors' identities. Even when inaccurate, referees' hunches regarding author identity introduce a source of bias into the review process.

Capricious Reviewers? The Lack of Interreviewer Agreement

Scientists concur about the criteria to be used in evaluating new research, including theoretical and practical significance, substantive interest, methodological competence, quality of presentation, adequacy of literature review, objectivity in reporting results, and value for future research (Sternberg & Gordeeva, 1996; Wolff, 1970). In fact, editors often provide such criteria in their instructions to reviewers. Yet, reviewers rarely agree regarding the merits of any given manuscript. On average, the Cohen's kappa for interreviewer agreement is less than .40 (Cicchetti, 1991), a value considered "poor" by psychometric standards. For key criteria such as design, analysis, importance, and recommendation regarding publication, reliability coefficients are even worse, ranging from .19–.28 (Marsh & Ball, 1989; Scott, 1974). There is the perception that interreviewer agreement is better in the physical sciences than in the social/behavioral sciences (Lodahl & Gordon, 1972), perhaps accounting for the relatively low manuscript rejection rates observed in those fields (Zuckerman & Merton, 1973). There does appear to be a codification of core knowledge in the physical sciences, as reflected by strong agreement among textbook authors on essential theories, analytic techniques, and findings; however, reviewer consensus is uniformly modest, even in the physical sciences, when the work under evaluation falls at the margins or frontiers of current knowledge (e.g., manuscripts, grant applications; Cole, 1992; Latour, 1987). For example, a study of National Science Foundation grant application reviews revealed substantial variability in reviewers' assessments and suggested that the "luck of the draw" with regard to reviewer selection played an important role in recommendations whether to fund any given proposal (Cole, Cole, & Simon, 1981).

Research on reviewer agreement in psychology journals reinforces this theme. In a content analysis of reviewer comments drawn from 400 reviews of 153 manuscripts submitted to APA journals, Fiske and Fogg (1990) found minimal overlap in the narrative comments offered by pairs of reviewers. Specifically, the same observation was made in only 0.44 instances per pair of reviewers. Reliability of the composite evaluation would be much higher if the manuscript were reviewed by a large representative sample of experts; Simonton (2004, p. 88) provides an example whereby 30 referees with an average among separate assessments of .20 would yield an impressive interrater agreement coefficient of .88. However, few critics of the current system, however, are pleading for more peer reviewers. Others have argued that the lack of agreement among reviewers is neither surprising nor problematic (Roediger, 1987). Fiske and Fogg (1990) noted that editors are quite aware of the lack of overlap among reviewers and observed that reviewers may be chosen to represent a range of perspectives and sensitivities to different types of defects. In Simonton's (2004) words, "Indeed those journal editors who deliberately solicit divergent reviews are probably enhancing the amount of creativity eventually seen. In contrast, if peer review were highly reliable, it would most likely exert a stifling influence on the discipline" (p. 90).

An Apparent Puzzle Resolved

If both the natural and social sciences exhibit low consensus at the frontiers of knowledge, then why is it that journals in the natural sciences have higher acceptance rates? The answer appears to be twofold. The natural sciences have more journal space, in part due to page charges, and so there is less competition than in the social/behavioral sciences (Beyer, 1978). Importantly, many natural science fields operate on a norm that submissions should be accepted unless they are patently wrong. A comment from Ziman (1968) is apt: "The general procedure is to allow all work that is apparently valid to be published; time and further research will eventually separate the true from the false" (p. 55). The fact that physicists are more content than psychologists to make Type 1 errors rather than Type 2 errors may give the reader pause (see Cole, 1992). One potential rejoinder might be that manuscripts in physics are overall of better quality than those in the social/behavioral sciences and therefore meet with positive reviews. However, the citation patterns for physics journals do not differ from those seen in the behavioral sciences. In both fields, the vast majority of published articles are rarely cited (Meyer, 1979).

This discussion raises an interesting point of curiosity. Our review suggests that page space in psychology is responsible for some of the distinctive editorial problems experienced in the social and behavioral sciences. In the age of online journals, page space is unlimited and becomes a nonissue. It remains to be seen whether acceptance rates will increase as more journals adopt exclusively online formats.

