

EDITOR'S COMMENTS

Diamond Mining or Coal Mining? Which Reviewing Industry Are We In?¹

In the editorial just published in the March 2009 issue of *MIS Quarterly*, Varun Grover, Pamela Galluch, and I argued that the IS field would develop more robust self esteem and identify were it to adopt dispositions that moved in virtuous circles rather than downward spirals (Grover et al. 2009). Prescriptions offered were that the field as a whole should celebrate our successes much more often, in view of the fact that the IS field has had a considerable number of these. We also suggested that group endeavors, like conference panels, workshops, and, in fact, authorial collaborations should focus on the positive, even in the event of criticizing the status quo. Finally, we tried to convince opinion leaders (as well as the rest of the community) that each of us has a stewardship responsibility to convey positive messages about the field. None of these prescriptions was intended to ignore realities, but simply to promote constructive actions rather than sheer negativity.

I would like to continue this line of thought, but to confine my remarks to reviewing journal submissions such as those papers being reviewed at *MISQ*. Here, once again, we are confronted with a choice to engage in either virtuous circles or downward spirals. These take form in the metaphors of diamond mining or coal mining.

Are We Seeking Out Diamond Lodes or Are We Rejecting Coal Lodes?

When she was Editor-in-Chief, Carol Saunders spoke often and eloquently, both in print and in speaking venues about our reviewers seeing themselves as diamond cutters rather than as gatekeepers (Saunders 2005a). Keeping with Carol's basic imagery, I depict the reviewing process as either diamond mining or coal mining, a change from her symbolism to account for the predispositions that reviewers have about the activity in which they are engaged.

We all know that predispositions (a.k.a. dispositions) are important. They frame subsequent actions and can dramatically affect decision-making.

Predispositions to trust are one simple example of this. As articulated by McKnight et al. (1998) among others, trusting beliefs can be shaped by one's priors. In the presence of other, stronger influences, these dispositions will weaken, but their impact is nonetheless always present. In IS studies, empirical evidence of the effect has been found in Gefen and Straub (2004), Pavlou and Gefen (2004), Lowry et al. (2008), and Vance (2008).

Suppose you are a miner. Your ultimate goal is to find diamonds, but this gem-in-the-rough is found amongst large quantities of pure and adulterated coal. Suppose you are convinced that the vein you are working in does not contain any diamonds. If you are predisposed to see coal (rather than diamond-bearing rock) in every lump of material you examine, then your chances of finding a diamond are much smaller. If, on the other hand, you are inclined to look for any evidence of the mineral diamond, and search hard for it, once again amongst large quantities of coal, you are more likely to accomplish this highly personal goal and identify the potential diamond in your search.

The attitude one brings into the reviewing process is similar to the predispositions of these miners. If you see yourself as a diamond miner who happens to be working with a lot of coal deposits in order to find diamonds, then you are well positioned to find diamonds. If your view is that of a coal miner, who only once in a great while and almost incidentally finds a diamond-

¹I would like to acknowledge the valuable suggestions made by former *MIS Quarterly* Senior Editors Varun Grover and Elena Karahanna for this editorial.

bearing rock, then you will experience “slim pickings” in the mines. The prize of finding diamonds is granted to those who *want* to find diamonds, and not to those who every once in a while stumble on them and cannot help noticing that they have found a diamond.

Manuscript Rejection as a Necessary Evil

In the course of reviewing, top journals, of course, must reject many papers. Whereas reviewers may believe that most authors have selected a good topic area and are well-intended, the prospect that the authors' submission will eventually be able to realize its intentions falls into serious doubt in many situations. In such cases, reviewers rightly reject papers, while hopefully still encouraging authors to develop their ideas in the constructive ways suggested by the review team. This has been called “developmental reviewing,” and past *MISQ* EICs like Carol Saunders have devoted editorials to this concept (e.g., Saunders 2005b).

What I am speaking about in this editorial, however, is NOT developmental reviewing, while at the same time fully supporting the concept. What I am speaking about here is reviewers assuming a positive attitude toward the research they are reviewing, so much so that, going in, they bend over backward to give every paper a fair hearing.

This is not an entirely new theme in that I have spoken about matters related to it in the past. A year ago (2008a), I presented a line of reasoning to convince readers that Type II reviewing errors (rejecting papers that the community would have relished) were more serious than Type I reviewing errors (publishing papers that the community views as weaker papers, as demonstrated by low citations). One major reason Type II errors occur, I argued, was that methodological considerations typically overruled good ideas in the review process. The inference of this line of thought was that the editors needed to assume control of the process and ensure that good papers were not being rejected.

