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“Circle the Wagons and Defend the Faith”

Slicing and Dicing the Data

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Commentators expressed contrasting views on the authors' examination of scientific achievement and editorial board membership in the management discipline. In response, the authors address key points with which they disagree and hold fast to their admonitory conclusions, neither compromising nor retreating from the recounting of base facts. If the authors' conclusions have prompted a measure of cognitive dissonance, they hope that any associated discomfort will lead to action on the part of all of the discipline's journals and their sponsoring organizations.

Keywords: *philosophy of science; scientific ethos; infometrics; bibliometrics; scientometrics*

We are pleased to have the opportunity to respond to the thoughtful comments of Hitt (2009), Klimoski (2009), and Tsui and Hollenbeck (2009) regarding our article on scientific achievement and editorial board membership (Bedeian, Van Fleet, & Hyman, 2009). We have long admired their work, and we value their perspectives on the results we report. In that they have noted many points with which we agree, we focus instead on several key points with which we disagree. It has been lamented that such dialogue seldom appears in the management literature (Bedeian, in press). As a form of academic discourse, these types of exchanges can be helpful in developing new insights as well as providing a means for authors to further develop and, if necessary, correct misleading or erroneous knowledge claims that have entered the scientific literature. In this respect, we view self-critique as the ultimate responsibility of a scientific community.

As stated in our article, our primary aim was

to examine the scholarly records of the editorial board members of two premier management journals published by the discipline's principal professional association, using the achievements

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of board members from leading journals sponsored by the primary professional associations of six other disciplines as a comparative backdrop. (p. 211)

It remains our contention that editorial board members play a critical role in authenticating knowledge claims, setting a discipline's expected standards of scientific rigor, and determining the advancement of individual scholars within the academic stratification system. Accordingly, we further contend that "the first and most obvious function of editorial peer review is to assure that decisions regarding publication are made by those most qualified to assess the quality and appropriateness of a submitted article" (Michels, 1995, p. 218). We see our position in this regard as generally accepted, in that the peer-review process and, by extension, the choice of individuals to serve as editorial board members is widely viewed as the very heart of science (Kochen, 1978; Ziman, 1978). More succinctly stated, "Science rests on peer review" (Daniel, Mittag, & Bornmann, 2007, p. 71).

We are not alone in our sentiments, as others have likewise maintained that "reviewer selection is one of the most important aspects of the peer-review process, arguably the most critical" (Hames, 2007, p. 43). Specifically addressing editorial board appointments, Starbuck, Aguinis, Konrad, and Baruch (in press) state that an editor's most important activity is "deciding who should be on the editorial board and which people should review each manuscript." Going further, however, it is our belief that the integrity of the peer-review process rests on the selection of editorial board members with the demonstrated competence to uphold scholarly standards and, in the words of one observer, separate "the academic wheat from the chaff" (Akst, 2008, p. W13). As we note in our article, the notion that individuals who serve as editorial board members should be individuals with recognized expertise within their disciplines is a cornerstone of the scientific ethos (Merton & Zuckerman, 1971/1973). Consistent with recognized norms of science, individuals selected to vet a manuscript should be true peers to its author in the sense of being equally versed in the field of research in which they are being asked to review. For this reason, we maintain that the merits of a manuscript should be assessed by scientific "peers" working in the same field of research as its author and that those peers be selected based on their scholarly abilities, as demonstrated by publications in peer-reviewed journals and evidence that their work is of value to others. In this connection, scientific norms dictate that those serving in editorial board roles should have demonstrated through their own peer-reviewed publications the competency required to understand the complexities of the work to be judged so as to make informed recommendations regarding which manuscripts should be accepted and rejected. As we also note in our article, unless editorial board members are appointed on the basis of their scientific achievements, the academic community may find it difficult to view their authority as legitimate.

The authors of the three commentaries commissioned to comment on our article seem to agree with our position that knowledge claims submitted for peer review should be evaluated by those competent to make an informed judgment. They take exception, however, with our results suggesting the possibility that at least some of the appointments to the *Academy of Management Journal* (AMJ) editorial board (for the period of our study) may have been based on particularistic or ascriptive criteria rather than merit. We find this puzzling in that, in taking exception to our results vis-à-vis AMJ, they offer no comments about the positive results we report for the *Academy of Management Review* (AMR)

editorial board and only a few comments about the comparative results of the other six journals included in our study.

The Hitt Commentary

Hitt's (2009) commentary is organized around four concerns. We comment on each in turn. He first expresses concern with regard to the systematic sampling procedure we used for randomly selecting editorial board members for four of the eight journals included in our analyses. As we state in our article, in each instance the one third sampling fraction we employed was sufficient to ensure an accurate estimate of population means for number of authored or coauthored articles within plus or minus 2 units with 90% confidence. Hitt nonetheless contends it would have been better to use the universe of all editorial board members for each of the journals included in our study. He bases this contention on two points. First, he asserts that the four journals for which we used a sampling procedure received the most negative assessments. This is simply not the case. Inspection of Tables 2 and 4 in our article, in particular, belies this claim. Specifically focusing on *AMR*, a journal for which the aforementioned sampling procedure was employed, the reader can clearly see that *AMR* received the highest Corrected Quality Index score and the second highest *h*-index value. Hitt's second point rests on an ad hoc analysis he conducted of the number of senior versus junior members serving on the *AMJ* editorial board. In drawing a one third random sample of *AMJ* editorial board members and comparing it against the remaining two third nonsampled members, he found "a difference in the number of senior versus more junior members in the two samples" (p. 254). Hitt does not report the direction of the difference or whether the difference was statistically significant or possibly due to random sampling variation. More obviously, the proper comparison for establishing the representativeness of the one third sample is a comparison of the one third sample to the total population of board members and not the residual nonsampled board members. The assumption that responses or characteristics of sampled units are "representative" of the responses or characteristics of nonsampled population units is problematic at best, as the latter do not represent a specific probability sample (McDonald, 2003).

