

## FROM THE EDITORS

### Raising the Bamboo Curtain

Benjamin Franklin said, “If a man empties his purse into his head, no one can take it from him.” In my position as an associate editor of *AMJ* for the past three years, I have had a great deal emptied into my head. As the present editorial team’s term comes to a close, I would like to place some of that back into my purse, leave the purse lying about in plain sight, and hope that others might help themselves to something valuable from it.

At the first meeting of our editorial team, Tom Lee noted that one of his goals for the journal was to “raise the bamboo curtain,” thus affording a clearer view of how the journal operates. We have worked hard to try to meet this goal. By the time our term ends in January 2005, I will have represented *AMJ* as a panelist or presenter at regional conferences, national conferences, and university colloquia more than 15 times, with each appearance aimed at providing a better understanding of the publishing process in general and of publishing in *AMJ* in particular. (I haven’t been alone on this; all of our team members have been active on this front.)

The authors who attend these sessions—some experienced and some aspiring—raise insightful and challenging questions. In the spirit of raising the bamboo curtain, I would like to offer some insights about three themes that emerge regularly in these sessions: (1) Prior to submission, what can I do to improve my chances? (2) Once the review process is underway, what is fixable, and what is not? and (3) Are there things that can save a paper from failure?

Then, after addressing those questions, I’d like to offer up an important secret that many authors do not know.

#### Improving Your Chances before You Submit

As it is with any quality journal, publishing in *AMJ* is difficult. Our acceptance rate hovers around 10 percent. But daunting as that figure may seem, it is possible to improve significantly upon that percentage by attending to a few basic issues prior to submitting your paper for review.

**Content and style.** One of the simplest ways to improve your chances is to familiarize yourself with the journal’s domain and style. The *AMJ* Web site ([aom.pace.edu/amjnew](http://aom.pace.edu/amjnew)) does an excellent job

of defining what *AMJ* does and does not attempt to do and provides well-defined guidelines for content, format, and style.

Errors on some of these fronts are immediate deal-breakers. For example, Tom Lee tells of manuscripts arriving on his desk that do not contain any data. Clearly, that would be a difficult sell for the Academy’s flagship empirical journal. Violating other guidelines may lead to significant slowdowns in processing reviews of your work. For example, 60-page submissions will generally be returned for editing, as our guidelines call for a maximum of 40 pages in most cases. Other guidelines, such as those involving format, structure, and reference style, are relatively minor by comparison and your flouting them may simply cause reviewers and editors to work harder than they need to in order to follow your story. But even errors in those areas increase the chance that your work will not be fully understood or appreciated. The other problem with errors of this type is that they may create a negative halo, one that may cause reviewers to evaluate other aspects of your paper more negatively, too. (“If they weren’t careful about headings, how about the analyses? And can I expect them to be careful with revisions?”) In all cases, these potential pitfalls are easily addressed prior to submission; it should not take a round of reviews to get format and structure right.

**Contribution.** Less easily self-assessed might be the contribution a study is capable of making. Is it new? Is it big? Is it theory-based? Is it surprising? Is it important? These are difficult questions that involve subjective judgments. However, my colleagues have offered some excellent insights into what constitutes a significant contribution (in, for instance, the editorials by Sara Rynes in April 2002 and Don Bergh in April 2003), and I encourage every potential author to take a few minutes to reflect on their thoughtful observations.

**You need a tough friend.** Of course, like many authors, I usually believe my own work hits all of the marks when it comes to interesting, important, and significant ideas and results. Yet, like many, I find my work rejected too often to suit my tastes. But although the vagaries of the peer review process are the subject of many scholarly conversations, I have learned something important while

reading and evaluating well over 1,000 reviews in the past three years: *The reviewers are not idiots*. There are many reasons why papers are not successful. But if I might be so bold as to propose “Schminke’s Law,” it is this: “If the reviewers don’t understand my paper, it is *not* their fault. If the reviewers don’t understand my paper, it is *my* fault.”

The solution? You need a really tough friend. Not someone who will simply blow smoke at you and confirm all of the positive aspects of your paper. You already know about those. What you need is someone who agrees to provide the toughest possible review, a person whose role it is to “outcritique” even the toughest potential reviewer. You need someone who will read your paper and then recite back to you what they think your paper was about, what you found, and what it means. If your tough friend cannot do that easily and accurately, without coaching or correction, your paper is at severe risk of being misunderstood by the editor and reviewers, too. Of course, not all friends are cut out for this role, and not all authors are adept at receiving such realistic critiques. (Warning: Friends often know this.) Therefore, I suggest a working partnership in which you and a tough friend perform this difficult task with each other’s work. You’ll share the pain and the benefits, and you’ll go through fewer friends.

