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Balancing Authorial Voice and Editorial Omniscience: The "It's My Paper and I'll Say What I Want To" versus "Ghostwriters in the Sky" Minuet

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As its title indicates, the purpose of the present volume is to "open the black box of editorship." My concerns about the integrity of the manuscript-review process as practiced by the management discipline's leading journals are well documented. These concerns, as they relate to the review process as a means for judging the quality and, thus, the credibility of scientific papers submitted for publication have addressed the social construction of knowledge (Bedeian, 2004); the proper roles of editors, referees, and authors (Bedeian, 2003); and ghostwriting by editors and referees (Bedeian, 1996a & b). In the remarks that follow, I will briefly summarize a few of these concerns and extend my previous thoughts by commenting on reservations I have about how the review process has evolved over the past fifteen or so years and how it may be improved.

Self-management and peer review

One aspect of the manuscript-review process that has always struck me as unique is the degree of self-management that exists within academia. This self-management is, perhaps, no more evident than in the matters of tenure and the peer review of scientific manuscripts submitted for publication. As Biagioli (2002) notes, peer review (in particular) "sets academia apart from all other professions by constructing value through peer judgment, not market dynamics" (p. 11). Whereas most professions are subject to government regulations, certifications, and even audits, we in academia are, for the most part, exempt from such constraints. Rather, through peer review, we supposedly regulate ourselves by engaging in "a series of rational judgments and decisions" (p. 35). In effect, by construing value based on peer judgments and not market dynamics, we have elevated peer review, as a quality-control mechanism, to a special status.

As a consequence, much like the depersonalized economic marketplace, the scientific marketplace of ideas, as enshrined in peer review, is portrayed as a well-behaved and disciplined entity that ensures the public of good science. As every academic doubtless knows, the actual reality of peer review is starkly different. Indeed, the ideal image of peer review as an objective arbiter of scientific merit is, as argued by Biagioli (2002) and others, little more than ritualized fiction for gaining public confidence and, thus, guarding the autonomy and authority we enjoy as academics (Bedeian, 1997).

Ghostwriting

My initial concern with the reality of the manuscript-review process, and how it has evolved in recent years emanated, from my own experience as an author. In revising and resubmitting a manuscript for a special issue of one of our discipline's premier journals, I encountered a guest editor who strongly felt that I should incorporate material suggested by a referee, but with which I philosophically disagreed. In two exchanges, I expressed discomfort at having the material appear under my byline and (before the notion of "ghostwriting" had occurred to me) indicated that I felt I was caught in a situation of "reverse censorship," in that, I was being told that if I wished to have my manuscript published, I would have to include material which I found offensive. The guest editor responded with a fury, upset with the idea of being associated with any kind of censorship – reverse or otherwise. Not surprisingly, the revised manuscript (after two rounds of reviews) was summarily rejected.

Rejection is, of course, a common experience in academic writing. What I find disturbing (among other things) is that it has been estimated that one-third of the authors who have a manuscript rejected not only abandon the manuscript, but "the entire line of research on which it was based" (Belcher, 2006b, p. 1). This is unfortunate because data indicate that many Nobel Prize winning authors have had their award-winning work initially rejected (Shepherd, 1995) and that, on average, over half the manuscripts initially rejected are ultimately published elsewhere (Weller, 2001, p. 64). This especially seems to be true for manuscripts reporting creative and unorthodox research (Frey, 2003). For such manuscripts, referee recommendations are evidently of limited value in judging the merits of unconventional knowledge-claims.

In this connection, whereas the most prestigious journals may receive more "good" manuscripts than they can publish, their ability to select from the "best of the best" is, regrettably, suspect. Miner (2003) 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" (p. 341). In support of Miner's contention, Starbuck (2005) has shown that articles published in top-tier journals do not necessarily exhibit significantly higher quality (as measured by the average number of citations they receive) than articles published in second-tier and third-tier journals.

The case of my rejected manuscript, however, ends well. I am one of the two out of three authors that studies suggest do not give up on manuscripts easily. Without hesitation, I submitted my original manuscript – saying what I wanted to say and not what the guest editor and anonymous referee wanted me to say – to another journal where it was accepted and, ultimately, selected to receive a Best Paper of the Year Award. To be honest, despite an invitation to attend an award banquet in London and being presented with an attractive plaque, I would still have opted to have had my manuscript published in the journal that was my first choice.