Hitt's (2009) second concern relates to the criteria that should be used to select editorial review board members. In this connection, he makes three points. First, the members of the *AMJ* editorial board have an average of more than 21 publications, and this "represents an effective number of publications for service on an editorial review board" (p. 254). The actual average, as reported by Rynes (2006), is 21.8 publications. It takes, however, two parameters to define a frequency distribution: a mean *and* a standard deviation. What Hitt does not report is that the standard deviation around the above-cited mean is 18.3. Thus, assuming *arguendo* that the frequency of the number of articles published by *AMJ* editorial board member is normally distributed, this indicates that 15.87% of the board has averaged 40.1 or more articles ($21.8 + 18.3$) but that 15.87% has averaged 3.5 or less publications ($21.8 - 18.3$). Thus, with 111 *AMJ* editorial board members, this further suggests that some 18 ($111 \times 15.87\% = 17.62$) board members have averaged fewer than 4 publications listed in the Web of Science (the source of the article counts). Of course, as our results indicated, the number of publications per *AMJ* editorial board

member is not normally distributed but is positively skewed. This then suggests that more than 15.87% of the *AMJ* editorial boards have fewer than 4 publications to their credit. Although the mean ($M = 21.80$) and standard deviation ($SD = 18.30$) reported in Rynes (2006) are based on more recent publication data than those used in our analyses, they are quite similar to the results we found when we reanalyzed our data using total counting rather than fractional counting for multiauthored articles ($M = 18.38$, $SD = 17.56$), further attesting to the representativeness of our sample-selection procedure. Hitt warns that “we must be careful in the selection of review board members to ensure not only depth but breadth, of knowledge and experience” (p. 255) Can board members have breadth or depth, however, if they have published as few as two articles? Does such a record merit editorial board membership? Note, too, that the above descriptive statistics are inflated because of the “multiplier effect” resulting from collaborative efforts. The analyses in our original article proportionally assign credit for multiauthored articles, as total counting (vs. fractional counting) essentially equates coauthorship with sole authorship. Moreover, by simply focusing on board member productivity, Hitt ignores quality of production, which typically would be considered a separate and more important issue in assessing a board member’s scientific achievements. We capture both performance dimensions (production and citations) by using Lindsey’s (1978) established CQI index.

Finally, Hitt (2009) also cites Rynes (2006) to the effect that, for *AMJ* board members, although “professional age” (experience) is not significantly related to review quality ($r = -.14$, *ns*), number of publications is negatively and significantly related to review quality ($r = -.21$).¹ In contemplating these relationships, a number of issues present themselves, the most obvious of which are who is judging quality and how. Although professional age and number of publications are objective measures, the assessment of quality is not. It may largely depend on one’s perspective (e.g., author or editor). Moreover, as we note in our article, editors simply cannot be experts in all areas, and, as scientific areas expand and become more specialized, editors are no doubt challenged in even knowing properly qualified referees in all research fields. We wonder whether some editors do not go into a default mode and judge a review’s quality by its length and degree of criticism when they are unqualified to review a paper in a subfield beyond their own expertise. As Amabile (1983) has shown, negative reviews are generally perceived as more competent than positive reviews. In addition, 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 (stressing the limiting aspects of manuscripts), wherein they fall prey to a negativity bias (Amabile & Glazebrook, 1981). This suggests that relying on referees of little or no academic standing can lend a decidedly negative bias to the review process. The inability of editors to render qualified judgments of reviews on papers outside their areas of expertise and the tendency of inexperienced reviewers to manifest a negativity bias may at least partially explain why Weller (2001) concluded that findings are “mixed for whether younger or more experienced reviewers produced a better review” (p. 313). In this respect, we would also note that Gordon (1980) suggested that in contrast to more eminent referees, younger referees are seen as being too concerned with detail and less likely to understand the “purpose and significance” of a study.

Whatever the case, we believe it is important not to commit the fallacy of assuming that experience does not matter. Although experienced and well-published scholars do

sometimes submit poor-quality reviews, this does not mean that experience and publication success are unimportant or that editorial review boards should, therefore, be composed of newly fledged PhDs of little or no academic standing. Indeed, the Academy of Management's (2006) own Journals Governance Task Force Report seems to recognize the fallacy in such logic, referring to editorial board members as "experienced scholars with strong . . . publishing experience" (p. 2). As Hitt's own data suggest, however, simply saying that this is so does not make it true.

Our sentiments regarding experience and publication as important for editorial board membership struck a responsive chord in one of the anonymous referees of our article—someone we are told who has served on four separate *AMJ* boards. In his or her review, he or she offered the following anecdote. In prefacing the anecdote, he or she commented,

More than any other journal I have served on, and I have done them all, *AMJ* places much more weight on reviewer timeliness and length, than any other journal I have ever seen. If your reviews are long and fast, then you are eventually placed on the board.

We believe the anecdote underscores our concerns about relying on referees of little or no academic standing and the motivation of neophyte referees to impress an editor so as to win an invitation to join the editorial team. We quote directly, using joint pronouns to mask the identity of the individuals involved:

One of our third year Ph.D. students met the editor of *AMJ* several years ago at a doctoral student consortium and apparently impressed him or her. He or she sent him or her a paper. He or she immediately wrote a ten page review that took him or her close to a week to write—a long review that happened fast. He or she got another one in two weeks. Ditto. Then again. After three or four of these he or she was elevated to the board. I am not making this up. He or she had zero publications and zero citations, and barely passed comps, but he or she was on the *AMJ* board. He or she was making accept and reject decisions on papers submitted by [an eminent management scholar]! How is that for peer review? He or she still serves on the board ["X"] years later and still has zero publications and zero citations. He or she is obviously shaping what gets published and in what form, but what possible legitimate or expert authority does he or she really possess?

Some readers may be content to continue to declare that such editorial board appointments do not affect the credibility of our discipline in the eyes of "key stakeholders" (see Klimoski anon). We are not. Moreover, we are concerned about the resulting damage of such editorial board appointments to public trust in our discipline's research. As we note in our article, at a time when our discipline is aspiring to "matter more" and influence public policy makers to use our research, its scientific claims must be beyond reproach. Such claims, however, will only be accepted as authoritative if they are recognized as having been appraised and authenticated by qualified referees. We more fully address this concern below.