But What About the Product? Does Peer Review Produce Sound Science?

Authors understandably want their articles to appear in high-status, competitive journals that enjoy wide circulation (Gottfredson, Garvey, & Goodnow, 1977). But whether the papers appearing in competitive outlets actually represent the most outstanding science is an empirical question. One index, albeit imperfect, of a paper's value is how often it is cited by other researchers. However, reviewer ratings of study quality are only modestly predictive ($r = .24$) of later citations counts (Gottfredson, 1978). In an intriguing study, Starbuck (2005) reported that although prestigious journals published papers that went on to be highly cited, such journals also published many low-value, infrequently cited papers. In addition, lower prestige journals published some excellent, highly cited articles, and it was common for highly cited articles to have a history of successive rejections from multiple journals. The bottom line is that "Evaluating articles based primarily on which journals publish them is more likely than not to yield incorrect assessments of articles' values" (Starbuck, 2005, p. 196).

One defense of peer review as currently practiced is that more trivial or flawed papers will be published in its absence. This idea was discussed extensively 30 years ago in the context of Latane's (1978) argument that reviewers in personality and social psychology were ". . . self-destructively stifling our airflow of scientific communication . . ." (p. 23). In response, one editor noted, ". . . of the 85–90% of papers we reject, 65–70% are absolute disasters by anyone's standards, . . . the rest are often methodologically sound, but say very little more than has been said in . . . the literature already" (Wyer, 1978, p. 29). In other words, there were concerns that more liberal acceptance rates would increase the noise-to-signal ratio and inundate the field with too much information.

The concern about information overload can be appreciated, but it may be useful to place this issue in the context of other fields. Rejection rates are lower in chemistry and physics than in psychology. Are these fields overwhelmed by information overload? Using the Web of Science, we found 426 journals (in the fields of biochemistry, physical chemistry, and organic chemistry) that published 99,253 articles in 2006. By comparison, the Web of Science listed 112 psychology journals that published 22,655 papers in the same year (if psychiatry is included, the total jumps to 190 journals and 28,883 papers; Web of Science, May 31, 2008). Chemistry journals publish three times as many articles, yet physical scientists have been among the most outspoken advocates for open peer review, a system that would yield even more scientific information. We discuss this option at greater length below.

Summary

Peters and Ceci summed up the lesson learned as follows:

> For years, scientists have assumed that the review process is basically objective and reliable. Is it? Unfortunately, the peer-review process has not received the experimental attention given to other research topics—most of which have considerably less significance for and impact on scientists . . . unless we . . . learn more about the variables that do influence peer review, we are left with little to defend it other than faith. (p. 195)

Since then, other critics also have referred to peer review as a faith-based rather than evidence-based process (Linkov, Lovalekar, & Laporte, 2006).

THE STUDY OF PEER REVIEW

Despite the work of a handful of psychological pioneers in this area—including Michael Mahoney, Douglas Peters, Stephen Ceci, E. Rae Harcum, and Ellen Rosen—most research on peer review or journal practices in general has been descriptive. This also has been the case in the field of the sociology of science, which has at least two journals exclusively devoted to the subject (*Social Studies of Science* and *Scientometrics*). In the biomedical fields, this has been an active area of inquiry. Special issues of the *NEJM* have been devoted to peer review and British medical scientists have conducted small randomized trials to examine the effects of blinding reviewers, identifiable reviews, reviewer tutorials, and publication recommendations for reviewers suggested by authors or by editors. None of these experiments are conclusive (see Jefferson, Alderson, Wager, & Davidoff, 2002), and, if we may risk hubris, they would benefit from the input of psychological methodologists. But the fact is that psychological scientists apparently have shown little interest in launching or joining such research (see Mahoney, 1977, for an exception). This is unfortunate because there seems little doubt in the context of the preceding sections that peer review is strongly influenced by psychological and social psychological forces. The resistance to this pursuit on the part of psychologists is reminiscent of the early resistance to the social psychology of the experiment (Berkowitz & Donnerstein, 1982; Suls & Gastorf, 1980), reflecting perhaps the defensiveness associated with being a "soft" science. We suspect that, for some, the study of research communication might seem akin to navel gazing. As we will see below, however, other disciplines have taken up this challenge.