In September 2008 (Straub 2008b), the argument continued with an example of one of the top cited papers in the IS field (DeLone and McLean 1992), a paper that was nearly rejected because of an overemphasis on methods. In fact, an updated study of DeLone and McLean (2003) was rejected at *Information Systems Research* in another mistaken stress on methodological issues to the detriment of the intellectual contribution. This editorial tried to persuade readers that when a rejection occurs, there is always a danger that the review team has missed an opportunity to enlighten the field.

These editorials focus on the reviewing process. But as for the incoming attitude of the review team, the topic has only been lightly touched upon in the term “culture of rejection” mentioned briefly in Straub and Ang (2008).

What is this very real problem and what can be done about it? I believe that many reviewers in the field have adopted a “show me” attitude and that this has become institutionalized so much so that the predisposition of many reviewers is heavily biased to reject papers out of hand. In short, to use the metaphor we are developing, many reviewers think like coal miners. They expect to see coal, and, lo and behold, that is exactly what they do see.

What is the alternative? The diametric opposite is a reviewer who thinks like a diamond miner. This reviewer wants to find diamonds. S/he realizes that the field cannot advance without these diamonds, even though others will eventually claim ownership of the diamonds and be well known for the luminescence of their properties.

These reviewers are predisposed to seek out the good in a paper. When they find sufficient signs of the potential, they help to develop it so that it can reach a publishable state. They still exercise their judgment and can recommend that a paper be rejected for its coal-like consistency, but they view this as a necessary evil of their job and not something that they relish.²

²Occasionally I encounter a reviewer who seems to exude this attitude and to actually enjoy rejecting a paper. From the start of the review to the end, they seem to want to belittle others, even in situations where it is clear that the authors are simply badly informed about the quality of the work they are producing. When I am recipient of such reviews as an editor, I try to persuade the reviewer that their insouciance or heavy-handedness does not suit the culture of reviewing we are attempting to promulgate. If that does not work, then I simply have to ignore that review and not burden the authors with it. Fortunately this occurs rarely, but the fact that it occurs at all shows that there is an extreme tail of the distribution of this disposition toward negativity.

Pure Speculation on Why Reviewers Have Negative Affect

Why do reviewers often come into the review process with negative affect? Not being aware of any in-depth study on this phenomenon, I'll speculate. I can think of at least four explanations for this: (1) they view the review process as a zero-sum game and the submission they have been asked to review as competition; (2) they have acquired new knowledge (especially true for new methodological knowledge) and their own standards have been raised with respect to that domain; (3) in a sense, the opposite of point 2 in that some reviewers feel defensive that they do not know enough and adopt the defensive stance of rejecting the paper; and (4) they want to uphold the standards of the journal, a point which is extremely tricky when the status of the journal has become elevated over the years.

Let's examine these in order, making the assumption that they could make sense under certain belief sets. Point (1): the view that accepting papers at top journals is a zero-sum game. The zero-sum game arises particularly when domains of study overlap, that is, when the reviewer is working on something similar or feels that the reviewed work, if accepted, would undermine her/his work in some way or compete for the limited journal space. This effect, if it exists, is mitigated in other cases.

As rational actors, reviewers for top journals may indeed believe sincerely that there is only so much space for accepted papers. Two widely read articles have made this point in recent years, in fact. Dennis et al. (2006) present evidence that IS faculties are disadvantaged in journal space and that this redounds to our detriment at tenure and promotion time. Kozar et al. (2006) report similar evidence of limited journal space. Therefore, it may not be out of sheer paranoia that reviewers (who also take on the dual role of authors themselves) see the reviewing process as a zero-sum game and for that reason bring a hypercritical attitude into the process.

I would counter this impression that reviewers may have with the following realities. Reviewing events are independent events. The rejection of one paper does not mean that the next paper has a higher probability of being accepted any more than the flip of a coin that results in five heads in a row increases the odds that the next flip will be tails. Moreover, papers in the regular review process are being reviewed as they are received, and most often by different editors; therefore, they cannot be readily compared with one another. Special Issues editors may have the ability to compare papers, but this is simply not the case for regular submissions.

There is one last piece of evidence that I hope would dissuade reviewers from holding a negative disposition for this reason. *MISQ* and *ISR* have made strides in increasing print journal space since these critiques appeared. At *MISQ*, we can publish more papers than in the past because we now have the capacity to do so. Under EIC Sambamurthy, *ISR* has also garnered more journal space from their parent organization, INFORMS. And, with regard to electronic journals like *Journal of the AIS*, there is no desideratum for space, but only for high quality.