In a final comment on selection of editorial review board members, Hitt (2009) concludes that "multiple criteria should be used for selecting editorial board members rather than focusing on one criterion above others" (p. 255). As we repeatedly state in our article, we likewise contend that editorial appointments should not be made solely based on number of publications and associated citations. Rather, as we explicitly acknowledge,

“referees must be objective, free from conflicts of interest, and able to prepare a timely critique that is helpful to both an editor and a manuscript’s authors.” We do, however, continue to maintain that

those serving in editorial board roles should have demonstrated through their own peer-reviewed publications the competency required to understand the complexities of the work to be judged so as to make informed recommendations regarding which manuscripts are accepted or rejected. (p. 214)

In this sense, as we also state in our article, scholarly credentials are a necessary but not sufficient basis for editorial board membership.

A third concern in Hitt’s 2009 commentary is that we do not “adequately account for the editor’s role in the review and decision process” (p. 255). We generally agree with Hitt’s comments, as they essentially mirror what we say in our article. For the record, however, we would note that Hitt incorrectly attributes the speculation that “editors of journals with greater numbers of submissions and greater breadth of topics . . . need to rely on a ‘vote-counting’ process more often than journals with fewer submissions and higher levels of specialization” (p. 227) to us rather than the action editor (Herman Aguinis) handling the current exchange. Hitt indicates that when serving as *AMJ* editor, although he placed “great weight on reviewers’ input” he did not simply “count votes” but read every submission in an effort to make a reasoned decision on its final disposition. This practice further underscores the importance of not only selecting qualified referees whose judgment can be trusted but also appointing editors who are themselves “experienced scholars with strong . . . publishing experience.” Moreover, it also highlights the importance of selecting editors who are generally familiar with the full breadth of a discipline and the identities of active researchers whose judgment can likewise be trusted in a wide range of specialties. As Ziman (1982) duly notes, “Knowing who knows about what is itself a personal resource that comes from active participation in a research field; it cannot be transferred to an index card or computer file” (p. 246). Some indication of the extent to which manuscripts are assigned to unqualified referees can be gleaned from a study of 173 lead authors of articles published in *AMJ* and *AMR* from 1999 to 2001 (Bedeian, 2003). Nearly 55% of the authors recorded that they had been asked to referee a manuscript they were not competent to critique. It is surprising that more than one third reported that they still submitted a “peer” review.

Hitt’s (2009) final concern relates to the question of whether management journals “may be publishing weaker articles because editorial review board members lack the necessary expertise to serve as legitimate evaluators” (p. 257). In response, he cites ISI Web of Knowledge 2004 and 2005 Journal Citation Reports (JCR) impact factor scores for *AMJ* and *AMR*, concluding that the impact ratings for both have improved. Based on the scores Hitt provides, this is true for *AMR*, as its impact rating rose from 3.72 to 4.25 for the years in question. Hitt’s own data, however, indicate the opposite for *AMJ*. According to the ratings he provided (that are accurate per JCR), the impact score for *AMJ* dropped from 3.43 in 2003 to 2.65 in 2004 and to 2.20 in 2005. To be completely fair, however, we should note that the 2006 rating for *AMJ* was 3.353, a number Hitt may not have had at hand. Moreover, during the period 2002 to 2006, the *AMJ* impact factor

bobbed up and down, ranging from 2.200 to 3.353 ($M = 2.8174$, $SD = 0.479411$). Whatever the case, any assertion that either *AMJ* or *AMR* may or may not be publishing weaker articles (because editorial review board members lack the necessary expertise to serve as legitimate evaluators) based on the fact that their impact scores have or have not improved is a classic example of the logical fallacy known as an *incomplete comparison*. Such assertions cannot be refuted. A complete comparison would require contrasting a journal's known performance with what its performance would have been with an alternative editorial board that never existed, that is, a comparison between a board that was and a board that could have been. We can nonetheless be sure of one thing. Had the manuscripts that were reviewed during 2004 and 2005 been reviewed by a different set of referees (of whatever expertise), the final set of manuscripts, as well as their content, would have been different (Bedeian, in press). Whether one set of manuscripts would have advanced learning and education within the management discipline more than the other can never be known.

The Klimoski Commentary

As we read Klimoski's (2009) remarks, his principal concern seems to be that we have not presented evidence that the "credibility of our discipline" may be at risk" (p. 239) to the extent that the peer review process within the management discipline is thereby suspect. He quotes from our article's abstract, where we note that "a cornerstone of the scientific ethos is that editorial board members should be selected based on their scholarly achievements, as demonstrated by publications in peer-reviewed journals and evidence that their work is of value to others in their disciplines" (p. 239). He then proceeds to argue that whereas "certain levels of accomplishment . . . are necessary," they are insufficient for staffing a journal's editorial board (as represented by *AMJ* and *AMR*). Klimoski's basic conclusion is that there is "little evidence that [the] current paradigm for vetting, selecting and recruiting Board members for key Academy of Management journals is problematic" (p. 239).

First, we wish to reiterate that the purpose of our study was to

examine the scholarly records of the editorial board members of two premier management journals published by the discipline's principal professional association, using the achievements of board members from leading journals sponsored by the primary professional associations of six other disciplines as a comparative backdrop. (p. 213)

It was not our intent to "offer evidence that the status or reputation of our discipline . . . in the eyes of key stakeholders is at risk" (p. 213). We continue to contend, however, that "to maintain public confidence and guard the autonomy and authority we enjoy as academics, it is necessary to demonstrate that the peer-review process within our discipline is conducted according to high standards of scholarship" (p. 213). We further hold "that as an academic community, the management discipline derives its scientific authority from the public's expectation that the evaluation and certification of new knowledge occurs following the complex of norms and values embodied in the ethos of science" (p. 213). What seems odd is that Klimoski acknowledges and agrees with our position in this regard when he states

that one plausible outcome of questionable staffing practices is to place the status or reputation of our discipline at risk. He seems to take exception to the fact that we have not conducted a study regarding whether the status or reputation of the management discipline has been harmed by the results that we report.

