

EDITOR'S COMMENTS

Why Top Journals Accept Your Paper

In my inaugural editorial of March 2008, I promised to discuss the everyday life of journals in that and subsequent editorials. 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."

Over the last year and a half we have covered all factors except 1 and 2, which I propose leaving to my final editorial, at which point we will have had several years worth of the online processing system, ScholarOne Manuscript, under our belts and, therefore, more accurate and fitting statistics.

In the current editorial I would like to ruminate about why top journals accept papers. This is a topic close to my heart and one that I have presented, in part, at numerous IS venues worldwide. I claim nothing more for this list than that, while consistent with the socio-metric literature across the business disciplines, they are "mine own." For all I know they may not resonate with other editors. Or they may be generally agreed to by senior IS scholars. I can only speculate about that. Therefore, please use them as you will, cautiously, with this caveat in mind.

The Importance and Difficulty of Publishing in Top Journals

It perhaps goes without saying that this topic is timely and relevant to academics. Publications in the top journals are critical for career advancement and for the advancement of science. Yet having papers accepted at the major journals in the field is a significant hurdle, one that cannot be enjoined by mere words of encouragement or exhortations to "try again" or "try harder."

How high is this hurdle? The stark reality is that a few hundreds of people publish a very small number of top journal articles over half a career while very few persons publish dozens upon dozens, with the middle part of the distribution being extremely sparse (Chua et al. 2003). Known as a power distribution, this description of IS publishing behavior has been found repeatedly in scientometric studies (Athey and Plotnicki 2000; Dennis et al. 2006).

Why is the central region of the distribution so sparse? Why is it not closer to normal, with a larger bulge in the middle, that is, a few hundred people publishing a half dozen or more top journal articles over the span of half of their careers?

The simplistic and often-offered explanation for this is that, even assuming the quality is present, the top journals do not have the space to publish all of these articles. Therefore, the gatekeepers restrict the pipeline to correspond to the space limitations.

While there is some evidence that this has been true in the past (Dennis et al. 2006), is it still the case? Since the Dennis et al. article appeared in 2006 with its clarion call for top IS journals to increase their ability to print more articles and help to level the playing field against other business disciplines (Kozar et al. 2006), *Information Systems Research* has explicitly increased its pages and *MIS Quarterly* has not only published an extra issue since 2006, but also now has the ability to print many more papers than it has traditionally published. Electronic journals like *Journal of the AIS*, of course, have the luxury of not having such space limitations. Thus, in 2009, this explanation may be overblown.

If the problem is not solely one of capacity, then why can't the field move away from a power distribution toward a more normal distribution, one where large numbers of people hit the "A" journals every so often and fewer who do so only once in their

Table 1. Criteria for High Quality Research (Reproduced from D. W. Straub, S. Ang, and R. Evaristo, "Normative Standards for MIS Research," *DATA BASE* (25:1), 1994, pp. 21-34. Copyright © 1994, Straub, Ang, and Evaristo.)

Criteria	Chase (1970)	Wolf (1970)	Price (1985)	Daft (1985)	Mitchell et al. (1985)
1. Statistical/mathematical analysis	X	X			X
2. Theory	X	X	X	X	
3. Coverage of significant literature	X	X	X		
4. Professional style and tone		X		X	X
5. Logical rigor	X		X	X	X
6. Contribution to knowledge	X	X	X		X
7. Contribution to practice	X	X	X		
8. Presentation level		X			
9. Research design		X		X	X
10. Adherence to scientific ethics	X				
11. Manuscript length		X			
12. Reputation		X			
13. Replicability of research	X	X			
14. Suggestions for future research		X	X		
15. Topic selection	X	X	X	X	X

careers? I believe that this underlying problem is that many do not fully understand the reasons top journals accept papers in the first place. This was the point that Soon Ang, Roberto Evaristo, and I made in 1994, one that had been articulated by numerous management researchers to that time and afterward. To raise the general level of appreciation of these factors in the profession, it is useful to know why papers are accepted at the top journals, a set of criteria that seems to be fairly consistent across the business disciplines at least. Table 1, reproduced from Straub et al. (1994) illustrates some of the criteria tabulated by previous scholars to that date.

There is some evidence that the predominant reasons for paper acceptance/rejection, such as those in Table 1, endure over time. By way of illustration of this tendency, I refer readers to Stout et al. (2006), who found that manuscripts were rejected when (1) the study was poorly motivated, (2) it was poorly designed, and/or (3) it did not make a significant contribution to the accounting education literature. Poor writing was also cited in this study as a common reason for rejection. Kekäle et al. (2009) conclude that papers fail to make the grade because they are not sufficiently original.

