

## FROM THE EDITORS

### Deriving Greater Benefit from the Reviewing Process

As an associate editor of the *Academy of Management Journal (AMJ)*, I spend much of my time studying correspondence between authors and reviewers. Through this experience, I have noticed how the contents of author and reviewer communications appear to influence publication outcomes. Often, submitted manuscripts have traits that are detrimental or beneficial to their acceptance prospects. Similarly, characteristics of reviewers' reports can facilitate or hinder conveying their intended messages to authors and affect whether the authors respond effectively.

My role in this process is to interpret the communications and make publication decisions. These decisions are based heavily on the advice and comments of the reviewers, which in turn are affected by the authors. My point of view is that I want authors and reviewers to recognize that their interactions play a central role in the continued development of our field. In what follows, I offer some suggestions to authors and reviewers as to how they might address some recurring difficulties that are apparent in their communications with each other. I hope that resolving these matters will help to improve the process for everyone and raise our level of scholarship.

#### To Authors

As anyone who has submitted work to a journal such as *AMJ* can tell you, any number of problematic issues can arise in a manuscript, and reviewers are usually quite adept at identifying these issues. However, although a large number of such matters could be discussed, I will mention only two that are becoming especially common. The first is what I call "premature submission" and the second is "comments and chances." I suspect that these issues are likely to be confounded with one another. However, there is merit to be gained by considering each one independently.

**Premature submission.** I believe that many authors submit their work prematurely. In my experience, a lot of manuscripts have superficial mis-

takes such as typos, errors in citations, missing tables, and the like. A similar theme concerns how manuscripts are written. Many are not clear and concise, and many have flaws in structure and presentation. Further, some submissions have unclear research questions, undeveloped theoretical models, vaguely described methods, and tautological arguments. What has been most surprising to me is that both inexperienced and experienced researchers exhibit these problems.

The downside of premature submission is considerable: reviewers often conclude that authors have not exercised due diligence in their work and develop a skeptical attitude toward their manuscripts' contents. Quite often, reviewers make negative inferences and extrapolations about the substance of such work, raising doubts about whether the studies were done appropriately and whether the analytical results are accurate. Moreover, authors may be unaware that this process is occurring because many of these comments are directed confidentially to the editor, rather than to the authors themselves.

By not presenting the ideas and intended contributions clearly and carefully, authors leave reviewers with unnecessarily negative impressions of their submissions. As the impression management literature has taught us, impressions *do* matter. From what I have seen, authors do not need to give reviewers any additional reasons to challenge their results.

**Comments and chances.** Recently I received an e-mail message from the author of a rejected manuscript noting that the manuscript had been submitted to *AMJ* primarily for developmental comments; if an invitation to revise were issued, then all the better. But, in the end, the author was concerned primarily with getting "good feedback" that could be used to improve the chances of getting the manuscript published elsewhere.

I sense that this motive may not be uncommon. A guiding goal of all the Academy's journals is to provide constructive feedback that will help authors improve their work, regardless of the publishing outcome. Of course, the fact that most manuscripts (more than 90 percent) are rejected is enough to make any of us less than optimistic about prospects at *AMJ*, and it seems reasonable to con-

---

Many thanks to Dov Eden, Tom Lee, Sara Rynes, and Marshall Schminke for their very insightful comments and suggestions.

clude that all one may get from a submission is some good feedback.

However, I suspect that this motivation does not serve the authors or their future submissions well. It seems that many of the manuscripts submitted with the comments-and-chances goal in mind are not very well developed. They may be written well and may address the most obvious problem areas associated with premature submission. But these manuscripts still have a rough feel about them: the ideas are not clearly developed, the contribution is not explicated convincingly, the argumentation is not balanced (that is, the benefits and costs of a relationship are not both addressed), the assumptions underlying the analysis are not considered, and so on.