Our argument is very simple. Broadly speaking, there are two kinds of academic journals: those that subject submissions to peer review and those that do not (Donovan, 2007). In academe, the greatest prestige is attached to publishing articles that appear in peer-reviewed outlets. In a real way, peer-reviewed journals perform a symbolic function. They “[encourage] credibility and trust insofar as they are perceived to be scientific” (Reay, 2007, p. 102). Peer review thus demarcates “true science” from “pseudo science.” The prestige that accrues to those who “successfully surmount the peer review process” (Lee, 2002, p. 9) and “the reason we trust journals . . . rests on the process by which manuscripts are evaluated before publication; that is the peer review system” (Campanario, 1998, p. 182). The relationship between a peer-review process conducted according to high standards of scholarship and a discipline’s credibility has been cogently expressed by Lee (2002) in an editorial comment authored while serving as *AMJ* editor:

The legitimacy and rigor of [the] peer review process affect our credibility with colleagues in management departments, other business school departments, other professional schools, liberal arts departments, and funding agencies. Needless to say, the outcomes of reviewing really do affect us both individually and collectively. (p. 9)

This relationship may be less obvious to some readers. Perhaps we should have been more forceful in stating the aim of our article and its underlying logic. We, however, suspect that, like Lee, most readers appreciate the relationship between the integrity of a journal’s review process and its credibility.

In this regard, we note in our article and elsewhere that one unique aspect of academe is the degree of self-management incorporated in the peer review of scientific manuscripts submitted for publication (Bedeian, in press). As Biagioli (2002) wrote, peer review (in particular) “sets academia apart from all other professions by constructing value through peer judgment, not market dynamics” (p. 11). Although 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. In our view, to betray the trust of what Klimoski calls “key stakeholders,” by failing to maintain the highest standards of peer review, risks damaging our discipline’s public trust. The trust our discipline enjoys is only possible because peer review “stands metonymically for credibility, for legitimate knowledge, for reliable and useful predictions, for trustable reality” (Gieryn, 1999, p. 1). Klimoski is correct, we note the potential, but do not “offer evidence that the status or reputation of our discipline . . . in the eyes of key stakeholders is at risk” (p. 240). At the same time, we surmise that the defensive positions taken by the authors of the other commentaries commissioned to comment on our article reflect an understanding of the relationship between a discipline’s credibility and a peer-review process that is conducted according to high standards

of scholarship. How one could contend that questionable editorial board appointments, such as that described by an anonymous referee of our article, do not undercut the credibility of our discipline is hard for us to fathom.

This aside, in the main, we agree with much of what Klimoski says but do have several reservations that merit comment. First, we largely agree with his list of desirable editorial board member attributes, as his discussion channels similar views expressed in our article. Contrary to what Klimoski maintains, however, we do not assume a linear relationship between number of publications and citations and board member suitability. Rather, we agree with Klimoski that achieving credibility as an editorial board member is not “simply a function of the length of one’s publication list.” Once again, we restate our position:

It is important that readers clearly understand that . . . we are not saying that editorial appointments should be based solely on number of publications and associated citations. We recognize that it takes more than a “high-power” name to prepare a thoughtful review and, however eminent, all humans are susceptible to the same biases and errors in judgment. (p. 214)

We also agree with Klimoski that the attributes of an editorial board serve a “signaling function.” In this regard, Klimoski notes that “it is not at all unusual for journals to have one or more individuals on the board because they provide ‘marquis value’” (p. 243). We wonder, however, what signal is sent about the credibility of a journal’s review process when individuals are appointed to a journal’s editorial board who have, as we note in our article, “authored only two articles that have garnered fewer than five (adjusted) citations or has authored only three articles that have attracted fewer than two (adjusted) citations (as in our *AMJ* sample)” (p. 224). Furthermore, we wish to make clear that we have no qualms about selecting more junior scholars to serve on editorial boards. We nonetheless maintain, together with the many desirable attributes Klimoski has identified, that in making such appointments due consideration should be given to an individual’s scholarly abilities, as demonstrated by publications in peer-reviewed journals and evidence that one’s work is of value to others. The question of whether more junior scholars possess analytic skills superior to “more mature and well published” scholars and that, in turn, more “mature and well published” scholars are more capable than younger scholars in evaluating “conceptual contributions” is an empirical one. As noted *supra*, the available evidence as to whether younger or more experienced reviewers produce better reviews is mixed. The notion that younger, more recently trained scholars are better methodologists than their more seasoned colleagues does not strike us as self-evident.

Contrary to what Klimoski suggests, nowhere in our article do we argue that only academic “stars” (his term) are “appropriate for journal board membership.” We simply maintain that all editorial board members should have a reasonable record of scientific achievement, such that there is no reason to question both their role in validating new discoveries in our discipline and corresponding impact on the advancement of others within our profession. Furthermore, we are uneasy with Klimoski’s suggestion that because the recent acceptance rates of *AMJ* and *AMR* “appear to be normative” implies that “the journal policy makers are staffing appropriately” (p. 247). Our uneasiness stems from the fact that, as Van Fleet, McWilliams, and Siegel (2000) have shown, reported acceptance rates

are not necessarily accurate, and even what is meant by the term “acceptance rate” is unclear. Indeed, citing data published by DeNisi (1997), Van Fleet, McWilliams, and Siegel showed that *AMJ*'s acceptance rate could have been reported as being as low as 8.3% or as high as 23.1% (p. 857). Furthermore, as Parnell (1997) noted, acceptance rates are artificially reduced by an “existing reward system that necessitates publication in [selective] journals and, thus, it is likely that many articles rejected from one selective journal, will be resubmitted to several other such journals before they achieve acceptance” (p. 71). As a result,

If a typical publishable article is submitted to four selective journals before publication, it also generates three rejections that will drive down the acceptance rates of this group of journals as a whole. If a typical publishable article is submitted to only two less selective journals before publication, it generates only one rejection within the group. (p. 71)

Parnell also noted that low acceptance rates may be more a function of space limitations than article quality.

