

# A Reviewer's Gold

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More than 15 years ago, when as a “fresh” scholar I submitted my first manuscript for review at a major journal, I had an experience that was remarkable only in retrospect. At that time, it appeared ordinary. After 3 months of submission and a wait that was measurably longer, I got the envelope back—a 9 × 12 brown packet that contained a fresh scholar’s first brush with the review system. The editor’s letter was unenthusiastic. True to folklore, the reviewers disagreed both about the contribution of the article and the proper direction for revision. True to probability laws, one of the reviewers was strongly opposed to, and challenged, my main conclusions, while the other reviewers encouraged me to press on and develop my arguments. True to the predilections of many of his kind, the editor perhaps saw the split vote as an opportunity to encourage a young though unproven scholar but offered no Solomonic solution to the divided reviews. With youthful enthusiasm, I earnestly engaged myself in the revision effort. I focused my revision around the reviewer who was the most opposed to my argument by directly addressing his or her concerns. In retrospect, this choice seems hard to explain. It was not necessarily the best one that could have been made. Nor optimal given the circumstances. I can only attribute it to my naïveté that I made the instinctive choice to engage in a debate in an area of my research interest.

Without fully recognizing the risk I was taking, I submitted my revision for a second round. After what seemed to be an eternity, the brown packet arrived. The second round of reviews, however, broke the common mold. Contrary to expectations grounded in conventional wisdom (as I realized later), the strongly opposed reviewer had changed his or her mind and now recommended conditional acceptance, while the other reviewers upheld their recommendations. Such a turnaround can be heady for an author, especially for a fresh scholar. One is prone to misattribute such a success, as I did. I saw my conditional acceptance as a personal victory, a triumph I alone had achieved through my powers of argument. In retrospect, I see how great a role that initially opposed reviewer must

have played in my first major publication. It is reviewers who carry perhaps the heaviest part of the burden in the review process, for they must meet a ferocious challenge—to remain open despite holding initial biases and to have the flexibility to perhaps overturn their initial disposition.

Further retrospection, and juxtaposing my submission with Holbrook’s (1986) lively but sadomasochist tale of the review process, which appeared the same year that I submitted my article, underscores the variability of the review process. Turnarounds of the type that I experienced are atypical. Indeed, considering Holbrook’s depiction of the review process as Willie and Frankie stories capturing Billy Crystal’s “imaginative exercises in self-mutilation,” it is apparent that most interactions between authors and reviewers often “mode out” at “I hate when that happens” and yield to a collective sense of “I know what you mean.” Within this background, my maiden experience came to seem as refreshing as it was rare.

A true appreciation of my first experience with the review process has grown with time, and often I look back at it as a beacon illuminating my role as a reviewer. Publishing in academic journals is often discussed as a “game” or as “prowess,” and reviewers are often characterized as masked “Sluggos” who need to be slain via exhaustion (e.g., by means of exhausting sets of notes they must wade through), or via exaggeration (by giving exaggerated praise for the reviewers’ “insightful comments”), or by use of both (Wright 2002). Authors are often viewed as languorous “Hagars” who need to be enlightened about their own limitations (information that is often culled from the authors’ own description of their works’ limitations) or educated as to how their current work makes no contribution (often via making vague references to theoretical misspecification and/or data problems). Turning such gaming expectations into refreshing experiences that produce a collective triumph for authors and reviewers and yield to a shared sense of “*Mahvelous!* I love it when that happens” remains a challenge. This is a challenge that we must collectively tackle.

Numerous commentaries, observations, criticisms, and suggestions have been offered to address this challenge. I want to take the opportunity of this invited essay to add my

voice to this effort. I see this challenge as real and as one that will likely shape the health and impact of individual marketing journals and, by extension, of the marketing field itself. By no means do I aim to offer the antidote to Holbrook's (1986) diagnosis of sadomasochism in the review process. I do hope my thoughts will trigger ideas that might shift the mode of review experiences from "I hate it when that happens" to "Mahvelous! I want to do it again," for authors and reviewers alike. In voicing these thoughts, however, I am not motivated by how-to concerns: the following contains no how-to-publish guidelines and how-to-be-constructive guidelines for authors and reviewers, respectively. Rather, a sense of personal reflection motivates my voice in this essay about my shortcomings as a reviewer, about things I have learned, and about those author encounters that lift the review process into golden moments. These golden moments offer a well-spring of learning and intellectual discourse that few other scholarly activities can match. I share these reflections with the belief that these moments are a reviewer's gold and guiding light.

