

EDITOR'S COMMENTS

Why Do Top Journals Reject Good Papers?

In my March 2008 editorial, I suggested that, “At a journal, everyday life is measured by factors like: (1) paper cycle times, (2) quality and usefulness of the reviewing, (3) readability of the articles, (4) managerial implications of published work, (5) policy constraints, and (6) quality of the papers,” and promised to spend some time in future editorials discussing these issues. Factors (5) and (6) have been the focus to date, with discussions about taking steps to ensure that good papers are not rejected, and policy constraints that restrict the scope of a journal to something less than the scope of the field it serves. The current editorial will be the last attempt for the nonce to deal with this critical topic. In the December 2008 editorial, we will turn to factor (3) readability of articles, which will segue later into (4) managerial implications of published work.

On factor (6)—how to assure that we publish the highest quality papers—I have argued that the biggest problem a journal has is when it rejects papers that its community would have welcomed. Since the purpose of a journal is the dissemination of the most advanced scientific knowledge, rejecting good papers subverts the mission of the journal. The policy at *MISQ* is to consider all papers that meet our mission, which has been recently enhanced to read:

The editorial objective of the *MIS Quarterly* is the enhancement and communication of knowledge concerning the development of IT-based services, the management of IT resources, and the use, impact, and economics of IT with managerial, organizational, and societal implications. Professional issues affecting the IS field as a whole are also in the purview of the journal.

If we miss publishing a good paper that fits this mission, then it hurts the journal more than publishing weak papers, which will not be deemed useful and, therefore, not widely cited.

In my June 2008 editorial, I proposed that we deal with this issue by adopting a very different reviewing stance toward papers, in so far as it is possible to do this while maintaining the high standards for acceptance. The topic is so important that, as indicated above, I would like to spend a few more moments of your valuable time by expanding on the concept, beyond what was stated in the June editorial.

First of all, it bears repeating again and again that journals should not be known for the papers they reject, but rather for the good papers they publish. When focused upon, high rejection rates convey the wrong message and, in fact, are not significantly correlated with journal rankings.¹ The right message, in my opinion, is that we actively seeking out the best work in the field. In short, journals (read here “journal editors and reviewers”) should be eager, even excited, about bringing out the best in the submissions and helping to craft some into well-wrought ideas.

Rejecting Good Work: A Case In Point

How does good work get rejected by the major journals? My argument in the June 2008 editorial was that methodological considerations frequently prevail even when, in fact, the ideas are liberating and ground-breaking (see also Dennis et al. 2006). Perhaps this would be more readily understood if it were not merely an abstraction, but if we had an actual case at hand.

¹Lewis et al. (2007) report that journal acceptance rates from Cabell and English (2004) show no statistical relationships with IS journal rankings.

I am privileged to be able to discuss, as a case in point, the reviewing process that lay behind DeLone-McLean (1992), the most heavily cited article in the IS literature between 1990 and 2004 (Lowry et al 2007). I will also discuss the reviewing errors that occurred in their follow-up article in 2003.²

Table 1 presents some of the results of Lowry et al.'s 2007 analysis of the most cited articles in the IS literature over a 15 year period from 1990 through 2004. It shows that, based on ISI citations counts, DeLone-McLean was the premier publication with the largest number of scholars referring to this article.

For those few persons in the field who may not have read or remember the essence of DeLone-McLean, here it is. Published in *Information Systems Research* when it was a relatively new journal, the title of the original article nearly says it all: "IS Success: The Quest for the Dependent Variable." The authors' focus is on the concept of success in information systems, and it arrives at its conclusions via a reading and interpretation of 180 articles that deal with systems success, and a taxonomy of the forms success takes. Its final contribution is a descriptive model, the famous ISM, or IS success model, where the dependent variable is placed in a nomology and broken up into numerous sub-constructs such as system quality, information quality, use, user satisfaction, individual impact, and organizational impact.

The follow-up article, which was published in *Journal of Management Information Systems* in 2003 has an equally incisive title: "The DeLone and McLean Model of Information Systems Success: A Ten-Year Update." This article takes into account the intervening years of criticism and research activity in system success. It adds the construct service quality to the ISM, and integrates individual and organizational impacts into a net benefits construct.

