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Peer Review and the Social Construction of Knowledge in the Management Discipline

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Prior research on the peer-review process has almost exclusively focused on its surface features—its impartiality, validity, and reliability. What has received relatively less attention is the influence of the social component that shapes the content of the discipline's published record and, in turn, determines its scientific progress. As the product of social processes, all knowledge-claims are socially constituted rather than the products of an absolute truth. Taking a sociology-of-knowledge perspective, I argue that the social processes underlying the peer-review process warrant closer scrutiny. In doing so, I contend that there must be a balancing of the inevitable author–editor–referee tensions operating throughout the editorial process so as to ensure that a clear authorial voice is preserved. I offer suggestions for assuring the integrity of the scientific enterprise, while respecting the prerogatives and ethics of authorship.

"When one of my articles is finally published, I always have a sense that I am only partially the author, something is lost; this something may well be a part of myself. There are so many other actors . . . who have . . . succeeded in making changes . . . or getting me to make it 'in a satisfactory way' that it no longer feels mine in a traditional sense."

—An author commenting on the peer-review process (Roth, 2002:15).

A basic canon of academic science dictates that all claims to knowledge, whether old or new, should be continually and impartially scrutinized for "possible errors of fact or inconsistencies of argument" (Ziman, 1984: 85). For more than 300 years, this canon has been institutionalized in the peer review of scientific papers submitted for publication.

The helpful comments of Achilles A. Armenakis, J. Scott Armstrong, Daniel B. Marin, Mark J. Martinko, John L. Michela, John B. Miner, and William H. Starbuck on preliminary draft manuscripts are gratefully acknowledged.

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As recounted by Merton and Zuckerman (1971/1973), with the initial formation of learned societies and academies in the 1600s, and the subsequent invention of scientific journals as a means for written interchange, scientists sought the competent appraisal and authentication of their work from disciplinary peers before it was entered into the archives of science as accepted knowledge. In turn, the new scientific societies and academies recognized that a system for critically sifting materials that would, in effect, bear their imprimatur was essential to gain public confidence and to establish and guard their legitimacy as autonomous and authoritative bodies. Today, peer-reviewed publications remain the gold standard for judging the credibility of scientific claims (L. Warren, 2003), and the peer-review system is such an unquestioned part of the scientific enterprise that its very existence furthers quality control, as authors, knowing that their claims will be critically assessed, attempt "to *imagine* the criticisms that reviewers might make of their work and take measures to forestall or remedy them" (Chubin & Hackett, 1990: 89).

Whereas there is little question that peer review, as a quality-control mechanism, remains essential for accepting or rejecting claims to knowledge prior to entering a scholarly discipline's published record, the system has been criticized on conceptual, methodological, and political grounds. In particular, a growing tide of criticism has questioned the impartiality, reliability, and validity of peer review as a means for determining scientific merit. Empirical studies do, in fact, suggest that biases associated with various social, intellectual, and political considerations enter into referee recommendations and that such recommendations generally have low levels of agreement. Given these results, and the crucial role peer review plays in determining the fate of ideas as well as individual career advancement, it is hardly surprising that, in the words of Siegelman and Whicker, "virtually everyone in academia seems to have an opinion about how well the peer-review process is working" (1987: 495). (For a summary of the extensive literature in this area, see Weller, 2001.)

My purpose here is not to revisit such weaknesses in the peer-review process and their effects on the professional advancement of individual scientists, but to discuss an equally serious consequence of peer review (as currently practiced) for substantive advancement and, in turn, learning and education, in the management discipline. This consequence has received only limited and indirect attention among management theorists and researchers (for notable exceptions see, Astley, 1985; Jacques, 1992; Locke & Golden-Biddle, 1997; Morgan, 1983) and yet is potentially much more destructive for the discipline as a whole than any other aspect of the peer-review process. Given that this consequence has yet to be acted upon, I argue it warrants closer scrutiny. Prior research on peer review has almost exclusively focused on surface features of the review process—its impartiality, validity, and reliability. What has received relatively less attention is the influence of the social component, which shapes the content of the discipline's published record and, in turn, determines its scientific progress.

My primary theme is that all knowledge-claims are socially constructed. Sociologists of science, especially those with a postmodernist discourse orientation (e.g., Derrida, 1972/1982), generally recognize this theme, which challenges the notion of objectivity, holding that knowledge is relative and subjective. Consequently, intellectual advancement is not a result of establishing objective truths, but rather a product of social definition. As Astley (1985) has described with regard to the management discipline, these definitions of truth are reinforced by institutional mechanisms that invest them with the stamp of scientific authority. Peer review is one, if not the principal, institutional mechanism within the management discipline by which "credible and creditable knowledge claims" are recognized (Kelly & Bazerman, 2003: 29).

Continuing this thread, I examine how, as a social process, peer review, with its characteristic exchange of referee comments and author revisions, influences how knowledge-claims are presented and validated and how as a consequence of this negotiation, the published version of a manuscript is almost inevitably a compromise between what its authors intended to say and the mandates of an editor and a set of referees. Next, in reflecting on the role of editors as "hubs" and referees as "lynchpins" in the peer-review process, I consider whether true "peers" can actually be identified to judge the merits of a manuscript, the fate of which may depend as much on the "luck of the reviewer draw" as it does on its merits. Third, I raise the question of what beyond scientific considerations guides referees in judging the merits of knowledge-claims. Fourth, acknowledging that the peer-review process is a social practice that requires balancing the triangular interests of authors, editors, and referees, I stress the importance of ensuring that a clear authorial voice is preserved throughout the process and not lost in an effort to please referees and editors. Fifth, I consider the marginal costs and benefits of the peer-review process as currently practiced. In conclusion, I offer ten suggestions for assuring the integrity of the scientific enterprise, while respecting the prerogatives and ethics of authorship.

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Although I will reference research from various academic areas, my comments will primarily be based on my experiences as an author, a reviewer, and a journal editor within the management discipline. This said, prevailing beliefs about the peer-review process are not unique to management journals. As Bakanic, McPhail, and Simon note, "most have been expressed in one form or another as long as scholarly journals have depended on editors and peers to decide what to print" (1987: 631). To draw a clearer picture than that which can be obtained by anecdote alone, I will also report data collected from 173 lead authors of articles published in the *Academy of Management Journal (AMJ)* and *Academy of Management Review (AMR)* from 1999 to 2001 (Bedeian, 2003). Conversations with colleagues (as authors) and the reading of critiques prepared by other referees of papers I have reviewed further inform my comments.

SOCIALLY CONSTRUCTED KNOWLEDGE

Modern science, as codified in its end product—the scientific article—is, by and large, ritualized fiction (Bedeian, 1997). Reflecting a positivist Kuhnian (1970) paradigm, the scientific article in its orthodox form generally adheres to a conventional style and format (viz., Introduction, Methods, Results, Discussion), which presents science as a purely rational sequence of activities cumulating in new knowledge. In reality, however, as the product of social processes, knowledge-claims are socially constituted rather than the consequence of an absolute or natural truth. Viewed from this perspective, scientists inhabit "a world of their own construction" (Sismondo, 1993: 529). Knowledge-claims are communally developed as scientists "check and recheck and revise and re-revise" their mental conceptions of the world. This is not to say, however, following Astley, that facts do not exist or that there is no such thing as objective reality, but rather "our *knowledge* of objective reality is subjectively constructed" (1985: 509).

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Within academic disciplines, knowledge-claims are socially validated through negotiation and eventual consensus among experts, with recognition and esteem accruing to those scientists who, in Merton's words, "have made genuinely original contributions to the common stock of knowledge" (1957/1973: 293). Writing in the field of biology, Myers (1990) shows how knowledge-claims are negotiated and, thus, socially constructed through the peer-review process, with its characteristic exchange of referee comments and author revisions. He illustrates this by analyzing the transformation and ultimate denouement of two manuscripts, each of which was revised multiple times in response to referees' criticisms before being accepted for publication. In doing so, he describes the negotiations that unfold as the manuscripts' authors try to make their claims to originality as strong as possible and the referees attempt to place the authors' assertions within a body of existing literature. Myers documents that such negotiations are flexible, but only within limits. Authors must claim some minimum level of novelty (or have their work dismissed as unoriginal). At the same time, however, if they venture too far beyond a discipline's established knowledge structure, they risk the charge that their work is irrelevant to existing research and, thus, unworthy of publication.

Myers (1990) observes that, whereas such negotiations perform an obvious screening function, they also lead to homogeneity of the scientific literature, as authors must bring their own interests in line with established theories to create a consensus on the nature of their knowledge-claims. More importantly, going beyond my earlier comments on the fiction perpetuated by scientific discourse in its orthodox form, for Myers the central question that presents itself is "not how reality is transformed in texts, but how it is made by texts" (1990: 98). In addressing this question, Myers argues that referees' comments about apparently superficial matters of organization and literary style are not just matters of taste, but help define the status of an author's knowledge-claim by constraining the acceptable content, positioning, and form of new contributions. (For more on this point, see Hyland, 2000.) Myers sees the unfolding negotiations between an author and referees as representing an unavoidable tension over the construction of a text's knowledge-claim as an author wishes to interpret it and referees who see their function as judging not only the validity of a claim and the status it should be accorded by the wider scientific community, but also its appropriateness for a particular journal. He observes that through trial-and-error, authors, in essence, must invent arguments that persuade editors and referees to acknowledge the novelty of their claims and assent to the ultimate publication of their work. As a consequence, the published version of a written text that reaches a journal's readers is almost inevitably a compromise between what its authors want to say, and the form in which they must say it, so as to be judged acceptable by an editor and, typically, two or three unidentified referees.

