

## OBSERVATIONS ON “ANATOMY OF AN R&R” AND OTHER REFLECTIONS

SARA L. RYNES  
University of Iowa

I think the R&R often feels more like a rebuke rather than a reward. . . . We did indeed receive some fairly devastating comments for this paper, which I can quickly paraphrase as: You’ve done nothing new here; you should have gotten better data, and here are some suggestions for a study that might actually have been interesting and worthwhile.

-Scott Seibert, this issue

Given this account, you might be surprised to know that the first version of the manuscript, “Taking Empowerment to the Next Level: A Multiple-Level Model of Empowerment, Performance, and Satisfaction, by Scott Seibert, Seth Silver, and Alan Randolph (2004), actually received unusually positive reviews for a first submission. One reviewer (an *AMJ* board member) recommended we accept the manuscript on the first round with only modest revisions. This is an extremely positive reaction that is rarely seen at *AMJ* or other top-tier journals. The other two reviewers also recommended revision, although one was a bit more positive than the other.

So, was Scott exaggerating in his characterization of the reviews? Perhaps a bit, but not by a lot. To be honest, his comments reflect pretty much how I think I would have felt had I been in his shoes, at least for the first few days after receiving the reviews. For the most part, I think his assessment was right-on—although the reviewers liked the basic *idea* of the manuscript, they expressed serious concerns about its particulars.

As action editor, I was actually quite worried that the author team might not be able to bring the manuscript through to acceptance. At the root of the uncertainty was the fact that all of Seibert et al.’s data were subjective individual perceptions, some of which had been aggregated to the work-unit level (e.g., the “empowerment climate” variable). Not surprisingly, one of the reviewers would have strongly preferred objective measures of management practices, or at least some measures ob-

tained from an independent source. Another wanted to see more data about the degrees of within- versus between-group variance on several variables, arguing that the manuscript would be doomed by the emergence of certain patterns (the much-feared “fatal flaw”). But Scott and his team handled a daunting set of challenges with diligence and an earnest desire to make the manuscript the best that it could possibly be. This effort became evident in the revision process and ultimately won over all three reviewers, even if reviewer 3 was still somewhat “grudging” in his/her acceptance. Let me elaborate on this particular review process from my own (editorial) perspective.

### THE SEIBERT, SILVER, AND RANDOLPH (2004) REVIEW PROCESS

#### The Initial Submission

Reading Don Bergh’s account of the review process for Agarwal, Echambadi, Franco, and Sarkar’s (2004) paper on spin-outs, you get a clear feeling for the role of each of the three reviewers. Similarly, the reviewers of the Seibert et al. paper also developed clear identities. For example, reviewer 1 (like reviewer 2 in Agarwal et al.) was clearly a cheerleader for the manuscript. Indeed, this reviewer suggested that we accept it outright, suggesting only modest revisions. His/her private comments to me read as follows:

This study is notably well formulated. From the development of conceptual relationships, through the design, measures, and analyses, to the results and discussion, the translations and actions are thoughtful, appropriate, and carefully undertaken. Although I know that we seldom accept manuscripts with only minor revisions, I feel compelled to make this recommendation in this case. I do offer an array of issues for these authors to consider, but none is sufficiently challenging to warrant a major revision or a rejection. I look forward to learning of your perspective.

Wow.

Reviewer 2 was the methodologist, an expert in multilevel analysis. Although Scott appears to have seen reviewer 3 as the most difficult to win over, in

---

Thanks to Seph Doliner, Amy Hillman, Duane Ireland, Brad Kirkman, Chet Miller, Nandini Rajagopalan, Scott Seibert and Debra Shapiro for their helpful comments on previous versions of this essay.

my mind, reviewer 2 posed the greatest challenges to the manuscript. Consider these private comments to me:

The key point here is #8. If there's shared perception on Empowerment Climate and not on Personal Empowerment, they can probably fix the rest. If not, it's toast. If this does go forward as a revision, I won't even look at it unless they can satisfy this point. It is pivotal.