ALTERNATIVE MODES OF EDITORIAL REVIEW

Open Peer-Review Policy

Since 1999, *BMJ* (which is one of the top four general medical journals, along with *NEJM, JAMA,* and *Lancet*) has instituted an open peer-review policy, whereby all reviews must be signed.[1] It is noteworthy, however, that 50% of the submissions *BMJ* receives are triaged by an in-house action editor. Those manuscripts that survive triage to be sent out for peer review typically go to one or two external referees, who receive about £50 for their services. Final decisions on manuscripts are made by the editorial ("hanging") committee, usually consisting of a statistician, an external editorial adviser, and the paper's editor; the hanging committee reads and discusses each article's importance, originality, and scientific quality. The final decision on publication is usually reached within 8 to 10 weeks of submission. The acceptance rate for original research articles is 7%. If an offer of publication subject to revision is issued, authors must return their articles within 4 weeks.

Hybrid Systems: Pre-Posting and Open Exchange

Since 2001, the online journal, *Atmospheric Chemistry and Physics* (*ACP*), has combined traditional peer review with open exchange (Koop & Poschl, 2006). Upon submission, relevant members of the standing editorial board are asked to give a submission a quick look for any technical problems. If the paper meets the basic standards, it is posted on *ACP*'s Web site. Registered researchers can then post either signed or anonymous comments online, to which the authors can publicly respond. The action editor moderates the online discussion and edits out any personal attacks or inflammatory comments. After 8 weeks, the authors have the option of revising the paper or submitting it for traditional peer review. The reviewers for this latter stage are selected before the paper goes online, and they can also comment during the initial stage, albeit anonymously. Manuscripts that survive the entire process are officially accepted for publication and made immediately available. To provide a lasting record of reviews and to secure the authors' and reviewers' publication precedence, the editorial board permanently archives every discussion paper and interactive comment and keeps them individually citable. Another online journal, *Electronic Transactions on Artificial Intelligence*, has a similar two-tiered editorial policy (Sandewall, 1997; see http://www.ep.liu.se/ea/apt/1997/001/2006).

In a variation on *ACP*'s approach, *Lancet*, *BMJ*, and some of BioMed Central's 184 journals started to offer authors the option of posting their work on a preprint server while it undergoes peer review. Anyone who wishes may post comments, questions, and critiques. This offers authors the opportunity to respond to the identified reviewers directly (all correspondence is shared with editor) prior to any revisions. For these articles, the prepublication history for each paper (including submitted versions, reviewers' comments, and authors' responses) is linked to the online published article (see www.biomedcentral.com/info). Obviously, this level of detail will not be relevant for all readers, but the information may be very beneficial to researchers in the same specialty. Moreover, the Internet provides the space for this kind of comment and rejoinder that paper journals cannot accommodate.

Public Library of Science (PLoS)

Some online journals that are part of PLoS (the most prominent publisher in the open-access movement) use a variation on the procedures used by *ACP* and *Electronic Transactions on Artificial Intelligence*. Every submission is initially reviewed by at least one member of its editorial board, but they check only for serious flaws in the way the experiment was conducted and analyzed. The editorial board explicitly requests these referees to ignore the significance of the results. In the absence of serious flaws, the paper is posted, and visitors to PLoS may read and attach comments to specific parts of the paper and rate the paper as a whole. This information and citation statistics are periodically downloaded by the journal staff, permitting notable papers to be highlighted by the attention they attract after publication. Even those journals in PLoS that employ traditional (i.e., unsigned) peer review provide readers the opportunity to add ratings, notes, and discussions to published articles to facilitate ongoing consideration of the research. Perhaps the most notable aspect of PLoS is the opportunity it provides to assess a research report's impact online. The responses the report gets soon after publication determine its prestige and value, independent of the

[1]Open peer review should not be confused with open access, which refers to easily accessible, electronic distribution of research reports without charge. Some electronic archives (notably arXiv) have both features, but many do not.

status of the journal. This is probably the most explicit presentation of Smith's (2006) observation that "Publication is not the end of the peer review process but a part of it" (p. 37).

