

## FROM THE EDITORS

### PEER REVIEW IN THE ORGANIZATIONAL AND MANAGEMENT SCIENCES: PREVALENCE AND EFFECTS OF REVIEWER HOSTILITY, BIAS, AND DISSENSUS

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Peer review is used in most scientific disciplines to assess the value of new knowledge presented in journal submissions. Individuals well versed in the appropriate research domain evaluate the quality of ideas, empirical rigor, and overall contribution conveyed by a manuscript. In essence, they serve a gate-keeping role by helping to filter out low-quality work (Crane, 1967; de Grazia, 1963). By so doing, they help to protect the integrity of the scientific enterprise and the status of established scholars, whose own claims to preeminence would be undermined by the publication of inferior work, particularly if that work were published in high-quality outlets (Cummings, Frost, & Vakil, 1985).

#### THE PERILS OF PEER REVIEW

Although this system of peer review is well accepted, it is far from perfect. Many would characterize it just as Winston Churchill characterized democracy—it is the worst possible system except for all others. As a set of human judgment and decision tasks embedded within a social world, the review system in fact does incur a number of risks. One issue is unnecessarily harsh critiques. Trained to spot flaws in research and to debate the merits of different theories and methods, reviewers sometimes focus solely on uncovering and aggressively highlighting the flaws in a submission. Complicating this situation is the possibility of reviewers seeing themselves not as peers but as superiors in the hierarchy of science (Starbuck, 2003). Placing someone in an evaluative role can have that effect. The net result can be very frustrated authors who develop bad attitudes, such as the one highlighted in the following passage:

We do understand that, in view of the misanthropic psychopaths you have on your editorial board, you

need to keep sending them papers, for if they weren't reviewing manuscripts they'd probably be out mugging old ladies or clubbing baby seals to death. (Baumeister, in Bedian, 1996)

A second issue for the peer review system is biased judgments. Biases can take many forms. In fields and journals without blind review, particularistic criteria related to authors' social relations, doctoral origins, and current affiliations can play major roles (see Zuckerman, 1988). Such criteria provide clues, albeit imperfect ones, as to the competency of a manuscript's author(s), and these clues can be used as a means to avoid the burden of deeply evaluating research or as unconscious or conscious shortcuts around any uncertainty about the value of a submission. In an oft-cited study, Peters and Ceci (1982) demonstrated the problem. They identified 12 papers published by individuals from prestigious psychology departments. Next, they resubmitted the papers to the same highly regarded journals in which the work had originally appeared, but with fictitious author names and affiliations (e.g., the Tri-Valley Center for Human Potential). Of the 9 papers that were not detected as resubmissions of recently published work, 8 were rejected on the basis of negative recommendations from 16 of 18 reviewers. What had changed? Only the author names and affiliations.

In fields with blind review, social relations and status based on institutional affiliations are less troublesome, at least at the reviewer level (they can still be operative at the editor level). But other types of bias unrelated to the actual merits of a submission can still affect the process (Miller, Glick, & Cardinal, 2005). Complexity bias, exemplified by the "Dr. Fox hypothesis," is one of the more interesting of these. Dr. Fox was an actor who delivered a complex presentation to several groups of well-educated psychologists, psychiatrists, social workers, and administrators (Armstrong, 1980). He managed to effectively engage the audiences and generate positive postpresentation assessments despite having a presentation based on double talk,

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contradictions, poor logic, made-up words, and references to irrelevant topics.

How significant is the complex language bias in journal reviewing? Evidence is scant but intriguing. In the most famous study of this issue, organizational science faculty from three prestigious universities evaluated the competence of passages taken from published research (Armstrong, 1980, 1997). Each passage had been rewritten in multiple versions, with one version having simple, straightforward language while other versions had more complex language. Faculty reading the simple version of a particular passage tended to evaluate it less positively than did faculty reading the same passage written in more complex language.