### THE ACHILLES' HEEL

It is true. Implicitly and tacitly, a manuscript emits a language that I as a reviewer understand well. The setup of the problem, the presentation of results, the organization and layout of material, the style of writing, and other features of a manuscript all send strong subliminal messages, often of "reject me, please." I am not alone in this linguistic capability. An editor of a major organizational behavior journal once commented that a "significant" number of articles are rejected not because they do not address important gaps in a literature or are not competently executed but because they are not "well written." This phenomenon is also evident in the enormous effort exerted to highlight the role of positioning and writing at doctoral consortia and national meetings, and the furious scrambling of researchers to get the positioning "right" and the writing "tight." But *what is "well-written"?* Indeed, great manuscripts tell a great story about a great idea. As such, authors should write well and communicate effectively, whatever that means at a given point in time. That is the authors' responsibility. As a reviewer, however, this is my Achilles' heel. Not writing well is not a fatal flaw. It can be overcome in the revision process. Strong theoretical development, addressing substantive problems, and achieving empirical insight are far more important. Yes, poor writing makes it difficult to decipher an article's fundamental contributions. But it can be done. Perhaps, then, an appropriate question is, *How can it be done?* As researchers, we learn to recognize our biases and struggle to work against them. Otherwise, we risk not being taken seriously. Likewise, as a reviewer, I have found it useful to recognize a reviewer's

Achilles' heel and to consciously block out a manuscript's implicit and audible messages (good or bad) to grasp its core contribution. If a submission shows me the potential for a meaningful core contribution in terms of some configuration of theory, method, and substance—the *fundamentals*—the writing and positioning can be crafted successfully. The reverse is not true. Crafting can hide frail fundamentals; it can't overcome them. Neither can the review process. Fundamentals are a fixed aspect of study. Fundamentals matter. Understanding problems as puzzles; constructing puzzles that address important managerial, consumer, and social dilemmas; and securing credible data that solve such puzzles are the grist of the fundamentals (Weick 2002). More collective energy, thought, and effort are needed to mill this grist for our fundamental choices as individual researchers and a discipline representing a body of useful applied knowledge. Yet the current intense focus on crafting can, and often does, shift our attention away from the fundamentals. We need to bring the fundamentals into focus. With strong fundamentals, reviewers can provide ideas, suggestions, and feedback to help authors to think through the crafting of the manuscript. This is the reviewer's obligation. I see no gold in rejecting a manuscript with strong fundamentals simply because it is not crafted well.

### ACCEPTING IMPERFECTIONS

As a rule of measurement, a single item is highly unreliable. Yet together, multiple items can very reliably tap a construct. Individual studies, like single items, are fallible almost by rule or principle. This makes it easy to identify imperfections in any manuscript. By focusing on identified imperfections, one can readily "justify" rejection of a work or invite "major" revisions posing often insurmountable hurdles. Think of the instances in which an editor or reviewer asks authors to collect new data (e.g., scanner/behavior data) because the cross-sectional survey data they used were "inherently problematic." Or vice versa. No data are inherently infallible (including scanner/behavior data). Indeed, only multisource data, like those from multiple items, can mitigate the fallibility problem. Yet it is not necessary for a single study to achieve this goal. Multiple studies using different sources of data, when viewed together, can do so. Likewise, think of the times when the editor/reviewer justifies rejection of a manuscript because the proposed model's omission of an "important" construct has produced a "fatal" model misspecification. With the unprecedented proliferation of "new" theories and constructs, often of increasing complexity, it is useful to contemplate the likely nature of a single model that includes all the constructs that could be deemed important in the context of some phenomenon. Probably it would be a model that is either empirically

unworkable or pragmatically uninteresting, or both. In fact, choices about what constructs to drop and which to keep draw a researcher into an internal discourse about a study's fundamentals. As in scale development, often more is learned about a study's fundamentals by analyzing what items were considered and dropped and why, rather than by examining what was retained and why. When researchers construct instruments that pull together several popular measures, hoping that empirical data will help them tell an interesting story, they stifle internal discourse about their study's fundamentals. Likewise, in the case of secondary or company data collected from a huge sample of consumers, employees, and/or firms, a discourse about fundamentals is marginalized if the authors rush to analyses of these data simply because they are there and are available. An internalized sense of fundamental discourse is likely to make researchers hesitate to place the data before the model. More important, within an ongoing discourse, researchers are less likely to feel the urge to develop fully specified models that close the conversation once for all. Rather, like single items on a scale, individual studies with designed misspecification may be entertained. Cumulatively, studies that carefully and systematically limit their individual models yet allow for overlaps and integration across studies can overcome the essential misspecification dilemma.