Reviewer 3, an expert on cognition and sense-making, was the one Scott characterized as posing the "So what?" question. Although s/he initially rated the manuscript as more promising than reviewer 2 did, it is easy to see how reviewer 3's comments to the authors raised alarm in Seibert's team:

I have questions about the conceptual contribution of the paper. As the authors note, the main contribution of the paper revolves around the empowerment climate construct. However, it is not clear to me what this concept adds to the literature. As the authors note, there already is an extensive literature showing that certain organizational practices, structures, and procedures are related to psychological empowerment. To me, the authors are simply adding the idea that "shared perceptions" that these practices, structure, and procedures are actually in place might also predict psychological empowerment. Methodologically, the authors are simply using a previously used construct, aggregating it across teams, and then calling it a new name. That does not seem like a significant theoretical contribution to me.

In my invitation to revise and resubmit the manuscript, I grouped the reviewers' concerns into three categories: conceptual and construct validity issues; analysis issues, and concerns about the discussion section. There were six fairly significant conceptual challenges dealing with levels-of-analysis issues, hypothesized relationships, and the construct validity of various measures. In the analysis category fell questions of unique versus shared variance, questions about the use of  $r_{wg}$  instead of ICC statistics, and questions about a possible need for additional variables. Concerns regarding the discussion section were less serious, mostly encouraging the authors to elaborate on the implications for future research. Here is the concluding paragraph of my decision letter, written October 4, 2001:

As you can see from these comments, revision will entail a substantial amount of work. Although all three reviewers are in favor of asking you to revise and resubmit your manuscript, there is considerable variance in their optimism about the likelihood of

success. In addition, it is possible that some of the additional analyses we are requesting will change the nature of your conclusions. Given these uncertainties as well as the multitude of issues that must be addressed, I would have to say that I see this as a relatively *high-risk revision*.

### The First Revision

Sixteen months after the initial decision letter and a few months after *AMJ* received a new version of Seibert and coauthors' paper, I again offered the authors an invitation to revise and resubmit their manuscript. Reviewers 1 and 2 both felt that the manuscript had been considerably improved. Reviewer 1 said:

Consistent with my review of your previous draft, I commend your thoughtfulness, precision, and care. Your revision follows this pattern and creates a more well-grounded argument. As is the case with any paper, there remains room for improvement, although my comments for this purpose are considerably fewer and all focus only on tightening the logic.

Reviewer 2 added:

I want to begin by complimenting the authors on the replies and the revision. It is evident that much effort, attention, and scholarship were invested in improving the paper. Thanks. . . . As I indicated in the prior review, I think that the research, which is intended to examine multilevel impacts of empowerment on performance, is a much-needed effort to help establish the meaning and importance of empowerment. I am very supportive of this effort.

However, in a twist reminiscent of the Agarwal et al. review process, reviewer 3 actually liked the revision less than the initial submission:

Even though the authors repeatedly claim that the major contribution of the paper is to introduce this concept of empowerment climate, I am still not totally convinced of the theoretical contribution here. . . . I also continue to believe that not measuring actual organizational conditions (and only measuring perceptions) is not just a minor limitation.

In summarizing the reviewers' comments in the decision letter, I indicated that levels-of-analysis issues continued to be the major obstacle to publication. However, at this point, I think it is fair to say that reviewer 2 truly did become Seibert et al.'s "best friend." Although s/he continued to critique (vigorously) what the authors had done with respect to this issue, s/he also offered a solution that, at the same time, helped to address reviewer 3's concerns about limited contribution. The issues involved were complicated, and reviewer 2 went into

them deeply and at length. I think you can get the flavor of reviewer 2's contribution from the following quote:

Given your variance on unit Empowerment Climate (EC) and your sampling of a single organization, EC cannot be an organizational level construct in this research! Obviously, the reconceptualization which I suggest above (which is consistent with levels theory and with contemporary climate theory) means that you have to recast the review of the EC literature and make clear that levels (of origin and focal level for analyses and generalization) have been confused, *and that one contribution of this work is to address such inconsistencies.* (emphasis added)

In a nutshell, reviewer 2 was suggesting a way to take the paper's main weakness and turn it into a strength. We should all be so lucky as to receive this type of help in the review process!