Confirmation bias is another interesting influence on reviewer judgment. Here, reviewers tend to favor research that does not deviate very much from prevailing or preferred wisdom. This tendency is consistent with Kuhn's (1962) assertion that findings in conflict with important beliefs are resisted. In one study, different versions of a paper were sent to 75 reviewers from the *Journal of Applied Behavioral Analysis* (Mahoney, 1977). For the first group of reviewers, the paper presented results that supported theoretical orientations common among individuals associated with the journal. For the second group, the paper presented results that were counter to those orientations. Other groups received versions of the paper that did not report empirical results or that presented mixed results. The reviewers who worked with results supportive of their theoretical orientations gave significantly higher ratings to the study's methodology, data presentation, and overall contribution. They also were much more likely to recommend acceptance or revision. In another study, two versions of an abstract were sent to members of the American Psychological Association (Goodstein & Brazis, 1970). The abstracts were identical but noted different empirical results; in one version, the results were consistent with common beliefs, and in the other version they were not. Individuals receiving abstracts supportive of common beliefs tended to rate the research as better designed and more important.

A third issue for the peer review system is dissensus among reviewers. Dissensus—disagreement among the reviewers evaluating a paper—has received a great deal of attention because it goes to the heart of peer review. Its potential to cause consternation is exceeded only by its perceived prevalence. Writing about the organizational and management sciences, Cummings and Frost commented, "Disagreement and differing opinions about the value of manuscripts . . . mark the land-

scape of editing" (1985: 420). Starbuck put it this way:

Of course, everyone who has submitted several manuscripts to journals has experienced inconsistent reviews, and every academic has heard many stories about the inconsistent reviews received by colleagues. Nevertheless, such experiences inevitably leave us wondering if we are the unfortunate ones, if others of more talent and greater skill receive helpful, positive reviews. (2003: 348)

Dissensus has often been studied in the social sciences, where relatively low paradigm development is taken as an article of faith. Low paradigm development implies disagreement over the criteria that should be used to evaluate research in an array of circumstances (evaluating the published research of new faculty candidates, evaluating grant proposals, and so on). When individuals in a field disagree about appropriate theory, methodology, techniques, and future directions, variation in evaluative criteria is a central feature of academic life (Lodahl & Gordon, 1972; Pfeffer, 1993). The journal review process certainly is not immune from this outcome.

An additional source of variance across reviewers is more psychological than sociological. Research on expert decision making suggests that reviewers may use the same criteria but apply them inconsistently because of factors such as information overload, distractions, and fatigue (Ashton, 2000). Such inconsistency would create variance both within and between reviewers.

A number of individuals have provided empirical evidence suggestive of dissensus. Scott (1974), for example, reported an intraclass correlation of .26 for reviewers completing work at the *Journal of Personality and Social Psychology*. Cicchetti (1991) reported an intraclass correlation of .38 for reviewers at *American Psychologist*, and Gottfredson (1978) reported intraclass correlations ranging from .16 to .50 for a set of expert judges who reviewed articles previously published in nine journals (e.g., *Journal of Applied Psychology*). Table 1 reports additional findings.

If a journal submission has a "true value" in some abstract sense, reviewer dissensus indicates a lack of convergence on that value. Starbuck (2005) assessed the implications of living in such a world. In his simulation-based results, reviewer dissensus drove publishing mistakes, with top journals repeatedly rejecting high-quality papers and accepting low-quality ones. For instance, assuming an average correlation of .45 between overall reviewer assessments and the true values of manuscripts (the most generous assumption that Starbuck allowed),

**TABLE 1**  
**Evidence for Dissensus among Reviewers**

Periodicals and Studies	Manuscripts Reviewed	Reliability/Agreement of Reviewers <sup>a</sup>
<i>Administrative Science Quarterly</i> (Starbuck, 2003)	500	$r = .12$
<i>American Psychologist</i> (Cicchetti, 1980; Scarr & Weber, 1978)	87	ICC = .50-.54 K = .52-.53
<i>American Psychologist</i> (Cicchetti, 1991)	72	ICC = .38
<i>American Sociological Review</i> (Cicchetti, 1991; Hargens & Herting, 1990)	322	ICC = .29
<i>Developmental Review</i> (Whitehurst, 1984)	73	ICC = .27
<i>Journal of Abnormal Psychology</i> , 1973 (Cicchetti & Eron, 1979)	215	ICC = .08-.17
<i>Journal of Abnormal Psychology</i> , 1974 (Cicchetti & Eron, 1979)	213	ICC = .29-.34
<i>Journal of Abnormal Psychology</i> , 1975 (Cicchetti & Eron, 1979)	191	ICC = .15-.19
<i>Journal of Abnormal Psychology</i> , 1976 (Cicchetti & Eron, 1979)	216	ICC = .07-.10
<i>Journal of Abnormal Psychology</i> , 1977 (Cicchetti & Eron, 1979)	232	ICC = .05-.21
<i>Journal of Educational Psychology</i> (Marsh & Ball, 1981)	325	ICC = .34
<i>Journal of Personality and Social Psychology</i> (Scott, 1974)	286	ICC = .26
<i>Law and Society Review</i> (Cicchetti, 1991)	251	ICC = .23
<i>Personality and Social Psychology Bulletin</i> (Hendrick, 1976, 1977)	177	ICC = .22
<i>Social Problems</i> (Smigel & Ross, 1970)	193	K = .40
<i>Sociometry</i> (Hendrick, 1976, 1977)	140	$r = .21$