### So What Can Be Fixed?

I have not been fortunate enough to accept a manuscript for publication after only one review. It seems that no submission is perfect and, thus, the question facing editors and reviewers is what to look for when determining whether a manuscript has a reasonable chance of successfully overcoming its inherent limitations. If we assume that all papers come to the table with flaws, how do we know which of these are fixable and which are not?

Of course, sometimes no single issue is fatal, but the cumulative effect of problems on many fronts contributes to a death of a thousand cuts. However, if most of a manuscript is sound, even significant issues—in isolation—are often surmountable, including those related to theory development, framing, and analyses.

**Theory development.** The *AMJ* mission states, “All articles published in the *Journal* must make a strong theoretical contribution.” Thus, theory lies at the heart of what we do, and often occupies a front-and-center position in reviews and decision letters.

One important question is whether a study’s hypotheses actually test the theoretical foundation

being offered. Are the relationships presented really key to, say, agency theory? Do your hypotheses really allow you to test an important tenet of institutional theory? If the answer is no, then the paper contains a serious flaw. However, such concerns are often fixable by modifying either the hypotheses or the theory.

This observation suggests that the theory offered as the foundation for a study need not be carved in stone. Nor, of course, is foundational theory like a box of chocolates, something to be tested with a series of small bites until a center that is to one’s liking is discovered. But reviewers provide all sorts of insights with the potential to strengthen the contribution of a paper. It’s been said that science isn’t about replacing theory that is wrong with theory that is right. Rather, it’s about replacing theory that is wrong with theory that is more subtly wrong. Thus, a more suitable or robust theoretical foundation might be uncovered during your review and revision process, and it would be inappropriate to ignore such a potential improvement.

**Framing.** A second front on which papers can be saved involves how they are focused or framed. The fundamental question here is, “What is this paper really about?” For any set of theory, data, and analyses, there is more than one potential answer to that question. A study might *include* five different constructs, but it can’t be *about* five different constructs. It might explore the relationships between leadership, ethics, organizational structure, organizational life cycle, and performance, for example. But it can’t be *about* leadership and ethics and structure and life cycle and performance. Each of those literatures has its own language, its own issues, its own theoretical foundations, and its own problems to be solved. Integrating all of these presents a wonderful opportunity to speak to a number of potential audiences. But at the end of the day, the study has to be *about* one of the constructs. It needs to be framed from that perspective, and a case needs to be made for its importance to the literature on that construct.

As does theory development, the review process often uncovers insights about the real “rich bit” a study offers—that little gold nugget of insight—that has the best chance to make a real splash in the field. A reviewer or editor may see a major potential contribution where an author saw only a minor one. And again, such adjustments to how a study is framed are not only fixable, but represent a prime opportunity for turning a smallish paper into an important one.

**Analysis.** Let’s start with an observation. No study has ever been rejected from *AMJ* because it used regression analysis rather than structural

equation modeling. Of course, reviewers often identify particularly appropriate or robust tools, ones that offer a better fit to the research question or the data. But I don't recall a reviewer ever recommending rejection because of an author's analytical choice. At worst, inappropriate or insufficient analytic tools call for a second look to determine whether existing effects are robust to more suitable analyses, or whether latent effects might emerge more clearly with improved techniques. Analytic procedures are eminently fixable and, thus, are almost never a fatal flaw for a paper.

### What Cannot Be Fixed?

So if theory, framing, and analyses are all fixable (at least in isolation), what things are not? There are several. Among these are poor data, poor measures, an uninteresting research question, and extraordinarily poor writing.

**Data.** Suppose your research question is longitudinal, but your data are cross-sectional. That's a very serious problem. Likewise, if your data on both the independent and dependent variable sides of the equation are exclusively self-reported, entirely perceptual, collected by a single method, and provided by a single informant—that, too, is a serious problem. Authors often attempt to address individual concerns like these by conducting a one-factor test for common method variance or by citing many other researchers who have used similar data. And, in isolation, reviewers and editors are usually willing to live with some of the weaknesses inherent in any particular data collection effort. But, again, the *cumulative* effect of limitations like these may eventually exceed a tipping point, a point at which reviewers and editors can no longer place sufficient confidence in the data to justify an acceptable level of confidence in the study's results.

**Measures.** Serious concerns with measures arise on at least two fronts. The first involves whether the measures employed display adequate psychometric properties. Are they reliable? Are they valid? Do they suffer from social desirability concerns? I know of no editor who would reject a paper solely because the reliability of a single measure fell slightly below .70 or because confirmatory factor analysis resulted in an RMSEA of only .06 or a CFI of only .91. But most editors would have considerable difficulty pursuing a manuscript in which several key measures displayed only marginal reliability, CFAs did not demonstrate at least moderate fits, theoretically independent constructs displayed problematic levels of multicollinearity, and so on.

