

EDITOR'S COMMENTS

Type II Reviewing Errors and the Search for Exciting Papers

In my March 2008 "Editorial Comments," I indicated that we would take up in greater detail why the top information systems journals, including *MIS Quarterly*, are, in my opinion, rejecting so many of the papers they receive and what we can do about improving this situation.¹ In the last issue, I centered this discussion around the concept of reviewing errors, and what they can mean. Repeated here as Table 1 is a graphical representation of the possible outcomes

Type I problems occur when there is a difference in opinion between the review team and the IS community about the worth of a published paper. The team (ultimately the senior editor as decision-maker, but with major input from the associate editor and reviewers) sanctioned the publication, but, in the long term, the community did not value the paper and, therefore, cited it infrequently. The journal may lose reputational capital in publishing weak papers, but the system on the whole is self-correcting in that weaker papers are merely not cited as much.

Type II problems result from another difference of opinion about the quality of manuscripts. In this case, the community would have responded well to the paper had it been published, but, alas, the review team did not see the contribution in the paper and so the paper was rejected. These errors are more troublesome because the damage occurs mostly as an opportunity cost. The only way in which anyone would know that an otherwise good paper had been rejected is if it appears in another strong venue, in particular another top journal. In this case, the original journal has lost the chance to publish what could be a highly cited paper.²

Why such an emphasis on citations? Although there are certainly other ways to judge journal quality, this is one established way. But I think there is greater value in using citations to assess whether journals are succeeding in their primary directive: to publish good, and occasionally great, papers.

		IS community's view	
		Accept paper!	Reject paper!
Review team's view	Accept paper!	A good paper is accepted	Type I error
	Reject paper!	Type II error	A weak paper is rejected

¹I am assuming that everyone, or nearly everyone, would agree that no field should place an inherently high value on rejecting papers. My sense is that there are ways that more papers can meet our high standards, and be legitimately publishable, if we can understand better why papers are rejected.

²I grant that this is a highly ephemeral loss in that there are no independent, trusted third parties rendering judgments on the true value of manuscripts. Perhaps the only way quality could actually be verified would be in an open marketplace of papers where journals bid on the privilege to review a paper, an idea that former AIS President Rick Watson floated a few years ago. In such a system, the potential value of a paper would be reckoned in the number of bids that a paper received from which journals.

Table 2. Top 50 Papers in Lowry et al.'s (2007) Analysis of Citations in Period 1990–2004

Top 50	<i>MIS Quarterly</i>	<i>Information Systems Research</i>	<i>Management Science</i>
1–10	2	5	3
11–20	5	1	4
21–30	8	2	0
31–40	3	5	2
41–50	7	3	0
Totals	25	16	9

Thus, the more important reason for stressing citations is that they are one established indicator that a journal has published good papers, papers that the community has found to be useful in subsequent research. Top journals, therefore, seek to bring exciting, new ideas to the awareness of the community, and the evidence is that, indeed, good journals do this. To wit, Table 2 shows how the most cited papers in the 1990–2004 period were distributed amongst *MIS Quarterly*, *Information Systems Research*, and *Management Science*.

The underlying meaning of citations is that researchers read prior research to find key ideas in the works of others that they can build on or refute. These papers they cite. If anything, the journal copy-editing process reduces the numbers of citations that authors are permitted, and so authors must focus on which citations have been most influential. These citations are what survive in the published article.

What is the bottom line? I am arguing that the job of a journal is to publish good (and as often as possible, great) papers, and to hope that these become heavily cited; moreover, this is quite different conceptually from rejecting a lot of papers. Citation analysis cannot calculate how many papers are rejected by a journal but only how many are read and utilized.³

If this line of reasoning makes sense, then we should be concerned that in the large percentage of papers that are being rejected, there are not a significant number that are good, but unappreciated. I am *not* contending that review teams are not doing a thorough job. What I am contending is that they may be focusing on factors that are not intimately associated with why a paper soars above the multitude and achieves the status of at least a “good” paper. Many times, for example, they focus on methodological factors rather than intellectual novelty. In short, review teams are in the mental mode of finding reasons to reject a paper rather than finding ways to accept a paper. Indeed, a culture of rejection is deeply set in many of our top journals. This tendency to focus on technical issues rather than to relish fundamental ideas has been observed to occur widely in the management disciplines (see Singh et al. 2007; Swanson 2004).