<sup>a</sup> This table is adapted from Starbuck (2005).  $r$  = product-moment correlation; ICC = intraclass correlation; K = kappa.

and assuming strong effects of reviewer inputs, 29 percent of the papers accepted in top-tier journals do not belong there.

### THE CASE OF THE ACADEMY OF MANAGEMENT JOURNAL

As suggested above, both sociological and psychological research paints a rather bleak picture of journal reviewing. But is this picture a reasonable facsimile of reviewing at *AMJ*? To what degree have this journal's stewards mitigated known problems in journal reviewing? The pages that follow address this question. I discuss possible mitigators (see Table 2) and then present evidence for their effectiveness.

#### Possible Mitigators

**Focused paradigm.** *AMJ* is sometimes criticized for being narrow. Critics have decried the *Journal's*

apparent emphasis on incremental advances produced through exacting quantitative methods. Even its own editorial review board recently suggested opening up the *Journal* to other forms of research. In a survey at the start of the current editor's term, the incoming board's leading suggestion for change was "Accept more innovative, less formulaic research" (Bartunek, Rynes, & Ireland, 2006; Rynes, 2005). Although only 17 percent of board members offered this advice, it was noteworthy. One board member put it this way, "Encourage and publish more ambitious and innovative research—not just incremental stuff that may be very tight methodologically but that offers little that is really new or interesting" (Rynes, 2005: 12).

Although the actual degree of narrowness at the *Journal* is debatable, the characterization no doubt has some validity. In terms of the present discussion, however, this apparent vice is a virtue. A narrow approach implies a focused paradigm and the shared values that go with that. If shared values

**TABLE 2**  
**Possible *AMJ* Mitigators of Reviewing Concerns<sup>a</sup>**

Concern	Focused Paradigm	Selection System for Board Members	One- and Two-Way Communication between Editors and Board	Performance Management for Reviewers
Harsh critiques		X	X	X
Conscious and unconscious bias		X	X	X
Dissensus among reviewers	X	X	X	

<sup>a</sup> The table shows connections highlighted in the text between mitigating factors and reviewing issues. These are the most apparent, but other connections are possible (e.g., performance management could impact dissensus by reducing careless errors).

exist for a particular approach to knowledge generation, then dissensus among the reviewers of a particular paper is less likely.

**Selection of board members.** Two features of how *AMJ* chooses editorial board members provide benefits. First, this journal's selection system strongly emphasizes merit. Candidates for the board typically have successful records of ad hoc reviewing at *AMJ* or have had successful board experience at other journals. Most have published in the *Journal* or in other top-tier journals. This emphasis on merit no doubt helps to screen individuals who have exhibited bias in an obvious way or have been highly caustic in their remarks. Former *AMJ* editors and editors of other journals who might be consulted on the performance of a prospective board member will probably note any obvious tendency toward bias or overt hostility.<sup>1</sup>

Second, there is an emphasis on fit with the fundamental mission and values of the *Journal*. Selecting on the basis of fit reinforces the *Journal's* focused paradigm and the lower dissensus associated with such a paradigm. More generally, fit as a selection criterion is a component of high-commitment management, an approach that has been associated with strong effort and commitment (e.g., Pfeffer, 1998). Selecting for fit also has a dark side—homosocial reproduction (Kanter, 1977)—which can limit new ways of thinking and acting. *AMJ's* answer to this potential problem is to emphasize fit with fundamentals, not fit with fine-grained preferences and beliefs.

**Communication.** Two-way communication between editors and editorial board members is reasonably frequent and serves to reinforce norms of thorough, developmental reviews. Board meetings at the annual AOM assembly, board surveys followed by feedback, e-mail bulletins with opportunities for response, and the “open door” policy of *AMJ* chief and associate editors all help to ensure that dialogue occurs. E-mail exchanges and telephone interactions about specific papers also take place.