Myers (1990) cautions that the claims produced by such author–editor–referee negotiations and reported in the final published form of a written text should not be seen as inherently more scientific and, certainly, not necessarily more correct than those originally proposed by a manuscript's authors. Given the view of science as being purely objective offered by the popular press, it is doubtful that the general public fully appreciates this warning (Gilbert, 1976). Seasoned authors and other members of the scientific community have, no doubt, come to this realization on their own. As

Myers relates, "Almost every scientific researcher I have interviewed has an anecdote about a referee who reviewed an article of his unfairly, or who required alterations that, in the writer's view, diminished the value of the article" (1990: 64). The author–editor–referee tensions operating in the publication process, and especially the arguments that authors must invent through trial-and-error to persuade editors and referees to assent to their claims, may explain why veteran researchers are fond of paraphrasing the opening lines of Elizabeth Barrett Browning's *Sonnets from the Portuguese*: "How do I know my data? Let me count the ways."

Discourse-process theory offers an alternative understanding of the peer-review process. Eco (1959/1979) has used discourse theory to analyze artistic performances. He notes that musical scores all share a common feature: autonomy granted to individual performers on how they chose to play a work. He sees musical compositions not as bounded works requiring exact repetition, but as "open works" offering a "field of possibilities" brought to their conclusions by performers at the same time they are experienced. In turn, as open products, artistic performances are susceptible to countless interpretations, as individuals supply their own sets of tastes, personal inclinations, and prejudices. As Eco says, an individual's "comprehension of . . . an artifact is always modified by his particular and individual perspective" (1959/1979: 49). There being no one "right way" to present an artistic performance, performers and their audiences engage in inescapable "psychological collaboration" along a spectrum of aesthetic potentialities.

Taking a sociology-of-knowledge perspective, however, and viewing the peer-review process as a social experience, much as Eco (1959/1979) describes artistic interpretation, it may be seen as a "transaction" between reader and text. Just as a musical score is a series of notes, a manuscript may be viewed as a series of words, each word possessing a subjective meaning interpreted according to its reader's idiosyncratic experience. As Rosenblatt maintains, "each individual, whether . . . writer or reader, brings to the transaction a personal linguistic-experiential reservoir, the residue of past transactions in life and language" (1993: 381). Meaning only emerges as all these elements reverberate upon one another. Reading is thus a "complex social act" that embodies what B. H. Smith has tagged the "total economy of our existence (1988: 16); that is, as explained by Crawford,

> the mood of the moment, the occasion of the reading, the expectation one brings to the reading task, the value we attach to the text or its author, the directions the text suggests for reading it, one's prior experience and knowledge not only with similar texts but with anything and everything that makes the reading of the text a meaning-making and purposeful activity. In short, everything informs our response—the morning newspaper, the brightness of the afternoon sun, our allergies, and finances (2001a: 1).

In this individualistic way, texts become endowed with meaning for each reader (Brent, 1992: 34).

Once peer review is seen as a social act, it is possible to understand how the criteria used to judge knowledge-claims are filtered through a personal "reading lens," which alters individual referees' understanding and shapes their thinking in an idiosyncratic fashion (Crawford, 2001b: 2). In this sense, two referees commissioned to review the same manuscript actually read different works. Recognizing that referees each construct a unique interpretation of a manuscript's content offers one explanation for the refrain "But the referees are making different criticisms of my paper!" (Fiske & Fogg, 1990), as well as the "low" inter-referee agreement (ranging from 0.19 to 0.54) commonly reported across manuscript reviews (for a summary of these findings, see MacCoun, 1998). This explanation is consistent with Fiske and Fogg's conclusion that diversity and uniqueness in referee comments result not from disagreement on particular points, but rather from each referee making appropriate and accurate points, but on separate topics. In such circumstances, it is not surprising for a former editor to reflect on the inconsistency of referee reports he had received and concluded that "knowing what one reviewer had said about a manuscript would tell me almost nothing about what a second reviewer had said or would say" (Starbuck, 2003a: 346). Furthermore, the discourse-processing theory notion that each reader constructs a unique interpretation of a written text is also consistent with the belief reported by more than one third (38.7%) of the *AMJ/AMR* authors in the Bedeian (2003) study that recommended revisions in their manuscripts were based on an editor's or a referee's *personal preferences.*

In a further complication, it should be realized that once read, it is impossible for the same reader to re-read a text from a previous viewpoint, as each experience of reading will have been modified by all previous readings. Thus, not only will different readers experience the same text in different ways, but it is impossible for individual readers to have the same reading experience twice (Brent, 1992: 33). This realization offers insight into the common referee remark, "Well, I didn't think much of the manuscript when I first read it, but it makes more sense now that I've read it again," and the experience of coming back to a manuscript after days, or even overnight, and interpreting it quite differently. For this reason, experienced referees know the wisdom of reading a manuscript and then setting it aside at least overnight, if not for a day or two, before making any final judgments about its merits.

At least two implications follow from juxtaposing the literature on the unreliability of the peer-review process with Eco's (1959/1979) notion of texts as open works. First, it is doubtful whether there is a universal and articulable latent dimension of "merit" or "publishability" against which manuscripts can be judged, as the definition of any such standard will inevitably vary from one reader to the next (Hargens & Herting, 1990). This, however, may not be a bad thing as such. Rather, it may simply reflect a healthy state of pluralistic criticism (Clark & Majone, 1985). Second, referee recommendations are of only limited value in judging the merits of a knowledge-claim. Given anywhere from two to five referees, and suspected levels of interrater subjectivity, manuscript acceptance or rejection largely depends on which small sample of referees is commissioned (Bedeian, 1996b). Thus, the referees commissioned to read a manuscript may represent, but may not be representative of, an entire discipline. As J. R. Cole observed, "The essential point is . . . that if the number of reviewers sampled is small, then the estimate of [a] population's opinion of a [manuscript] can be quite biased" (1989: 59). The denial or acceptance of knowledge-claims may in this way be quite accidental, largely a function of who reviews what research rather than its quality per se (Langfeldt, 2001: 821).

HUBS AND LYNCHPINS

As Myers's (1990) analysis shows, and Ziman (1984: 64) before him observed, the peer-review process is a highly reflexive and, at times, convoluted social activity requiring the balancing of three distinct interests: Those of an author, of an editor, and of peer referees. Given the stakes at play, it is not surprising that opinions on the current operation of the peer-review process run strong. Both critics and defenders of peer review acknowledge that editors are at the "hub" of the process and that referees serve as its "lynchpins." Whereas critics rarely question the need for peer review, they do, however, rail at what they see as inadequacies in the process itself. Horrobin (1982), for one, has argued that the concept of peer review is philosophically flawed, being based on two myths: "The first is that all scientists are peers, that is, people who are roughly equal in ability. The second . . . is that in those rare instances in which someone who is exceptional does appear, the ordinary scientist always instantly recognizes genius and smooths its path" (p. 34). To this he adds: "No one who knows anything at all about the history of science can believe for one second either myth" (p. 34). Regardless of whether one agrees with Horrobin, he does raise several cognate issues. For instance, can "peers" be identified for most scientific work and, thus, are they capable of responsibly appraising its worth?

Few editors can be expected to possess the professional expertise to competently assess papers cutting across all the subspecialties of a discipline. Thus, they generally seek the specialized advice of others (i.e., referees) regarding whether a paper should be "published as it stands, or revised in detail, or be rejected out of hand" (Ziman, 1984: 64). Although the origins of peer review date back more than three centuries, the notion of a "trial by jury of one's peers" stems from the Magna Carta of 1215. As Eisenhart notes, "In academic peer review, peers are asked to judge another's academic contributions and potential in the context of a field of scholarship they presumably share" (2002: 248). In this sense, peer review demands "nothing less than a critical re-examination of an academic piece by another *equally versed* in [a] field" (Nash, 1996: 8). As such, a "peer," by definition, would be someone who possesses technical expertise at least roughly equivalent to that needed to have originally authored the work to be reviewed (Roth, 2002).

The requirement that the merits of a manuscript be assessed by a scientific "peer" working in the same field of research as its author, however, is further grounds for critics of current peer-review practices. In particular, concern has been expressed that manuscripts are often not consigned to acknowledged peers for scrutiny and, thus, the quality of not only manuscript reviews, but also published work has commensurably suffered (Miner, 2003b). Evidence for this concern is not hard to find. In one instance, the readers of a leading management journal have been told that its editors seek reviews from inexperienced referees as a means of "grooming" future "talent" (Schminke, 2002: 498). This practice is especially worrisome as inexperienced referees are relatively more likely to go into what Roediger (1987) has dubbed "reviewing mode," and engage in what Van Lange (1999) has tagged SLAMing (*S*tressing the *L*imiting *A*spects of *M*anuscripts), wherein they fall prey to a negativity bias (Amabile & Glazebrook, 1981). This overmotivation to be brutally critical has been attributed to not only insecurity and self-doubt among neophyte referees, but also a desire to impress editors so as to win an invitation to join the editorial team. This tendency is likewise documented in various autobiographical accounts (e.g., Ashford, 1996; Romanelli, 1996).

Additional fodder for critics who question whether referees have at least peer-level expertise is that among scientists asked to serve as ad hoc referees, the "decline-to-review rate appears to be correlated with reviewing expertise, stature in the field, and professional rank" (Northcraft, 2001: 1079). As explained by Glenn, what this suggests is that, "if the number of competent and willing referees has not increased . . . journal editors have had to increase the workload of the willing and qualified potential referees and/or turn to referees of doubtful qualifications" (1976: 180). If they have followed either or both strategies, one can only wonder if the effect on the quality of resulting reviews has been positive. Of special concern to critics, of course, would be the use of referees with doubtful qualifications—those who are not true "peers." One critic has offered the opinion that "pretense at expertise is as much a matter of malpractice as is an intentional hatchet job" (Bauer, 1984: 33). Data from the Bedeian (2003) *AMJ*/*AMR* author survey suggests the frequency with which "malpractice" occurs. More than a third (36.6%) of the respondents acknowledged submitting a critique for a manuscript they felt incompetent to review.