Why did reviewer 2 spend quite a bit of time helping these authors solve their problems? Although I will never know for sure, three things come to mind. First, the potential "fatal flaw" had been averted, so the manuscript was not automatically "toast" in the reviewer's eyes. Second, multi-level research is truly a passion for reviewer 2, so I would guess that s/he literally could not help being intellectually engaged in solving the problem. Third, I believe that the authors' conscientious first revision accomplished the task that Scott talks about in his essay: namely, they managed to convert reviewer 2 from a skeptic to a supporter (Meyer, 1995).

In my decision letter of February 14, 2003, I noted that this was still a risky revision and that the two major areas of concern continued to be levels of analysis and incremental contribution. Privately, I was now optimistic that the authors could bring this manuscript to acceptance, partly because it was clear that they were very diligent and partly because reviewer 2 had given them such great suggestions for how to address the remaining problems.

### The Second Revision: Conditional Acceptance!

In this round, something really quirky happened. In fact, this is the only time I can ever remember this happening in my four-and-a-half years as an *AMJ* editor, and both reviewer 2 and I were a tad embarrassed. Let me explain.

Here is my introductory statement in the decision letter, which was written on July 29, 2003:

All the reviewers (and I) are impressed with the further progress that you have made on this manuscript. In fact, two of them are impressed enough

that they recommend acceptance with only minor changes. The other reviewer still sees the manuscript as "promising," but believes more substantial changes are warranted before making a final decision.

My own reading of the manuscript is somewhere in between the two views. That is, I believe that some sections of the manuscript still need non-trivial revision. On the other hand, I believe the manuscript is close enough now that I am comfortable **conditionally accepting** your manuscript, contingent on your making a satisfactory final round of changes. Congratulations!!

Interestingly, it was now reviewer 3 (along with reviewer 1) who was recommending acceptance, despite his/her earlier frustration with aspects of the manuscript that really couldn't be changed (i.e., the basic data set). Yet reviewer 2 was still being a stickler, although continuing to rate the manuscript as "promising." I was a little puzzled and—okay, I'll admit it—frustrated, because I thought the authors had done a lot better job than reviewer 2 was giving them credit for. You can sense some of the awkwardness of my being personally much more positive than the reviewer in this excerpt from the decision letter:

That brings us to Reviewer 2's comments. As in previous rounds, Reviewer 2 makes some very good points about levels of analysis. As such, I would like to see you incorporate them even a bit more than you already have. That said, however, I do not feel as strongly as Reviewer 2 (#1) that "the underlying conceptualization of EC – its rationale, underpinnings, and content – is presented as *organizational* level." In fact, I think you've come *almost* as far as you can toward describing it as a "work unit" level construct, given the fact that the content of your measure often uses the word "organization" instead of "work unit." What I would like you to do here is to focus on seriously representing Reviewer 2's points 3 and 4 in the next version of the manuscript. I agree with the reviewer that because of the way things were measured, you need to spin more of a story about how consensus emerges "through processes that promote perceptual convergence." Personally, I was pretty much okay with the way you described things until about page 10.

I not only sent this letter to the authors, but also (as is *AMJ* policy) sent a "blinded" copy to each of the reviewers. A day later, I received a profusely apologetic e-mail from reviewer 2, indicating that s/he had mistakenly re-sent the review from the last round rather than the new review, which recommended acceptance with only very minor revisions. Whew!!

