

## FROM THE EDITORS

### OBLIGATIONS AND OBFUSCATIONS IN THE REVIEW PROCESS

I'm *that* guy.

Yep, I'm that clown who torpedoed your paper. I'm that lamebrain who couldn't tell a contribution from a confabulation. I'm that ingrate who wouldn't know good scholarship if it plowed over him in a full-sized SUV hauling bathroom tiles. I'm that arrogant, know-nothing cretin who turned the editor away from your brilliant, ground-breaking work. I'm all those other things you were thinking of calling me, and worse.

Because I'm that guy—a long-time *AMJ* reviewer—and because I have other experience in ruthlessness as chair of *AMJ*'s advisory committee and as an associate editor of *Organizational Behavior and Human Decision Processes (OBHDP)*, Tom Lee asked me to write a short bit about how to balance reviewing with other responsibilities. What he didn't realize is that I'm completely unbalanced. Instead of doing what he asked, I tortured his original request into a chance to address other issues of balance, namely, the frames of reference we all take on during the review process.

#### Reviewers are Authors

Everybody in our profession is one of *those* guys. Or gals. I'll go out on a (perhaps short) limb and say that we've all recommended rejection of some paper at some journal or conference at some time. We are all reviewers. And yes, simultaneously we are all authors. Do we adopt different identities as reviewers versus authors? Does Mr. Hyde creep into our personae when we put on a reviewing hat?

Apparently so for me. A few months ago, my eight-year-old daughter, Monica, knocked uncharacteristically softly on my home office door. "Daddy, are you doing PowerPoint or are you doing reviewing? 'Cuz if you're doing reviewing I'll wait 'til later to tell you about what happened all over the kitchen floor."

Monica knows I have fun when I'm doing PowerPoint. Sometimes she gets to sit on my lap

and do PowerPoint with me. We get to draw. We get to change colors and lines and shapes. We get to make something. On good days I feel that way when I'm writing my own papers. As an author, I am playing with ideas. I am creating.

In contrast, I almost *never* feel that way when I'm composing reviews or decision letters. And everyone I talk to seems to feel the same. Does the reviewing hat fit that badly? Why are we all so cranky as reviewers?

First, reviewing is supposed to be critical. It has to be. When we slip into a reviewer's identity, we buckle on our double-blind armor and assemble our verbal weaponry. We are defending the realm of the *Journal's* reputation; there may be barbarians at the gate. Less than 10 percent of what's submitted to *AMJ* will appear in print. It is a reviewer's job to help separate those papers from the other 90-plus percent that aren't quite as good. All papers have strengths. Those strengths should be and usually are duly noted. Yet the main purpose of the review process—to cull the best from the rest—inevitably focuses attention on a paper's weaknesses. So, reviewing seems more like destroying than creating. Finding and writing about what's wrong ain't fun.

Second, reviewing is mainly a volunteer activity. We do it because it is a *professional obligation* in our discipline, and especially in our own journals, something I fear is not being socialized strongly enough in freshly minted Ph.D.'s. Without voluntary, time-consuming, high-quality reviews, there would be no *AMJ*. There would be no *OBHDP*. There would be no pubs. There are few extrinsic rewards for reviewing. Occasionally, reviewers singled out for the quality of their work might be rewarded with a lifetime subscription to *Modern Reviewer*.

At *AMJ*, reviewing awards are meaningful, and in the long term, sustained good reviewing builds a positive reputation and a social network of grateful editors and colleagues. There is also much to be learned by taking this active role in the publication process. However, the rewards are intellectual and social capital rather than financial capital. From my own informal survey of colleagues, it is also clear that reviewing receives virtually no weight in individual performance evaluations. Indeed, no de-

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Thanks to Linda Treviño, Karen Jansen, John Hollenbeck, Dov Eden, Sarah Rynes, Marshall Schminke, Don Bergh, and Tom Lee for reining me in on previous drafts. I hope their comments keep me from getting kicked out of the Academy.

partment chair has ever asked me to provide a count of the number of reviews I do, or has asked to read any of my 250+ reviews. Moreover, good reviewing means almost nothing in the job market. In the short term, reviewing has low subjective expected utility.

Economics journals pay their reviewers. Go figure.