The Tale of the DeLone-McLean Reviewing Process

Let me begin the story of how the original submission was reviewed, and finally published, by foreshadowing that it was nearly rejected by *ISR*. Bill DeLone was an assistant professor at the time and his coauthor, Eph McLean, was already a senior distinguished IS scholar who had recently accepted an endowed chair at Georgia State University. Whereas Bill and Eph had had success publishing articles in *MISQ* in the 1980s, *ISR* was a new journal in the early 1990s, but one that everyone believed was destined to become a top-ranked journal. Therefore, Bill was amenable to Eph's suggestion that they try the "new" journal.

The manuscript immediately had rough sledding in the reviewing process. One can easily imagine this based the précis of the paper that I just gave. The reviewers were highly critical of the methods since they involved a qualitative assessment of a fairly large research stream. They focused on ways in which the authors may have gone astray in determining that this was the best taxonomical reduction and were critical of the theoretical basis of the paper.

Had the reviewers been simply polled and the majority won, the paper may never have been published. Fortunately, the senior editor stepped in, believing that the paper would be well received and possibly even seminal. The decision, thus, was to publish. This was fortuitous, of course.

Please note that consistent with the arguments I have been making in the March and June editorials just past about the meaning of citations, we can further reason that the senior editor was correct and the reviewers were wrong. Why? Because they focused on methods to the detriment of ideas.

It was perhaps predictable that the follow-up manuscript developed after the turn of the millennium would suffer from the same reviewer overemphasis on methodology. The authors did not change their approach by adopting statistical meta-analysis or content analysis techniques. Instead, as in the original, they read the pertinent research and carefully interpreted it. It became clear to them that service quality was an important new dimension and that the split between individual and organizational impacts was no longer supportable. So they proposed a refined version of the ISM.

²This analysis was undertaken with the permission and involvement of Drs. DeLone and McLean. They have read my retelling of the tale for accuracy and have graciously agreed to let me use it as an example of my main point. They were particularly hopeful that it would help those just entering the field to better understand how the journal reviewing process works (or does not work).

Table 1. The Top 25 Cited Articles in Lowry et al. (2007)

	Authors	Title	Journal	Year	Cites
1	Delone, W. H., and McLean, E. R.	Information Systems Success: The Quest for the Dependent Variable.	ISR	1992 (3:1)	432
2	Moore, G. C., and Benbasat, I.	Development of an Instrument to Measure the Perceptions of Adopting an Information Technology Innovation	ISR	1991 (2:3)	278
3	Orlikowski W. J., and Robey, D.	Information Technology and the Structuring of Organizations	ISR	1991 (2:2)	256
4	Adams, D. A., Nelson, R. R., and Todd, P. A.	Perceived Usefulness, Ease of Use, and Usage of Information Technology: A Replication	MISQ	1992 (16:2)	209
5	Mathieson, K.	Predicting User Intentions: Comparing the Technology Acceptance Model with the Theory of Planned Behavior	ISR	1991 (2:3)	209
6	Taylor, S., and Todd, P. A.	Understanding Information Technology Usage: A Test of Competing Models	ISR	1995 (6:2)	205
7	Cooper, R. B., and Zmud, R. W.	Information Technology Implementation Research: A Technological Diffusion Approach	MS	1990 (36:2)	190
8	Venkatesh, V., and Davis, F. D.	A Theoretical Extension of the Technology Acceptance Model: Four Longitudinal Field Studies	MS	2000 (46:2)	151
9	Compeau, D. R., and Higgins, C. A.	Computer Self-Efficacy: Development of a Measure and Initial Test	MISQ	1995 (19:2)	148
10	Bakos, J. Y.	Reducing Buyer Search Costs: Implications for Electronic Marketplaces	MS	1997 (43:12)	144
11	Brynjolfsson, E., and Hitt, L.	Paradox Lost? Firm-Level Evidence on the Returns to Information Systems Spending	MS	1996 (42:4)	138
12	Melone, N. P.	A Theoretical Assessment of the User-Satisfaction Construct In Information Systems Research	MS	1990 (36:1)	133
13	Orlikowski, W. J.	CASE Tools as Organizational Change: Investigating Incremental and Radical Changes in Systems Development	MISQ	1993 (17:3)	132
14	Brynjolfsson, E., and Smith, M.	Frictionless Commerce? A Comparison of Internet and Conventional Retailers	MS	2000 (46:4)	130
15	Weill, P.	The Relationship between Investment in Information Technology and Firm Performance: A Study of the Valve Manufacturing Sector	ISR	1992 (3:4)	126
16	Bakos, J. Y.	A Strategic Analysis of Electronic Marketplaces	MISQ	1991 (15:3)	123
17	Hartwick, J., and Barki, H.	Explaining the Role of User Participation in Information System Use	MS	1994 (40:4)	123
18	Thompson, R. L., Higgins, C. A., and Howell, J. M.	Personal Computing: Toward a Conceptual Model of Utilization	MISQ	1991 (15:1)	120
19	Goodhue, D. L., and Thompson, R. L.	Task-Technology Fit and Individual Performance	MISQ	1995 (19:2)	118
20	Iacovou, C. L., Benbasat, I., and Dexter, A. S.	Electronic Data Interchange and Small Organizations: A Test of Internal and External Factors	MISQ	1995 (19:4)	117
21	Niederman, F., Brancheau, J. C., and Wetherbe, J. C.	Information Systems Management Issues for the 1990s	MISQ	1991 (17:4)	117
22	Brancheau, J. C., Janz, B. D., and Wetherbe, J. C.	Key Issues in Information Systems Management: 1994–95 SIM Delphi Results	MISQ	1996 (20:2)	111
23	Alavi, M., and Leidner, D. E.	Review: Knowledge Management and Knowledge Management Systems: Conceptual Foundations and Research Issues	MISQ	2001 (25:1)	110
24	Webster, J., and Martocchio, J. J.	Microcomputer Playfulness: Development of a Measure with Workplace Implications	MISQ	1992 (16:2)	109
25	Orlikowski, W. J.	Improvising Organizational Transformation Over Time: A Situated Change Perspective	ISR	1996 (7:1)	107

