1-2016

Optimizing the Business Side of Science: Publication Review Cycles and Process Management Considerations

Thomas F. Stafford
University of Memphis, tstaffor@memphis.edu

Optimizing the Business Side of Science: Publication Review Cycles and Process Management Considerations

Thomas F. Stafford

University of Memphis

tstaffor@memphis.edu

Abstract:

What's the most valuable thing in scientific research? Data? Theory? Insight? Results? No; it is the reviewer. For, without the reviewer and the peer-review process, science becomes a specious thing, indeed. Who could trust prose not vetted by peers as being objective and generalizable? The reviewer is the backbone of the scientific publication process, and, yet, is among the most underappreciated and over-vilified party in science. Reviewers are often considered to be "late", "wrong", "biased", and, worse in the minds of many who submit to the premier journals of our field. In this paper, I deal with the issue of publication cycle times from behind the scenes and leverage my own views as someone who has commissioned innumerable reviews for potential journal articles as an editor and who freely recognizes the frailties of the process. Giving voice to a debate that recently transpired in our association's listserv, I speak to, respond to, and expand on points related to the time it takes for reviewers to do the work they do for us as authors, and I offer some insights as to the factors implicit in free service peer reviewing in the service of science and publication.

Keywords: Reviewers, Peer Review, Review Cycles, Cycle Time.

Peer reviewing is critical in the process of legitimizing new scientific knowledge. Yet, there are concerns about its quality. (Iivari, 2016)

High quality reviewers should be able to qualify a paper in few hours, like medical experts can establish a critical diagnosis (even in minutes). (Mora, 2015)

Long review cycles are not caused by poor reviewer incentives. This is a red herring, designed to divert criticism of the extraordinarily inefficient way we review papers, and the editors-in-chief who have the authority to improve but choose not to. (Ralph, 2015)

I still agree with [Professor Manuel Mora]: it only takes a few hours to render a quality evaluation of a paper. The slowdown is in the intake/assignment process, if one puts aside the notion that reviewers should jump immediately to assignments when received. (Stafford, 2015)

1   Introduction

I noted recently in these pages Juhani Iivari's paper on the state of reviewing in our field (Iivari, 2015). I was primed to notice it, I suppose, because I had just engaged in an extended round-robin of public discourse via listserv postings on AISWorld about review cycle issues in business research (cf., Jennix, 2015; Mora, 2015; Palvia, 2015; Ralph, 2015; Stafford, 2015). Having just engaged in a public debate, the issue was fresh and vital in my mind, and this vitality was made even more cogent by the introspections I had undertaken in my own editorial postings on how best to revise review processes to improve review cycle times.

I appreciate the fact that colleagues are motivated enough to *want* to comment on the process we undertake as editors, the procedures to which we submit ourselves as authors, and the outcomes we await with interest as readers in our branch of science. (By using the term "comment" here, I speak to the stimulus that Mora's (2015) publicly expressed concerns provided for my own thinking and to the even and measured discourse of Iivari's (2016) essay on the matter, which I reply to here). Critical comments mean engaged customers, and I like that. It far surpasses apathy, don't you think?

We have introspected on the state of the review process for decades in our field (cf., Gray et al., 2006; Kohli & Straub, 2011; Saunders, 2005; Saunders & Benbasat, 2007; Weber, 1999). Periodically, in our world of technology research, a thought leader speculates on reviewing, how it could be better, how it could be different, how it could be faster, or even how we could do *without* it (we can't, but kudos for creative thinking). My two greatest editorial mentors have spoken widely on this topic (and by this I do not specifically exclude any of the worthies cited here, for each has generously and ably advised me over the course of my editorial career). My favored mentors of finely considered advice on the editorial process are the former *MIS Quarterly* Editor, Carol Saunders, and the founder of this fine publication, Paul Gray, of blessed memory.

Carol became my hero when she publicly wrote of the need for developmental reviewing at a time when "fatal flaw" was the buzzword so often used by poorly unmotivated reviewers to truncate a review to its closest brevity. In her "diamond cutter" characterization of reviewers who contributed the precious treasure of thoughtfulness and guiding advice in their reviews even when (and, perhaps, *especially* when) rejecting papers, Carol set a standard we now benchmark and reference reviewer excellence, which we can see in follow-on essays that cite and expand her views, such as those by Straub (2009) and Kohli and Straub (2011).