As a reviewer, I have taken this to mean that it is useful to begin with the presumption that we are in the business of recommending imperfect studies and misspecified models. With this presumption in mind, I can then weigh a study's core contributions—the *fundamentals*—against its imperfections and misspecifications—the *failings*—on a balance that is adjusted for past literature. When this adjusted balance shifts toward its fundamentals, a manuscript merits consideration. By unhesitatingly identifying the failings of their work as clearly and forthrightly as they identify its fundamentals, authors can lift the review process. In so doing, the authors' sense of the balance is bared, and the internalized fundamental discourse is revealed. This revelation yields useful knowledge. As a reviewer, I gain a better appreciation of the trade-offs made in a given study. As the members of a field, we become better prepared to engage in the fundamental questions of puzzle making and puzzle solving. Hesitating authors may be understandably less forthright, fearing that their stated failings will be used against them. Like witnesses invoking the Fifth Amendment, authors may feel that they need the protection of withholding information to counter reviewers' tendency to "reject when given a chance" in an us-versus-them environment. Overcoming this fear, in my mind, remains a challenge for reviewers in particular and for the review process in general. The burden of this challenge, however, falls on reviewers and editors both. Gold can be mined by overcoming this challenge.

## INTELLECTUAL DISCOURSE

It is a misperception that reviewers are the "experts" for a given manuscript. Rather, the authors are. Usually, authors spend several years mapping the germane literature, selecting and rejecting constructs for inclusion in their model, designing and redesigning their study, and performing hundreds of analyses (many of which often go unreported) to better understand their data. A 35- to 40-page manuscript cannot adequately capture all that its authors wish to communicate. To facilitate this communication, a reviewer can help by asking critical questions. In an us-versus-them climate, critical questions can provoke defensive, lengthy, and generally obtuse responses that amplify the drudgery of the review process. Golden moments occur when the critical questions trigger thoughtful, direct, and authentic responses that go beyond the original concerns to address the substructural, underlying issues. In one such instance, an author responded comprehensively to my rather modestly developed criticism of model misspecification by specifying the potential constructs that could be involved in such misspecification and analyzing the possible effect of each construct and so circumscribing the limits of my concern. It was "*Mahvelous!*" The author's open and thoughtful response turned the review process into an authentic intellectual discourse. It built a trustful us-with-them climate that served ultimately to enhance the quality of the manuscript. I have wondered, *What can I do as a reviewer to promote this climate?* Cynics argue that, when 90 percent of submissions are rejected in the first round of reviewing, the hope for a trustful us-with-them climate is futile. Optimists observe that it is the very high probability of negative outcomes that places the burden on reviewers and editors to ensure that the review process is perceived as fair and trustful (Epstein 1995; Harrison 2002). Although several suggestions for lightening this burden have been offered, none is more compelling than the simple idea that reviewers be open and thoughtful: open to authors' points of view (especially when these run counter to the reviewers' own) and open to providing thoughtful recommendations based on careful and thorough reading of submitted papers.

In this simple paradigm, a reviewer also needs to be open to other reviewers' points of view and open to recognizing that the important intellectual discourse is not *between* authors and the individual reviewer but *among* the authors and the reviewers. Authors often get distressed when they find that reviewers disagree about the contribution of their work. Some studies have computed correlations among reviewers' judgments. When these are found to be dismally low, it is concluded that the review process is "broke." This conclusion appears logical. But it is not necessarily correct. A manuscript is a cohesive configuration of different elements, including substantive