Like Hitt, Klimoski also suggests that the current *AMJ* and *AMR* impact factors may be a marker to infer that the work being published is read and useful. We refer readers to our comments above concerning the logical fallacy incorporated in such an incomplete comparison. Klimoski goes on to claim that “producers of scholarship and research manuscripts” appear satisfied as evidenced by the fact that both *AMJ* and *AMR* continue to receive submissions for possible publication. Data presented in the Bedeian (2003) study referenced above do suggest that authors of articles published in *AMJ* and *AMR* are generally satisfied. This is perhaps not surprising, however, given that these authors' works were accepted for publication. At the same time, nearly 20% of these same 173 authors questioned the competence of the referees assigned to vet their work. More than one third of the authors reported that recommended revisions in their manuscripts were based on either an editor's or a referee's personal preferences. Similarly, a full one third had experienced pressure to make a revision conform to the personal preferences of either an editor or a referee. Nearly 25% indicated that in revising their manuscript they had actually made changes they felt were wrong. More than one third reported having been treated like an inferior by an editor or a referee, and more than half felt that an editor had regarded a referee's knowledge about original research reported in the author's own manuscript as more important than the author's own. Furthermore, more than 56% believed that their judgment was probably better than that of a referee, and some 82% felt that referees should not have the power to make authors conform to their opinions. These numbers suggest that there is adequate room for improvement in author satisfaction. Moreover, if anything, survey responses from authors whose works were accepted for publication should lend a positive bias to the above numbers.

Furthermore, Klimoski suggests that because we favor populating editorial boards with scholars of academic standing, we are somehow opposed to selecting board members whose thinking is unique or who are innovative. This is clearly not the same as selecting individuals to serve on editorial boards who have not published or have published so little that their credentials and, thus, their credibility are suspect. Let it be known that not all well-published authors are “totally invested” in the status quo or are irretrievably socialized in prevailing paradigms. We would also note that consensus is not all bad. Research suggests

that “consensus leads to an increase in the amount of academic debate” within an academic field because “only issues ready for substantive consensus are broadly debated” (Evans, 2007). Contrary to what many may think, consensus can amplify debate, as in the case of the present exchange.

Finally, Klimoski suggests that the extent to which *AMJ* and *AMR* are cocited by the larger scientific community would provide an indication of their relative standing in the larger academic arena and laments the absence of such information. Such information is available, however. Using data gleaned from the Social Science Citation Index (SSCI) for the years 1997 to 2001, Lockett and McWilliams (2005) created citation counts for 24 topical journals in management, economics, sociology, and psychology. Using these data, they created a cross-citation matrix whose individual elements p_{ij} give both the percentage of references in a discipline i that were directed to discipline j and the percentage of citations discipline j received from discipline i . Although *AMJ* was found to be a net exporter of knowledge (being cited more than it cites), *AMR* was found to run a trade deficit in citations. Overall, the Lockett and McWilliams data suggest that management as a discipline imports a larger share of citations from other disciplines than it exports. Given the diverse audiences to which the management discipline’s leading journals appeal, however, this may arguably be a plus, indicating that the discipline’s journals are open to multiple information networks, thereby facilitating the cross-fertilization and synthesis of ideas (Bedeian, 2005).

In conclusion, Klimoski states the belief that our critique is incomplete, overstated, and even alarmist. In contrast to his own criticism of those he perceives as “being invested in existing ways of thinking,” he concludes that the current practices for selecting editorial board members are “appropriate and defensible,” and in his closing line he quotes former President Jimmy Carter’s budget director Bert Lance, who declared, “If it ain’t broke, don’t fix it.” We find this ironic in that, in a commentary commemorating his classic 1977 article “Managers and Leaders: Are They Different?” Zaleznik (1972/1992) also cites the same Lance quote. He does so, however, to illustrate that managers and leaders are, indeed, different. They differ in their vision and the realization that success contains its own seeds of decay. As Zaleznik explained, “Leaders understand a different truth: ‘When it ain’t broke may be the only time you can fix it’” (p. 130). Agreeing with Zaleznik, we undertook our study in the belief that knowing more about the ultimate costs and consequences of the peer-review process and understanding it better may suggest how it can be improved, enhancing our discipline’s productivity and growth. To this end, we have followed the facts where they led. Thus, contrary to what Kilmoski intimates, we do not feel it is in our discipline’s best interest to have its premier journals rest on their oars in the belief that their past success is a guarantee of their future success.

The Tsui and Hollenbeck Commentary

In reading the Tsui and Hollenbeck (2009) commentary, we largely come away agreeing with their general conclusions and, in particular, their thoughts on increasing the proportion of “successful authors” that serve on editorial boards and the importance of enhancing the transparency of alternative criteria, other than success as an author, for editorial board membership. Tsui and Hollenbeck concur with our position that board

members should be “carefully selected” and possess the “requisite [subject matter] expertise to assure high quality manuscript reviews” (p. 259-260). They also share our view that past success as an author should be the “*primary requirement* for elevation to our editorial boards” (p. 271) and that, as we have discussed above and in our article, it takes more than a “high-power” name to prepare a thoughtful review. The center point of their commentary—what they identify as the “critical question—is how to balance “success as an author” with “other criteria that contribute to being an ‘effective reviewer’ and that help comprise an effective board as a whole” (p. 261). We very much appreciate Tsui and Hollenbeck’s goal of “not taking a side in this debate, but rather clarifying the issues on each side” (p. 261). In this sense, we are one in that, as we state in our article, “we hope that the implications suggested by our findings prompt serious reflection and provide a platform for open discussion and free dialogue about the qualifications necessary to judge new knowledge that enters our discipline” (p. 233). There are, however, a few issues in Tsui and Hollenbeck’s commentary that require elaboration. Like Hitt and Klimoski, Tsui and Hollenbeck principally focus on our findings as they pertain to *AMJ*. They do so because, as they explain, *AMJ* “has the most disturbing results.” Following Tsui and Hollenbeck’s lead, we likewise restrict our remarks.