The simplest solution to this problem is due diligence *prior* to undertaking a study. Smart people have studied lots of things. Chances are good that somebody has looked at a construct very similar to yours. Chances are also good that somebody has developed a sound measure of it, too. A thorough search of the literature—and I do mean thorough—is likely to uncover one or more well-established measures that are far less likely to draw significant fire from reviewers or editors. Real estate investors follow the rule of OPM (other people's money) whenever possible. Researchers should follow that rule, too, using *other people's measures* whenever they can.

The second issue related to measures involves misfit between the constructs about which a study theorizes and the measures employed to test the relationships between them. A thought problem from philosophy asks the question, "If you call a dog's tail a leg, how many legs does a dog have?" The answer, of course, is four. Calling it a leg does not *make* it a leg. However, many studies seem to try. Editors recognize that in many research domains, perfect measures of important constructs do not exist. Often, proxies are—at best—fairly rough indicators of important underlying constructs. We recognize that, and can often live within those limitations. But studies that rely on tails posing as legs are not likely to be fixable.

Thus, the only real solution is searching for appropriate measures *before* you begin your research. A great place to begin is with publications that provide collections of scales, their histories, and their characteristics. Jim Price's *Handbook of Organizational Measurement* (most recently reprinted in 1997 in the *International Journal of Manpower*) and Robinson, Shaver, and Wrightsman's *Measures of Social Psychological Attitudes* (Academic Press, 1991) are just two examples of excellent starting points. Remember: OPM.

**The research question.** The issue of significance of contribution has been addressed elsewhere, so I won't delve into it much more deeply here. However, we often see a paper provide—as the primary justification for its existence—evidence of a "hole" in a literature. Usually, the authors have it right; nobody has explored the moderating effect of A on the relationship between B and C. And sometimes these studies are extraordinarily well executed. But as a former colleague used to state, "Research not worth doing is not worth doing well." In other words, not all holes need to be filled.

**Writing.** Manuscripts sometimes arrive in such poor condition with respect to grammar, spelling, and style that even our most conscientious reviewers cannot make heads or tails of them. Occasion-

ally, problems are due to the challenges faced by authors for whom English is not their first language. The international mission of *AMJ* makes us sensitive to that situation, and my experience has been that our reviewers are patient, gracious, and accommodating in dealing with such challenges. But from time to time we receive manuscripts that are so opaque, so poorly written, and so difficult to decipher that the quality of the research question—or even its *potential* quality—simply cannot be deciphered. Perhaps it is unfortunate, but more often than not, this problem is not fixable. For a paper to get a second bite at the apple, reviewers or editors must have some chance of sighting and recognizing the “rich bit.”

Think of it this way: When I go to the hardware store to buy drill bits, I don't want drill bits. What I want is holes. The same is true for us at *AMJ*. We invite manuscripts and revisions, but we don't really want manuscripts and revisions. What we want is *AMJ* articles. *Great AMJ* articles. Your project may be of *AMJ* quality, but if your paper does not allow reviewers and the editor to see that *AMJ*-level article within, or to see how one is likely to develop, it is not likely to be successful.

### What Can Save a Paper?

In view of all of this, is it possible to identify the characteristics that can save a paper from failure? Are there trends in the sort of things that lead reviewers and editors to take a second look at a paper, even one that might be saddled with more than its share of weaknesses? The answer is yes.

**Topic.** An especially interesting topic is capable of pulling a paper back from the brink. Let me provide an example from outside the domain of management research. I recently read about a series of studies performed on capuchin monkeys. It seems that these monkeys like cucumbers but *love* grapes. When two monkeys are asked to perform independent (but equal) tasks in exchange for a cucumber slice, they happily comply and gladly accept the reward. However, if one of the monkeys receives a grape for performing the task (or for not performing at all), the other monkey will often throw away the cucumber slice or refuse it entirely. Even monkeys are able to recognize unfair treatment, and react to it negatively!

As someone interested in organizational justice research, I am fascinated by these studies. I find myself talking about them even to people who don't do research in the area. In fact, I find myself telling friends and family who aren't researchers at all. Of course, not everyone is as fascinated by them as I am, but from time to time we come across

submissions at *AMJ* that evoke similar reactions; this research is just so interesting that I want to go tell people about it, even scholars outside of management or individuals outside of academia. Papers like that get a second look.

**Data and measures.** A second feature that may compel reviewers and editors to look even more deeply into a paper's potential is a compellingly unique data set. Student samples, EMBA participants, and *Fortune* 500 CEOs all have their places in our research. But it would be difficult for most reviewers to dismiss quickly a quality data set comprised of research teams stationed in Antarctica over 20 years, rocket launch crews from NASA and Russian space programs, Navy SEALs teams, or the top draft picks from the NBA. We might know little or nothing about the specific contexts in which these participants execute their work. But we know interesting data when we see it. If the measures using those data are sound and measure what they purport to measure, we are likely to take a hard second look at the studies.