significance, theoretical foundation, and empirical insight. Great manuscripts excel on each dimension. However, great manuscripts are rare. Usually, manuscripts are uneven. For instance, a manuscript may break new ground in its use and/or development of theory but address a substantive problem of less significance or rely on weak empirical work to test the theory espoused. Thus, if such a manuscript were to be rated along the three noted dimensions, the correlations among the ratings would, unsurprisingly, be weak. In this light, it is easy to understand why different reviewers' judgments may not correlate well. For a given manuscript, editors usually select reviewers with disparate expertise. In turn, reviewers approach the manuscript from different perspectives, and their judgments likely reflect the unevenness of a typical manuscript. My inference is that it matters little if reviewers' judgments do not correlate well; it may actually be a good thing when they don't. What matters more is whether these correlations improve as the manuscript goes through the review process. When reviewers are locked into their views from the start—when their minds are closed rather than open to a discourse—it is unlikely that the initial correlations will budge. By contrast, when reviewers are engaged in an intellectual discourse not only with the authors but also among themselves, perspectives will begin to converge, and consistent directions for the revision effort will be identified. Now, the correlations will swell. To make this work, each reviewer needs to do more than read the other reviewers' comments for confirmation and general interest. Viewing every review as a three-way intellectual discourse, a reviewer needs to adjust, refine, and back off from his or her own initial judgments as the discourse unfolds, issues are raised and addressed, and the manuscript's contribution is sharpened and enhanced. I find that it is easy for me to blank out the other reviewers from my intellectual discourse with authors. This limits the potential of the process (and keeps the correlations from growing). Working against this easy trap is an important obligation of my role as a reviewer.

Because this intellectual discourse unfolds within a "double-blind space" of anonymity and the written word, how the discourse is inked matters. Just as an author's manuscript emits silent messages, so does a reviewer's report. Avoiding disparaging and degrading language is only the beginning here. The very structure of a review, the underscoring of issues, and the style of communicating together send strong signals, often signals like "I gotcha" or "you dummy." Such signals, even if not intended, may be interpreted as signifying a reviewer who is "locked in" and unlikely to change his or her position, no matter what evidence is presented. Sometimes reviewer anonymity undermines the regulation of such signals. I find it useful to amplify this regulation by imagining that I am speaking to authors in person. In so personalizing my reviewer's report, I find I become better disposed toward an

intellectual discourse and allow more space for authors to counter and build on my comments through additional evidence, insight, and/or argument. This discourse has often been a source of learning for me. There is lots of gold in that.

### ONLY A GAME?

The process of scholarly review is ultimately more than a game. Games have rules, implicit and explicit. So does the review process. Games have winners and losers. Indeed, the outcome of the review process is also a win or a loss, an acceptance or a rejection. Games pose challenges and build skills. So does the review process. Yet, for me, there is an important difference. Games lock the players into the role of rivals who seek to outdo, outmaneuver, and outsmart each other in a struggle of us *versus* them. The review process can be such a game. But it does not have to be. Players can instead be collaborators. Together, they can work to uplift the review process and the focal work itself. A manuscript is but a single step in the program of its authors' research. Working as collaborators, reviewers can help authors think through the fundamentals both in respect to the article under review and the connected long-term program of research. Because reviewers render judgments about authors' work, this privileged position *per se* can shift the power balance. This disequilibrium can reduce the review process to a game. I have not found an effective method to successfully and consistently diffuse this potential. While several suggestions have been proposed, including allowing authors to provide feedback on the quality of reviews obtained and releasing the identity of reviewers (Epstein 1995), none appear particularly compelling. In some instances, concretizing objections into testable alternative hypotheses (alternatives to those entertained by the authors) can help shift the game of "I know best" to a constructive process of letting the data speak for themselves. Doing this is, of course, not always possible. Yet, wherever possible, opportunities to frame criticisms into hypotheses that are testable within the framework of the data collected should not be missed. In one instance, in response to my concern that the authors of an article had not considered an alternative mechanism embedded in a competing theory, they presented detailed empirical results from both models. The authors could have responded by forcefully arguing their position. They could have cited studies and noted precedents. They could have identified flaws in the competing theory. However, by allowing the data to speak directly to the question, the authors offered a far more compelling answer. I learned from this experience, taking away the conviction that I can help in the review process by presenting my concerns in terms of testable hypotheses. Having data matrices available during the review process (as they are when an article

is published) and applying today's flexible statistical software can further lift the review process. Indeed, authors may worry that releasing their data matrices will foster misuse and abuse. As the members of a discipline, we need to work out this potential problem by setting ethical boundaries within which authors feel safe to share summaries of their data and research materials for the sake of knowledge development and integration. Otherwise, golden opportunities will slip by.