Paul Gray, generous and selfless soul that he was (and I feel privileged to have known him early in my career), gave his time and support extensively to any author he encountered during *CAIS*'s fledgling fays, which seems to me precisely the reason *CAIS* became such an important publication so quickly. People such as Paul set the standard for service generosity, and people such as Carol established the baseline for developmental reviewing. They have each been important to improving the review process. Improvement starts at the top with the editors.

2   The Importance of Reviewing in the Business of Scientific Production

Who does *not* want good reviews? The question seems rhetorical since we all, as a community of scholars and scientists, clearly find benefit in fair and objective evaluation of our research submitted for

publication. We'd have to be fools to say, "No, really, I prefer to get three bad reviews and try to figure it all out on my own" or "Oh, by the way, take your time with it; I'm in no hurry".

The fact that folks want to complain in highly public forums about the state of peer reviewing in our research field signals three things to me. First, we have engaged customers for our journals (apathetic customers don't complain, please note), which signals that we have a vital and vibrant science, not a stagnant one. Second, I think the vocal desire for improvement signals that that the colleagues who use our journals are indeed discerning customers of the work that editors provide as a service to the field, which suggests that everybody, at least implicitly, recognizes the fundamental and central role of reviewing in science production. Third, it signals that editors have some work yet to do in almost every respect and at almost every publication. Just ask any author; you will hear, anecdotally, but chapter and verse, what's wrong with the review process: it's not timely, quality is low, and reviews don't support authors (Weber, 1990).

I'm citing anecdotal assertions here, albeit from a highly knowledgeable source; the actual research on the topic of review process performance tends to suggest that most authors are satisfied with peer review (Armstrong 1997), though this may well be in the same sense that Winston Churchill was satisfied with democracy[1]. Certainly, we know peer review to be the most critical part of the scholarly research publication process because researchers have affirmed that those who volunteer to read and evaluate our papers in the research publication process are our field's scarcest resource (Armstrong, 1997; Gray et al., 2006, p. 276). We can benchmark the scarcity of such a critical resource with the cost of providing it; peer review is quite costly with direct costs approaching USD$300 per person/event and a global cost of well over USD$2 billion (Smith, 2010).

The editorial decisions that such reviews support are critical to authors; they are nearly life and death in their importance to one's career trajectory, so the process's quality *is* important (Fletcher & Fletcher, 1997). Sadly, in blind peer reviewing, the temptation to let quality slide is not inconsiderable in some cases (Armstrong, 1997, p. 65). On the other hand, some research questions whether peer review even contributes to research's quality at all (Smith, 2010). These findings lead to many informed speculations about the possibility of doing *without* reviewer anonymity in the peer review process (e.g., Armstrong, 1997; Stamps, 1997; Weber, 1990). Looking to the anecdotal accounts published about blind review's seeming downside, we find the related authorial concerns to be resulting poor quality guidance for authors, slow review cycle times (evidencing the erstwhile "accountability" problem) and uncomfortable levels of subjectivity in assessment.

Hence, in one of the few published empirical studies of author dissatisfaction with the peer review process, of the top ten issues authors complain about with regard to peer review, concerns about blind reviewing ranked right at the top of the list (Stamps, 1997). Review reliability was fourth, bias came in fifth, but concerns for time lags in the review process came last at tenth. Which is to say that, while review cycle time *is* a problem since it did (however barely) make the top ten list, it pales in comparison to other, less publicly decried issues with peer review.