The Adversary Model

Bornstein maintains that editors and reviewers in psychology make poor decisions because they are aware that the majority of papers must be rejected due to the shortage of page space; thus, the cost of accepting a flawed manuscript exceeds the cost of rejecting a manuscript of which one is unsure. Because many submissions fall somewhere in the middle with respect to quality, reviewers tend to focus on small problems or make vague statements to rationalize a negative decision. Reviews often exhibit a stilted or forced quality because reviewers are given fundamentally incompatible tasks: identify sound research—but not too much of it—and articulate the scientific, but not pragmatic, reasoning underlying their review.

Bornstein (1991) proposes that this can be remedied by an adversarial approach that asks reviewers to "... make every effort to challenge and refute" (p. 456) the authors' claims (see also Finke, 1990; Harcum & Rosen, 1993). Reviewers essentially play the role of prosecuting attorneys. The action editor then asks the authors, who are responsible for representing the paper's virtues, to submit a rebuttal (i.e., they play the role of defense attorneys). The action editor makes the final decision regarding the submission's disposition with the manuscript, reviewer attack, and author rebuttal in hand.

Bornstein thinks that redefining the role of the reviewers as prosecutors would make them "... stay close to the methods and results because they would be held accountable for their assertions and be aware the author will soon be rebutting their criticisms" (p. 457). Even a reviewer who was highly motivated to prevent a particular manuscript from being published could not hinder publication simply by asserting that the manuscript is flawed or trivial. Bornstein also thinks that anonymity of reviews would be less important in the context of exclusively negative reviews.

The adversarial approach does not formally address the page space issue. Bornstein (1990) proposes this could be handled by imposing modest page charges (as in the natural sciences), requiring a small submission fee for all manuscripts, and/or increasing the number of brief reports. Since the publication of Bornstein's proposal, we have seen the development and widespread use of the Internet for the posting of supplementary material for readers interested in more detail. This relatively new strategy is likely to make the introduction of page fees less onerous.

The Avant Garde: arXiv

arXiv, an online open access resource, was founded in 1991. Prior to this time, high energy physicists already tended to use preprints to communicate new findings to the scientific community rather than print journals because they wanted rapid access. This practice was transferred to the Internet by creating a highly automated electronic archive and distribution server for research articles. Currently, arXiv, which is maintained and operated by the Cornell University Library with guidance from the arXiv Advisory Board, includes physics, mathematics, computer science, nonlinear sciences, quantitative biology, and statistics.

Researchers wishing to post their studies submit them to arXiv section moderators, who verify that the reports are topical and refereeable scientific contributions that follow accepted standards of scholarly communication. New users and those without a recognized academic affiliation are required to attain endorsement to verify that they are active members of the scientific community before their submission is processed; users with recognized academic affiliations are exempt from the endorsement process. Endorsers are not asked to review the paper for errors, but to check only that the paper is appropriate for the intended subject area. If the submission passes these screening procedures, it is posted electronically without further review. If the authors decide to revise or correct the manuscript, then the new version is posted along with the original. arXiv has committed to providing perpetual availability of all submissions.

The absence of peer review and open exchange might be seen as a negative. In particular, how is junk to be distinguished from good science? arXiv proponents claim that research specialty groups are self-policing with regard to the quality of research claims. Proponents of arXiv observe that some very influential papers remain purely as e-prints and are never published in traditional peer-reviewed journals because their authors feel that their work already has reached its intended audience. However, a majority of the posted papers are also submitted to traditional journals for publication (70% in journals, 20% in convention proceedings; O'Connell, 2002). An analysis by Gunnarsdottir (2005) notes that most authors use both arXiv and traditional journals, but they do so for different purposes:

> Formal publication is still a necessary gatekeeper to assure outsiders that a piece of work has significance when they need to make decisions about appointments, promotions and funding (Bohlin, 2004). Conventional journal publications have a symbolic role for these outsiders, whereas the preprint dissemination bears the burden of information exchange in the scientific workplace. (p. 551)