Touching briefly on the second myth, Horrobin (1982) alleges that scientists possess an inbred lack of imagination, which prohibits them from recognizing true genius and, thus, distinguishing major breakthroughs from lesser findings; critics again find reason to doubt the efficacy of peer review. The list of seminal works that met initial peer-review rejection is lengthy and includes those reporting research judged to be of Nobel-prize quality (Campanario, 1996; Gans & Shepherd, 1994). As Rosalyn Yalow, herself a Nobel laureate, observed in commenting on the peer-review process, "The truly imaginative are not being judged by their peers. They have none!" (1982: 60). To drive home this point, as part of her Nobel lecture, Yalow published the initial editor's letter rejecting the work for which she was eventually honored. Tongue-in-cheek, she observed that now both she and the editor were famous!

An editor's assignment of referees is a pivotal phase of the peer-review process and plays a crucial role in shaping the intellectual landscape of an academic discipline. Since the publication of S. Cole, J. R. Cole, and G. A. Simon's (1981) study of the National Science Foundation's peer-review system for grant proposals, it has been widely held that the fate of a particular proposal might best be characterized as resting on the "luck of the reviewer draw," half being determined by the merits of the proposals and the attributes of its principal investigators and half by apparently random elements.

Cole and colleagues concluded that whether a proposal was funded depended "to a significant extent" upon a program director's choice of referees. The prevalent (and lasting) impression that seems to have been created by this conclusion, as it was subsequently extended to journal submissions, is that the fate of a manuscript's suitability for publication is determined as much by luck ("which way the dice tumble") as by manuscript quality. This conclusion confirmed what many had long suspected—that convergence in peer judgments about a manuscript's quality occurred about as often as expected by chance. In the words of one vocal critic, "If a paper is not conspicuously poor, the outcome of its submission to a journal depends more on luck than anything else" (Glenn, 1976: 185). Or, as expressed by another contemporary in direct response to the Cole et al. conclusion, "If experts cannot agree at a level that exceeds chance, there is no reason to consult them in making editorial decisions" (Whitehurst, 1984: 27). Two other observers, however, seem to take some solace in the Cole et al. finding. Looking at the upside, they reasoned, "If errors of judgment are randomly distributed, 'good scholars' would occasionally have their manuscripts rejected and 'poor scholars' would occasionally have their papers accepted. Over time, everyone would have the same chance of being treated unfairly" (Perlman & Dean, 1987: 207). An interesting application of equity theory, to say the least!

A complete reading of the Cole et al. (1981) study, however, presents a more balanced picture than often portrayed. They point out that referees' disagreements may as readily be due to "real and legitimate differences of opinion among experts about what good science is or should be" (p. 885) as to caprice. Critics responding to the notion that the peer-review process is nothing more than a "crap shoot" have contributed to an extensive literature cataloging its purported (and real) conceptual, methodological, and political weaknesses. As argued above, however, what has been virtually ignored is that knowledge-claims are socially constructed, being subject to the inevitable author–editor–referee tensions operating throughout the publication process. The impact of this social component, and the influence it has on referee judgments and, in turn, upon claims entering the list of science, offers an (as yet) overlooked explanation for the seeming randomness with which manuscripts are either accepted or rejected.

REFEREE EYES

Viewed from a sociology-of-knowledge perspective, the relative independence of referee reports suggests that the authentication of knowledge-claims involves more than reason, good methods, and evidence. This naturally raises the question of what, beyond scientific considerations, guides referees in judging the merits of knowledge-claims. More specifically, what extra-scientific beliefs and peculiarities might potentially influence an author's success in navigating the "peer-review maze"? (Mahoney, 1978: 41).

In this connection, Bakanic, McPhail, and Simon (1987) note that some editors may purposely select referees who will read a manuscript from different angles and try to keep their differences in perspectives in mind when rendering their editorial decisions. In doing so, editors may attempt to "exploit the disciplinary and institutional allegiances of peer reviewers, and even their geographical, racial, political, or sexual biases" (Jasanoff, 1985: 30). In other instances, editors may attempt to select one referee who is a methodological expert and another who is a subject-area specialist. Critics, however, have argued that this practice will not "add up to a well-rounded critique." As one explains, "The only really appropriate referee is someone who has the relevant technical skills, theoretical sophistication, *and* substantive knowledge" (Glenn, 1976: 180n). This argument recognizes that editors have wide latitude in selecting referees and may thus predetermine a manuscript's fate. Acknowledging this latitude, one editor admitted, "I feel certain that if I were allowed to pick reviewers of manuscripts, I could, holding constant the papers under question, produce either a high positive correlation among their opinions, a zero correlation, or even a negative correlation for papers that are free of technical errors" (Roediger, 1987: 251n).

Beyond noting that each reader constructs a unique interpretation of a written text, how such a variance in outcomes is possible may be explained by realizing the dependence of referees' evaluations on what Travis and Collins (1991) have termed "cognitive particularism," wherein strong social networks (as have been shown to exist in the management discipline; Bedeian & Feild, 1980; Duncan, Ford, Rousculp, & Ginter, 2002) lead to strong cognitive similarity. A similarity in cognitive boundaries is believed to lead to a correspondence in decision making. Indeed, Wilkes (1994) has presented evidence suggesting that scientists who share the same cognitive style bring similar emphases to their work and respond more favorably when vetting one another's work. Thus, whereas this sharing of cognitive boundaries shapes their interests, it also functions as a cognitive constraint by defining their evaluative conception of what constitutes "good and valuable" research. Not only do the facts of reality appear differently for scientists wearing different research lenses, but the importance of those facts also differs (Mahoney, 1979: 251), as they "tend to apply favored theoretical perspectives in a more or less exclusive manner" (Astley, 1985: 500). Moreover, as Burrell and Morgan argue, because these favored perspectives are so dominant and strong, their adherents often unquestionably accept them as "self-evident" (1979: xi). Consequently, as described by Roediger, "What may be perceived as a great discovery or a novel viewpoint by some workers may be greeted with a yawn or derision by others" (1987: 234).

Thus, it should come as no surprise that Crane (1967) reports that a journal's contributors tend to have the same characteristics (e.g., academic affiliation, doctoral origin, and professional age) as those of its editorial-board members. Blind review of manuscripts did not change this relationship. Moreover, in situations where editors and editorial-board members share the same cognitive biases, it is not surprising that "editors typically act as if reviewers are more competent than authors and as if reviewers' opinions have more validity than authors' opinions" (Starbuck, 2003a: 345). Indeed, in the Bedeian (2003) survey of *AMJ*/*AMR* authors, more than one third (34.1%) reported having been treated like inferiors by editors or referees and 56.1% felt that editors had regarded a referee's knowledge about original research reported in the authors' own manuscripts as more important than their (the authors') own.

Crane (1967) suggests that academic partisanship also results from common academic training, which encodes schematic expectations about how content goes together and, thus, guides information processing. She argues that, as a consequence, editorial-board members respond positively to "aspects of methodology, theoretical orientation, and mode of expression in the writings of those who have received similar training" (p. 208). In providing structure to an otherwise ambiguous social experience, academic training affects the ostensibly neutral judgments that referees make about the authority of alternative knowledge-claims by producing a form of cognitive insulation between groups subscribing to different theoretical orientations (cf. Jasanoff, 1985: 26). Support for this reasoning comes from a study conducted by Miner (2003a), who asked a group of organizational behavior (OB) and strategic management scholars to rate the importance of various OB theories. Those with a strategic-management orientation judged the theories less favorably than their OB counterparts. In extending this result, he reasoned, "one would expect that manuscripts reviewed by those whose disciplinary orientation did not fit the material would be recommended for rejection more often" (Miner, 2003b: 342). Results of a second study suggest that such training effects also extend to journal editors. In a study of manuscripts published in the *Academy of Management Journal, Academy of Management Review,* and *Journal of Management* for the years 1982–1996, Martinko, Campbell, and Douglas (2000) found the respective editors' primary fields of interest were related to the content of the articles they selected for publication.

From a sociology-of-knowledge perspective, these findings further question the portrait of scientists as dispassionate creatures "capable of suppressing personal biases in the interest of objective inquiry" (Mahoney, 1979: 351). Moreover, they raise what Mitroff and Chubin consider to be "one of the pivotal issues in the peer-review debate" (1979: 209). That is, whether the peer-review process is prey to a biased mind-set on the part of journal editors and referees and, hence, only favors work conforming to an orthodoxy defining what constitutes "good" research. The publication difficulties encountered by researchers pursuing unconventional ideas outside accepted cognitive boundaries, as well as the resulting dampening effect on the creative spirit of a discipline, should be of no small concern (Moran, 1998). Both set an upper limit on an academic discipline's rate of scientific change. Recognizing this point, Kayes (2002) has argued, following Geertz (1983), that the best way to influence intellectual advancement within the management discipline and, thus, learning is to encourage the adoption of diverse perspectives and, perhaps, even the actual blurring of theoretical representations.

This argument aligns with Myer's (1990) concern that the peer-review process leads to homogeneity of the scientific literature, as authors must bring their own interests in line with those of established theories. Mone and McKinley (1993) have specifically commented on the value of uniqueness in the conception and execution of management research programs, arguing that multiple perspectives are important for advancing existing knowledge. Carrying this thinking forward, Bailey and Ford (2003), however, have cautioned that unless new advances are based on previous findings, the management discipline risks further fragmentation and a growing inability to address the needs of practicing managers.