If we were to consider the research on authors' perceptions (e.g., Stamps, 1997) in leveraging our views on improving peer review as opposed to editors trying to guide operational decisions based on feedback from anecdotal concerns and listserv postings, then the blinded nature of most peer reviews and the related objectivity issues are what seem to predominate in most authors' minds. Researchers have challenged the notion that blinding reviewers' identity results in more objective outcomes on the grounds that the evidence indicates that blinding the identity of authors from less prestigious institutions does not statistically enhance their publication potential (Armstrong, 1997, p. 69). Rather, they have found that reviewers' apparent bias in the form of subjective or culturally bound assumptions seems to stand in the way of success (Pauleen et al., 2006; Stamps, 1997; Trauth, 2014), and one remains uncertain whether registering reviewers' identity would obviate that or not. There is an engaging suggestion that comes from the best introspection of a forum of informed editors: register reviewers' identities, but register them with the *editors*, not the authors. That is to say, accrediting reviewers and storing their identities and performance profiles in a database would be an enormous resource for editors hard pressed to find able, capable, and responsive reviewers with whom to improve review cycles and outcomes (Gray et al., 2006). It should also lead reviewers to be mindful of the notion that their "work" is evaluated and notice taken of their diligence.

---

[1] Winston Churchill was famous for his observation that democracy was the worst possible way of doing things *except for the alternative*.

www.manaraa.com

3   Reviewing: It's About Time

Notwithstanding the empirical evidence on authorial concerns with peer review issues, cycle time from initial submission to final editorial decision seems a most compelling operational issue, speaking for myself as an editor. While most editors would agree that fast is better than slow, I believe that there are problems with asserting that, if reviewers are slow in producing reviews, they actually harm (as opposed to simply discommoding) authors. I say this because a standard part of the anecdotal assertion that slow reviewing is a grievous harm to authors includes the implicit presumption that reviewers are indebted to, that they owe, or are in some way objectively accountable for the speed of their work. I suppose the main point of my essay, residing among many other points of interest and moment, is the important notion that peer review is not prized well enough by anyone but the receiving authors who are the ones who want it to be accounted for. To wit, those complaining about some anonymous reviewer "not doing their job" by returning a review more slowly than desired are, in reality, making their complaint about a party that is "doing a job" (for which the recipient would like to hold them accountable) for *free.*

We all know this, each of us, at center and implicitly, but I don't think we actually stop in our rush to judgment to directly consider and evaluate our responses to the review cycle time issue in the context of the important notion of somebody doing us a favor that they really don't have to do and are in no way compensated for. Hence, when we issue or reply to strident cries for "reviewer accountability", I, having served as an editor who could not have operated without the cheerful and voluntary service of dedicated reviewers, feel it incumbent on myself to say "to what do we account?". For to call a party to account is to implicitly speak the language of transaction between parties, which then raises the question of compensation and value in the exchange. Since reviewers in our field are not compensated and are also not widely rewarded in tenure track considerations for the important service they provide, it's like asking the Pope, if you'll pardon the religious metaphor, to give one back their money when they don't get the blessing they expected.

For shame. Pogo the Possum in the funny papers once said (and I'm dating myself here), "We has met the enemy, and We is Him". Just so. We are the enemy when everyone wants a good review and nobody has time to give one back. Science has become selfish when it once was selfless; subjective and mean when it once was objective and fair. "Those [damn] reviewers ought to be held accountable!"[2]**.** Is that science at all, I wonder?

   I don't blame you; I'd want my money back, too ☺[3].

In addition to my sarcastic metaphor, above, about wanting my money back for a critical service that was rendered to me free of charge, I have two other metaphors more proper to the notion. My old friend Dennis Gonier, a man who lived his business life on Internet time having founded America Online's internal research division, was fond of saying:

   In every managerial decision there are three things desired: you want it fast**,** *you want it* cheap, *and you want it* good*. You get your choice of any two of those.*

The managerial wisdom of this is that anything fast will either be good and expensive or cheap and bad, and, by corollary, that anything free cannot be both fast and good. For those who insist that free is part of the bargain in peer review as is the case in our field (but not in other fields, which charge submission fees and then pay review honoraria and where review returns are often subsequently blindingly fast), then the truism of managerial worth dictates that either quality or speed must suffer in a free review process. Leaving aside other truisms about good work taking sufficient time, I simply remark that those who complain about the speed of returns in the peer review process in this field *are only getting their money's worth,* plain and simple.