Other forms of editor–board member communication exist. “From the Editors” essays (such as this one) are important for building and maintaining norms and expectations. In these essays, the editor-in-chief, an associate editor, and/or a guest provide information relevant to one or more constituencies

of *AMJ*—authors, board members, and so on. They discuss general functioning of the *Journal* (e.g., Rynes et al.'s “Everything You've Always Wanted to Know about *AMJ*” [2005]) or issues of strategic importance (e.g., Gephart's “Qualitative Research and the *Academy of Management Journal*” [2004]). Also, the editor-in-chief and associate editors attend numerous forums and colloquia to discuss the *Journal*. Some of these take place at the annual AOM meeting; others take place “on the road.” Schminke (2004) reported making 15 such road trips as an associate editor. These forums put the norms of the *Journal* on display, thus reinforcing and reproducing those norms; the likely net result is paradigm focus among authors, editors, and board members.

**Performance management.** A performance management system has been in place at *AMJ* for quite some time. The system seems to have been one of the first in the organizational and management sciences. After crafting the decision letter to the authors of a submission, the action editor for the paper evaluates each reviewer's input to the review process. A review's thoroughness, developmental tone/message, commentary on contributions to theory, and attention to technical detail are among the issues considered. Most board members score well, but in rare exceptions problems exist and persist. These board members may be assigned few or no manuscripts or excused from the board. For ad hoc reviewers, low ratings generally mean discontinued use and a low probability of being asked to join the *Journal's* editorial board. Evaluating reviewers in this way reinforces existing norms, particularly norms supporting timely, developmental, and high-quality reviews.

### Evidence for Effectiveness

In the opening portion of this essay, I highlighted the problems of dissensus, bias, and hostility and provided illustrative evidence from other fields and journals. Next, I presented four factors that may work against these problems. Here, I discuss empirical evidence related to *AMJ*. This evidence provides insights into the functioning of our journal.

**Dissensus.** Cummings, Frost, and Vakil (1985) analyzed processes and outcomes for 81 manuscripts from the period in which Cummings was *AMJ's* editor (1976–78). They identified 34 of these as special cases, meaning the process had some unusual feature or the reviewers disagreed on the recommendation. My reading of synopses of the 34 special cases (see Cummings et al.'s appendix) suggests that 23 cases exhibit reviewer disagreement

<sup>1</sup> Merit is not unique to the selection system at *AMJ*, but this and other top-tier journals may use it more fully than do other publications. Editors at top-tier journals likely enjoy larger pools of talented individuals interested in being board members.

for first-round submissions, yielding an overall disagreement rate of 28 percent (23/81). This rate of disagreement is not extremely high. Moreover, the inferred reliability estimate (in which chance agreement is taken into account) would seem to be reasonable. Unfortunately, it is not clear that the research assistants working with Cummings assigned all cases exhibiting disagreement to the special cases category. It is possible that some cases not assigned to that category exhibited modest disagreement, but this could not be examined with available materials.

Beyer, Chanove, and Fox (1995) analyzed processes and outcomes related to 400 manuscripts from Beyer's *AMJ* editorship (1985–87). Translating ordinal data for the reviewer recommendations ("reject," "doubtful," "major revision," "minor revision," "accept") to a 1–5 scale, Beyer and coauthors reported an average within-paper standard deviation of .69 for new submissions. A standard deviation of .69 derived from a scale with a range of 4 (5 minus 1) is not extremely high, but it is not low either. Also, .69 is partially the result of both ceiling and floor effects (average within-paper ratings of 1 and 5 had standard deviations of 0 by mathematical definition; note that disagreement was not a focus of the Beyer et al. study).

Bedeian (2003) surveyed 173 authors with successful submissions to *AMJ* and *AMR*. These authors responded to the following question: "I was satisfied with the consistency of the referees' comments among one another." Only 31 disagreed with this statement (18%), but another 34 were neutral rather than in agreement (20%). Considering the biased nature of the sample (only successful authors), these results provide only limited support for the positive effects of the mitigating factors.