Whatever the case, Langfeldt (2002: 64) has observed that editors and referees are unlikely to look upon cognitive particularism as a source of bias, but rather as a basis for authoritative evaluation. Indeed, as noted by Frey, "cognitive dissonance reduction . . . ensures that, in most cases, the referees [and editors] do not perceive any conflict between what *they* consider to be the 'common good' and their own private interests" (2002: 8). This is not to suggest that editors and reviewers are not doing their work honestly and conscientiously, but to highlight (once again) that all knowledge-claims are a result of social processes. What is suggested, however, is that bias exists on both ends of the peer-review process. Whereas author complaints are commonly passed off as Aesopian "sour grapes" and research does suggest that authors do offer self-serving external attributions in response to negative editorial decisions (Wiley, Crittenden, & Birg, 1979), editors and referees are often no less guilty of overestimating their objectivity. On balance, it would seem that a "holier-than-thou" attitude from either party is unwarranted (Kruger & Dunning, 1999) and, perhaps, at least on occasion, authors should not be the only recipients of rejection letters (cf. Millman, 1982: 125).

One aspect of the holier-than-thou attitude on the part of editors has always struck me as particularly odd. That is, when an editor's rejection letter concludes with the suggestion that although a manuscript is "unsuitable for publication in this journal," the accompanying referee comments might be helpful in crafting a revision for submission to another journal. Such rhetoric implies that although an author's work fails to meet "our standards of scientific worth," by incorporating the revisions that have been offered, it may still be pawned off on a less discriminating journal. That one journal would refuse to certify an author's knowledge-claims as credible, but then suggest that another might, provides further support for the notion that all knowledge-claims are socially constructed.

AUTHORIAL VOICE

As a social practice, the peer-review process requires balancing the triangular interests of authors, editors, and referees. The tension that is created by the role-induced perspectives of the involved parties distinguishes the scientific enterprise from mere opinion, custom, or tradition (cf. Logan, 2002). A manuscript's authors try to make their knowledge-claims as strong as possible, and referees attempt to weaken and, if possible, reject these claims. As manuscripts pass through what may be multiple requested revisions, the interests of authors and referees are inherently at odds (Merton & Zuckerman, 1971/1973: 492). The negotiations that ensue inevitably involve tension as authors try to show that they "deserve credit for something new," while editors and referees try to access the acceptability of the authors' claims (Myers, 1990:67–68).

Both anecdotal evidence (Biggart, 2000) and recent empirical work (Bedeian, 2003) suggest that the characteristic triadic tension among the goals of editors, referees, and authors has developed into a deep conflict, as it has become increasingly common for journals to request that manuscripts be extensively and repeatedly revised. In line with the concerns expressed above, the consequences of this inflation in the review process for the management discipline's published record and, in turn, its scientific progress, have likewise received virtually no attention. And, yet, as I have argued, these consequences are potentially much more destructive for the management discipline as a whole than any other aspect of the peer-review process. As described by Spector,

> Over the past few decades there has been an inflation in the journal review process in our field. Reviews that at one time were typically short overviews (one page or less) of major strengths/weaknesses have grown in length and thoroughness, to where now eight or more single-spaced pages are not uncommon. By the middle 1980s, journals began to require detailed point-by-point replies to reviewer comments to accompany resubmissions. These too have undergone inflation from short lists of whether or not each reviewer comment was addressed to lengthy companion documents that can be longer than the submitted manuscript. Such point by points often include detailed background, ancillary analyses, references, tables, and figures that are not in the submitted manuscript. Often the review process becomes a struggle between authors and reviewers with editors serving as referee through round after round of resubmission (1998: 1).

No one questions either the role of referees in advising editors regarding the "publishability" of a submission or that referees can, on occasion, protect authors from themselves, as in cases involving slipshod work. At the same time, there is a delicate line between incisive criticism commenting on "irremediable errors and corrigible faults" and the rights of authors to protect the intellectual integrity of their work. In the view of a growing number of commentators (e.g., Miner, 2003a; Starbuck, 2003b), this line has been increasingly broached in the management discipline. This view has particularly come to the fore with the notion that peer reviews should serve a "developmental function" that is largely pedagogical in nature (Schminke, 2002: 487). According to this notion, a key aspect of reviewing is providing authors with a "compensatory education" concerning how best to achieve their objectives, as well as how to analyze their data, frame their arguments, and express their ideas (Sutton & Staw, 1995: 380). One editor has described his job as providing a "seminar by mail." He describes such seminars under his direction as involving "trenchant critiques" of an author's efforts, including "very detailed suggestions for revising and perfecting the research procedures, the strategy of presentation, the craftsmanship of the paper, or all of these" (Patterson, 1994: 16). Reflecting a similar instructional tone, the guidelines provided referees for one prominent management journal pointedly advise, "DO try to make your revisions *developmental*. We are trying to develop authors as well as evaluate their work." Although well intentioned, this advice suggests a perplexing riddle. That is, when referees and authors are the same people, how is it that they are qualified to "teach and tell" as referees, but must be "taught and told" should they submit a manuscript for review? It also creates a dynamic in which referees are expected to offer pages of detailed suggestions for "improving" a manuscript, and unlettered authors are expected to abide (Kinicki & Prussia, 2000).

At an opposite philosophical extreme are those critics who believe that evaluation is the heart of review and that the education of authors should be left to graduate programs. Noting the unreliability of referees' judgments, and that referee suggestions may, in fact, make a manuscript worse rather than better, these critics contend the "details of writing, arrangement, emphasis, [and] conclusions drawn" should remain with a manuscript's authors. As an example of this philosophy, consider one editor's thoughts on the question of developmental reviews:

> . . . [W]e believe in letting the author tell his own story, without having referees acting as nannies and telling him how he could tell his own story better . . . we do not believe that it is the function of the editor of a scientific journal to teach contributors what they should have learned from their Ph.D. supervisors (Eysenck, 1980: 1).

Given the social variables that abound within the peer-review process and a need to allow for the interests of authors, editors, and referees, it would seem more meaningful to consider a *balance* that allows for the prerogatives of all parties to be respected. Ideally such a balance would redress the problem of authors being treated as if they were apprentices and allow them to publish manuscripts that would reflect their ideas rather than the demands of an editor or set of anonymous referees. Open-ended responses to the Bedeian (2003) *AMJ/AMR* author survey convey a belief among some respondents that their intellectual property rights have been arrogated by the peer-review process: "I believe that [the journal] . . . has gone overboard in rewriting manuscripts according to reviewers' and editors' preferences," and "In the end, [the editor] actually rewrote sections of the paper to include his preferred terminology. I'm somewhat surprised he didn't take authorship credit." As noted by Myers (1990), editorial comments about matters of literary style are not just matters of taste, but affect a manuscript's persona. Thus, editors should be certain that recommended revisions are not simply a matter of personal preference, but are required for clarifying obvious ambiguities or correcting errors. This, of course, would equally apply in the instance of copy editors, who likewise should be sure that their proposed changes do not alter an author's substantive intent but are necessary for enhancing clarity or to match a journal's format requirements (Bishop, 1984: 109–110).

Bedeian (1996a) has likened the demand that authors conform to the conceptual and stylistic preferences of editors and referees to "ghostwriting." As Frey has noted, "sometimes the papers published reflect more the referees' than the authors' ideas" (2003: 213). In such situations, one has to wonder if the referees' demands for revision have not become so overly invasive as to border on coauthorship and may have actually slowed scientific progress by delaying publication of important work or by even discouraging some authors from attempting to publish their findings. Further, although generally unappreciated, ghostwriting also has serious historical ramifications. In years to come, when historians are parsing an author's work, they will be unable to tell if they are responding to the author's own words or those of an unidentified editor, referee, or copy editor, all of whom will escape responsibility for what is attributed to the author. This corruption of the historical record has obvious implications for tracing the true origins and, by extension, evolution of managerial thinking.

A particularly irksome form of ghostwriting occurs when referees essentially ask authors to rewrite a manuscript as they would have written it, rather than evaluating it within the author's intended framework (Leblebici, 1996). Sometimes this involves testing additional hypotheses at a referee's suggestion or, possibly, recasting or dropping hypotheses, especially those that yield null results. Readers are seldom told about this. Kerr (1998; Garst, Kerr, Harris, & Sheppard, 2002) has referred to the practice of recasting or dropping of hypotheses as HARKing (Hypothesizing After the Results are Known), wherein a post hoc speculation is inserted into the manuscript as if it were an a priori hypothesis. As statisticians have long recognized, when such hypotheses are data driven, they are inherently susceptible to capitalization on chance and are nothing more than a disguised form of data dredging (MacCallum, Roznowski, & Necowitz, 1992; McPherson, 1976). HARKing thus not only leads to a biased sample of findings being published, but also to a misrepresentation of true underlying relationships. Moreover, forming hypotheses only after findings are known and then conjuring up supporting theories results in what McNemar long ago lamented—an academic discipline that "rests on a foundation of mathematical quicksand and psychological bog" (1960: 300). (For two authors' experiences with the review process and HARKing, see Starbuck, 1994, and Humphreys, 2002.)

From a sociology-of-knowledge perspective, a basic truth emerges. Depending on the elements that a referee brings to reading a text—noting these elements will vary from one referee to the next—referees can always find problems with a study and can always think up other relevant tests or alternative theoretical frameworks. Further, given their unique perspectives, referees can always interpret a study's results in their own way and call upon a different set of supporting references to write a different manuscript (Gilbert, 1977). The subjective nature of knowledge construction thus suggests that the search for a perfect manuscript could go on *ad infinitum* as referee after referee adds his own twist to a study's narrative. Indeed, in a conundrum reminiscent of St. Luke's parable of the lost sheep, a common dilemma encountered by authors confronted with multiple peer reviews is that in responding to one referee's recommendations for revision, they run the risk of alienating others who either have a different take on a particular issue or may not even see the issue as relevant. In such a circumstance, the content of the resulting manuscript will be as much a function of the idiosyncratic opinions of the referees selected to vet a work as its author's original intent. Further, in line with Myers's (1990) observation that peer review leads to homogeneity of the scientific literature, there is reason to believe that as the number of referees for a manuscript increases, manuscripts become "more complex," but "less interesting" (Higgins, 1992), with their main thrusts becoming less accessible to readers (Zanna, 1992), as they read more like committee-authored reports (M. G. Warren, 2000). Moreover, it should be obvious that had a manuscript been read by an entirely different set of referees, according to their own subjective perspectives, the final product would have been equally different, if not less pedantic.