How could this have happened, given that the

authors had, as in previous rounds, sent detailed responses to reviewer comments? Wouldn't I have seen that the new review was the same as the one the authors had just responded to? The answer is actually no, because in the role of *editor* (as opposed to reviewer), I want to read each new version of a manuscript with fresh eyes. I am trying to discern the "gestalt" of the paper—to comprehend the forest rather than the trees, the holistic effect rather than the authors' responses to a myriad of specific concerns. As such, I do not read author responses to reviewers unless there is a very specific reason to do so, relying instead on my own reaction to the manuscript in combination with the new, updated round of reviews. I do not know whether this is the way other editors do it, but I have chosen this way because I want the way I see the paper to be as close as possible to the way an *AMJ* reader will see it. Because readers don't get to see the behind-the-scenes drama, I don't want to, either.

There is one last amusing part to this story. In Scott's essay, he says:

To counteract [the feeling of rebuke from an R&R], I try not to delve too deeply into the details of the reviewers' comments when I first receive the letter. Instead, I read quickly through the editor's comments and give myself some time—at least one weekend—before actually starting to struggle with the specific content of the reply. I let myself have a little celebration, administer whatever self-reinforcers seem appropriate, and build up that ever-precious supply of self-efficacy for the next part of the effort.

Apparently, this is a man of his word! When I e-mailed him the great news that he should scrap the comments from reviewer 2 because reviewer 2 in fact now really liked the paper and had almost no remaining concerns, Scott wrote back that he hadn't actually read the recently sent reviews yet. He had just read the decision letter and was still in celebration mode.

After all this, did the reviewers and I "know" that the paper was special? Although I realize that editors and reviewers are often not good at predicting the subsequent fate of the manuscripts they receive (Gottfredson, 1978), I'd like to think that maybe we did. In my final letter to the authors, I wrote the following:

On a personal note, I would like to tell you that I believe the manuscript now reads extremely well and is very cogently argued. I think this may become a very important paper—an exemplar for careful multi-level research. Given how difficult it is to publish in the *Journal*, your hard work, careful analyses, and perseverance have produced a major ac-

complishment. I know that the revision process has not been trivial, so I offer my heartiest congratulations!

## TWO LAST THOUGHTS

In their contributions to this Editors' Forum on the Review Process, Rajshree Agarwal and her colleagues; Don Bergh; and Scott Seibert have all offered a few personal observations that stand out as key in their experiences. I will do the same. However, because so much has already been said along these lines, I limit my advice to two points.

### Make Good Use of Your Emotions during the Revision Process

I'm not going to suggest that you deny your emotions in responding to reviews of your work. First, I think it is impossible to do so, particularly if you are not sure that you can deal successfully with the reviewers' comments. Second, attempting to suppress your emotions will not produce the best outcome for your paper. As Damasio (1994) has shown, emotional reactions are essential for effective responses in social exchanges, and review processes are indeed "social exchanges." Third, noting the issues over which you have very strong emotions can sometimes help you to discover what you believe to be the core, nonnegotiable aspects of your manuscript. For Agarwal and colleagues, for example, the decision not to split their paper into smaller subparts became a core, nonnegotiable issue in the face of reviewer 3's suggestion to do otherwise.

Therefore, I agree with Agarwal and her colleagues, who suggest that it was "helpful to experience the feelings the reviewer comments evoked." However, I also agree with their assessment that "these 'normal' reactions could not, however, become our guiding spirits; instead of making the review process a test of our self-esteem, we recognized the need to deal with it rationally and professionally."

Allowing yourself to emote *outside* the review process decreases the chances that you will do so inappropriately inside of it. So, how do you deal with some of the more difficult emotions? Are you angry? Try humor. Buy a voodoo doll, name it "reviewer 2," and get yourself a big bunch of pins. Write a limerick: "There once was a reviewer named Three, who a great scientist pretended to be. . . ." Paraphrase the reviews and make them even worse than they are, as Seibert did in the opening quote to this essay.