The example of arXiv helps to differentiate between the kinds of information scientists need "on the ground" of daily scientific work versus the kinds of information used by administrators and granting agencies. The psychological sciences have used the traditional peer-review system in both capacities. Of course, the high energy physics community may have features that make arXiv more appropriate for them than for psychologists. They are described as strongly collaborative and have "..little stress over priority and intellectual property" (Gunnarsdottir, 2005,

p. 558). Although psychology would seem to embrace a different ethos, it should be noted that our field has become much more interdisciplinary and collaborative, especially with the advent of the Internet, globalization, and advanced technologies.

CONCLUSIONS AND RECOMMENDATIONS

> *I was challenged by two of the cleverest researchers in Britain to publish an issue of the BMJ comprised only of papers that had failed peer review and to see if anybody noticed. I wrote back 'How do you know I haven't already done it?'*
>
> —Smith, 2006, p. 84

Scientific psychology has used the same set of editorial practices for nearly 50 years. During that time, evidence has mounted to suggest that the peer-review system has some fundamental flaws. If a research hypothesis had encountered similar problems, researchers in the field would have actively discussed it, undertaken procedural changes, and, if they continued to find persistent negative results, they would have abandoned the hypothesis in favor of something new. Oddly, although sporadic questions have arisen, traditional peer review never has been seriously debated in psychology. In fact, we suspect that many of the things we report in this article may be novel to many of our readers.

Our first priority in undertaking this article was to pique curiosity among psychologists and provide sufficient information to stimulate dialogue. Other fields already are actively engaged in discussing and testing alternatives to traditional peer review. Psychology has not yet fully exploited the opportunities offered by the Internet and other technologies for the dissemination of scientific findings or those offered by alternative practices evolving in other disciplines; in fact, psychology appears to be in the rear guard in this respect. The reasons for psychology's lag are not critical. But if we wish to maintain the traditional peer-review approach, we should develop cogent and empirically sound reasons for doing so. Traditional peer review should be deemed to be the best that is possible based on evidence rather than custom or tradition. Therefore we hope that the work we have drawn on in this article will inspire discussion and debate at all levels of our discipline, including researchers, administrators, and policy makers. Such conversations should be undertaken locally within academic departments and nationally at conferences and within editorial boards and publication committees, and they should seek to address critical questions such as why the rejection rate at prestigious journals is so much higher in the behavioral sciences than in the natural sciences and whether all research journals should operate on the same peer-review principles.

In recommending incisive dialogue on this topic, we recognize that the academic tenure and promotion system has the potential to be a barrier to undertaking change in the peer-review system. Although journals routinely articulate the dissemination of cutting-edge research in their mission statements, journals play a key role—albeit unacknowledged—in assisting the field in evaluating junior scientists who seek to establish their research careers. In addition, publication record factors into the evaluation of applications for external grant funding. Thus, any changes in the peer-review system will have ripple effects at multiple levels of scientific evaluation. In our view, a question that merits careful thought is whether the process of research review and dissemination should be isomorphic with decisions related to funding, tenure, and promotion. Clearly the proponents of arXiv decided early on that their primary—and perhaps only—mission was the dissemination of research findings and that their audience consists of hands-on researchers actively engaged in the generation of new knowledge. The evaluation of scholarly productivity for promotion, tenure, and grant support falls in the purview of provosts and deans and, as such, incorporates motives, priorities, and needs that may or may not be compatible with the scientific enterprise.

Our second recommendation concerns the urgent need for a psychological science of research communication. The sociology of science has been active for several decades and provides some foundations, but this work is primarily descriptive and correlational (see Cole, 1992, for a conspicuous exception). There already seems to be sufficient evidence to suggest that halo effects, self-fulfilling prophecies, prestige suggestion, and impression management influence the peer-review process. Much as the psychology of the experiment refined and expanded our grasp of behavioral mechanisms in the laboratory, psychology provides the optimal models and methods for the exploration of the processes that shape peer review. In particular, experimental testing of different peer-review strategies is urgently needed. Such inquiries have occurred on a limited basis, primarily in the biomedical sciences. But a science of research communication seems well-suited to the portfolios of psychological methodologists and evaluation researchers.