The first issue in Tsui and Hollenbeck’s commentary on which we wish to elaborate is their discussion of the “percentage of ‘successful authors’ on the *AMJ* board.” In conducting their own analysis of this topic, they purport to have deviated in three ways from the methodology we outline in our article. First, they claim not to have relied on “between-discipline comparisons,” as they feel that too many factors confound such assessments. Second, rather than using Hirsch’s well-established *h*-index, they employ an ad hoc measure they refer to as *h'*. Third, instead of drawing a sample of *AMJ* board members, they base their analysis on a census of the board’s entire membership. With regard to the first point above, we acknowledge in our article that coauthorship patterns, citation habits, definitions of authorship, and quality norms vary across disciplines and that these factors should be considered when making interdisciplinary comparisons and that it is important to consider such differences when assessing research productivity and citation patterns across disciplines. Although we do report data on other disciplines, we do so principally as a backdrop or point of reference. Readers wishing to solely focus on within-discipline comparisons can easily consider the results we report for *AMJ* and *AMR* and disregard our other findings. We believe it is telling that none of the commentators chose to do so. This said, we find ourselves baffled by Tsui and Hollenbeck’s ensuing attempt to measure the overall research productivity of *AMJ* board members.

In an effort to measure overall *AMJ* editorial board member research productivity, rather than rely on what Tsui and Hollenbeck refer to as a “straight *h*-index” to access “quality,” they improvised an alternative measure wherein they simply counted the number of articles authored by each *AMJ* board member in a restricted set of eight journals. For each article so identified, its author was given 1 point on Tsui and Hollenbeck’s (2009) *h'*-index. They justify their new index by arguing that

an author with a single publication that was cited twice would receive an *h*-score of 1, but this paper has not yet, in our opinion, had enough impact to count as a successful authoring experience. In contrast, the straight *h*-index would *not* count a paper with 60 citations by an

author with 61 publications as being successful, yet a paper cited this many times has clearly had a significant impact on the field. (p. 262)

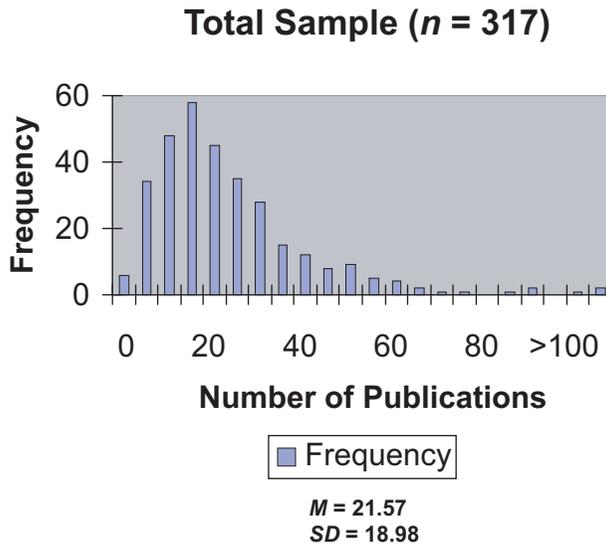
Putting aside the magnitude of the numbers in the preceding example (Nobelist Herbert A. Simon's h -index is 42), the example offered fails to take into account that scores on the h -index metric are not linear. In a manner analogous to percentile scores, h -scores are compressed at the upper tail of their distribution. Thus, by design, just as it is a longer way from, for example, the 99th percentile to the 100th percentile than from the 50th to the 51st percentile in a set of test scores, the difference between h -scores of 21 and 22 is much greater than the difference in h -scores of 1 and 2. Thus, the further you move out on the tail of the distribution of a set of h -scores, the greater and more impressive the difference. We fail to see the logic behind Tsui and Hollenbeck's h' -index, especially given the acknowledged difficulties associated with using cut scores for what are otherwise continuous data (MacCallum, Zhang, Preacher, & Rucker, 2002). Furthermore, we are at a loss as to how the concern Tsui and Hollenbeck express about the standard h -index not counting an article with 60 citations as successful as compared with one having 61 citations does not also apply under their h' -index scoring scheme with respect to an article with 19 citations as compared with another with 20 citations. The article with 19 citations would count no more in the Tsui and Hollenbeck h' -index calculation than the article with 60 citations would in Hirsch's standard h -index formula.

We are further perplexed by Tsui and Hollenbeck's decision to limit their analysis to only eight journals rather than access all the journals indexed in the Web of Science. We are also puzzled by why, after acknowledging that between-discipline comparisons are too confounded by other factors to make meaningful comparisons, they proceeded to include two journals in their analysis that are outside the management discipline and a third journal that is multidisciplinary. SSCI classifies both *Journal of Applied Psychology* and *Personnel Psychology* as applied psychology journals, and *Administrative Science Quarterly* (ASQ) is generally considered a multidisciplinary journal (Allen, 2003). In selecting journals for our sample, as explained in our article, we purposefully selected journals sponsored by professional associations rather than those affiliated with individual universities (e.g., ASQ) or departments to avoid any in-house bias that might be reflected in editorial board membership or citation patterns. We likewise limited our selections to journals published by professional associations, as opposed to for-profit journals (e.g., *Organizational Behavior and Human Decision Processes* [OBHDP]) owned by publishing companies to avoid any questions concerning editorial independence. In addition, we selected only journals in which, in principle, the full membership of a discipline might publish, as opposed to narrower specialty journals with more limited constituencies. By selecting journals with a broad reader base, we avoided confounds associated with publication preferences across subfields. Two of the journals in the Tsui and Hollenbeck analysis (viz., *Strategic Management Journal* and OBHDP) are, in our judgment, specialty journals and thus subject to confounds analogous to those that Tsui and Hollenbeck stated they wished to avoid in making between-discipline comparisons. Other than AMJ and AMR, we question whether the six other journals used in Tsui and Hollenbeck's analysis are representative of the management discipline *as a whole*.