What if you are anxious, perhaps bordering on desperate? (This happens, of course, particularly

when tenure decisions loom.) Here, I have seen a number of things work. One of the most common solutions is mentioned by both Agarwal and her coauthors and by Seibert: writing out reviewer concerns and addressing the easiest ones first. Susan Ashford (1996) describes a variant of this process:

One practice has helped me remain open to reviewers and to keep my spirits up for responding. Shortly after I receive reviews, whether the decision is a revise and resubmit or a reject, I break each review up into specific points. I then create a setup for responding to each review, a template in which I state the reviewer's points in order, each followed by "Response:" and room for my future response. Thus I create a lengthy document, most of it blank, awaiting my responses. No matter how gloomy I feel after receiving the reviews, I get this done. Then, after several days (sometimes weeks), I pick up the response templates and begin by answering those points that are easiest (sometimes all I can do at that time is fix typos and find missing references if my self-efficacy is still low). Once I am engaged in the task and the task is broken into small pieces, I generally can keep going and soon build a head of steam to deal with the more difficult issues. (Ashford, 1996: 125)

A second approach that many people use is to try to suppress the full surge of emotion that comes with the first read of the R&R letter. Seibert "quickly reads through the editor's comments" and gives himself some time before digging into the reviewers' specifics. One of my colleagues asks her spouse to do the initial reading and to give her a "big picture" synopsis of what the editor and reviewers have said. Only after she has absorbed this big picture does she dig into the substance of the reviews on her own. Another friend takes the electronic file of reviewer comments and edits out all the "tonal" words that make him feel discouraged or combative.

However you do it, building up self-efficacy and positive affect facilitates the creative process (e.g., Amabile, Barsade, Mueller, & Staw, 2005). Susan Ashford takes the decision letter to a private place and conducts a mental preparation, reminding herself that "the feedback I am about to receive is about my *work*, not about *me*, and that my work is only a small part of myself" (1996: 124). Seibert "administers whatever self-reinforcers seem appropriate, building up that ever-precious supply of self-efficacy for the next part of the effort." However you do it, it is important that you reward rather than punish yourself for surviving this first hurdle.

### See Your Situation through the Editor's Eyes

One thing I never really appreciated until I became an editor is how different the role is from that of a reviewer. Prior to becoming an editor, I thought that my job as an author was to sway each and every one of the three reviewers—if I did that, surely my paper would be accepted. And in 99 percent of cases, it is probably true that if you manage to swing all three reviewers to your side, the editor will not disagree. But note the "if" in my last sentence. What about those (many) cases where one reviewer simply does not like your manuscript and cannot be swayed? Or where one reviewer is positive, one negative, and one on the fence, even after two revisions?

What I had failed to appreciate prior to my editorial role is that *it is ultimately the action editor—not the reviewers—who makes the decision on a manuscript*. This means that in the end, it is the editor who must be persuaded. Knowing this, one of my main pieces of advice is to try to conceptualize the entire process from the perspective of the editor. Of course, persuading the editor is much easier to the extent that you can also persuade each reviewer but, as we have seen, sometimes the editor must "break ties" or even go with a minority view. Thus, it is important to keep the editor's broader view and ultimate decision-making power in mind.

Why does the editor have such a strong role?<sup>1</sup> Is it because the editor knows more about the topic of the paper than the reviewers? Probably not. Reviewers are often chosen specifically for their expertise in the core areas of a manuscript. Although action editors, too, are matched with manuscripts' subject matter and methods, the connection is frequently looser. Rather, one key reason each *AMJ* editor has so much discretion is that he or she has the most complete set of information from which to make a decision.