Rejecting rejection and crossing the line

My experience highlights the vagaries of the review process and what some have come to call the "luck of the reviewer draw" (Bedeian, 2004). This situation is captured in one aspiring author's observation that the best career advice she ever got came at a seminar on publishing. She was told, when she was ready to submit a manuscript for review, to prepare three envelopes addressed to three different journals. "Send it to the first – if it gets rejected, then send it to the second. If it gets rejected again, then send it to the third." The point being that "the process is so subjective that you need to give your work the benefit of the doubt a few times before pulling the plug on it" (Belcher, 2006a, p. 2).

The painful validity of this anecdote is familiar to all those who have revised a manuscript as requested only to have it rejected and then, abandoning the revised manuscript, submitted their original manuscript for publication elsewhere and – lo and behold – had it accepted. A colleague and I still joke about one of his manuscripts that had gone through three rounds of reviews at the *Academy of Management Journal (AMJ)* only to be bounced. Frustrated, he submitted his original manuscript to the *Journal of Applied Psychology*, where it was accepted after one round of reviews.

I can go this one better, however. Several colleagues and I submitted a manuscript to an *Academy of Management* – sponsored journal and received an "invitation" to resubmit the manuscript based on a lengthy set of revisions. We dutifully made the revisions and the manuscript was ultimately accepted after two more rounds of referee comments. To our surprise, the manuscript received the journal's annual Best Paper Award. The editor seemed quite pleased that the "developmental review process" had resulted in such a fine paper. In effect, what had happened was that the editor essentially

passed judgment on a manuscript that he had ghostwritten and (surprise, surprise) judged to be superior. In short, he liked that which he had created. In essence he had served as author and reviewer of his own work. Should you wonder if I am exaggerating, the initial manuscript we submitted was so different than the manuscript that was accepted we went on to submit it to another Academy journal and it has since been published. Somewhere, at some point, the line between reviewing and ghostwriting had been crossed.

These examples aside, let me be clear in stating that I do not endorse simply repackaging and resubmitting rejected manuscripts without carefully considering referee comments. Whatever the reason for a manuscript's rejection, the benefits of outside feedback for improving a manuscript should not be minimized. In this respect, I agree with Starbuck's (2003) contention that referees' comments should not be viewed as judgments about the value of one's work, but as data about potential readers' reactions to what an author is trying to say. This, however, does not mean that an author should always follow a referee's or editor's bidding before submitting a manuscript elsewhere.

I stress this point because I recall attending a seminar on publishing where an associate editor of a journal, which is published by one of our discipline's primary professional associations, stated that he felt it was "unethical" for an author to resubmit a rejected manuscript without first incorporating the revisions of the rejecting journal's referees. It seems ludicrous to me that anyone would argue that authors are acting unethically if they chose not to revise their work based on the comments of an anonymous referee of unknown pedigree. This is especially true when, by one estimate, 25 percent of editors' and referees' comments "might be wrong, overstated, or off point" (Feldman, 2005, p. 654). Add to this the fact that whereas "peer review" should mean that the merits of a manuscript are assessed by a scientific "peer" working in the same field of research as its author, evidence suggests that this may not be the case when referees are selected on the basis of particularistic criteria rather than their scientific achievements (Bedeian, Van Fleet, & Hyman, 2007).

Faux rigor and playing the game

All this strikes me as an example of what might be termed "faux rigor." There seems to be a belief that having three, four, or more referees submit multiple pages of comments is proof that our journals are truly top-tier. What appears to have been overlooked is that "the referees commissioned to read a manuscript may represent, but may not be representative of, an entire discipline" (Bedeian, 2004). Admittedly, we are handicapped as a discipline in that, without a generally accepted criterion for scientific quality, decisions by referees will always be, to some degree, subjective. As a result, in an effort to nevertheless engender faith in publication decisions, we seem to have

focused on the extent to which consensus exists across referees as an indication that the review process is a valid indicator of scientific quality. This has always struck me as odd, as we regularly teach in our courses that high reliability (i.e., consensus across referees) is no guarantee of validity. As Daniel (1993) offers, "A high level of agreement between reviewers in itself proves very little, since two reviewers might reach equally erroneous conclusions" (p. 6).