We also wish to comment on data Tsui and Hollenbeck present pertaining to the potential number of what they refer to as “successful authors” within our discipline. In particular, we wish to suggest that the representativeness of the data that Tsui and Hollenbeck rely on to gauge the number of such successful authors is, in our opinion, suspect. Admittedly, generalizable data regarding the number of articles published by members of our profession are sparse. We would caution, however, against using the Miller, Glick, and Cardinal (2005) data that Tsui and Hollenbeck rely on for this purpose. The Miller et al. data come from two sources: (a) a nonrandom sample of 16 “organizational science doctoral programs” in the United States and Canada and (b) a random sample of 20 additional doctoral-granting departments also located in the United States and Canada. Miller et al. tracked the publication records of 445 graduates of these 36 programs and found that “41 per cent of these individuals never published in any journal tracked by the *Social Science Citation Index* . . . while the average individual only published 4.6 articles over a 20-year period” (Glick, Miller, & Cardinal, 2007, p. 822). On one hand, it should be obvious that the first of the two sources Miller et al. drew on for their data represents a convenience sample. Alternatively, it should be equally evident that neither the 20 additional departments in their sample nor the combined total of 36 departments can be considered representative of the general population of management departments (doctoral granting and non-doctoral granting) in the United States and Canada and, furthermore, that by combining data from two countries, confounds are present because of national and societal differences in educational systems and cultural norms. Thus, given the sampling bias in both sources used by Miller et al., it would seem likely that they each systematically differ from the broader population to which Tsui and Hollenbeck are attempting to generalize (i.e., members of the management discipline). We would also note that the Miller et al. samples were not restricted to academics but included 48 cases simply assigned “industry” because their affiliation could not be determined. In our experience, these individuals would not be expected to have produced articles for SSCI-indexed journals. The inclusion of these 48 cases in the Miller et al. analysis, thus, attenuates the descriptive statistics that they report.

To assess the degree to which the Miller et al. data are suited for the purposes Tsui and Hollenbeck intended, we accessed an alternative database (Jauch, Hunt, & Bedeian, 1998). The database contains complete curricula vitae for 317 individuals who graduated with a doctorate degree in management from a U.S. college or university between 1977 and 1987. Because data collection began in 1998, at least 11 years had passed since members of the sampling frame had received their graduate degrees. As a group, the 317 respondents had authored 6,839 articles in journals tracked by the SSCI (see Figure 1). Of this number, only 6 failed to publish an article in a recognized journal. The number of articles published ranged from 0 to 175 ($M = 21.57$, $SD = 18.98$, $Mdn = 17$, mode = 12). As the magnitude of this standard deviation suggests, and as would be expected given the nature of the data, the distribution is right skewed ($S_x = 2.93$). Using Tsui and Hollenbeck’s tipping point of 20 published articles as defining a “successful author,” however, 69.5% had reached or exceeded this benchmark. Moreover, as shown in Figure 2, the 317 respondents had, on average, received 184 citations to their work ($SD = 270.99$, $Mdn = 67$, range = 0 to 1,510). As would be similarly expected, given the frequency distribution for number of articles published, the distribution of citations is also right skewed ($S_x = 2.15$).

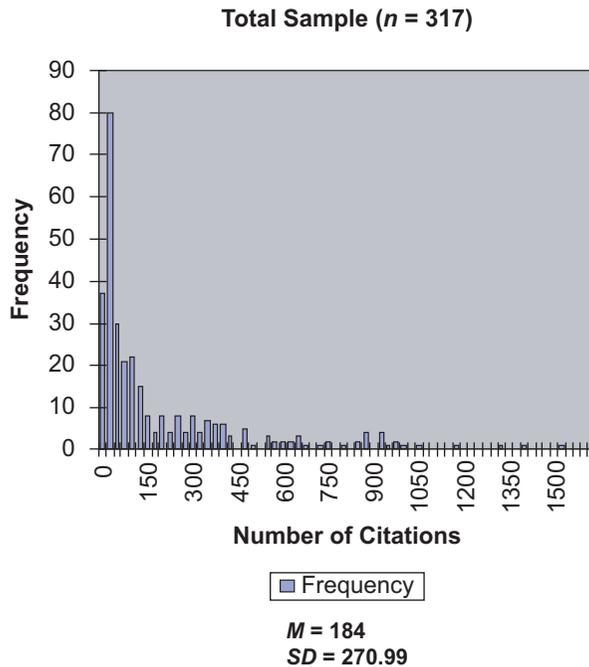
Figure 1
Number of Articles Published by 317 Faculty Members Who Graduated With a Doctorate Degree in Management From a U.S. University Between 1977 and 1987 and Teaching at a U.S. University in 1996 for the Period 1977 to 1998



Comparing the Jauch et al. (1998) data to those in Tsui and Hollenbeck's Table 1, we find that the cohort in question has a higher mean number of published articles than *AMJ* editorial board members (21.57 vs. 19.03) but also a greater variance in number of articles published ($SD = 18.98$ vs. 14.25). The mode number of articles published is about the same (12 vs. 13), but the median number of articles published is higher for the Jauch et al. cohort than *AMJ* board members (17 vs. 14). In all, looking at both Figures 1 and 2, the Jauch et al. data set presents a much healthier picture of the management discipline's productivity than do the Miller et al. data and suggests that the number of successful authors (at least as defined based on number of articles) is not as limited as heretofore envisioned.

In their concluding pages, Tsui and Hollenbeck offer various suggestions for reducing the gap between the demand for and supply of "effective" referees. We are in general agreement with their suggestions and are particularly attracted to the possibility of an open application process for editorial board membership. The American Psychological Association (2005) has used such a process for a number of years. At present, the exact criteria for membership on the *AMJ* and *AMR* editorial boards are gauzy at best. Readers of *AMJ* are assured that a merit- or performance-based system has long been used as "the foundation for forming" its review board (Ireland, 2008, p. 9). This system for selecting editorial board members has been described as involving "evaluations of multiple 'work samples,'" that is, ad hoc reviews that have been prepared by members of *AMJ*'s ad hoc reviewer panel (Rynes, 2006, p. 1098). More recently, this merit-based system has been expanded, such that the "best reviewers" from the academy's annual conference are asked to be ad hoc referees for the academy's journals (Lee, 2007). How these "best reviewers"

Figure 2
Number of Citations to Articles Published by 317 Faculty Members Who Graduated With a Doctorate Degree in Management From a U.S. University Between 1977 and 1987 and Teaching at a U.S. University in 1996 for the Period 1977 to 1998



are identified and by whom, however, are unclear. We contacted four division program chairs for the 2007 academy meeting to gather details about this practice. Two indicated no specific knowledge of any such practice, whereas the other two thought there was some sort of author evaluation of referees but were unsure how it worked. Adding to this confusion, what is meant by a “merit-based process” for selecting board members seems to vary from one pronouncement to the next. In some instances, “merit” is discussed in terms of “strong track records of publication and citations” as well as preparing “high-quality reviews in a timely fashion” (Rynes, 2006, p. 1098). In other instances, success as an author is not mentioned, and “merit” is solely defined with respect to being an “outstanding reviewer” (Lee, 2007).