Point (2): The second reason is that reviewers may be holding others to a higher standard because they themselves have gained new knowledge. Perhaps the clearest case of this is newly minted Ph.D.s in the field. Earning a doctoral degree is a long, involved process with one dominating feature being that it is mostly a degree in methods. This is the way most faculties plan their doctoral programs and this is most often exactly the outcome they get.

In and of itself, education should be beneficial across the board and one can hardly raise an objection that the reviewers are too highly educated for their own good. We strive to educate doctoral students well, and should be proud that they have mastered this knowledge. But it would be certainly better for the field if with this great knowledge came a strong dose of humility. Rather than adopting a negative affect toward the failings of others, the review process is better framed as a golden opportunity to transfer knowledge to others. Reviewers can be usually as involved in the reviewing process as they wish. I have served as editor in many cases where reviewers have asked for and gotten the authors' data (or correlation matrices), have independently analyzed it, and then offered a new way of looking at the data freely to the authors. This in my view is the positive disposition that we want in reviewers. The reviewers feel better about themselves when they engage their own knowledge base in this way, and it also helps the authors and the journal.

Point (3): Reviewers who feel that they are lacking in their knowledge and thus reject papers from a defensive position. Some reviewers no doubt feel that they lack the knowledge to evaluate an *MISQ* paper well (or, perhaps more accurately, well enough). They are likely proud to be reviewing for the journal, but feel subliminally unqualified. They then take a "defensive stance" to

reviewing. In doing so, any small aspect of the paper that may be unconventional or easy to criticize is a reason to raise the standards of the journal onto a pedestal. Indeed, newly minted Ph.D.s, typically lacking experience in reviewing for top journals, could either take pride in their knowledge or feel inadequate in their knowledge... both responses leading to a negative disposition toward the paper. (Very much like defensive medicine where doctors don't use their judgment to diagnose what they think is wrong, but instead use a battery of tests to come to a conclusion through a process of elimination.) This reviewing stance does not assess the paper holistically, but instead uses a set of checklists to determine ways to reject a paper.³

Point (4): The fourth possible reason that reviewers bring a hypercritical point of view to their reviewing is that they are, quite rightly, trying to maintain the high standards of the journal. Again, no EIC in his or her right mind could possibly object to that noble goal. But as I have been trying to argue over the last year in my editorials, we do not really have a Type I problem at our top IS journals. If weak papers are published, they will not be cited and that is the only true downside.⁴ Weak papers take up space that could go to good papers, but this is completely hypothetical at *MISQ* since we are not rejecting papers because we cannot afford to print them. I am concerned, as I have expressed in other places (Straub 2008a), that we are more likely to have a Type II problem, that we are rejecting papers that the community would welcome in print and thus would be highly cited. In short, we are missing out on papers that have good ideas but are vulnerable in their methodology or for other "fixable" reasons. The bottom line is that reviewers should not worry that they are undermining the journal if they refocus their efforts to help papers with solid intellectual content through the process. It may take more work on the part of the entire review team, but this is what will advance knowledge in the field and result both in highly cited work and a higher journal impact factor.

Knowing which papers make acceptable contributions or earth-shaking contributions is more of an art form than a science. So there will be mistakes, assuredly. Very few of us are saints, after all. But, as we argue in our recent editorial on vicious, downward spirals (Grover et al. 2009), if we give in to negativity because we are seeing it all around us, we do not merely perpetuate the downward movement; we also strengthen it. Negative dispositions are perhaps not morally superior to Pollyanna dispositions, but all things being equal, overly optimistic affect does not have the same power of contagion that pessimistic affect has and is, therefore, preferable, if these are the only choices.

Fortunately, these are not the only choices. We can become aware that we have a negative affect toward reviewing and change this disposition. Once aware, we can attempt to influence others, which is precisely what I am trying to do with this editorial. Bringing a negative disposition into the review process truly defeats everything that we are trying to accomplish. It hurts the journal; it hurts the field; and it hurts individuals. It results in the rejection of papers that would have made solid contributions, which damages both the field and the journal. It also discourages authors who do not understand why their considerable efforts to improve their work are not clearing the hurdle. When editors and reviewers take the attitude that authors are actually blessing the journal with their work, it is even possible that rejections can be made more palatable to authors. There is no question in my mind that rejecting papers is a necessary evil, but there is likewise no reason for it to be carried out in a dour state of mind.