That is, to be sure, a sarcastic observation and not helpful to what I perceive is a problem, but it sure feels good to say it; so true it is, and so sympathetic I am to the plight of the poor overworked and underpaid reviewer. Sure, I would like the review process to move more quickly, and I think most right-minded editors work hard to that end. That said, other than sweat equity, there are few means at any editors' disposal to

---

[2] These are nearly the actual words (taking minimal license) that a member of a publication board I know of used to describe how to deal with "the review problem". Hold them accountable, he said, as if there were an account to hold. It is not an economic transaction and we are not the bosses nor the managers of the folks who volunteer to vet our papers for us. Have we overlooked that these fine and rare colleagues who agree to review are doing so for free? Out of good will?

[3] My own snarky sentiment, in the sprit of "we typically get what we pay for" in the review process.

speed the review process other than redesigning the process. The actual human resources of the journal review system, which are the esteemed review board members, are serving the review process out of sufferance, and one dares not push them too hard lest they also run the mental calculus of the cost-benefit analysis we all carry with us as rational economic customers and conclude that "this job does not pay enough" and take their work elsewhere in the style of Johnny Paycheck[4]. The research shows that pushing volunteer reviewers too hard for better performance leads to alienation that will result in longer, not shorter, cycle times (Stamps, 1997, p. 4).

I am glad, on the one hand, for authors who reach out to express their concerns about the process of peer review. Editors are marketers (selling the research publication service against worthy competitors in the form of other journals), and good marketers roundly prize customer engagement. Engagement permits editors to improve the quality of journal offerings, which is all to the good. As a marketer, I would preach the wisdom of *prizing* complaining behavior because we'll never learn more about improving processes than we will from an unhappy customer. Nor will we ever have a more loyal customer than the complainer we hear and effectively respond to.

On the other hand, editors as production managers are more challenged to reconcile the notion that the customers who sometimes complain are also the catchment group for providing the services of which they complain. To wit, each author is also of very necessity a reviewer. And reconcile this editors must because the people who complain about the service are also the people who are able to pitch in and contribute to improving the service. Some editors feel that those who complain loudest about slow reviews are those who provide the least service to the review process (Weber, 1990), a notion that I suspect any editor might agree with out of sympathy. The review process is inherently asymmetric (per Carol Saunders, in Gray et al., 2006), but there is no cure for that, only patience.

Everybody wants a get a good review for their own papers, and few people have the time to perform a good review for somebody else's. I have countless anecdotes of entreating an individual with a rare skill to review a unique paper specific to their rare knowledge only to be blithely informed "I've got a lot on my plate right now" or, "I won't be able to get to it until next month". These are common response to editorial requests for help. Any editor would tell you the same. And who among us could complain back against such grounds? Editors are not exempt from being too busy to take additional tasks on, of being over-worked and underpaid, and so on. We can all well understand the reasons why reviewers approach are sometimes reluctant to take on additional assignments.

4   Come On and Take a Free Ride[5]

All who write might well also review given that peer review for juried publication is largely a free and unrewarded service that journals provide to better the career trajectories of participating authors. But all who submit and do not review ought most properly sit down and shut up when it comes to objections about cycle time because they are taking a free ride on a system that relies on the goodwill of colleagues who serve for the love of science and not the desire for pay. Brother Elwood Blues, channeled by actor Dan Aykroyd, said it best in one of his most famous songs: "Whadda ya want for nothin'? Rubber biscuit?"[6].

We all know that publishing boosts careers, earns promotions, leads to better jobs, pay raises, course reductions, and all manner of desired outcomes, and the corollary to its effects is that publishing relies on the service of reviewers whether blind or not, compensated or not, rewarded or not. We equally know, if we are completely candid with ourselves on the matter, that peer reviewing brings few if any tangible rewards, does not figure highly in promotion and tenure, and likely never was a significant consideration in the awarding of a raise or endowed professorship. Put simply, I know of no circumstances where reviewing was an activity that differentiated a colleague for a tenure decision, whereas most any tenure decision made in this world will hinge in some way on research productivity. Yet, the research that brings tenure requires the service of reviewers who do not gain tenure in providing it. The popular novelist Joseph Heller had a term for that sort of quandary: Catch-22.