As Tsui and Hollenbeck state, and we agree, the selection criteria for editorial board membership can only be stretched so far before they “snap” and people are left “head scratching,” unable to “figure why somebody is on the board.” If this happens, Tsui and Hollenbeck predict an “unhealthy cynicism” will develop within the discipline. Based on the reader feedback we have received on our article to date, the data presented in Bedeian (2003), and stories such as the above anecdote concerning the graduate student commissioned to review for *AMJ*, such cynicism already exists. Incidentally, we suspect that the previously quoted anonymous referee’s anecdote may not be that unusual. Based on the

number of job applications we have received from ABDs who list experience as an ad hoc referee for one or more Academy of Management journals, reliance on graduate students as referees is not rare. That we as a profession feel that our unfledged PhDs should be authenticating our discipline's knowledge claims, setting its expected standards of scientific rigor, and determining the advancement of individual scholars within the academic stratification system is hard for us to understand. That anyone could argue that doing so does not diminish our discipline's credibility is beyond our ken.

Finally, Tsui and Hollenbeck claim that their "data fail to support the implied conclusion of Bedeian et al. (2009) that reviewers of *AMJ* are less accomplished scholars than would-be authors of *AMJ* are." As readers no doubt recognize, this was not our conclusion. Rather, our data show that, in general, relative to the members of the *AMR* editorial board and all save one of the other journals included in our study, *AMJ* editorial board members were less accomplished scholars. In advancing the *incorrect* notion that we had concluded "reviewers of *AMJ* are less accomplished scholars than would-be authors of *AMJ* are," Tsui and Hollenbeck miss our point, that is, not that someone with less of a scholarly record may be a referee but that someone with virtually *no* scholarly record may be a referee. If this were the case, it seems likely that a substantial number of authors would have had their work reviewed by less qualified referees.

Conclusion

Despite our admonitory conclusions, we have a high regard for the peer-review process. We do maintain, however, that it can be improved. Indeed, as we state above and in our article, "knowing more about the ultimate costs and consequences of the peer-review process and understanding it better may suggest how it can be improved, enhancing our discipline's productivity and growth." Thus, our aim was to better understand the peer-review process within the management discipline, not to "trounce and denounce." Like Hambrick (2007), who has recently voiced his concern over our discipline's fetish for theory, there are no personal motives underlying our intent. Moreover, as Hambrick was also careful to make clear, we are not "pointing fingers." Two of us have served the Academy of Management, and the management discipline, in virtually every conceivable role and, like Hambrick, are "part of the establishment." Our unease with the current state of the peer-review process has steadily increased as we have witnessed (as Tsui and Hollenbeck warn) a growing "unhealthy cynicism," especially among graduate students and new faculty, about the scientific authority of our discipline. This cynicism is evident in both the satirical portrayal of the manuscript review process as a game and a decreasing confidence in having one's work fairly and competently reviewed (Bedeian, 2003). As we note in our article, it is our personal observation that being asked to serve in a gatekeeper position is seen as a distinct honor—a testament to the significance of one's work—and that it is particularly disconcerting to young scholars when it appears that a record of scientific achievement is not the primary qualification for editorial board membership.

As we hope we have made clear, we do understand the difficulties associated with identifying and enticing qualified peers to serve as editorial board members. Nor do we expect the peer-review process to work fairly and efficiently all the time. Nonetheless, peer review

means approval by independent experts who, as Cain (2004) explained, “understand (at least claim to understand) the subject and have the expertise to assess rigorously a manuscript’s representation against the facts, methods, and experiences of the field” (p. 181). If it turns out that *peer review* is not done by one’s *peers*, then we have at best reduced our journals from being “peer reviewed” to “pseudo-peer reviewed” and should not be surprised if their legitimacy, and our discipline’s credibility, is diminished. To avoid any suspicion in this regard, we would like to see not only our discipline’s journals publish a definition of what they mean by *peer review* (Lock, 1991, p. x) but also their editors conduct (and make public) periodic internal and external evaluations of their peer-review process, with participation of authors (both successful and unsuccessful), editorial board members, ad hoc referees, and readers (Hojat, Gonnella, & Caelleigh, 2003, p. 76). Publishing such periodic self-assessments would help avoid the “head scratching” about which Tsui and Hollenbeck have expressed concern and possibly lead to suggestions for improving the peer-review process.

In closing, we agree that the academy’s journals are its “crown jewels” (Lee, 2007). In responding to the accompanying commentaries, we have attempted to recount the base facts as they have been revealed in our analysis and to do so in the same spirit of scientific inquiry that spurred our study. If our results have prompted a measure of cognitive dissonance, we hope that any associated discomfort will lead to action on the part of all our discipline’s journals and their sponsoring organizations. With respect to a concern for the academy’s “crown jewels” and those charged with maintaining their credibility, we offer Saint Paul’s admonition: *Depositum custody*, “guard what has been entrusted to you” (I Timothy 6:20).

Note

1. Actually, the correlations Rynes (2006) reported between “professional age” (experience) and review quality and between number of publications and review quality in her text discussion and those in the intercorrelation matrix that accompanied her discussion are reversed. We follow Hitt in assuming that the correlations in the matrix are correct.

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