Third, reviewing takes boatloads of time. For most of us, time is our most precious resource. The opportunity cost of reviewing is brutal. I am a slow thinker and clunky typist. It takes me at least a full day to complete a decent review. If I weren't reviewing, I could be doing PowerPoint with Monica. I could be helping my son, William, build his Pine-wood Derby race car. I could be working on any of the innumerable and immeasurably more interesting research projects that have been languishing in my files. I could be taking a nap.

Add these briquettes together, toss in a few incendiaries, such as having been on the receiving end of a sneering review, or feeling that if someone else's paper is rejected there may be one more slot left open for one's own submission, and we have a roaring barbecue. The reviewing hat is the same goofy-looking white one we'd only wear leaning over an outdoor grill. How do we keep from charring each other?

### A Guy Walks into a Bar . . .

One thing I try to do is *lighten up*. To keep the latent crankiness from seeping into my prose, I set a difficult, specific goal of working at least two jokes or lame witticisms into each review package. It's a difficult goal because even one snicker is hard-won amongst us know-it-all academics. It's also difficult because most topics are off-limits or off-target given the stakes involved. The off-limits zone includes anything the author did (aside from perhaps a funny misspelling or tow). No one wants to be made fun of, especially while their paper is being sizzled. So, my humor is usually self-deprecating. I note some dim-witted thing I said or did. There are plenty to choose from. Or, my humor is about something universally funny—like Elvis—unless the authors are from Memphis or Mississippi.

Another element of lightening up is to ease away from the stilted language we use in technical writing. Good reviewing is good conversation. We are authors; authors are us. As reviewers, we should speak to authors in the second person, rather than the third. Move from the declarative to the imperative. Go ahead, use contractions. Say "ain't." Say "dagnabbit."

Of course research and publishing are sober businesses. Reviews of an *AMJ* manuscript can send entire careers in spooky directions. Reviewers must take an author's ideas and efforts seriously. But as a reviewer, I hope I don't take myself too seriously.

### A Bill of Rights for Manuscript Authors

As authors, we know that negative journal decisions are much more likely to occur than positive ones. In fact, the ratio can be greater than 9:1 for top-tier outlets such as *AMJ*. We also know from research that the perceived fairness of decision processes is especially important for those who receive negative outcomes (Brockner & Wiesenfeld, 1996). Nobody who's ever gotten an "A" in my class has complained later about their final exam. How can we make reviews seem less unfair?

A bill of rights for authors is one, rather pretentious, answer to the fairness question. Each right outlined below is coupled with an example of how it can be violated or obfuscated. The rights themselves incorporate some of the basic findings from procedural and interactional justice literature, as well as principles for treating our own research participants decently (e.g., Leventhal, 1980; National Commission for the Protection of Human Subjects of Biomedical and Behavioral Research, 1979; Tyler & Bies, 1990). At best, this list of rights may spark some thinking and dialogue about reviewing. At worst, it may trigger a knowing smile.

**1. Manuscript authors have a right to respectful and courteous interpersonal treatment. Always.** As a budding professional and first-time reviewer, I mimicked what I saw—or what I thought I saw—in reviews of my own unsuccessful submissions. I sent in a six-page tome that steadily ratcheted up its negativity. By the end of the review, I was a screaming banshee. I oozed frustration at the dreadful paper that was wasting my time. I posed ugly rhetorical questions to the authors. When I eventually received the full packet of reviewer comments and the editor's decision letter, one line stood out: "Ignore the vitriole in Reviewer 3's report, and pay attention to his/her substantive points." Lesson learned. Unfortunately, it might have been learned at the expense of an author's goodwill, or worse yet, an author's self-efficacy for doing research.

Reviewing is not primal scream therapy. Keep the bluster and negative affect to yourself, *even if the authors have not and even if you think the paper is colossally bad*. Would you say those things to your spouse? Er . . . don't answer that. Would you say those things to your nieces and nephews?

Hunt for severe language: "extremely," "never."

"always," "bonehead." Purge it. Then, turn down the volume. As a general rule, the information quality of a written product—e-mail, review, student theme paper, or article submission—is inversely proportional to the number of all-caps words and exclamation points it contains. REALLY!!!!