It is perhaps understandable that my current thinking about why papers succeed in getting through the rigorous review process of the top journals has evolved since our 1994 study. Based on an analysis of empirical data collected directly from published IS scholars, Straub et al. put forth criteria that were normatively important. These factored into four groups: (1) conceptual significance, (2) practical significance, (3) research design, and (4) presentation. Indeed, these four categories now form the evaluation criteria for all *MISQ* reviews.

What these categories do not do, however, is make clear which combination of required and enhancing characteristics lead to a successful manuscript. I would like to reprise the results of this 1994 study for contemporary times, simultaneously augmenting them with characteristics that I personally feel are key. We can foreshadow this discussion with all 10 reasons summarized in Table 2.

Let's discuss each of these in turn, noting at the outset that reasons 1 through 4 are required elements whereas items 5 through 10 are enhancing elements. By "enhancing," I mean that if they are present, then they heighten the chances of acceptance. I do believe, however, that there is latitude in the extent to which the latter six reasons factor into the decision to accept.

Table 2. Ten Reasons Why Top Journals Accept Your Paper

A paper is accepted at a top journal because...	
Required Elements	1. Its basic idea is exciting (blue ocean strategy).
	2. Its research questions are nontrivial.
	3. It hits themes that are popular.
	4. It sufficiently uses or develops theory.
Enhancing Elements	5. It follows a recognizable formula.
	6. It covers the key literature sufficiently.
	7. It is clean (grammatically, typographically, appearance).
	8. It effectively uses or applies new methods.
	9. It does not vehemently contradict the work of major movers and shakers.
	10. It has a respectably large field sample (empirical, quantitative-positivist work).

Reason #1: Exciting, Blue Ocean Ideas

Kim and Maubourgne (2007) have set the marketing profession afire with their business logic about blue and red ocean product innovations. They argue that products like the extremely inexpensive automobile, for example, the Model T popularized by Henry Ford, were breakthrough ideas that essentially made competition (red, as in bloody waters) irrelevant. By creating a product that was in an entirely new market space, the Model T was in a blue ocean, uncontested for many, many years and enormously profitable for Ford. By the same token, I believe that the single most critical reason papers are accepted by our best journals is that they move into intellectual territory that is unexploited—in short, blue oceans.

This reason is a primary focus of good reviewers. The code word for this is “contribution.” Contribution will also show up below under nontrivial research questions and theory, but the quintessence of this factor is novel ideas. Both reviewers and readers are members of the IS community, and what stimulates them intellectually is a new perspective on an old problem, since there are seldom brand new problems in life. “The sun also rises,” as Ecclesiastes says, and this could be nowhere truer than with scholarly articles in the business disciplines. The first questions readers ask themselves are, “What is new here? Where is the ‘hook’?”

An example of such a blue ocean article is Mata et al. (1995). This paper was the 27th most cited paper in the key IS literature from 1990–2006 (Lowry et al. 2007). In that one of the authors was Jay Barney, one of the founders of the resource-based theory of the firm, it is perhaps not surprising that this article introduced the concept of sustainable competitive advantage from IT and detailed the logic whereby IT applications qualify as rare, valuable, inimitable, and nonsubstitutable resources that could lead to a competitive edge. The paper brought the resource-based view of the firm solidly into the IS field.

Another article that aptly illustrates this point is DeLone and McLean’s IS success model (1992). The most heavily cited paper in the 1990–2004 period (Lowry et al. 2007), this article focuses the field on the key variables that lead to the successful systems. It offered such a powerful, macro-level view of much of the extant literature in the field that it is still widely used and cited to this day. Their update in 2003 showed how the model could be reinvigorated and reapplied by contemporary IS scholars.

Clearly not all papers are this groundbreaking and yet they can still be acceptable in the top journals. One reason would be that, while not blue oceans nor groundbreaking, they pose fascinating research questions—puzzles to be solved (Kuhn 1970).

Reason #2: Nontrivial Research Questions

When is a research question nontrivial? To some extent, it is a question that has neither been addressed nor answered satisfactorily before. What exactly does this mean?