Having said this, however, I do not wish to convey the notion that I have lost faith in the review process, as I do sincerely believe that editors and referees play an invaluable role in saving authors from embarrassing errors. Nonetheless, I also believe that we should be honest in acknowledging that the manuscripts which ultimately appear in our journals are often no less a reflection of the interests of the referees selected to serve as reviewers as the intentions of the authors themselves. It bothers me that publishing, at times, seems to have been turned into a game in which some referees try to find things to object to in a manuscript just to convince an editor that they have done a conscientious job in preparing their review and, in turn, authors admit to having included a specific reference in a manuscript primarily because they hoped that its author would be selected as a referee (Bedeian, 2004).

Further, I continue to wonder if the manuscripts ultimately accepted as a result of the review process make any greater contributions to advancing knowledge than the manuscripts as originally submitted. Additionally, I firmly believe that had these manuscripts been reviewed by sets of different referees (according to their own subjective perspectives) their final content would have been different and, perhaps, even rejected. Whether or not this differing content would have made a greater contribution to knowledge than the content in the original manuscripts remains an open question.

Given my rejected manuscript story, it may come as a surprise to some that I do not encourage arguing with editors and referees and agree with Dov Eden (this volume) only to do so if an issue is of "prime importance" and would otherwise be "intellectually dishonest." I also, however, agree with Dov that authors should not be "obsequious." Like Dov, I have learned from experience not to argue with editors and referees over "little things," such as editors and referees who insist that their own work be cited in a manuscript, regardless of how tangential the connection. I have found, however, that one can disagree with editors and referees without arguing. What I find works in those situations where I am directed to make alternations that I believe to be uninformed is to simply explain my understanding based upon my reading of various sources and then request that the editor or referee advise me about how I may have misunderstood the sources or how the sources are incorrect. If the editor and referee wish to argue with Cohen, Cohen, West, & Aiken (2003) or Pedhazur & Schmelkin (1991) or whomever, let them. Perhaps they do know more than these authorities and,

if so, I will have learned something. Nonetheless, I admit to often (not always) agreeing with Nobel Laureate Paul Samuelson, who in addressing referee comments, has confessed that "in my heart of hearts I question that, *net*, they have improved the merits of my papers' contents or expositions" (quoted in Shepherd, 1995, p. 125). Interested readers should know that not everyone is so reticent about arguing with editors and, indeed, have developed quite successful careers doing so, going as far as resubmitting rejected manuscripts to the same journal and ultimately having them accepted (Starbuck, 2006, pp. xi-xv).

What gives me pause, though, are situations in which fledgling authors feel pressured to have their work published (to avoid jeopardizing their careers) such that they do compromise on more than the "little things." The extent of the pressure felt in this regard is suggested by a study of 173 lead authors of articles published in the *AMJ* and the *Academy of Management Review* from 1999 to 2001 (Bedeian, 2003). Nearly 25 percent of the authors reported that to placate a referee or editor they had actually made changes in their manuscripts that they (as authors) felt were incorrect. It seems to me that in a Six-Sigma era, we can do better as a profession than having one out of four of the manuscripts published in our discipline's premier journals being flawed (at least in the eyes of their biddable authors).

The best of all worlds

In the best of all worlds, the review process would function in a manner consistent with its ideal image. Editors and referees would enable and amplify rather than sometimes stifle and even replace an author's voice (Beebe, 2006). In such a world, editors would "request" revisions no more than necessary and, all the while, be sensitive to the prerogatives and ethics of legitimate authorship. In this respect, it struck me as odd that a recent editor's forum in the *AMJ* (see Bergh, this volume) seemed to take pride in the fact that the paper selected as the journal's Best Article for 2004 required a full 24 months between initial submission and final acceptance. A third of this time was required for various reviews and 16 months were needed to satisfy referee requests for revision. Allowing, perhaps, for a 12-month lag before publication, one has to wonder about the timeliness of our discipline's research, let alone its prospects for reporting dramatic scientific breakthroughs that will change the world.