Most times, the disrespect and discourtesy in a review aren't blatant. They linger just below the surface and pop up in code words. Here is one (real) example of how a reviewer might obfuscate this way: "The authors demonstrate a shallow understanding of research on \_\_\_\_." Here is how the authors will likely read that same comment: "You're too stupid or too lazy to stay current in the \_\_\_\_ area." A better, less inflammatory way to express a nearly equivalent criticism would be: "There are some important developments that might help to inform your ideas about \_\_\_\_\_. Those developments include . . ."

Other times, the disrespect and discourtesy are blatant. One tactless editor, in a response to a methodological wrinkle in a resubmitted manuscript, stated: "I took your idea about handling \_\_\_\_ down the hall to a colleague who has expertise in the area. He laughed." If authors could sue for a violated right in the review process, this is where the court documents would start.

**2. Manuscript authors have a right to full and careful readings of their submissions.** I started out my career doing reviews in bits and pieces, milking usefulness out of the times I'd normally be unproductive. I'd read a few pages during timeouts and halftime at my son's basketball games. I'd push the manuscript a little further bouncing home on a propjet. I'd punch through a couple more pages waiting in the dentist's office while the novocaine took hold. I might finish reading it while I hid in the cheap seats during a faculty meeting, the kind where we'd have another critical vote about charging undergraduates a Restroom Facilities Fee.

Once, a senior colleague snuck into the back of the room and caught me. "Waddya doin'?"

"Knocking out a journal review."

"Hmph. Sure glad it isn't *my* paper." Embarrassing pause. "What if it was *yours*?"

I hadn't entertained this twist on the Golden Rule before. But my colleague was right. Nobody wants their magnum opus to be sampled in bite-sized chunks while motions are being seconded. Dude, a manuscript isn't a commercial for Mountain Dew. It needs to be deeply processed and elaborated. It is a whole work; it should be read that way. Nowadays I won't start to do a review until I can first read through the paper in an uninterrupted, distraction-free, single sitting.

I know a former editor who processed manu-

scripts using an explicit, two-by-two matrix. One dimension is summarized by the question, Does it address an important problem (pose an important question)? The other dimension is, Does the investigation solve the problem (answer the question)? Judgments in yes-yes and no-no cells are easy. In the other two cells they're hard. I think this process is implicit in most reviewing situations. I also think a holistic, one-time reading is fairer and better for filling in the cells of that matrix than a stutter-step approach.

Violations of the second right are not always obvious. However, a one giveaway is when a review is little more than a list of unconnected trivialities. Another is when factually incorrect statements abound. Witness: "The authors don't provide a clear explanation of how they sampled the firms in this study." And pages 14–15 were about what, belly-button lint? Bless their hearts, authors who are asked for a revision respond with true penitence: "Sorry. Perhaps we weren't clear in our two-page exposition of Sampling and Data Collection. Now we've rewritten it in day-glo purple ink using words that have no more than five letters. Thanks for pointing out how easy it was to miss that entire section." As authors, each of us has probably encountered a review in which it was clear the reviewer didn't read the entire paper in one sitting—if they read the entire paper at all. In fact, I have a friend who reviews papers by skipping the Introduction, jumping into the Methods and Results, and then skimming the Discussion.

I'm sure glad they're not *my* papers.

**3. Manuscript authors have a right to expect criticisms of their work to follow the same standards of logic and evidence applied to themselves.** Reviewing is conversation, but it is contentious conversation. Arguments in those conversations need to be properly developed and supported. I believe this is one of the most egregious problems and most poorly handled parts of the review process. It is agonizing to us authors when our papers are derailed by a few overstated and unjustified claims made by seemingly all-powerful referees. Some common examples are given below. It is time we adopted a single, two-way standard. It is time to wear the author hat when writing major arguments in reviews.

*Everybody knows.* This is a frequently damning criticism of the interestingness or potential contribution of a paper. It takes almost no effort to state, as little as a single sentence or incidental phrase: "It is well-established that . . .," "hypotheses are simply not convincing . . .," "it is well-known that . . .," and "there is better (more integrated, more comprehensive, more articulated, more stain-

fighting power) theory . . . ." These can be level-headed, sensible statements, *as long as they are accompanied by relevant references, sustaining facts, and/or deductive logic.* These are maddeningly empty statements without such justification, equal to the grade-school comeback "I just know." Surely we wouldn't allow such arguments to go unsupported by *authors*. Why should they be acceptable for reviewers? If providing better justification and clearer explanation makes a journal review stretch an extra page, so be it.