The tale of moving the work into print was similar to the original tale except that the paper was rejected at *ISR* this time round, but found a welcoming home at *JMIS*. Why was it rejected at *ISR*? The authors logically assumed that since *ISR* had published the original paper, an update would be a natural and welcome extension.

The background story is straightforward. The reviewers found fault with the methodology and felt that the above-described modifications and additions to the ISM did not justify publication, but they were, in this situation, not overruled by the senior editor. Was the review team right or not? To quickly test this rival hypothesis, I ran a citation analysis using the Harzing.com "Publish or Perish" software. This software employs Google Scholar, so it likely overstates the influence of a work, but the numbers are still impressive. Since its publication in 2003, this DeLone-McLean piece has had 501 citations, or 83.5 per year. This, in my experience with the range and character of Harzing's typical numbers, is impressive.

I would hazard that, once again, the citations tell the tale and *ISR* missed out on what will be another highly cited paper, which I am equating with excellence. The reviewers were once again wrong, and I would go further by stating that the editor was also wrong, even more so. I am further humbled by this tale since it happens that I was the *ISR* senior editor on the paper.

You may rightly ask how my actions at that time can be reconciled with what are now my repeated arguments in favor of an editorially directed process at *MISQ*, one where editors should be duly influenced by reviewer expert opinions, but not so deferential to these that reviewer votes in favor or against a paper determine its fate.

The answer is simple enough. At the time, I lacked the insight (and possibly even courage) to confront the rest of the review team and to push the paper forward in spite of the reviewers' heavy opposition. My current view of what can happen in the reviewing process has been strengthened by what I learned about myself in giving in to such expediencies. Good editors need to have courage in their own convictions and they need to take risks.

Taking Risks

This brings me back to my central point. Great journals need to publish good work, and, whenever possible, great work. The responsibility for finding this great, or at least good, work lies with the editors. Whether the work is unsolicited and has been submitted in the regular review process, or whether the editors seek it out, the job of the elite editorial boards is to move papers through to publication. And this involves taking risks at times, as with these two DeLone-McLean papers.

The job of journal editors is NOT to reject papers. Let me repeat this again for emphasis: The job of journal editors is NOT, I repeat NOT, to reject papers.

Rejection of manuscripts is undeniably a frequent outcome in the normal reviewing process. But it is not an outcome that is meaningful since it does not have a direct connection to the overriding purpose of scholarly journals, which is to publish work that advances and reshapes the field. Restated, the objective of our editors and reviewers should be to find the most intellectually stimulating papers, even if they are diamonds in the rough that will need a significant amount of polishing.³ This is vastly preferable to publishing incremental work that is more methodologically pure and requires less polishing. Good ideas should always prevail over good methods, all things being equal.⁴ We need to move from a journal culture of rejection to one of active search for the best work being produced by our best scholars.

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³This was the hallmark of Carol Saunders' editorship at *MISQ*. I am merely continuing Carol's clarion calls for this kind of developmental reviewing and am not claiming any mark of innovation in this regard.

⁴Please revisit my June 2008 editorial, where this idea is developed in much greater detail.

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