Indeed, in a conundrum reminiscent of St. Luke's parable of the lost sheep, a common dilemma encountered by authors confronted with multiple peer reviews is that in responding to one referee's recommendations for revision, they run the risk of alienating others who either have a different take on a particular issue or may not even see the issue as relevant.

This last realization adds a second layer of randomness to the peer-review process, in that it underscores that not only may the acceptance or rejection of a manuscript be due to chance, but that its ultimate content may be as well. It also suggests a correlative observation. Whereas one might argue that an initial submission has been improved by successive rounds of referee comments, this should not be taken to imply that in its published form it is necessarily superior to that which would have resulted had it been reviewed by a separate set of referees. That the peer-review process is thus characterized by multifinality (i.e., a condition in which the same initial conditions lead to a different result) rather than equifinality (i.e., a condition in which different initial conditions lead to the same result) further attests to the fact that knowledge-claims are socially constructed.

None of this is meant to imply that authors do not want referee suggestions for revising and polishing their work and are not appreciative of such guidance (Bedeian, 2003). When properly matched to manuscripts, qualified referees do add value in the review process (Laband, 1990). A balance of the type called for above, however, would allow editors to hold the ground on factual errors, yet allow authors leeway in responding to disagreements growing out of debatable issues, keeping what *they*, the authors, find useful and discarding what *they* do not (Frey, 2003: 216). Revisions that overstep the bounds of peer review and shift the focus of an author's knowledge-claim would be the exception. This balance would also allow authors to maintain their own persona as reflected in their writing styles, choice of language, and construction of arguments.

Above all, this would avoid a final product that its author may not have intended to write, not to mention expressing in someone else's words thoughts an author may not have intended to convey. Moreover, it would ensure that a clear authorial voice remains at the center of a manuscript and circumvents basic questions related to both the ethics of legitimate authorship and the intellectual responsibility for the work being reported (L. Smith, 1998). Beyond the historical ramifications mentioned above, these latter concerns have obvious contemporary consequences for the transparent development of our discipline's common stock of knowledge, as well as for the recognition bestowed upon individual scholars by our academic community.

In summary, a sociology-of-knowledge perspective underscores that knowledge-claims are a collective product bearing the mark of many social relations (Roth, 2002: 15). The process of preparing a peer-reviewed manuscript is social from beginning to end, incorporating an inherent tension that makes negotiation among authors, editors, and referees essential, as they strive to achieve their respective goals (Myers, 1990: 67). The results of these trilateral negotiations is a literature that represents an academic discipline's published record and, more important, determines the general course of its substantive advancement.

"PUBLISHING AS PROSTITUTION"

The true emphasis of the peer-review process should be on advancing a common body of knowledge. Some authors, being "beaten down," respond to the process by withdrawing their work completely. Others actively enter into the referee–author negotiations described by Myers (1990). Recognizing that the likelihood of refusing to make any recommendations for revision and still have their work published is slim or none, these authors give ground in some areas and hold fast in others. This jockeying, of course, introduces a further element of risk into the review process, as editors and referees may not look fondly on authors who question their judgment. Seeking to minimize all risk, others simply comply, choosing to "accept" referee and editor demands and "fall in line," revising their manuscripts, sometimes considerably, to get them published. Still, others seek more hospitable venues and, if they are persistent, their manuscripts are eventually published elsewhere.

For many, the choice to pursue is bound in a host of related considerations. What Larochelle and Désautels (2002) call the "referee regime," in reference to the fact that referees and editors govern access to the academic playing field, has wide ripple effects. These effects are most obvious in criteria for tenure and promotion but also translate into economic considerations relating to wages, grants, and research contracts (Bedeian, 1996c). The pressure to conform is, thus, great. As Frey has noted, for most scholars the invitation to resubmit a manuscript "*according to the demands* exactly spelled out by two or three referees and [an] editor . . . is a proposal that cannot be refused because their survival in academia crucially depends on publications in referred professional journals" (2003: 206). In protest, he goes further in contending that "the system of journal editing existing in our field at the present time virtually forces academics to become [intellectual] prostitutes," as they "sell their souls" to please referees and editors so that their work will be published. The extent to which academics may be willing to go against their convictions in order to be published is suggested by data from the Bedeian (2003) *AMJ/AMR* author study. To wit, nearly 25% of the respondents admitted to revising their manuscripts to placate a referee or editor and as a result actually making changes they as authors felt were incorrect.

Given the prevailing referee regime, new entrants to our discipline would seem to be the most vulnerable to the dramatic personal and professional consequences that underlie Frey's (2003) concern. Seeking tenure and job security, and recognizing the opportunity cost of time, aspiring authors may have little recourse but to surrender their voices to secure the approval of referees and editors (Neck, 2001). One novice author recalls the disappointment of such a situation, wherein an editor made acceptance of a manuscript contingent on including material that seemed to have no relevance to the author's basic argument. Recognizing the editor's strategic position, however, the author plaintively explained, "Having spent so much time in the writing of [my] article, and being a young scholar, I complied" (Roth, 2002: 3).

For more senior academics the situation is, of course, different. Rather than subjugate themselves to the rituals of peer review, some simply drop out of the publication process altogether, believing that the reviews they typically receive do not come from qualified referees (Ashford, 1996: 125). Others turn to authoring or editing books or contributing to anthologies or, in some instances, writing newspaper columns (Frey, 2003: 215). Such outlets provide an opportunity to be more independent and creative in one's thinking, being free of the conventional strait jacketing associated with the scientific orthodoxies and demands of unidentified referees.

EQUATING MARGINAL COSTS AND BENEFITS

A final issue that presents itself is the relative benefit of the peer-review process as currently practiced. What Ellison (2002) has noted with regard to economics is no less true for management as a discipline. That is, "the review process is the major determinant of how [academics] divide their time between working on new projects, revising old papers, and reviewing the work of others" (p. 949). It thus influences the productivity of our discipline as a whole, as well as how enjoyable it is to be an academic. Further, Ellison wonders whether the time spent revising manuscripts is either necessary or valuable. Reasoning as an economist, it could be naturally argued that, to operate optimally, recommended revisions would equate marginal costs with marginal benefits. Although not recognizing this argument per se, Roediger (1987) has suggested that an interesting study would be to compare initially submitted manuscripts with their published versions to determine if the peer-review process leads to a significant improvement in the finished product (for one such effort in the medical sciences see, S. N. Goodman, J. Berlin, S. W. Fletcher, & R. H. Fletcher, 1994). In an intriguing twist, Ellison has noted that it is plausible "to imagine that in a parallel universe another community of [academics] with identical preferences could have adopted the norm of publishing papers exactly as they are submitted, figuring that any defects will spur academic discourse and reflect on the author" (2002: 984–985).

Ellison's (2002) musings lead to an additional issue. That is, "[w]hether any one set of articles will ultimately be most beneficial" for advancing a discipline (Perlman & Dean, 1987: 212). From the perspective of an editor interested in improving the impact and importance of the scientific work being published in a discipline, Roediger similarly questions if "it matters whether or not one publishes what one perceives as the best 25 percent of the submitted papers, rather than (say) the next 25 percent" (1987: 224). Miner (2003b), a former editor, has speculated on this very point. He contends that the peer-review process as currently configured "rejects a substantial number of articles that are just as good, if not better, than what is published." He explains, "This occurs because when we get down to something similar to a 10 percent acceptance rate, it is impossible to discriminate effectively."

A second interesting study would be to determine the fate of rejected manuscripts, especially looking at the citation rate of those manuscripts that were subsequently published, to determine their impact on the discipline. Some rejected manuscripts are, no doubt, lost to the discipline forever (and, possibly, properly so). There are, however, dozens of publication outlets, and if authors encounter unfavorable reviews at one, they can "roll the dice" at another. Eisenhart correctly notes, however, "Often other outlets are found, but often too, they are not as mainstream, not as prestigious, and not as influential as [an] author's first choice" (2002: 250). Finally seeing their work in print is, of course, little solace to those faculty members denied tenure due to a lack of publications in first-tier outlets. Moreover, as Eisenhart commiserates, once their tenure clock expires, and they are denied a permanent appointment, they may be unable to locate another faculty position.

There may yet be some consolation for those affected. Articles in top-tier journals do generally garner more citations than do those in lower tier outlets. At the same time, however, research shows that articles published in top-tier journals do not necessarily exhibit significantly higher quality, as measured by the average number of citations they receive. As Starbuck explains,

> Journals reject some manuscripts that genuinely belong in the top 20 percent of all submissions after a first review and reject more after repeated reviews. Indeed, some manuscripts that genuinely belong in the top 20 percent of all submissions appear in second-tier and third-tier journals. Conversely, one-half or more of the articles in top-tier journals are not among the best manuscripts submitted (2003b: 1).

Simply stated, reasonably assuming that some number of the best submissions appearing in second- and third-tier journals were initially rejected by higher tier journals, we can assume these data further attest to the impreciseness of the peer-review process and underscore the fallacy of evaluating the quality of articles (not to mention promotion or tenure candidates) simply based on the journals in which they were published. The also suggest a third study: the veracity of the common belief that, regardless of tier, *every* journal publishes articles that are never cited. It would be especially interesting to know what percentage of articles published in top-tier journals are never cited and, thus, while taking coveted journal space, have no documentable impact on the discipline's intellectual capital.