Beyond what has just been noted, there are a host of reasons why this happens. I will try not to over-generalize, but to anchor the discussion to what I have observed in my personal experience as SE for *Information Systems Research*, *Journal of the AIS*, and *DATA BASE* (as well as *MISQ*, naturally), and an AE for *Management Science*. Throughout those years of managing review teams, I have noticed three powerful tendencies. First, reviewers tend to focus on methodology. Second, the editors (SEs and AEs) are heavily influenced by the reviewer input, often to the point of setting aside their own judgment. And third, editors do not exert influence and signal their *a priori*s about the paper to those reporting back to them in the hierarchy. Let me detail each of these problems after noting, by way of preview, that the solution to this problem, as I see it, is a new, albeit reprised way of reviewing termed “editorially directed processing” (EDP).

³ Another caveat to be noted here is that papers that are cited may or may not be read, or have their nuances reflected in a citation. Even authors who argue that IS research is not cited as much as it should be (Loebbecke et al. 2007) acknowledge that citation analysis is one of the best ways to determine how influential is the research of the field.

Table 3. Two Reviewing Approaches and Type II Errors

Strong Methodology		Published only under Review Approach #1	Published under either review approach	
Good Methodology				
Acceptable Methodology			Published only under Review Approach #2	
Poor Methodology				
	No New Ideas	Some Incremental Ideas	Good Ideas	Frame-Breaking Ideas

Legend: Review Approach #1 (Dominant): Evaluate methodology first, and reject unless at least Good Methodology; then evaluate ideas and reject unless at least Some Incremental Ideas; Review Approach #2 (Proposed): Evaluate ideas first, and reject unless at least Good Ideas; then evaluate methodology and reject unless at least Acceptable Methodology.

Dominance of Methodological Considerations in Review Recommendations

It is unhappily the case that many reviewers focus on methodology nearly to the exclusion of novel ideas. This is a special variation of the Type II reviewing error in that good papers are being rejected because they are not technically perfect even though they are otherwise interesting or even inspiring. Table 3 shows the dimensions of the problem. As the legend indicates, review approach #1, which I would posit is the current and dominant reviewing posture in top IS journals, stresses methodology at the expense of intellectual content. The grayed out areas represent manuscript acceptances under different conditions. The white cells represent rejections under all conditions.

Why do I say this is a problem? After all, is not methodology a critical element in validating that the science passes muster and the results are credible? Assuredly.⁴

But it is one thing to argue that methodology is the *sine qua non*⁵ in manuscript evaluation and another to argue that if the methodology is not in the upper range, manuscripts with highly original ideas do not suffice. Let me clarify my argument. Quality in a journal is reflected foremost in the quality of its ideas (given no poor methods), not in the quality of its methods (given no poor ideas).

Put straightforwardly, our top journals should be seeking out exciting research, and if the methodology is minimally “acceptable,” the assessment of the intellectual content should dominate the decision. This is what I characterize as review approach #2 in Table 3.⁶ In my opinion, the key to citable articles is groundbreaking work. When the work is stimulating but is primarily building on past work, then it is still worthy of publication even though it will not be cited as much as articles that break the frame.

⁴Let me hasten to add that I have devoted a lot of effort over the course of a career in encouraging quantitative researchers to be rigorous in validating their instrumentation. I do not see this framing of exciting ideas foremost to be at odds at all with arguments for better methods. Recently I heard an economist issuing a challenge to traditional economic theory on the topic of creative destruction. His methodology lent weak credence to his thesis, and was neither strong in itself nor in the results from the methods. Nevertheless the ideas were so exciting that I can easily see why the work ended up in a top journal in finance/economics. In short, if the ideas are fascinating or ground-breaking enough, methods that are minimally acceptable should not stand in the way.