In his stimulating book on the persistence of scientific revolutions, Thomas Kuhn (1970) argued that most science operates within what he calls the “normal science” paradigm, where the researcher is making incremental additions to a body of knowledge. According to Kuhn, studies that by and large replicate other work become commonplace so that scientists can learn where the paradigm fits and where it needs to be reformulated to fit better. When the need to reformulate grows too unwieldy through anomalies that cannot be readily explained, the time is ripe for a dramatic revolution in thought and the emergence of a new paradigm for explaining the natural world.

To the extent to which Kuhn’s model can be applied to social science research, one has to wonder why we do not see more strictly replicative or largely incremental studies in our top journals.¹ The premium here is placed rather on novelty and newness, especially in the realm of research questions. Normal science favors replications, with extensions in persons, settings, and times in order to build a more robust paradigm, but I would never advise a junior faculty member to adopt this publication strategy because the editors in our top journals prefer research questions that depart in significant ways from what we have seen in the past²—in short, research questions that have not been well investigated before.

Even when scholars are invested in a theory base that has been popular in the past, a theory like the technology acceptance model (TAM), they typically must seek out “strange new lands” and ask questions that are very different from those asked in the past. In our paper on the impacts of gender on TAM (1997), David Gefen and I brought in the socio-linguistic literature to show how men and women differ with respect to preferences for certain forms of communication. Our research question was simple enough: How does gender affect the causal relationships in TAM? But, given that this particular topic had not been examined before, the review team apparently thought that the issue was nontrivial and worthy of publication (and also, of course, sufficiently well done).

Reason #3: Popular Themes

Ironically, even though review teams are highly receptive to new ideas, if the ideas are too radical and lie too far beyond the reviewers’/evaluators’ experience, their comfort zones will be stretched beyond their limits and the sad result will be rejection of the paper. The good news, though, is that nearly all new topics can be linked to popular themes in the IS field or other fields. Many papers dealing with trust in online vendors have been welcomed by our top journals, I would argue, because IS scholars tied their work to marketing and management studies where face-to-face interactions with vendors were more typical (e.g., Pavlou and Gefen 2004; Wang and Benbasat 2005). Applying trust to the online environment was originally a blue ocean for IS (Gefen 1997), in spite of the fact that the theme had been popular in marketing studies for years before this (e.g., Moorman et al. 1996).

Once a theme has been introduced into the field, the resonance of the theme within the field spurs new work. Whereas it is difficult to introduce wholly new, full-blown ideas into the field at this time, mini- or micro-revolutions in thought are still possible,³ and this is where most scholars focus their subsequent attentions. In this way it is possible to seek out blue oceans even when a radical revolution in thought is no longer possible.

Reason #4: Theory

Theory is King and it is in the evaluation of the theoretical contribution that most reviewers become convinced, or not. What may not be clear is how this attitude relates to blue oceans in that authors very seldom are developing brand new theory; they are most often applying a theory and then showing how variations or refinements of this theory can be applied to a new domain.

¹As noted under “popular themes” below, studies do appear in our top journals that retest the nomology of particular research streams. Without this significant overlap between studies, it would not be possible to conduct meta-analyses, of which there are many examples in the IS field, as in other business disciplines.

²For those who see value in Kuhn’s work, there is something of a conundrum here. Whereas we likely should be publishing more incremental work (normal science) to extend the generalizability of results (external validity) and build a cumulative tradition, the field has a low appetite for this. The coin of the realm is novelty and blue oceans.

³I would define mini- and micro-revolutions as studies that offer major or minor refinements to the prevailing nomology.

In the last issue of *MISQ*, Angst and Agarwal (2009) applied the elaboration likelihood model (ELM) to the phenomenon of resistance to electronic health records (EHR). The authors apply the theory faithfully to this domain, but what is most exciting about the paper, in my opinion, is that the application of the theory lends credence to ways in which managers can make these systems more attractive to potential users. In short, this theory seems to be a puissant means for showing managers how to manage EHR.

As I interpret the acceptance of this paper, it was not the theory base *per se* that made the difference. In fact, it is a quite old theory base (for business schools), dating from 1981 (see Petty and Cacioppo's book where they elaborate on the elaboration model). And ELM has been applied in IS before (e.g., Bhattacharjee and Sanford 2006), so the mere application of this theory was not an innovation.

Although it is clear from prior studies of paper acceptance criteria that theory is important, I believe that it is the unbeatable combination of a reasonably well applied theory answering interesting, novel questions in a well known thematic stream of work that leads to blue oceans. Ideas become striking when we instantly know that we have not read something like this before, at least not from this perspective. And Angst and Agarwal achieved this admirably.