RECOMMENDATIONS

Recognizing that all knowledge-claims are socially constituted places the role of peer review in a different light. Rather than an omniscient arbiter of truth, peer review is more properly understood as a social process whereby knowledge-claims are socially constructed. Whereas there is little question but that peer review is an important mechanism for authenticating knowledge-claims prior to entering a scholarly discipline's published record, I have argued that there must be a balancing of the inevitable author–referee tensions operating throughout the editorial process so as to ensure that a clear authorial voice is preserved. I have further argued that this is important not only to avoid questions relating to both the ethics of legitimate authorship and the intellectual responsibility for the work being reported, but that to do otherwise permits *sub-rosa* influences to be exerted on a discipline's current character and its future development.

What follows are ten suggestions for assuring, if not enhancing, the integrity of the scientific enterprise, while respecting the prerogatives and ethics of authorship. If enacted, it is my belief that several of these suggestions will not only bolster the discipline's common stock of knowledge, but also stimulate its intellectual vitality. Whereas enacting these suggestions would require some additional costs and time, these would be more than outweighed by the benefits that would accrue for the management discipline as a whole.

1. *Allow authors of potentially controversial papers to submit a "Note to Referees."* A brief 1- to 2-page note would not only provide contextual background for the thesis underlying the authors' work, but also permit them to address any theoretical, conceptual, or methodological issues they believe might be unfamiliar or mistakenly misconstrued by a manuscript's referees (Armstrong, 1982). This would enhance the review process by giving author–referee communication a more dialogic tone.

2. *To enhance the dialogic tone of published manuscripts, the use of discursive footnotes should be encouraged.* Footnotes provide a parallel text for disclosing idiosyncratic, but important, research considerations, as well as a means for authors to provide observations on their own research, what Zerby has called the "inner workings of scholarship" (2002: 9). Such observations humanize scholarship by providing insights into how an author's ideas developed and may also suggest or inspire ideas for further research. The frequent and longstanding use of explanatory footnotes in disciplines such as sociology and economics attests to their discursive value. At times, the footnotes may even be more illuminating, if not entertaining, than the text being annotated (Grafton, 1997).

3. *If referees' comments are of scientific value, they should be published with the manuscripts they review.* This should especially be the case with manuscripts that address controversial topics. Publishing the views of dissenting referees or the contrasting views of qualified commentators would allow readers an opportunity to judge a manuscript's merits on their own and also provide protection to naive readers. Peer-commentary journals such as *Behavioral and Brain Sciences* (*BBS*) and periodicals such as the *Journal of Educational and Behavioral Statistics* (*JEBS*), which occasionally include commentaries with author responses on selected articles, provide examples of this suggestion in action. Each issue of *BBS* publishes a "target article" followed by a set of commentaries authored by peers actively involved in research on the topic under discussion. The content of the commentaries inevitably varies, with some noting inconsistencies in a target-article author's reasoning or citing conflicting data and others being supportive and suggesting ideas for future research. The final goal is to achieve a state of "creative disagreement" through the interaction of data, ideas, and minds (Harnad, 1979). Such exchanges would add a new dimension to the management literature by providing a recognized forum for considering alternative perspectives. One suggestion would be to use the *BBS* format and, as is now done with theme issues, annually set aside a portion of one or two of a journal's issues for the discussion of a controversial topic. An alternative would be to follow the *JEBS* model and provide commentaries on a selective basis. The management discipline needs more debate. It could stand to be livelier, as well. Point–counterpoint exchanges would help in both regards.

4. *Publish more letters to the editor and author replies.* Subscribers either to the *American Psychologist* or the *American Sociological Review* often find that the sometimes-heated interchanges appearing in the Comment and Reply sections can be more intellectually stimulating than the original works being disputed. These types of back-and-forth exchanges invite reader participation in critiquing and developing new insights. For the most part, such dialogue seldom appears in the management literature. As rhetorical forms of academic discourse, such venues also provide a means for authors to further develop and, if necessary, correct misleading or erroneous knowledge-claims. Simply stated, self-critique is the ultimate responsibility of a scientific community (Chubin, 1985).

5. *Identify the referees of published manuscripts.* It is my impression that most referees take their responsibilities seriously and see reviewing as a professional obligation that comes with membership in our academic community. At the same time, as referees for most journals are unpaid, there are few overt rewards or incentives beyond professional courtesy and an occasional mention on "Acknowledgments" pages, for serving as a referee. Indeed, Glenn refers to refereeing as "charity work" and notes, "All too often, the 'reward' a referee receives for prompt and conscientious work is receipt, within a few weeks, of another manuscript from the same journal, often less closely related to his or her interests and areas of expertise than the previous paper" (1976: 180). As Glenn suggests, individuals identified as dependable referees will generally be invited to do many reviews, perhaps more than they can reasonably perform. This will especially be the case if they publish with reasonable frequency and, thus, their names appear in reference lists and computer indexes, common sources used by editors for identifying qualified referees. Given the few tangible benefits associated with refereeing, the fact that the peer-review process functions so well is evidence of our discipline's communal norms. An opening footnote acknowledging an article's referees would provide the referees with more than a modicum of recogni-

tion for their service and further motivate them to be as thoughtful as possible in their reviews (Peterson, 1975). Referees should, of course, have the option of being identified or remaining anonymous.

6. *Authors of all manuscripts submitted for publication should be asked to rate the adequacy of a journal's administrative procedures and the usefulness of referee comments*. Were administrative procedures slow or prompt? Efficient or bureaucratic? The printing of submission and acceptance dates would indicate the length of time manuscripts typically take to pass a journal's editorial screen and be published. Such information would be helpful to authors choosing where to submit their work, especially those who might anticipate needing an acceptance letter for securing or keeping a job (Herxheimer, 1989).

Similarly, were referee comments useful, useless, or even offensive? Author responses to such questions would be of value to referees in improving the quality of their future reviews and to editors in evaluating the effectiveness of individual referees (Schwartzbaum, 1976). Referees have an obligation to both authors and readers and should be accountable to both. Although actual editorial misconduct may be rare (Street, Bozeman, & Whitfield, 1998), a mechanism for soliciting such feedback would allow authors to feel less vulnerable, permitting them to voice concerns related to the fairness of the review process, as well as the author-friendliness of a journal's administrative procedures (Fine, 1996). It is ironic that though editors and editorial-board members routinely assess the work of others, their own work as referees is seldom formally judged.

7. *Individuals selected to vet a manuscript should be true peers to its author(s) in the sense of being equally versed in the field of research in which they are being asked to review*. It has been repeatedly observed that "[t]he choice of referees . . . is the single, most important decision that editors make in dealing with submitted manuscripts" (Bishop, 1984: 53). Therefore, the selection of referees should be done with great care. For reasons mentioned above, referees should not be asked to review manuscripts outside their areas of expertise and should accept the responsibility of notifying editors what their expertise does (and does not) cover. Further, referees should refuse invitations to review manuscripts that are beyond their ken. To allow authors input in this regard, *Organization Science* (http://web.gsm.uci.edu/orgsci/INFO.htm) is unique among management journals in encouraging authors to nominate up to four individuals to review their submissions. Finally, as Humphreys (2002) has noted, egalitarianism in selecting referees is to be encouraged, as long as it is given *secondary* status to training and research experience.

8. *Referees should not be trained "on the job."* As noted, publication success is a key consideration in tenure and promotion deliberations and, in turn, translates into a host of economic considerations. With so much at stake, potential referees should be provided with hands-on training prior to conducting an actual review. Such training should include stressing the importance of preserving authorial voice and underscoring that, although the interests of authors and referees are inherently at odds, peer review should not be seen as adversarial. Further, as envisioned by Evans and Woolridge (1987), referee training should also include seeing exemplary reviews and preparing and receiving feedback on mock reviews. They also suggest that such training should be a standard part of doctoral education, wherein students are required to prepare reviews of published works and to compare their reviews with those of other students. Miner (2003b) has likewise suggested holding peer-review workshops at professional meetings. Such workshops would target graduate students, as well as others wishing to hone their reviewing skills.

9. *Editors should not be trained "on the job."* One former editor has noted that journal editors are typically selected for their research skills, their political connections, or their professional visibility (Goodstein, 1982: 28). He suggests, however, as well intentioned as good researchers may be, it is erroneous to assume that they will necessarily be good editors. He adds that he is unaware of any journal in any discipline that "requires, expects, or even encourages" editors to seek the training necessary to fulfill their roles. This seems odd given an editor's indisputable role in assuring a journal's success and our discipline's focus on the importance of training for enhancing individual performance.

10. *A meaningful formal appeal procedure should be provided for authors who believe that a manuscript has been improperly reviewed*. This procedure should be under the direction of someone other than the editor to which a manuscript was originally submitted and be governed by uniformly applied and well-publicized procedures (Epstein, 1995). Having an independent third party function as an ombudsman would more evenly balance the power differential between authors and editors, thereby prompting editors to be even more cognizant of their obligations to the discipline and its individual members (Roth, 2002).

Lest one thinks that all rejected authors are wounded egotists incapable of being objective about their own work, research suggests otherwise. Of 74 authors who requested reconsideration of manuscripts rejected by the *American Sociological Review* over a 4-year period, 13% were judged to have valid complaints and succeeded in having their work accepted for publication (Simon, Bakanic, & McPhail, 1986).

CONCLUSION

In closing, my goal has been to bring more attention to the influence of the social component that shapes the content of the management discipline's published record and, in turn, determines its scientific progress. If in doing so I spark debate, research, and further ideas for improving the peer-review process, I will prize these outcomes. To paraphrase Winston Churchill's (1947) assessment of democracy, it has been often observed that, "[p]eer review is the worst form of scientific evaluation, except for all others that have been tried" (Roediger, 1987: 239). This, however, should not be taken to imply that the peer-review process cannot be improved. To this end, I have argued that the development of our discipline warrants a more sophisticated understanding of how knowledge-claims are socially constructed and validated. We each have an obligation, distributively and collectively, to advance this understanding and, thereby, further the spirit of scientific inquiry that brings us together as a community of scholars striving to further learning and education within the management discipline.