What I see happening is that these types of manuscripts are less likely to receive the detailed, nitty-gritty feedback that is needed to help the authors develop their ideas to the degree necessary for acceptance in any high-quality journal. Instead, the reviewers tend to focus on very general concerns because the manuscripts are so general in the first place. Typical reviewer comments include the following: "The research question is unclear," "The motivation is not established," "The concepts are not defined clearly," "The hypotheses are not linked with the theory," "The study does not provide a fair test of the hypotheses," "There are alternative explanations of the study findings," and "The discussion does not make theoretical sense of the findings," to name but a few. Certainly, comments like these are important, but they are not the kind of specific criticism needed to strengthen an author's ideas.

The point is that the comments and chances strategy can backfire because it does not yield the specific feedback the authors were hoping for, the insights they can draw on to improve their work's chances at the next journal. Thus, the authors may not be getting what they really wanted to get in the first place. The bottom line is clear: the more you invest in an *AMJ* submission, the more you will get back.

### To Reviewers

I have noticed two recurring issues in reviewer reports. The first I call the "breadth versus depth trade-off," and the second, the "sink or swim" approach. Like my concerns pertaining to authors, these matters may also be interdependent, but there is value in considering each independently.

**Breadth versus depth.** Reviewing a manuscript requires a decision about the breadth and the depth of the overall assessment. By breadth, I mean the

variety of topics that are addressed, and by depth, I mean the extent of the evaluation. It is not uncommon for reviewers to focus mostly on one or the other of these aspects.

It's easy to see why reviewers often make this trade-off. They are expected to evaluate a wide range of issues: research questions and their motivation; contributions to theory development; the structure, foundation, and logic of theoretical models; the internal validity of research designs; the reliability and construct validity of measures; the appropriateness of analyses; the presentation and interpretation of results; and discussions of implications for theory development and of limitations and overall implications for management knowledge. In other words, much breadth is needed. Simultaneously, depth is called for; all of the topics identified above have many layers, and for any one of them, a reviewer can ask detailed questions (for instance, What is new for theory? Is that interesting—why or why not? Are the concepts defined? Do the concepts represent the theory adequately?).

When reviewers address only one of these areas, they produce either breadth or depth reviews. A breadth review emphasizes broad concerns; the reviewer gives general comments about a manuscript's proposed contribution and about whether its theoretical model is developed adequately and its research method provides a fair test of the conceptual arguments. On the other side, a depth review typically plunges into the theoretical model or the research method, sometimes addressing both, but usually just one. This type of review focuses on the nuts and bolts of the arguments or the measures.

Although I appreciate the tremendous service our reviewers who write either breadth or depth reviews provide, in the spirit of improving their contributions to the evaluation process, I will express my belief that striving for an equal balance between breadth and depth provides the best reviews. Such balanced reviews are the most effective foundations for publication decisions and are also the most effective means for reviewers to communicate their suggestions to authors. In contrast, if breadth and depth are not balanced, comments may be too broad to help action editors see how to assess an author's ideas, or the comments may be too specific, limiting editors' comprehension of the overall value of a manuscript and its contribution. For my money, reviewers provide the best evaluations when they balance the breadth and depth of their reviews equally. With those reviews, they can more effectively identify the strengths and weak-

nesses of manuscripts and engage the authors in improving their work.

**Sink or swim.** There are many schools of thought regarding the role of reviewers in the publication process; some writers have seen reviewers as prosecuting attorneys (Pondy, 1995), and others have seen them as umpires (Meyer, 1995). I have noticed a different kind of reviewer style: the "mean coach." These reviewers' evaluations focus solely on problems and offer little in the way of suggested solutions; it seems the reviewers will let authors sink unless they can learn to swim on their own.

Far more effective than such sink-or-swim reviews are reviews that provide insights into how particular problems might be addressed, even if the recommended responses require a complete overhaul of a theoretical model or research design. Of course, in some cases, an elaboration of solutions is not feasible, such as when a manuscript does not contain a theoretical contribution. Clearly, all that can be said in that instance is that the "contribution is not developed adequately." But in many cases, additional comments can be offered that will help the authors understand what they need to do to resolve problems effectively. In these circumstances, we ask that reviewers provide the authors with clear and defined recommendations that will help them improve their manuscripts and assist in the continued development of our field.