Reasons 5–10: The Rest

Reasons 5 through 10 are enhancements or things not to do when submitting manuscripts to our highest tier journals. In the best of all possible worlds, if one did numbers 1 through 4 well, the reviewers would view the rest of these reasons as sufficient conditions and their absence or ineffectiveness would only be an irritant. Take reason 5, following the standard structure for a paper of this type/paradigm (the recognizable formula). Readers, for example, come to expect a positivist, quantitative paper to assume the structure of (1) introduction/motivation/research questions, (2) literature review, model and hypothesis development, (3) methodology, including instrument validation, (4) data analysis, and (5) discussion, including implications for scholars and managers, limitations, and future research directions. If an author ignores this structure and skimps on the methodology section, as a case in point, the reviewers will be vaguely unsettled and they will then give the authors a long list of methodological issues to deal with in the revision, if they are disposed to even invite a revision. Just having all of these sections at sufficient length does not in any way guarantee acceptance, naturally.

Reason 6, similarly, raises the issue of whether the authors have cited the key works, or missed some highly relevant citations or, occasionally, an entire body of literature. Good developmental reviewers will be perplexed by such oversights, but they will provide the relevant citations (but not ask the authors to spend the next three years of their lives reading the corpus of works in the socio-psychological literature, for instance) and wait to see if the authors are able to incorporate them successfully into the revision. Doing this element well seldom gets kudos from the reviewers, but not doing it well almost always raises hackles.

Good writing, reason 7, makes a paper a joy to read, but when the paper is ungrammatical, filled with sloppy errors, badly organized, and awkwardly phrased, it only serves to put readers off. Developmental reviewers, again, will spend a lot of time helping authors correct such mistakes, albeit grimacing as they labor with the paper. In brief, a well written paper lacking exciting ideas and an applicable theory is still a show stopper whereas reviewers are more likely to grit their teeth and spend effort on poorly written papers, as long as the potential contribution is present.

Authors who introduce new methods to the study of perennial problems in the field (reason 8) add value by giving new perspectives primarily, but not entirely, through the methodology and the fresh insights it provides. This was a significant factor, as I see it, in the acceptance of Sidorova et al. (2008). In this article, the authors used latent semantic analysis to determine the underlying thematic elements in the IS field over a 20-year period. This technique had not been applied to IS before. The paper was also a blue ocean in that it offered some reasonably good empirical evidence that the field of IS was not fragmented, as advocated by Banville and Landry in their landmark 1989 *Communications of the ACM* article, but was held together via a fivefold thematic core. Whether one agrees with their interpretations or not, the approach and evidence assembled broke new ground.

Another example of this is where the methodological insight is itself the blue ocean. In the current issue, Burton-Jones' article entitled "Minimizing Methods Bias Through Programmatic Research" says it all. The paper creates a new category of methods

bias that has not been recognized previously by methodologists. This kind of methods bias can severely damage the scientific credibility of our work and, therefore, needs to be carefully considered when developing and exploiting instruments.

Reason 9, that a manuscript does not contradict the work of movers and shakers, may invite controversy, but I will defend it on the following grounds. I believe that there is an underlying conservatism in science and scientific endeavors, which is the basis for Kuhn's argument about normal science. This body of knowledge is painfully accumulated through the efforts of the entire invisible college. In many cases, forward-looking researchers advance a true revolution in thought and draw support for their blue ocean. At this juncture the paradigm shifts and a new paradigm instantiated. And it is at this point that most normal science takes over again and support for the still-new thesis goes on, with variations that ascribe novelty to refinements in the model or proof/counterproof through new methods.

In information systems, I think that this is not an unreasonable description of the TAM research that has accumulated since 1986 (Lee et al. 2003; Venkatesh et al. 2003). Even the paper by Sharma, Yetton, and Crawford in this issue, which offers strong evidence that much of the explained variance in TAM is explained by common methods bias, is still in this tradition. This paper disputes the strength of the TAM paradigm, but does not at base contend it.

Once a new scientific paradigm is set in place, the bar for proof against it is raised (Kuhn 1970). This is the background for my argument that positioning one's paper as a totally new way of looking at a phenomenon while at the same time implying that the previous literature was simply wrong will place one firmly in opposition to many of the movers and shakers in the field. A strongly stated contention of the current paradigm will meet with natural resistance by those invested in the prevailing thought—the movers and shakers and their disciples. It will, thereby, significantly decrease the likelihood that the paper will receive a warm welcome from the reviewers, many of whom have been influenced by the movers and shakers.