To further investigate the dissensus issue, I examined 68 cases from the current Rynes editorship of this journal. These cases involved initial submissions and were randomly drawn from the files of two associate editors, one known for micro work and one for macro work. In 37 percent of the cases, reviewers did not offer the same recommendation, nor did they offer recommendations in immediately adjacent categories (e.g., "rejection" and "remediation doubtful"). Applying the Beyer et al. approach to the Rynes data yielded a similar within-paper standard deviation. Overall, the Rynes data seem to be consistent with the data from earlier studies of *AMJ*.<sup>2</sup>

<sup>2</sup> Although dissensus is usually defined conceptually as simple disagreement, the real underlying issue is reliability. As shown in Table 1, most individuals examining

The three published studies and the new data suggest dissensus is present at *AMJ* but certainly not to the extent that it could be. Thus, dissensus is an issue, but a focused paradigm seems to dampen its role.<sup>3</sup>

**Bias.** The same studies discussed above present data on the issue of bias. Cummings et al. (1985), for example, reported few problems with detectable bias. Their commentary, particularly their casual but detailed discussion of 34 special cases, suggested little concern. Beyer et al. (1995) found little evidence for particularism (as noted earlier, this is more a matter of editor than reviewer behavior for a journal with blind review). Instead, they found some evidence for gatekeeping, in which reviewers and editors use valid indicators in evaluating manuscripts. Bedeian (2003) reported that most authors in his survey were satisfied with reviewer "objectivity." Although that term means different things to different people, authors probably would not report satisfaction on this dimension if they had detected bias against nonconfirmatory results, simple writing, or any other aspect of their work unrelated to the merit of content. Jauch and Wall (1989) also provided relevant data. In their work, *AMJ* and *AMR* board members reported details of their reviewing practices. What is interesting here is the level of effort reviewers reported; they frequently read a submission, making notes, and then reread it before beginning their reviews. With this level of effort, bias is less likely to creep into the process through mental laziness.

The above evidence is admittedly loose and indirect. It is far from conclusive. The evidence does, however, offer some indication that life at *AMJ* is at least a little better than some critics might suggest.

**Harsh critiques.** Cummings and his coauthors (1985) examined two dimensions of reviewing. The first, labeled "coaching," involved supportive commentary and an emphasis on helping authors to improve. The second, labeled "criticizing," focused

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dissensus have calculated true reliability estimates. None of the published work on *AMJ*, however, includes such estimates. To provide information comparable to past analyses of *AMJ*, I calculated disagreement rates and standard deviations in the present rudimentary analysis.

<sup>3</sup> A factor in limiting the effects of existing dissensus is *AMJ*'s strong editor system. In such a system, action editors do not function as simple vote counters (Rynes, 2006). Although significantly influenced by reviewer input, they make final decisions. In an intriguingly titled piece ("Publishing as Prostitution"), Frey (2003) highlighted the positive role of a strong editor system while acknowledging its limitations in completely avoiding imperfect results.

on quality evaluations. Some reviewers had high scores on one or the other of these dimensions, and others had high scores on both or neither. Important for the present essay, reviewers with high criticizing scores did not necessarily provide scathing feedback. Although pointed in some ways, the prose these reviewers offered was not necessarily harsh, as the insignificant differences between critics and coaches on such dimensions as emotional intensity and tone indicated (see Cummings et al.'s Table 6). Thus, the reviewers who would have seemed the most likely to engage in overly harsh critiques did not do so. Also, my reading of Cummings et al.'s (1985) 34 special cases revealed little concern with caustic comments.

In my own work with *AMJ* reviewers, caustic comments have been few and far between. The culture of the institution calls for developmental reviews, and this value is respected.

### CONCLUSION

Starbuck penned this apt summary:

Peer review arouses very diverse emotions, beliefs, and ambitions. It angers, it reassures, it intimidates, it tramples egos, and it puffs them up. For some, peer review demonstrates the vacuousness and unreliability of social science; for others, the substance and reliability of social science. (2003: 348)

The peer review process is subject to a number of problems, including harsh critiques, bias, and dissensus. At *AMJ*, several factors help to mitigate these problems. Although this journal certainly does not have a lock on virtue, we do have a reasonably well defined paradigm and a set of process norms that help us to provide useful, developmental feedback.<sup>4</sup> Moreover, we have wonderfully dedicated people serving as editorial board members and ad hoc reviewers. More than any other feature of the system, they ensure a healthy journal.

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<sup>4</sup> Writing this essay presented more than a few moments of hesitation. On the one hand, *AMJ* does appear to have some unique advantages. On the other hand, the field is blessed with a number of journals that deserve our respect.

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