⁵Latin, literally “without which, nothing.”

⁶The articulation of this approach via Table 3 is the brainchild of Dr. Dale Goodhue. We are in complete agreement about the preferences expressed.

Many editors already know and believe these things because they have observed them in practice. And the remaining two points I would like to make about misplaced democratic sentiments in the reviewing process and editorial signaling are ways in which top-notch editors will reassert their views about the worth of papers and reassume their roles as leaders of the field.

Misplaced Democracy Degrading the Reviewing Process

It is perhaps comforting to some when the reviewing process is a fairly simple vote-counting system. Reviewers are chosen for their expertise, after all, and if we weigh their recommendations equally (is this a reasonable assumption, by the way?), then majority rule would tell us whether a paper should continue in the process or not. Or, in an era when the numbers of submissions to top journals are reaching an Olympic scale, an even simpler decision rule is the black ball. One negative vote and the paper is rejected.

This *Weltanschauung* begs the real question, however. Who is in charge of the decision-making? Reviewers render their judgments as recommendations, as they should, but journals delegate authority to the editors to accept or reject papers. Notably, not to the reviewers. If the editors defer entirely to the reviewers, then editors are superfluous. The Editor-in-Chief can count votes as easily as others, one would hope, and if this were a viable way of finding good papers, we could de-layer our journals, keep only a single editor, and reduce the number of eyeballs looking at papers.

So my argument here is that editors (which in the case of *MISQ* includes both SEs and AEs) should be forming their own views about the paper independent of the reviewers, and, in the long run, their judgment should prevail when there is doubt about the quality of the paper. An underlying lynchpin of the reasoning here is that the appointment of editors to top journals is thoroughly vetted (as it is) and, all things being equal, their expertise should be what is most heavily counted in the review process.

Let me build on what I said in my last editorial about the critical importance of having the right people to direct the editorial decision-making and EDP, which I remind you, is “editorially directed processing.” Alan Hevner and Chris Kemerer are new SEs for *MISQ* specifically charged with building up the design sciences and economics of IS areas, respectively. I have complete faith in their ability to seek out and accept good papers for *MISQ*. Equally so, I confidently delegate decision-making authority to the other new SEs appointed last December. These include, in alphabetical order, Henri Barki, Shirley Gregor, and Joe Valacich. There is no doubt in my mind that these editors are thought-leaders in our field and are endowed with excellent judgment about the types of papers *MISQ* should be publishing.

Moreover, these appointments make it clear that we are seeking out good papers across the board now in the information systems discipline. For greater diversity, we are developing the areas of design science and the economics of IS, but we want the best papers in all of the areas that *MISQ* has traditionally served. Good papers are well cited, and the espoused goal of the journal is to seek out work that deals with “the enhancement and communication of knowledge concerning the development of IT-based services, management of information technology resources, and the economics and use of information technology with managerial and organizational implications” (*MISQ* Website at MISQ.org).

It would be straining the point to describe the strong AEs that were either already on our board or that were added last December. I invite you to visit our Website at MISQ.org to see the level of expertise that can be brought to bear to completely invoke EDP. The essential point to be made here once again here is that the journal has appointed leading scholars so they can exercise their judgment about which papers pass the hurdle of contribution. Assuming I can convince them that review approach #2 is preferable, they can also be relied on not to overstress methodological considerations when balancing these against substance.

Editorial Influence and Signaling to Reviewers

Bear with me while I carry forward the logic of an editorially directed process to the *logistics* of an EDP. Assume that the editors reach a consensus on the worth of a paper and, in general, believe that it passes muster. Indeed, they believe that the paper could, with revision, make an important conceptual, empirical, and/or theoretical contribution. As editors in charge of the review team, it behooves us to communicate this clearly to the reviewers, keeping to the fore the possibility that the reviewers may have insights into the weaknesses of the paper that would, in fact, counter the expressed editorial advocacy. In short, reviewers always should

be encouraged to exercise independent opinions, but whether the problems they identify can be remedied or not is still left to the purview of the editors.