Blue oceans enter a field not because they make strong statements about how they are radically different from previous views, but because there is an almost intuitive reaction on the part of readers to this difference. In short, to best position papers as the latest development in an established research stream, the authors' contributions should be stated as gaps or new perspectives and not as a fundamental challenge to the thinking of previous researchers. To reframe, papers should be in apposition rather than in opposition. My understanding is that Einstein did not position his new theories in opposition to Newton, but in apposition, that is, as a possible, novel interpretation of reality that he believed would eventually find empirical evidence in fact. As it did.

The tenth reason is targeted to positivist, empirical researchers, and articulated because it is an unspoken "rule" that there are a set of magic numbers that describe an acceptable sample size for a particular kind of study. If the reviewers do not believe that the sample size is sufficient, then the author has to either gather new data and return with that in hand or else convince the review team that the sample size is indeed large enough already.

And this is a nontrivial task. For some deepset reasoning that I only dimly understand, reviewers seem to have these magic numbers in mind and hold to these beliefs as if they were factual and generalizable. Often times the logic that is used to reject a paper is not even based on sound statistics. If, for example, an experiment has 2 cells and each has 15 subjects and the hypotheses are all significant and have high explained variance, this sample size of 30 is even better than a sample size of 100 or 200. Why? A student's T test with independent sample sizes of 15 is robust to violations in normality and other statistical assumptions. If the effect is strong with small samples, then it will be even easier to detect in larger samples. Finding significance in a sample of 30 tells us that the differences between means yields a large effect size.

Small samples are indeed a problem if some of the hypotheses are insignificant. At this point and only at this point does the possibility of a Type II error arise. But I have seen reviewers question sample sizes even when all hypotheses in a study were found to be significant. This is the problem with "magic" numbers.

As is obvious, I wish that this desideratum in which reviewers uncritically require their own magic sample numbers were not the case, certainly more so than the other reasons. But, alas, it is. As a general rule of thumb, samples of more than 100 seem to be acceptable. Except for the more complex models, statistical power often reaches the community standard of .80 at this number (Cohen 1988). We know in reality that there are many instances where smaller numbers are not only reasonable, but noteworthy, given the difficulty of collecting certain forms of data. Matched dyadic data from vendors and customers springs to mind.

Tension in the List

I would be the last person to deny that there is palpable tension in this list, perhaps even built-in contradiction. Wouldn't a true blue ocean have to step on the toes of movers and shakers, for example? At least to some extent? But that being the case, then this tension would be sorely missed if we are to see this as anything other than typical human, and social, experience. How many times in our own lives do we not find ourselves poised between two goals, goals that are inherently contradictory? My final and closing argument is that we may need to see contradiction for what it is, and draw closer to Walt Whitman's "Song" to himself when he says

Do I contradict myself?
Very well then, I contradict myself,
(I am large, I contain multitudes.)

Concluding Thoughts

It is equally likely that my 10 reasons will meet with some resistance, with phrases like "of course" and "so what?" That may be true for authors who have already had great success in publishing. But for many others, I hope that at least a few of these reasons will spark new thinking about why papers are accepted. If only a few people develop deep insights into how to rework their research so that it adheres to the critical success factors implied by this list and they later meet with better results in the reviewing process, then the purpose of the editorial will have been served.

We could have refocused this entire discussion on why papers were *rejected* and the logic would have been inverse, but essentially the same. But this would be just another instance of focusing on the negative rather than the positive. My coauthors Varun Grover and Pamela Galluch and I invite you to read (or reread) our March 2009 *MIS Quarterly* editorial on the harmful effects of downward spiraling or vicious cycles. We believe that the job of journals (and its editors and reviewers) is to publish exciting papers; it is not our job to reject papers. The latter is a necessary evil in maintaining high standards and requiring superior papers. But it is a consequence of this quality goal, not the goal itself. As I have argued in editorials before, it is not something we should feel good or brag about. We should be focusing on the high quality papers that have appeared and will appear in the future, assuming we remain careful and conscious of quality control.

My personal belief is that we have such deep talent in the IS profession that individuals can in fact learn how to more effectively conduct their research and articulate their ideas and that this can eventually lead to higher quality drafts coming into our top journals. I trust that our experienced editors will recognize these blue oceans when they appear and be eager to fully cooperate with our authors in developing a publishable paper. A much higher percentage of papers accepted from those submitted means that the input has been dramatically improved. And a higher acceptance rate will not be in vain as we now have enhanced capacity to print these papers. I believe that this is an attainable goal for the field.

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