Each reviewer approaches a particular manuscript with certain predispositions, training, and beliefs. These background characteristics cause reviewers to zero in on certain aspects of a manuscript while ignoring others, or to agree with some points while disagreeing with others. In addition, because the *set* of three reviewers is often selected precisely to provide these differing perspectives, reviewers' reactions to a manuscript may not converge. Indeed, they did not converge in the Agarwal et al. process, and they almost didn't converge for Seibert et al., either. Someone has to decide, and the person with the most information—the edi-

<sup>1</sup> Other journals may give less authority to their action editors than *AMJ* does.

tor—is usually the one in the best position to make a well-informed decision.

A second reason for vesting considerable authority in the editors, at least at *AMJ*, is that our editorial team and review board are selected on the basis of merit as reviewers. In other words, most of our associate editors have been promoted to the position by virtue of their excellence as reviewers, and most members of *AMJ*'s review board are selected on the basis of their strong performance as ad hoc reviewers. Therefore, while action editors may not be experts in all specific subjects assigned to them, they are generally at least conversant in the broad area and have a long track record of polishing gems and distinguishing between fixable and fatal flaws in manuscripts.

Although the idea of a strong editor may strike some as unattractive, I think it offers some real benefits to authors. For example, as an author, I used to worry about such things as whether the editor had noticed that reviewer 1 was in conflict with reviewer 3 about appropriate methodology, or that reviewer 2 was misinformed in alleging a "fatal flaw" in the design, or that reviewer 3 had written comments that were very difficult to respond to without appearing confrontational or groveling. As any author who has faced an R&R knows, these are not trivial concerns.

In those situations, trying to look at your manuscript through the eyes of the editor, who is sitting with the manuscript and all three reviews, may be very clarifying in terms of developing an overall strategy for your revision. It can also be very helpful in shaping the letter to the editor describing the changes that have been made. The first part of an author's formal response to the reviews is an executive summary of the major changes in the manuscript, and the second is the response to the editor's comments; responses to reviewers come last. The front end sets the stage for the responses to individual reviewers and, in that role, is probably the most important part of the letter—not to mention the one most likely to be read by all three reviewers.

Finally, after reading Agarwal et al.'s and Seibert's editorials in this forum, I would be remiss if I didn't also reinforce their suggestions that you keep the core idea of your contribution in mind as you consider both the editor's and the reviewers' comments. In Seibert's case, he and his team never wavered from the core message. In discussing his response to reviewer 3's "So what?" question, he says:

As has been said many times, "The reviewer is never wrong," but there are some comments that you sim-

ply must disagree with if you value your work or hope to have it survive the review process. How do you deal with this question and, more generally, with reviewers with whom you basically disagree? . . . In the case of our paper on empowerment, we decided not to disagree with one reviewer about his or her assessment of our contribution. Rather, we simply emphasized the main points of our contribution in the clearest way possible. . . . In the end, I think the reviewer accepted the third point, but that acceptance and a strong action editor were enough.

In Agarwal et al.'s case, the authors believe they were diverted somewhat from the paper's core contribution in the first round of revisions by trying too hard to respond to each and every reviewer comment:

We spent intense effort in not only addressing each and every concern, but more importantly, in trying to incorporate them all as we "integrated across" a wide swathe of literature in order to create as comprehensive a story as possible. Treating every comment as critical, we lost coherence and focus. We gained considerable insight into each specific concern and learned immensely from the attempt to integrate everything, yet we ended up with a manuscript that was appropriately judged as "longer, less readable" and so "presenting arguments not relevant to the main story."

One way to try to avoid this happening is to treat the core contribution of your paper as the touchstone of the whole process, and the editor's decision letter (rather than the individual reviews) as the mirror in which the manuscript is reflected.

Clearly, the fact that editors have lots of information from multiple reviewers does not make them infallible. Indeed, I can remember a few cases where I still wonder whether I made the right decision. However, the combination of three expert reviews from individuals who (usually) do not know the identity of the authors<sup>2</sup> does make editors relatively well-informed and, in most cases, well equipped to make good decisions.