REFERENCES

Amabile, T. M., & Glazebrook, A. H. 1981. A negativity bias in interpersonal evaluation. ***Journal of Experimental Social Psychology,*** 18: 1–22.

Armstrong, J. S. 1982. Research on scientific journals: Implications for editors and authors. ***Journal of Forecasting,*** 1: 83–104.

Ashford, S. J. 1996. The publishing process: The struggle for meaning. In P. J. Frost & M. S. Taylor (Eds.), ***Rhythms of academic life: Personal accounts of careers in academia***: 119–127. Thousand Oaks, CA: Sage.

Astley, W. G. 1985. Administrative science as socially constructed truth. ***Administrative Science Quarterly,*** 30: 497–513.

Bailey, J. R., & Ford, C. M. 2003. Innovation and evolution: Managing tensions within and between the domains of theory and practice. In Larisa V. Shavinina (Ed.), ***International handbook on innovation:*** 248–257. Hillsdale, NJ: Elsevier.

Bakanic, V., McPhail, C., & Simon, R. J. 1987. The manuscript review and decision-making process. ***American Sociological Review,*** 52: 631–642.

Bauer, S. H. 1984, December 24. Ethics (or lack thereof) of refereeing. ***Chemical & Engineering News,*** 2: 33.

Bedeian, A. G. 1996a. Improving the journal review process: The question of ghostwriting. ***American Psychologist,*** 51: 1189.

Bedeian, A. G. 1996b. Thoughts on making and remaking the management discipline. ***Journal of Management Inquiry,*** 5: 311–318.

Bedeian, A. G. 1996c. Lessons I learned along the way. In P. J. Frost & M. S. Taylor (Eds.), ***Rhythms of academic life: Personal accounts of careers in academia:*** 3–9. Thousand Oaks, CA: Sage.

Bedeian, A. G. 1997. Of fiction and fraud. ***Academy of Management Review,*** 22: 840–842.

Bedeian, A. G. 2003. The manuscript review process: The proper roles of authors, referees, and editors. ***Journal of Management Inquiry,*** 12: 331–338.

Bedeian, A. G., & Feild, Jr., H. S. 1980. Academic stratification in graduate management programs: Departmental prestige and faculty hiring patterns. ***Journal of Management,*** 6: 99–115.

Biggart, N. W. 2000, Spring. From the chair. ***Organizations, Occupations, and Work***: 1–2. [Newsletter of the Organizations, Occupations, and Work Section, American Sociological Association.]

Bishop, C. T. 1984. ***How to edit a scientific journal.*** Philadelphia: ISI Press.

Brent, D. 1992. ***Reading as rhetorical invention.*** Urbana, IL: National Council of Teachers of English.

Burrell, G., & Morgan, G. 1979. ***Sociological paradigms and organizational analysis.*** London: Heinemann.

Campanario, J. M. 1996. Have referees rejected some of the most-cited articles of all times? ***Journal of the American Society for Information Science,*** 47: 302–310.

Chubin, D. E. 1985. Research malpractice. ***BioScience,*** 35(2): 80–89.

Chubin, D. E., & Hackett, E. J. 1990. ***Peerless science: Peer review and U.S. science policy.*** Albany, NY: State University of New York Press.

Churchill, W. S. 1947, November 11. Orders of the day. House of Commons, Hansard: 207. London: Her Majesty's Stationery Office.

Clark, W. C., & Majone, G. 1985. The critical appraisal of scientific inquiries with policy implications. ***Science, Technology, & Human Values,*** 10: 6–19.

Cole, J. R. 1989. The paradox of individual particularism and institutional universalism. ***Social Science Information,*** 28: 51–76.

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

Crane, D. 1967. The gatekeepers of science: Some factors affecting the selection of articles for scientific journals. ***American Sociologist,*** 2: 195–201.

Crawford, W. 2001a. Reading as a socially-constituted act. Retrieved February 25, 2003, from http://www.wiu.edu/users/mfwc/wiu/socialact.html

Crawford, W. 2001b. Teacher-reader as cultured role. Retrieved

February 25, 2003, from http://www.wiu.edu/users/mfwc/wiu/culturedrole.html

Derrida, J. 1982. ***Margins of philosophy*** (A. Bass, Trans.). Chicago, IL: University of Chicago Press. (Original work published 1972)

Duncan, W. J., Ford, E. W., Rousculp, M. D., & Ginter, P. M. 2002. Community of scholars: An exploratory study of management laureates. ***Scientometrics,*** 55: 395–409.

Eco, U. 1979. The poetics of the open work. In ***The role of the reader: Explorations in the semiotics of texts***: 47–66. Bloomington, IN: Indiana University Press. (Original work published 1959)

Eisenhart, M. 2002. The paradox of peer review: Admitting too much or allowing too little? ***Research in Science Education,*** 32: 241–255.

Ellison, G. 2002. The slowdown in the economics publishing process. ***Journal of Political Economy,*** 110: 947–993.

Epstein, S. 1995. What can be done to improve the journal review process. ***American Psychologist,*** 50: 883–885.

Evans, K., & Woolridge, B. 1987. Journal peer review: A comparison with employee peer performance appraisal. ***Journal of Social Behavior and Personality,*** 2: 385–396.

Eysenck, H. J. 1980. Editorial. ***Personality and Individual Differences,*** 1: 1–2.

Fine, M. A. 1996. Reflections on enhancing accountability in the peer review process. ***American Psychologist,*** 51: 1190–1191.

Fiske, D. W., & Fogg, L. 1990. But the reviewers are making different criticisms of my paper! ***American Psychologist,*** 45: 591–598.

Frey, B. S. 2002, June 6. Publishing as prostitution? Choosing between one's own ideas and academic failure. Zurich, Switzerland: University of Zurich, Institute for Empirical Research in Economics, Working Paper 117.

Frey, B. S. 2003. Publishing as prostitution?—Choosing between one's own ideas and academic success. ***Public Choice,*** 116: 205–223.

Gans, J. S., & Shepherd, G. B. 1994. How the mighty have fallen: Rejected classic articles by leading economists. ***Journal of Economic Perspectives,*** 8: 165–179.

Garst, J., Kerr, N. L., Harris, S. E., & Sheppard, L. A. 2002. Satisficing in hypothesis generation. ***American Journal of Psychology,*** 115: 475–500.

Geertz, C. 1983. ***Local knowledge: Further essays in interpretive anthropology.*** New York: Basic.

Gilbert, G. N. 1976. The transformation of research findings into scientific knowledge. ***Social Studies of Science,*** 6: 281–306.

Gilbert, G. N. 1977. Referencing as persuasion. ***Social Studies of Science,*** 7: 113–122.

Glenn, N. D. 1976. The journal article review process: Some proposals for change. ***American Sociologist,*** 11: 179–185.

Goodman, S. N., Berlin, J., Fletcher, S. W., & Fletcher, R. H. 1994, July 1. Manuscript quality before and after peer review and editing at the Annals of Internal Medicine. ***Annals of Internal Medicine,*** 121: 11–21.

Goodstein, L. D. 1982. When will the editors start to edit? In S. Harnad (Ed.), ***Peer commentary on peer review: A case study in scientific control***: 28–29. Cambridge, England: Cambridge University Press.

Grafton, A. 1997. ***The footnote: A curious history.*** Cambridge, MA: Harvard University Press.

Hargens, L. L., & Herting, J. R. 1990. A new approach to referees' assessments of manuscripts. ***Social Science Research,*** 19: 1–16.

Harnad, S. 1979, September. Creative disagreement. ***The Sciences,*** 19: 18–20.

Herxheimer, A. 1989. Make scientific journals more responsive—and responsible. ***The Scientist,*** 3(6): 9–11.

Higgins, E. T. 1992. Increasingly complex but less interesting articles: Scientific progress or regulatory problem? ***Personality and Social Psychology Bulletin,*** 18: 489–492.

Horrobin, D. F. 1982. Peer review: A philosophically faulty concept which is proving disastrous for science. In S. Harnad (Ed.), ***Peer commentary on peer review: A case study in scientific control:*** 33–34. Cambridge, England: Cambridge University Press.

Humphreys, L. G. 2002. Problems in individual differences research with peer review, some peer reviewers, and suggestions for reform. ***Multivariate Behavioral Research,*** 37: 282–295.

Hyland, K. 2000. ***Disciplinary discourses: Social interactions in academic writing.*** Harlow, UK: Longman.

Jacques, R. 1992. Critique and theory building: Producing knowledge "from the kitchen." ***Academy of Management Review,*** 17: 582–606.

Jasanoff, S. 1985. Peer review in the regulatory process. ***Science, Technology, & Human Values,*** 10: 20–32.

Kayes, D. C. 2002. Experiential learning and its critics: Preserving the role of experience in management learning and education. ***Academy of Management Learning and Education,*** 1(2): 137–149.

Kelly, G. J., & Bazerman, C. 2003. How students argue scientific claims: A rhetorical-semantic analysis. ***Applied Linguistics,*** 24: 28–55.

Kerr, N. L. 1998. HARKing: Hypothesizing after the results are known. ***Personality and Social Psychology Review,*** 2: 196–217.

Kinicki, A. J., & Prussia, G. 2000. From members of the editorial board. ***Academy of Management Journal,*** 43: 799–800.

Kruger, J., & Dunning, D. 1999. Unskilled and unaware of it: How difficulties in recognizing one's own incompetence lead to inflated self-assessments. ***Journal of Personality and Social Psychology,*** 77: 1121–1134.

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

Laband, D. N. 1990. Is there value-added from the review process in economics?: Preliminary evidence from authors. ***Quarterly Journal of Economics,*** 2: 341–352.

Langfeldt, L. 2001. The decision-making constraints and processes of grant peer review, and their effects on the review outcome. ***Social Studies of Science,*** 31: 820–841.