Finally, we hope the reader will recall that our strong words about reviewers were offered tongue in cheek, as a means of provoking discussion. In fact, we love reviewers. Some of our best friends are reviewers. We ourselves have been and continue to serve as reviewers; having both been editors, the tremendous efforts and contributions of reviewers are foremost in our minds. Our goal is not to disparage reviewers, but to improve the system through which reviews are rendered. It is an undertaking that has the potential to make the review process more interesting, engaging, and scientifically rigorous for reviewers, editors, and authors. The conventional peer-review process is not quite like the proverbial "smoke-filled room" where decisions are made, but opening the window may nevertheless introduce a breath of fresh air.

REFERENCES

Adair, R.K. (1982). A physics editor comments on Peters and Ceci's peer-review study. *Brain and Behavioral Sciences*, *5*, 196.

Alpert, J.S. (2007). Peer review: The best of the blemished? *American Journal of Medicine*, *120*, 287–288.

Amabile, T.M. (1983). Brilliant but cruel: Perceptions of negative evaluators. *Journal of Experimental Social Psychology*, *19*, 146–156.

Armstrong, J.S. (1982). Barriers to scientific contributions: The author's formula. *Brain and Behavioral Sciences*, *5*, 197–199.

Berkowitz, L., & Donnerstein, E. (1982). Experimental validity is more than skin deep: Some answers to criticisms of laboratory experiments. *American Psychologist*, *37*, 245–257.

Beyer, J. (1978). Editorial policies and practices among leading journals in four scientific fields. *Sociological Quarterly*, *19*, 68–88.

Bohlin, I. (2004). Communication regimes in competition: The current transition in scholarly communication seen through the lens of the sociology of technology. *Social Studies of Science*, *34*, 364–391.

Bornstein, R.J. (1990). Epistemic progress and journal page limitations: A proposal for increasing the base rate of manuscript acceptance in psychology journals. *American Psychologist*, *45*, 673–674.

Bornstein, R.J. (1991). Manuscript review in Psychology: Psychometrics, demand characteristics, and an alternative model. *Journal of Mind and Behavior*, *12*, 429–468.

Bradley, J.V. (1981). Pernicious publication practices. *Bulletin of the Psychonomic Society*, *18*, 31–34.

Burnham, J.C. (1990). The evolution of editorial peer review. *Journal of the American Medical Association*, *263*, 1323–1329.

Cicchetti, D.V. (1991). The reliability of peer review for manuscript and grant submissions: A cross-disciplinary investigation. *Behavioral and Brain Sciences*, *14*, 119–186.

Cole, S. (1992). *Making science: Between nature and society*. Cambridge, MA: Harvard University Press.

Cole, S., Cole, J.R., & Simon, G. (1981). Chance and consensus in peer review. *Science*, *214*, 881–886.

Eichorn, D.H., & VandenBos, G.R. (1985). Dissemination of scientific and professional knowledge. *American Psychologist*, *40*, 1301–1316.

Finke, R.A. (1990). Recommendations for contemporary editorial practices. *American Psychologist*, *45*, 669–670.

Fiske, D., & Fogg, L. (1990). But the reviewers are making different criticisms of my paper! Diversity and uniqueness in reviewer comments. *American Psychologist*, *45*, 591–598.

Garcia, J. (1981). Tilting at the papermills of academia. *American Psychologist*, *36*, 149–158.

Garvey, W.D., Lin, N., & Nelson, C.E. (1970). Communication in the physical and social sciences. *Science*, *146*, 1655–1659.

Ginsparg, P. (1994). First steps towards electronic research communication. *Computers in Physics*, *8*, 390–396. Retrieved from http://arxiv.org/blurb/blurb.ps.gz

Glenn, N.D. (1982). The journal article review process as a game of chance. *Brain and Behavioral Sciences*, *5*, 211–212.

Godlee, F. (2002). Making reviewers visible: Openness, accountability and credit. *Journal of the American Medical Association*, *287*, 2762–2765.