Fn2

### Implications of Studying the Best of the Best

Although I believe that we can learn a great deal from studying exemplary papers and the backstage processes that produced them, it is also important to be aware of the potential limitations to their generalizability. For example, these papers obviously started with some "big ideas" that enhanced

<sup>2</sup> I say "usually" because we cannot verify this in every single case.

their chances of acceptance from the very beginning. As Seibert says:

Most of what you have to work with comes from the original intent and design of the research project. No amount of craftsmanship in constructing a reply can make up for a lack of basic theory, limited contribution, or a poor research design. The art of the reply is critical for a diamond in the rough but won't transmogrify a lump of coal.

Second, the authors had already exerted a *lot* of effort on their first drafts, as all the reviewers noted. These authors did not rely on the reviewers to clean up messes that should never have been submitted in the first place, and they gained the reviewers' respect as a result.

Third, they were very open to feedback, as their papers reveal. Elsewhere, Ashford (1996: 125) has commented on how important openness to feedback can be to career success:

In evaluating our PhD students recently, one faculty member summarized the discussion of seven different students by saying that what seemed to differentiate the top and bottom performers was their openness to feedback. Top performers were open to faculty input, actively sought it, and incorporated it into their work. Less effective students were defensive regarding feedback and, occasionally, openly hostile.

Fourth, Agarwal and her colleagues' and Seibert et al.'s cases ended well—very well, actually. As such, their experience was different from those of about 90 percent of the authors who submit their manuscripts to *AMJ* and other top-tier management journals. It is almost certainly easier for people like Scott Seibert to feel that “reviewers are an author's best friend” than it is for those whose papers have been rejected. Still, if my own experiences as an author are any guide, the passage of time often allows a person to see the wisdom of a reviewer's comments, even if those same comments felt incredibly unfair when first received. I can remember a few cases, for example, where reviewers' comments were so negative that I decided to abandon certain lines of work. Although the reviewers didn't feel like “friends” at the time (and in some cases, still don't), the fact that they prodded me to shift directions was probably a good thing from an overall career or life perspective. And I can hon-

estly say that the negative emotion from those cases is now gone—something I would not have believed possible in one particular case.

Despite these caveats necessitated by the rather unique circumstances of the review processes described here, I believe there is great value in seeing how master craftsmen (and women) navigate the revision and resubmission process. Even after 25 years in this field, I learned something from reading these editorials. I hope you did, too.

## REFERENCES

- Agarwal, R., Echambadi, R., Franco, A. M., & Sarkar, MB. 2004. Knowledge transfer through inheritance: Spin-out generation, development, and survival. *Academy of Management Journal*, 47: 501–522.
- Agarwal, R., Echambadi, R., Franco, A. M., & Sarkar, MB. 2006. REAP REWARDS: Maximizing benefits from reviewer comments. *Academy of Management Journal*, 49: 191–196.
- Amabile, T. M., Barsade, S. G., Mueller, J. S. & Staw, B. M. 2005. Affect and creativity at work. *Administrative Science Quarterly*, 50: 367–403.
- Ashford, S. J. 1996. The publishing process: The struggle for meaning. In P. J. Frost & M. S. Taylor (Eds.), *Rhythms of academic life: Personal accounts of careers in academia*: 119–127. Thousand Oaks, CA: Sage.
- Damasio, A. R. 1994. *Descartes' error: Emotion, reason, and the human brain*. New York: Quill.
- Gottfredson, S. D. 1978. Evaluating psychological research reports: Dimensions, reliability, and correlates of quality judgments. *American Psychologist*, 33: 920–934.
- 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* (2nd ed.). Thousand Oaks, CA: Sage.
- Seibert, S.E. 2006. Anatomy of an R&R (or, reviewers are an author's best friends.) *Academy of Management Journal*, 49: 203–207.
- Seibert, S. E., Silver, S. R., & Randolph, W. A. 2004. Taking empowerment to the next level: A multiple-level model of empowerment, performance, and satisfaction. *Academy of Management Journal*, 47: 332–349.