Langfeldt, L. 2002. ***Decision-making in expert panels evaluating research***. Oslo, Norway: Nork institutt for studier av forskning og utdanning.

Larochelle, M., & Désautels, J. 2002. On peers, those 'particular friends'. ***Research in Science Education,*** 32: 181–189.

Leblebici, H. 1996. The act of reviewing and being a reviewer. In P. J. Frost & M. S. Taylor (Eds.), ***Rhythms of academic life:***

Personal accounts of careers in academia: 269–274. Thousand Oaks, CA: Sage.

Locke, K., & Golden-Biddle, K. 1997. Constructing opportunities for contribution: Structuring intertextual coherence and "problematizing" in organizational studies. ***Academy of Management Journal,*** 40: 1023–1062.

Logan, C. A. 2002. When scientific knowledge becomes scientific discovery: The disappearance of classical conditioning before Pavlov. ***Journal of the History of the Behavioral Sciences,*** 38: 393–403.

MacCallum, R. C., Roznowski, M., & Necowitz, L. B. 1992. Model modifications in covariance structure analysis: The problem of capitalization on chance. ***Psychological Bulletin,*** 111: 490–504.

MacCoun, R. J. 1998. Biases in the interpretation and use of research results. ***Annual Review of Psychology,*** 49: 259–287.

Mahoney, M. J. 1978, February. Publish and perish. ***Human Behavior,*** 7: 38–42.

Mahoney, M. J. 1979. Psychology of the scientist: An evaluative review. ***Social Studies of Science,*** 9: 349–375.

Martinko, M. J., Campbell, C. R., & Douglas, S. C. 2000. Bias in the social science publication process: Are there exceptions? ***Journal of Social Behavior and Personality,*** 15: 1–18.

McNemar, Q. 1960. At random: Sense and nonsense. ***American Psychologist,*** 15: 295–300.

McPherson, J. M. 1976. Theory trimming. ***Social Science Research,*** 5: 95–105.

Merton, R. K. 1973. Priorities in scientific discovery. In N. Storer (Ed.), ***The sociology of science***: 267–278. Chicago, IL: University of Chicago Press. (Original work published 1957)

Merton, R. K., & Zuckerman, H. 1973. Institutionalized patterns of evaluation in science. In N. Storer (Ed.), ***The sociology of science***: 460–496. Chicago, IL: University of Chicago Press. (Original work published 1971)

Millman, J. 1982. Making the plausible implausible: A favorable review of Peters and Ceci's target article. In S. Harnad (Ed.), ***Peer commentary on peer review: A case study in scientific control***: 40–41. Cambridge, England: Cambridge University Press.

Miner, J. B. 2003a. The rated importance, scientific validity and practical usefulness of organizational behavior theories: A quantitative review. ***Academy of Management Learning and Education,*** 2(3): 250–268.

Miner, J. B. 2003b. Commentary on Arthur Bedeian's "The manuscript review process: The proper roles of authors, referees, and editors." ***Journal of Management Inquiry,*** 12: 339–343.

Mitroff, I. I., & Chubin, D. E. 1979. Peer review at the NSF: Dialectical policy analysis. ***Social Studies of Science,*** 9: 199–232.

Mone, M. A., & McKinley, W. 1993. The uniqueness value and its consequences for organization studies. ***Journal of Management Inquiry,*** 2: 284–296.

Moran, G. 1998. ***Silencing scientists and scholars in other fields: Power, paradigm controls, peer review, and scholarly communication.*** Greenwich, CN: Ablex.

Morgan, G. 1983. In research, as in conversation, we meet ourselves. In G. Morgan (Ed.), ***Beyond method: Strategies for social research***: 405–407. Beverly Hills, CA: Sage.

Myers, G. 1990. ***Writing biology: Texts in the social construction of scientific knowledge***. Madison, WI: University of Wisconsin Press.

Nash, F. 1996. Peer review and reproduction of knowledge. Retrieved February 10, 2003, from http://www.psa.ac.uk/Publications/psd/1996/nash3.htm

Neck, C. P. 2001, August 5, 4, 3, 2, 1 . . . The horn has sounded, but the game is not over. In A. G. Bedeian & C. P. Neck (Chairs), ***Has the editorial review process gone awry? Lacunae, emphases, and surfeits.*** Symposium conducted at the sixty-first annual meeting of the Academy of Management, Washington, DC.

Northcraft, G. B. 2001. From the editors. ***Academy of Management Journal,*** 44: 1079–1080.

Patterson, S. C. 1994. The itch to publish in political science. In R. J. Simon & J. J. Fyfe (Eds.), ***Editors as gatekeepers: Getting published in the social sciences***: 3–19. Lanham, MD: Rowan & Littlefield.

Perlman, D., & Dean, E. 1987. The wisdom of Solomon: Avoiding bias in the publication review process. In D. N. Jackson & J. P. Rushton (Eds.), ***Scientific excellence: Origins and assessment***: 204–221. Newbury Park, NJ: Sage.

Peterson, R. A. 1975. Too many manuscripts? ***American Sociologist,*** 10: 54.

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

Romanelli, E. 1996. Becoming a reviewer: Lessons somewhat painfully learned. In P. J. Frost & M. S. Taylor (Eds.), ***Rhythms of academic life: Personal accounts of careers in academia:*** 263–268. Thousand Oaks, CA: Sage.

Rosenblatt, L. 1993. The transactional theory: Against dualisms. ***College English,*** 55: 377–386.

Roth, W.-M. 2002. Editorial power/authorial suffering. ***Research in Science Education,*** 32: 215–240.

Schminke, M. 2002. From the editors: Tensions. ***Academy of Management Journal,*** 45: 487–490.

Schwartzbaum, A. 1976. Letter. ***American Sociologist,*** 11: 152.

Siegelman, L., & Whicker, M. L. 1987. Some implications of bias in peer review: A stimulation-based analysis. ***Social Science Quarterly,*** 68: 494–509.

Simon, R. J., Bakanic, V., & McPhail, C. 1986. Who complains to journal editors and what happens? ***Sociological Inquiry,*** 56: 259–271.

Sismondo, S. 1993. Some social constructions. ***Social Studies of Science,*** 23: 515–553.

Smith, B. H. 1988. Contingencies of value: Alternative perspectives for critical theory. Cambridge, MA: Harvard University Press.

Smith, L. Z. 1998. Anonymous review and the boundaries of 'intrinsic merit'. ***Journal of Information Ethics,*** 7: 54–67.

Spector, P. E. 1998, Fall. When reviewers become authors: A comment on the journal review process. ***Research Methods Forum***: 1–4. [A publication of the Research Methods Division, Academy of Management.] Retrieved March 2, 2003, from http://www.aom.pace.edu/rmd/1998_forum_reviewers_become_authors.html

Starbuck, W. H. 1994. On behalf of naïveté. In J. A. C. Baum & J. V.

Singh (Ed.), ***Evolutionary dynamics of organizations***: 205–220. New York, NY: Oxford University Press.

Starbuck, W. H. 2003a. Turning lemons into lemonade: Where is the value in peer review? ***Journal of Management Inquiry,*** 12: 344–351.

Starbuck, W. H. 2003b. ***How much better are the most prestigious journals? The statistics of academic publication***. Unpublished manuscript, New York University.

Street, M. D., Bozeman, D. P., & Whitfield, J. M. 1998. Author perceptions of positive and negative editor behaviors in the manuscript review process. ***Journal of Social Behavior and Personality,*** 13: 1–22.

Sutton, R. I., & Staw, B. M. 1995. What theory is ***not. Administrative Science Quarterly,*** 40: 371–384.

Travis, G. D. L., & Collins, H. M. 1991. New light on old boys: Cognitive and institutional particularism in the peer review system. ***Science, Technology, & Human Values,*** 16: 322–341.

Van Lange, P. A. M. 1999. Why authors believe that reviewers stress limiting aspects of manuscripts: The SLAM effect in peer review. ***Journal of Applied Social Psychology,*** 29: 2550–2566.

Warren, L. 2003. Galileo didn't publish his observations in scholarly journals. ***National Geographics,*** 203(5): 15.

Warren, M. G. 2000. Reading reviews, suffering rejection, and advocating for your paper. In R. J. Sternberg (Ed.), ***Guide to publishing in psychology journals***: 169–186. Cambridge, England: Cambridge University Press.

Weller, A. C. 2001. ***Editorial peer review: Its strengths and weaknesses.*** Medford, NJ: Information Today.

Whitehurst, G. J. 1984. Interrater agreement for journal manuscript reviews. ***American Psychologist,*** 39: 22–28.

Wiley, M. G., Crittenden, K. S., & Birg, L. D. 1979. Why a rejection? Causal attribution of a career achievement event. ***Social Psychology Quarterly,*** 42: 214–222.

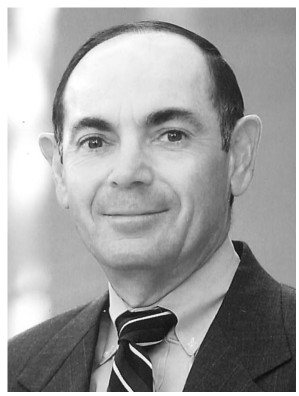
Wilkes, J. M. 1994. Characterizing niches and strata in science by tracing differences in cognitive styles distribution. In W. R. Shadish & S. Fuller, (Eds.), ***The social psychology of science***: 300–315. New York, NY: Guilford.

Yalow, R. S. 1982. Competency testing for reviewers and editors. In S. Harnad (Ed.), ***Peer commentary on peer review: A case study in scientific control***: 60–61. Cambridge, England: Cambridge University Press.

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

Zerby, C. 2002. ***The devil's details: A history of footnotes.*** Montpelier, VT: Invisible Cities.

Ziman, J. M. 1984. ***An introduction to science studies: The philosophical and social aspects of science and technology.*** Cambridge, England: Cambridge University Press.



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