### The Secret

In sessions related to publishing, I'm often asked some version of the question, “So are there any secrets we should know about?” The answer is yes. The secret that many authors do not know is this: *We want to publish your study.*

It's been said that reviewers may look for reasons to reject papers, but editors look for reasons to publish them. The second part of that statement is undoubtedly true. From the very beginning of the review process, from the moment your manuscript first lands on my desk, your interests and mine are perfectly aligned: *We want to publish your study.* (And when we publish it, we want it to be as interesting, sound, and impactful as it possibly can be.) That does not always happen, of course, but the phrase “publish or perish” is exactly on the mark. *If AMJ does not publish, it perishes.*

It should also be noted that this alignment remains true deep into the review and revision process. We view ourselves as members of the profession first and representatives of *AMJ* second. Thus, even if things don't work out as planned at *AMJ*, our reviewers and editors work closely with authors toward a common goal of improving articles so as to reach their fullest potential.

### Conclusion

Like most rewarding things, publishing in *AMJ* is difficult. But it is not impossible. A perusal of recent issues of *AMJ* reflects the fact that we em-

brace high-quality research from a variety of perspectives, performed by scholars from around the world. A great many of these hail from institutions other than the largest and most research oriented, and many perform research considered to be outside the “mainstream.” The system is necessarily rigorous, but it is as close to a pure meritocracy as I can practically imagine.

Predictable issues sink manuscripts, and equally predictable ones can save them. My hope is that the comments I have provided here may have raised the bamboo curtain an inch or two, providing a little additional insight into the process. Of course, my experience at *AMJ* has been a personal one. Other individuals have different insights to offer, as evidenced by these editorials in the *Journal* and other fine essays, like Richard Daft’s (1995) “Why I Recommended That Your Manuscript Be Rejected and What You Can Do about It” (pages 164–182 of the second edition of L. L. Cummings & P. J. Frost’s *Publishing in the Organizational Sciences*). It is my hope that readers will try to learn all they can about this important topic. It is also my hope that, as a result, you will continue to think of *AMJ* as the journal of choice for your best work.

### Closing Thoughts

A final and somewhat unrelated note is in order before I close. This is a rewarding job, and a difficult one. However, it would be far less rewarding and far more difficult (impossible, in fact) without the help of many incredibly talented individuals. The past three years have afforded me the pleasure of working with some of the smartest, most committed people I’m ever likely to meet. And, amazingly, to a person, they were also individuals of unfailing good cheer and optimism. I love working with people like that, and on those days in which the intrinsic rewards of the job were almost not enough to keep me going, these people were.

I cannot name them all, of course, but the list would begin with my department chair, Foard Jones, and my dean, Tom Keon, who provided the support that allowed me to accept this position. My colleagues at the University of Central Florida

picked up the burden created by my almost exclusive attention to *AMJ* over the past three years. My assistants throughout this—Jessica Ramirez, Marie Mitchell, and Diane Sullivan—pulled my tail out of the fire (and perhaps, more importantly, kept it from falling in) more times than I can possibly count. And the staff at *AMJ*, past and present—Tonia Allred, Persephone Doliner, Martin Evans, Nancy Grandjean, Jennifer Isom, and Janet Thompson—provided a rock-solid home base that all of us knew we could turn to at any time for information, assistance, and perhaps most importantly, an upbeat word or something to laugh about when things didn’t go quite right.

I cannot sing the praises of our reviewers loud enough, both our “ad hocs” and those wonderful people whose names grace each issue as members of our Editorial Board. I cannot name them all here, but a special few have been more than essential to me. You know who you are, and when things got tight, I pulled water from these individuals’ seemingly bottomless wells time after time. Their insights and professionalism carried the day more times than I can recall. And none of them ever let me down, not once.

My colleagues as associate editors—Don Bergh, Dov Eden, and Sara Rynes—have made me proud to be part of this team every one of the thousand days we’ve spent together so far. Their talent, insights, compassion, and facility with this job humbled me each time I interacted with them. And my mentors in this role—Maureen Ambrose, Jeff Edwards, Greg Northcraft, and of course, Tom Lee—provided better professional guidance than anyone has ever had in any job, period.

As others have said before me, this has been an incredible experience. Nevertheless, I’m terribly anxious to get back to my own work, my own students, and my own colleagues. I don’t know yet how much I’m going to miss this job; I suspect it will be a lot. But holy cow, I’m going to miss these people.

Marshall Schminke  
Orlando, Florida

Please note that Professor Sara Rynes (University of Iowa) begins receiving new submissions on July 1, 2004, and that she will assume the editorship on January 1, 2005. Beginning July 1, 2004, send new submissions to her at [amj-srynes@iowa.edu](mailto:amj-srynes@iowa.edu).

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