And please, if citations are offered, enough information (i.e., author, year, journal) should be given to at least let the author find them on the first couple of tries in an electronic database. Let's end the wearisome afternoons tracking down cryptic references to "that famous guy who said that one thing about job performance back in the seventies." Once I had a reviewer cite "Smith, 19xx."

*Tyranny of pet ideas.* We all have favorite theories and paradigms. Unfortunately (for the review process at least), mine aren't the same as yours. Nor do I have a hammerlock on what it means to do organizational Science with a capital "S." There is seldom a justification for reviewer comments such as "It is hard to believe that in 2002 people are still studying \_\_\_." "Examining \_\_\_ in this way is no longer acceptable," "The area has moved well beyond this idea," "The constructs are too narrow . . .," or "The constructs are too broad . . . ." All of these are barely concealed versions of "mine is better than yours." Again, these can be fair statements if foundational theories in a research domain have changed, or if systematic evidence has set aside an earlier paradigm. Anyone remember cold fusion? But *those changes and that evidence need to be made explicit.* The logic underlying such claims must be as transparent as the logic leading up to the author's hypotheses and methods. And if I can only rustle up citations to my *own* work as support for such claims, my reviewer's arguments may as well be an Enron quarterly report. Yes, self-referencing is acceptable, as much in reviewing as in authoring—if I wasn't an expert, I probably wouldn't be reviewing the paper. But no, I'm not the only person to have written pithy things about the relative value of research ideas. There ought to be an acknowledgment of other work and opposing drumbeats (e.g., Schneider, Hough, & Dunnette, 1996).

*Methodological ascendancy.* We all have favorite methods, too: data-collection vehicles, measurement instruments, and statistical techniques. Increasingly often, however, those favorites crowd out methods that can yield equally good and perhaps unique insights. Methodological triangulation

is waning rather than waxing. As shown by Scandura and Williams (2000), fewer multistrategy investigations have been published recently in *AMJ* than in the past, which is profoundly disturbing. All of those past and current investigations came through the review process. Generic statements such as "Laboratory methods provide no evidence about \_\_\_ that could be ported to real organizations . . ." "Computer simulations in this context are merely garbage-in, garbage-out . . .," and "I vomit uncontrollably when I see the word 'policy-capturing' . . ." are examples of the kinds of methodological obfuscation that lead to substantive tunnel vision. They have no place in reviews unless a reviewer can pinpoint when and why their favorite method is discernibly superior (Spector, 1998).

**4. Manuscript authors have a right to expect criticisms of their work to be prioritized.** If I could make a State of the Reviewing Union Address, I'd say the era of big laundry lists is over. I think this reviewing technique is fading, but I recently received a report with 84 separate, unclassified points (hey, you'd have counted them too). It wasn't a lousy manuscript. Honest. But it did have a lot of missing punctuation marks where the reviewer thought I could have interjected a few commas and colons and semicolons here and there so that eventual readers wouldn't be all out of breath and gasping for oxygen by the time they finished a sentence or a paragraph or even a page or a subsection of the overall paper.

The Biggest Problem with this reviewing-via-spewing approach is that it doesn't focus author attention on The Biggest Problem. Of course, the editor is supposed to handle a major chunk of that task. Still, reviewers should send importance signals to authors (Schminke, 2002). As authors, we expect to shed some blood in our revisions. As reviewers, we should at least clarify the issues to shed it over.

Such a right doesn't translate into a purely transitive ranking of a manuscript's weaknesses. It might simply be a grouping of the one or two most troublesome features of the paper versus all the rest. I've seen excellent reviews where such points are split into "Major" and "Minor" issues, "General" relative to "Specific," or "Whoppers" versus "Trifles."