## THE REFEREE

To ensure fair play, games need referees. Being a referee is not easy. Some players, for instance, are famous, command great respect, and have huge egos. When such a player is fouled, the reaction is to question, contest, and pressure the referee to back off. This reaction is part psychological play. The players are preparing for the next time they play rough, hoping the referee won't call a foul then, out of fear of another show of defiance. Then there is the audience, the public at large, whose members are always watching a referee's every move for signs of partiality or weakness. Miss a few calls, appear to take sides, or give in to player tantrums, and the public writes off the referee and the game. Like the father in the proverbial story of the father, son, and the donkey, the referee seems unlikely to ever please everyone.

In the ongoing intellectual discourse among authors and reviewers, the editor is the referee. Although it is not possible to please everyone, it is largely up to the editor to ensure fair play, resist pressure, and build confidence in the outcome of the discourse surrounding a review. Editors have an obligation to call the fouls, keep the discourse focused on the fundamentals, and blow the final whistle. Thus, for instance, while reviewers may want to continue the discourse and ask for more revisions and explanations, it is the editor's obligation to terminate the review process for a given manuscript and make a decision, one way or another. Likewise, when reviewers disagree and suggest divergent directions for revision, it is the editor's obligation to sort out the differences, focus the discussion around promising fundamentals, call out some reviewers' suggestions as unnecessary, and promote an intellectual discourse that seeks convergence (not necessarily consensus). When editors abdicate this obligation and let reviewers and authors slug it out, confidence in the review process is undermined, and the discourse suffers. Given this, a reviewer's role must be to accept graciously when an editor asks an author to politely disregard the reviewer's concerns or move the revision in a direction contrary to what the reviewer recommended. As the reviewer, I may disagree. But I must not disapprove. I must let go. By letting go, I can still participate usefully in the discourse by

working in step with other reviewers and authors to move the manuscript forward. By holding on and rehashing my "neglected" concerns, I risk derailing the intellectual discourse. Lots of gold is lost in that.

## CONCLUSION

People often ask, *why do it?* Reviewing takes time; effort; energy; and, as a time-bound activity, often demands attention just when one can least afford to divert it from one's own research pursuits. Common answers to the "why" question are that reviewing can bring recognition as a scholar, offer influence in shaping a field, serve as a stepping-stone to higher ambitions, and confer a sense of being of service to one's field. There may be a kernel of truth in each of these answers. Yet these answers don't tell the whole truth. They miss the key point. Reviewing, like an essential food group, feeds the intellectual health of a scholar. Reading and critiquing scholarly outputs, listening and responding to alternative viewpoints (of authors and of other reviewers), and learning and growing from this discourse are unsubstitutable goods for this reviewer's mind. That is the reviewer's gold. Gold is mined when a review process and reviewers allow authors the space to be open, direct, and nondefensive. Gold is lost when the review process resembles a battleground where each side is locked in a strategy of "winning this one." Gold is gained when the process wins regardless of the fact that a particular article is accepted or rejected. Undoubtedly, effective mining requires the cooperative efforts of authors, reviewers, and editors.

In closing, I can't claim that this commentary represents shared ideas. Neither do I claim that these ideas represent my final thoughts on these issues. These are merely my personal reflections at this time on a process that I think is critically important for individual scholars and for the discipline itself. Like other intellectual discourses, my ideas are likely to evolve as I learn more and understand better. I do hope, however, that this and other essays provide a foundation upon which others can build, even if the views they develop differ from my own. Our field needs to bring apparently disparate voices into a unity that can be the bedrock for revitalizing the review processes. We will all be the richer for it.

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**REFERENCES**

- Epstein, Seymour. 1995. "What Can Be Done to Improve the Journal Review Process?" *American Psychologist* 50 (10): 883-885.
- Harrison, David. 2002. "Obligations and Obfuscations in the Review Process." *Academy of Management Journal* 46 (6): 1079-1084.
- Holbrook, Morris. 1986. "A Note on Sadomasochism in the Review Process: I Hate When That Happens." *Journal of Marketing* 50 (3): 104-108.
- Weick, Karl E. 2002. "Puzzles in Organizational Learning: An Exercise in Disciplined Imagination." *British Journal of Management* 13:S7-S15.
- Wright, Peter. 2002. "Marketplace Metacognition and Social Intelligence." *Journal of Consumer Research* 28 (March): 677-682.