Going Beyond Developmental Reviewing

Developmental reviewing is a highly desirable trait in journal reviewing, but it is not sufficient for journals to continue to be truly great journals, in my view. To sustain a journal's greatness calls for doing more than waiting for papers to be thrown over the transom by authors. It calls for a strongly favorable predisposition that can then lead to a virtuous circle, a belief that there is much work that deserves to be disseminated to the community at large. It calls for a proactive stance for strenuously moving into practice the good work that is being produced. In short, it calls for diamond miners.

³It is very possible that senior faculty are not doing a sufficient job in helping doctoral students transition into the constructive reviewer role. Being able to discriminate between *theoretical and methodological concerns* and *fatal flaws* and then balancing that against the overall contribution of the research is not an insight that comes naturally. In short, it would be well if senior faculty wrestle with their own tendency to coal-mine or diamond-mine, and, concluding within themselves that diamond mining is a more fruitful business, convey this to the students.

⁴Some might object, and correctly so, that too many weak papers can obviously dilute the quality of any journal. After all, in an emergent paradigm field, potential is often in the eyes of the beholder and there may be a low correlation of agreement between reviewer pairs. Therefore, choosing editors who are willing to take risks on paper and sensitizing them to moving the line should ultimately help the journal—and should result in the desired movement. However, there is the ever-present danger that the line becomes too soft—resulting in too many weak papers. Taking risks, therefore, also requires vigilance that the journal benchmarks and standards are held to.

What are the manifestations of this proactive stance? It takes at least three forms, as I see it. They are (1) exciting forays into new research domains via Special Issues; (2) role modeling and dedicated surveillance by the editors; and (3) predispositions by the reviewers to seek out and then develop the best that can be found in submissions. Let's deal with each of these in turn.

- (1) Special Issues offer editors, reviewers, and authors alike the chance to bring out the best in each other. Since roles are often mixed in these scenarios, reviewing takes on a special quality, one that may impart more positive thinking because of the overall shared goal to make a statement about the importance of the research domain to the community at large. The workshops that sometimes accompany Special Issues are ways to accentuate the positive and eliminate the negative. Those who participate in Special Issues either as editors and reviewers or as authors are more likely to adopt proactive stances in the future toward diamond mining rather than coal mining. We live in hope.
- (2) Editors can assist in a wider adoption of proactive stances by conveying positive affect, by being constructive themselves, and by encouraging reviewers when they are being helpful rather than being defeatist. It is perhaps unrealistic to think that one can induce a profound change in another's disposition through such simple proscriptions, but the recalcitrant reviewer's behavior might change if s/he is rewarded for certain behaviors and cautioned about others. And behavioral change can lead to attitudinal change (cognitive dissonance). Result: more diamond miners and fewer coal miners.

Senior Editors at *MISQ* can also take on the role of diamond miner by seeking out promising papers. In an administrative experiment that is now in its second year, *MISQ* SEs can choose to fast track papers that they see as extraordinary. These papers cannot be self-promoted by authors, but must be discovered by the SEs, primarily through an experience with a previous vetting of the work.

- (3) Individual reviewers can promote proactive stances in others by seeking to influence the discourse in reviews. If one is a reviewer in a revision round, it is possible to note the other review comments that are constructive and to build on these. It is also possible to take a stand on what others see as fatal flaws and to offer solutions instead. Although the reviewing process is not a democracy (Straub 2008b), good editors will gravitate to those who are seeking diamonds rather than those who never see anything but coal. Thus can a proactive stance lead to greater individual influence on papers and, in the long term, greater opportunities for professional service in the editorial ranks.

Why Rejection Rates Should Not Be the Issue

This editorial is just one in a long chain of editorials that takes the position that our focus as a field should not be on negatives like rejection, but positives like acceptance. For this reason, I have vowed not to seek out and certainly not to promulgate the rejection rate of manuscripts at *MISQ*. Sadly, in this case, if I stress how many good papers we have accepted and printed out of all we received, the rejection rate is tautological and so it is really the number of submissions that needs to remain a mystery.

Isn't this a bit radical? It may be, but I believe strongly that our strength lies in the good and great papers that we produce for the field and does not lie in the number we reject, which I see as a necessary evil. We must reject papers. Of course. But this is not what we should be dwelling on, or even worse, what we should be proud of.

We should be proud of the new authors who have succeeded in publishing in our best journals. We should be celebrating the diversity of our journal boards and the involvement of the international community of scholars. We should recognize the editors and reviewers who take a proactive stance as diamond miners. We should be celebrating the leaders who published early, middle, and late in their careers and mentored others to do the same.

And we should be celebrating those authors whose papers were inspiring and headed in the right direction, but fell short. If the authors were still encouraged, though, to develop their ideas further by the review process, it may be that in their next submission, they will achieve a victory, for themselves, for the journal, and for the field.

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