In thinking about this situation further, I was reminded of Dick Daft's (1983) comments on the "machine-gun fire of referee criticisms" (p. 544) and also wondered how many within our discipline have simply withdrawn from "the game" as a result of battle fatigue. I have likewise wondered what cost our discipline has incurred in lost knowledge and disenfranchised colleagues who have simply dropped out of the publication process altogether. Is it necessary to spend 24 months to revise a manuscript? Can it be justified in the sense of equating marginal costs with marginal benefits? As Ellison

(2002) has noted, "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 entire discipline, as well as how enjoyable it is to be an academic. Finally, I also wonder to what extent the review process as currently practiced distorts the true record of authors' contributions to our discipline. How are readers to know if they are responding to an author's own words and ideas or those of an unidentified editor or referee, both of whom will escape responsibility for what is attributed to the author?

Beyond these considerations, an additional question looms for our junior colleagues striving to earn tenure. Can 24 months be seen as a viable time-frame for submitting and ultimately having a manuscript (award winning or not) accepted for publication when their tenure clock is ticking down? I've previously argued in favor of re-evaluating our tenure and promotion system so as to reward faculty for doing a few pieces of high-quality research rather than grinding out multiple publications and simply playing a numbers game (Bedeian, 1989). The traditional model, however, with its emphasis on number of publications, still prevails (De Rond & Miller, 2005). As I have stated, it seems to me that we have straightjacketed our junior faculty at the most crucial, formative stage of their careers and that the editorial-review process, with its bias toward established paradigms, not only discourages creative and unorthodox research, but disadvantages those wishing to enter our profession (Bedeian, 1989).

Others have come to share my concern. Acknowledging "the relatively short tenure clock and strong emphasis to publish in 'A' journals in today's business school world," Nifadkar and Tsui (2007) have likewise bemoaned the fact that the contemporary-review process not only discourages the "fainthearted or thin-skinned," but suppresses the "intellectual potential" of our discipline. In this vein, they quote Freeman (2005) to the effect that "overemphasis on reviews, reviewers, revisions, and the socialization of the paper-writing process can lead to a kind of collective group think" (p. 433) that they see as "detrimental to creativity and originality." Echoing sentiments that I have expressed here and elsewhere, Nifadkar and Tsui (2007) similarly conclude that the review process, as currently practiced, has become a barrier to scientific progress in our discipline, stifling intellectual boldness. In this respect, we are at one in the belief that "the community of scholars has a responsibility to ensure that [our discipline's] intellectual environment facilitates rather than inhibits creative scientific activities" (p. 302). Simply put, the future development of our discipline is otherwise at risk.

Conclusion

The scientific marketplace of ideas is unique in the extent to which it is self-managed. In this regard, the manuscript-review process is central to gaining

public confidence and, thus, guarding the autonomy and authority we enjoy as academics. It is important that we remain open to examining the "black box of editorship" to ensure that it remains a reliable and valid means for assessing the content of our discipline's published record. To do otherwise would have chilling implications for our scientific progress and be an affront to the prescriptive norms that guide our common pursuit of new knowledge.

In summary, I offer the following conclusions for consideration by authors, editors, referees, and the academy in general:

- 1 Aspiring authors should not give up on manuscripts easily as most manuscripts are ultimately published.
- 2 Editors should be especially cognizant of biases favoring established theories and against research reporting creative and unorthodox findings.
- 3 Although the referees commissioned to read a manuscript may represent, but may not be representative of, an entire discipline, the benefits to authors of outside feedback for improving a manuscript should not be minimized.
- 4 Authors should not necessarily feel compelled to always follow a referee's or editor's bidding in revising a manuscript or before submitting a rejected manuscript elsewhere.
- 5 The manuscripts that ultimately appear in our journals are often no less a reflection of the interests of the referees consigned to serve as reviewers as the intentions of the authors themselves.
- 6 Had a published article been reviewed by different referees (according to their own subjective perspectives) its final content would have been different.
- 7 Deans and tenure and promotion committees should recognize that articles published in so-called top-tier journals do not necessarily exhibit significantly higher quality than articles published in second-tier and third-tier journals.
- 8 To preserve the prerogatives and ethics of legitimate authorship, editors and referees should enable and amplify rather than stifle an author's voice, requesting revisions that are no more than necessary.

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Part III Editing Different Types of Journals

Opening the Black Box of Editorship

Edited by

Yehuda Baruch

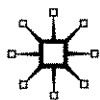
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