Problems can be ranked in two, partly overlapping, ways. The first way is the more straightforward, capturing and relating to the author their perceived severity in a paper. An almost universal and perhaps even shopworn criticism in the severe category is that a paper doesn't have compelling theory. Here's an inventory of verbal tip-offs: "The authors' conceptual framework is . . . chockablock-

... fragmented ... Rube Goldbergian ... piecemeal ... disjointed ... a hodgepodge ... higgly-piggly ... pell-mell ... [apparently someone else had *The Poky Little Puppy* when they were a toddler] ... a patchwork ... pastiche ... loose collection ... assortment ... Whitman's sampler ...," and, my all-time favorite, "... a gumbo." Anytime you can work Creole cuisine into a review, do it. This approach corresponds to what might be termed the "Fatal Flaw Model" of reviewing. In that model, if the investigation contains what the reviewer believes to be an unrecoverable error (e.g., nonrandom assignment in a lab experiment), that error should jump to the top of the review. But for that fatal flaw, and for all other major ones, I think it is requisite for a reviewer to offer suggestions on how it might be avoided the next time around.

The second way to rank problems involves abstraction of minor issues into larger categories. It might be termed the "Cumulation and Threshold Model" of reviewing. Too many problems and you've broken the reviewer's back. For example, one-item scales, homegrown measures, lack of a priori pilot work, and suspiciously high correlations between ostensibly similar independent variables might be abstracted into "Construct Validity Issues." This form of prioritizing moves what could be a clerical task of noting typos and awkward wording into an intellectual task of seeing smaller issues as manifestations of deeper and broader problems. Intellectual tasks are more fun.

**5. Manuscript authors have a right to get feedback about their work in a reasonable span of time.** I don't have time for reviewing. Nobody does. And the "juggling act" and "spinning plates" metaphors don't seem right for it. We make time for reviewing by *dropping* other balls and plates, often our own research and sometimes our own family responsibilities. Although it is a professional obligation, I'm not suggesting we must affirmatively answer every reviewing call. Selfish as it might sound, my saying yes to a review request involves whether or not I read papers in the requesting journal, and whether or not I'll submit papers there.

But I'm sidestepping the main issue. Is this author's right anything like a defendant's right to a speedy trial? How long is a "reasonable span" of time? I can only offer a confidence interval, and a fuzzy one at that. Six or seven months seems too long, even for reviewers stationed at the University of the Mid-Atlantic Trench. One and perhaps even two months seems too short.

Too short? Is that possible? Here I must adopt a stance that opposes current trends for faster re-

turn at journals, and even *AMJ's* own speedy goals. The rights I've outlined above call for more care, deeper thought, and greater idea germination in the review process. That takes more time. Published papers would be better if reviewers took more time. Did I say I'm a slow thinker and clunky typist?

Unfortunately, what we've all come to expect as a reasonable review span has become increasingly less time. I've had authors call me as an associate editor after six weeks and ask what stage of review their manuscripts are in. Even the letter that accompanies *AMJ* manuscripts states (and has for several years), "authors are understandably anxious about the fates of their manuscripts" and "I have allowed you four weeks to complete your review." Apparently, as a reviewer I should feel blessed by this temporal bounty I've been granted. I'm just as guilty at *OBHDP*, where we give the same short deadline. But at least we're not vicious about late reviews. One morning I woke to find a severed horse's head at the foot of my bed. A note was attached: "Your *AMJ* review was due *yesterday*."

So, for this final right I'd like to turn the tables again. Putting the more comfortable author hat back on, we need to be more patient about receiving reviews. That might make the reviewing task a little less onerous. It also might make those reviewers—us reviewers—a little less cranky.

### Responsibilities

Speaking of little, would the review process improve a little if we all lightened up, or if we adopted this mini bill of rights? Are there five, parallel, author *responsibilities* that would balance these rights? I don't know. I'm still unbalanced. More importantly, I need to get back to doing PowerPoint.

I want Monica to come in this time.

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#### Editor's Note:

For your information:

Trieschmann, Dennis, Northcraft, and Niemi published a ranking of business school research performance in *AMJ* in 2000. Recently, they updated their rankings. These updated rankings can be found at [www.kelly.iu.edu/ardennis/rankings](http://www.kelly.iu.edu/ardennis/rankings).

Tom Lee  
Seattle