If editors do not signal their *a priori* about a paper to the reviewers, then the natural inclination of the reviewers is to judge the paper against very high methodological standards first, which leads them to subsequently have a different, less favorable view of the overall contribution of the paper.

The reason for appointing veteran scholars to high editorial positions is to take advantage of their ability to adopt a holistic point of view. The ultimate value of a good paper is not that its methodological rigor is near perfection. What is important about good papers is that they change the way we look at a phenomenon. Scholars who are deeply affected by the work will, no doubt, strengthen the methodological approach in the course of, naturally, citing the seminal article.

Benefits to Authors and the Field

Suppose some or many of the mental frames being advocated here were widely adopted at *MISQ*. What is the likely outcome? How would this personally affect authors?

Authors would have the benefit of having early editorial advocacy for their papers, or else they would know quickly why their paper was not being sent out for review (and hence rejected). The single most frequent complaint about top IS journal reviewing is long delays and the uncertainty this creates for authors. What's more, it was not always the case in the IS field. At one time a decision for "revise and resubmit" was a strong signal to authors that their paper stood an excellent chance of eventual publication. Sadly, at some point, both editors and reviewers began hedging their bets and subjecting authors to lengthy and grueling revisions only to reject the paper in the final analysis.⁷

Another decided advantage to EDP is that good ideas would not sink as readily as when methodological considerations predominate. As scholars and authors themselves, editors have often had their own work beset by overly technical reviewers, and may heed this clarion call for a greater latitude for good ideas.

The trade-off is clear enough. Authors may have a unique perspective on an age-old problem, but their methods of attacking the problem may be lacking. But if the focus is on newness rather than methodology, researchers using the work can adjust the research artifacts and test theoretical propositions in alternative settings. The same cannot be said, of course, if the paper is immediately killed and hence beyond salvaging, for a given journal at least.

The outcome for top journals is perhaps not as obvious, but if the line of reasoning is sound, a higher percentage of good and great papers will appear in print, as defined by intellectual content rather than technical achievement. And the citation counts should go up since the articles in the journal are moving in blue oceans of uncontested space.⁸

Detmar W. Straub
Editor-in-Chief
dstraub@cis.gsu.edu

References

Kim, W. C., and Maubourgne, R. 2007. *Blue Ocean Strategy: How to Create Uncontested Market Space and Make the Competition Irrelevant*, Cambridge, MA: Harvard Business School Press.

⁷Like many other authors, I have been in the position of having a paper rejected after a third or even a fourth round of revisions. I know that this is sometimes unavoidable, but, in my opinion, it should be an extremely rare event rather than the ordinary event it seems to be.

⁸In tribute to Kim and Maubourgne for their fascinating book on seeking out untraveled highways.

- Loebbecke, C., Berthod, O., and Huyskens, C. 2007. "Research Importance in the Information Systems Field: A Citation Analysis," *Proceedings of the 28th International Conference of Information Systems*, J. Webster and S. Rivard (eds.), Montreal, December 9-12, pp. 1-15.
- Lowry, P. B., Karuga, G. G., and Richardson, V. J. 2007. "Assessing Leading Institutions, Faculty, and Articles in Premier Information Systems Research Journals," *Communications of the Association for Information Systems* (20:Article 16), pp. 142-203.
- Singh, G., Haddad, K. M., and Chow, C. W. 2007. "Are Articles in 'Top' Management Journals Necessarily of Higher Quality?," *Journal of Management Inquiry* (16:4), December, pp. 319-331.
- Swanson, E. P. 2004. "Publishing in the Majors: A Comparison of Accounting, Finance, Management, and Marketing," *Contemporary Accounting Research* (21:1), Spring, pp. 223-255.