Also, it is important for reviewers to remember that their reviews are being read very carefully and that authors may derive unintended meanings from their comments. By holding themselves to the same standards of clarity and organization they hold the authors to, reviewers can help authors avoid repeating mistakes or going down a different, but wrong, road in future revisions.

## Conclusion

An action editor makes publication decisions in large part on the basis of the recommendations and comments of reviewers. Given that reviewers respond to the manuscripts of authors, I believe that both should take stock of how they approach their role as communicators. First, of both authors and reviewers, I ask that you focus heavily on theoretical contribution. Previous researchers (Daft, 1995) and members of our own editorial team (e.g., Rynes, 2002) have stated that the nature and extent of theoretical contribution is the most significant factor influencing the publication prospects of a manuscript. I strongly encourage all authors to articulate the overall contributions of their manuscripts to theory. Does your study present new theory? Does it revise or ex-

tend previous theoretical explanations in an interesting way? If so, how? As for reviewers, I encourage you to provide additional insights and advice on how a manuscript's contributions can be better explicated.

Second, I suggest to authors that you consider getting critical peer review of your manuscript prior to its submission. Ask these "friendly" reviewers to really hone in on the adequacy of your proposed theoretical advance and evaluate whether it is fairly tested and adequately discussed. In crafting your manuscript, it may be helpful to think about *AMJ* as an institution in the market for ideas, a market in which you seek to sell your product. Also, I recommend that you address basic writing and organizational concerns, so that errors do not distract from your contribution (see Bem's [1987] chapter on writing empirical journal articles for an excellent primer on this matter). Finally, in the spirit of strategy and competition, let me invoke the lesson "know thy rival." Although reviewers sometimes play the role of umpire or defense attorney, it is probably fair to say that their most typical role is that of prosecutor (Pondy, 1995). Given that reality, try to anticipate the reviewers' objections and preempt them through frank discussion of your study's limitations and a clear defense of your chosen approach. Daft (1995) and Meyer (1995) should provide you with some insights as to how your "rivals" might think.

In conclusion, I believe that the contents of author and reviewer communications influence publication outcomes. By understanding that manuscripts can have features that are detrimental to their publication prospects, authors can take preemptive steps to improve their manuscripts' odds for success. Similarly, characteristics of reviewer reports can obscure or distort the messages they want to send an author. Reviewers can contribute more to the *Journal*, as well as to the field, by providing clear and constructive suggestions for improvements in the manuscripts they evaluate. Both authors and reviewers should remember that their relationship does not have to be an adversarial one and that, by working together, we all extend the quality of our knowledge and our field.

Donald Bergh  
Pennsylvania State University

## REFERENCES

- Bem, D. J. 1987. Writing the empirical journal article. In M. P. Zanna & J. M. Darley (Eds.), *The compleat*

**academic: A practical guide for the beginning social scientist:** 171–201. New York: McGraw-Hill. Available on-line at [http://comp9.psych.cornell.edu/dbem/online\\_pubs.html](http://comp9.psych.cornell.edu/dbem/online_pubs.html).

- Daft, R. L. 1995. Why I recommended that your manuscript be rejected and what you can do about it. In L. L. Cummings & P. J. Frost (Eds.), ***Publishing in the organizational sciences***: 164–182. Thousand Oaks, CA: Sage.
- Meyer, A. D. 1995. Balls, strikes, and collisions on the base path: Ruminations of a veteran reviewer. In L. L. Cummings & P. J. Frost (Eds.), ***Publishing in the organizational sciences***: 257–268. Thousand Oaks, CA: Sage.
- Pondy, L. R. 1995. The reviewer as a defense attorney. In L. L. Cummings & P. J. Frost (Eds.), ***Publishing in the organizational sciences***: 183–194. Thousand Oaks, CA: Sage.
- Rynes, S. 2002. From the editors. ***Academy of Management Journal***, 45: 311–313.