Godlee, F., Gale, C.R., & Martyn, C.N. (1998). Effect on the quality of peer review of blinding reviewers and asking them to sign their reports. *Journal of the American Medical Association*, *280*, 237–240.

Godlee, F., & Jefferson, T. (Eds.). (2003). *Peer review in health sciences* (2nd ed.). London: BMJ Books.

Gottfredson, S.D. (1978). Evaluating psychological research reports: Dimensions, reliability, and correlates of quality judgments. *American Psychologist*, *33*, 920–934.

Gottfredson, S.D., Garvey, W.D., & Goodnow, J.E. (1977). Quality indicators in the scientific journal article publication process. *JSAS Catalog Selected Documents*, *7*, 745.

Gunnarsdottir, B. (2005). Scientific journal publications: On the role of electronic preprint exchange in the distribution of scientific literature. *Social Studies of Science*, *35*, 549–575.

Harcum, E.R., & Rosen, E.F. (1993). *The gatekeepers of psychology: Evaluation of peer review by case history*. Westport, CT: Praeger.

Hargens, L.L. (1988). Cognitive consensus and journal rejection rates. *American Sociological Review*, *53*, 139–151.

Horrobin, D.F. (1990). The philosophical basis of peer review and the suppression of innovation. *Journal of the American Medical Association*, *263*, 1438–1441.

Jefferson, T., Alderson, P., Wager, E., & Davidoff, F. (2002). Effects of editorial peer review: A systematic review. *Journal of the American Medical Association*, *287*, 2784–2786.

Justice, A.C., Cho, M.K., Winker, M.A., Berlin, J.A., & Rennie, D.The PEER Investigators (1998). Does masking author identity improve peer review quality: A randomized trial. *Journal of the American Medical Association*, *280*, 240–242.

Koop, T., & Poschl, U. (2006). *An open, two-stage peer-review journal*. Retrieved from www.nature.com/nature/peerreview/debate/nature04988.html

Kronick, D.A. (1990). Peer review in 18th-century scientific journalism. *Journal of the American Medical Association*, *263*, 1321–1322.

Kuhn, T. (1970). *The structure of scientific revolutions* (2nd ed.). Chicago: University of Chicago Press.

Latané, B. (1978). Notes for a talk on our scientific publication system. *Personality and Social Psychology Bulletin*, *4*, 22–23.

Latour, B. (1987). *Science in action*. Cambridge, MA: Harvard University Press.

Linkov, F., Lovalekar, M., & Laporte, R. (2006). Scientific journals are 'faith based": Is there science behind peer review? *Journal of the Royal Society of Medicine*, *99*, 596–598.

Lock, S. (1985). *A difficult balance: Editorial peer review in medicine*. London: Nuffield Provincials Hospital Trust.

Lodahl, J.B., & Gordon, G. (1972). The structure of scientific fields and the functioning of university graduate departments. *American Psychological Review*, *37*, 57–72.

Mahoney, M.J. (1977). Publication prejudices: An experimental test of confirmatory bias in the peer review system. *Cognitive Research and Therapy*, *1*, 161–175.

Mahoney, M.J. (1985). Open exchange and epistemic process. *American Psychologist*, *40*, 29–39.

Marsh, H.W., & Ball, S. (1989). The peer review process used to evaluate manuscripts submitted to academic journals: Interjudgmental reliability. *Journal of Experimental Education*, *57*, 151–169.

McNutt, A., Evans, A.T., Fletcher, R.H., & Fletcher, S.W. (1990). The effects of blinding on the quality of peer review: A randomized trial. *Journal of the American Medical Association*, *263*, 1371–1376.

Merton, R.K. (1968). The Matthew Effect in science. *Science*, *159*, 56–63.

Meyer, G.S. (1979). *Academic labor and the development of science*. Unpublished doctoral dissertation, State University of New York at Stony Brook.

Nickerson, R. (2005). What authors want from reviewers and editors. *American Psychologist*, *60*, 161–162.

O'Connell, H.B. (2002). Physicists thriving with paperless publishing. *High Energy Physics Webzine*, *6*. Retrieved from http://library.cern.ch/HEPLW/6/papers/3/

Peters, D.P., & Ceci, S.J. (1982). Peer-review practices of psychological journals: The fate of published articles, submitted again. *Behavioral and Brain Sciences*, *5*, 187–255.

Rennie, D. (2003). Editorial peer review: Its development and rationale. In F. Godlee & T. Jefferson (Eds.), *Peer review in health sciences* (2nd ed., pp. 1–13). London: BMJ Publishing Group.

Roediger, H.L., III (1987). The role of journal editor in the scientific process. In D.N. Jackson & J.P. Rushton (Eds.), *Scientific excellence* (pp. 222–252). Newbury Park, CA: Sage.

Rosenthal, R. (1966). *Experimenter effects in behavioral research*. New York: Appleton-Century-Crofts.

Sandewall, E. (1997). *A neo-classical structure for scientific publication and reviewing*. Retrieved from http://www.ep.liu.se/ea/apt/1997/001/2006

Scott, W.A. (1974). Interreferee agreement on some characteristics submitted to the *Journal of Personality and Social Psychology*. *American Psychologist*, *29*, 698–702.

Simonton, D.K. (2004). *Creativity in science: Chance, logic, genius, and zeitgeist*. New York: Cambridge University Press.

Smith, R. (2006). *The trouble with medical journals*. London: Royal Society of Medicine.

Starbuck, W.H. (2005). How much better are the most-prestigious journals? The statistics of academic publication. *Organization Science*, *16*, 180–200.

Sternberg, R. (Ed.). (2006). *Reviewing scientific works in psychology*. Washington, DC: American Psychological Association.

Sternberg, R., & Gordeeva, T. (1996). The anatomy of impact: What makes an article influential? *Psychological Science*, *7*, 69–75.

Suls, J. (2001). Report from the editor of *PSPB*: Turning the corner! *Dialogue*, *16*, 28.

Suls, J., & Fletcher, B. (1983). Social comparison in the social and physical sciences: An archival study. *Journal of Personality and Social Psychology*, *44*, 575–580.

Suls, J., & Gastorf, J. (1980). Has the social psychology of the experiment influenced how research is conducted? *European Journal of Social Psychology*, *10*, 291–294.

Tesser, A., & Martin, L. (2006). Reviewing empirical submissions to journals. In R. Sternberg (Ed.), *Reviewing scientific works in psychology* (pp. 3–30). Washington, DC: American Psychological Association.

Triggle, C.R., & Triggle, D.J. (2007). What is the future of peer review? Why is there fraud in science? Is plagiarism out of control? Why do scientists do bad things? Is it all a case of: All that is necessary for the triumph of evil is good men do nothing? *Vascular Health and Risk Management*, *3*, 39–53.

van Rooyen, S., Godlee, F., Evans, S., Smith, R., & Black, N. (1998). Effect of blinding and unmasking on the quality of peer review: A randomized trial. *Journal of the American Medical Association*, *280*, 234–237.

Web of Science. (2008). Retrieved from http://www.thomsonreuters.com/products_services/scientific/Web_of_Science

Wolff, W.M. (1970). A study of criteria in journal manuscripts. *American Psychologist*, *25*, 636–639.

Wyer, R.S. (1978). Excerpts from letters received by Bibb Latane. *Personality and Social Psychology Bulletin*, *4*, 29–30.

Zanna, M. (1992). My life as a dog (I mean editor). *Personality and Social Psychology Bulletin*, *18*, 485–488.

Ziman, J. (1968). *Public knowledge: An essay concerning the social dimension of science*. Cambridge, United Kingdom: Cambridge University Press.

Ziman, J. (1969). Information, communication, knowledge. *Nature*, *224*, 318–324.

Zuckerman, H., & Merton, R.K. (1971). Patterns of evaluation in science: Institutionalization, structure and functions of the referee system. *Minerva*, *9*, 66–100.

Zuckerman, H., & Merton., R.K. (1973). Institutionalization patterns of evaluation in science. In N. Storer (Ed.), *The sociology of science* (pp. 460–496). Chicago: University of Chicago Press.