---

[4] Author of the cultural mythos represented in the classic country song "Take this Job and Shove It".
[5] The musically informed reader has already noticed I have a proclivity for pop metaphors. This metaphor is from the Edgar Winter Group song of the same name.
[6] Johnson, Epps and Fulton's, "Rubber Biscuit" as performed by The Blues Brothers.

That, in a nutshell, is the pivotal issue around which the debate over review cycle time and review quality ought to revolve even if the empirical evidence suggests (as it does) that the key issues in reviewing are its blind nature and perceived connected issues of objectivity and bias. All who write can also, but often do not, review. Yet, all who write require (and, one might say, demand) reviewing and are increasingly discerning about its speed, quality, and content. As Carol Saunders said: *asymmetry.*

5   Money for Nothing[7]

I write here to speak to these issues in a forum more permanent and enduring and even perhaps more impactful than the unmediated listserv I frequently contribute to. Recently, some colleagues and I had an interesting interchange on the list. My paper appears now in response to the *CAIS* editor's interest here in more of the same, which Junahi Iivari (2016) has expressed after witnessing the listerv interchange and feeling motivated to write thoughtfully and in depth on the broader topics regarding the issues with peer review in our field.

To that end, the genesis of this paper was an interchange that took place in the closing days of March, 2015, on the Association for Information systems listserv called "AISWorld". Professor Manuel Mora from the Autonomous University of Aguascalientes initiated an interesting interchange (in which I and others participated) about issues with peer review cycle times at major business journals. The issue is clear to most readers: for a work task that really only takes a few hours (reading a manuscript to evaluate it for publication), the actual realized times for completing a review process from initial submission to final editorial decision can be extensive and frequently irritating to research authors who are anxious to attain decisions about their research submission. Anecdotes abound of papers taking many rounds and several years in review to achieve the final outcome at major journals, and, certainly, this is troublesome to editors and authors alike. Empirical evidence suggests wide variability on cycle time: typical reviewers evaluate less than 10 papers per year and, while a good review can take up to 6 combined hours to complete, the process itself can last from mere days to several years (Stamps 1997).

In that light, the question is why are cycle times such a culturally prominent issue in our field? I refer mostly to Mora, whose initial AISWorld posting decrying the current state of review cycles prompted me to write an AISWorld post in reply. Mora, an author frustrated with delays in review cycles, speaking at large to the colleagues of our field, was replied to and re-replied to yet again, by me responding from an editorial perspective in defense of highly-prized but voluntary reviewers. In that interchange, Mora said that a review should only take a few hours and so what justification is there that a reviewer can offer for, essentially, not retuning an assignment the day after reviewing it?

> *Accountability! Reviewers are not earning their pay! Blues Brother Elwood would endow the Rubber Biscuit Award for that notion.*

I freely acknowledge in my own quote from that series of listserv postings that appear at the opening of this paper that, leaving aside the notion that reviewers should drop everything and render instant service to papers, editors are the next in line for criticism because they are the operations managers of the process. The review process operational design is the only other point for discernment and improvement left after one dispenses with issue of reviewer dedication.

The notion that highly skilled business professors who are involved in peer review process would *not* be oriented toward quality control and process improvement seems patently absurd. We are in the business of teaching our students how to run businesses well; it stands to reason we also have a similar orientation toward conducting our academic business, the publication of research. Yet, to judge by the rising debate, constituents of the journal publication process fully believe that the managers of the process are unconcerned with process quality. Maybe editors are not earning their pay either[8]?

6   Mutiny on Bounties

Paul Ralph (2015) from the University of Auckland opines as part of the listserv dialogue that the review process is inherently inefficient and that the editors who manage it are reluctant to change it. Speaking as one such editor, I find I must agree: 1) the process is truly inefficient, and 2) I, an editor, am truly

---

[7] Dire Straits; the classic pop paean to imbalanced economic transactions

[8] I never get paid for editing any more than reviewers get paid for reviewing. It actually ends up costing editors a bit out of pocket to do the (voluntary) job when considering travel, supplies, and various marketing incidentals.

overwhelmed by the notion of changing it in the ways that would alleviate *all* of the complaints about the process. In operations parlance, the optimal solution is always more costly than enduring low levels of the problem.

I suppose that raises a couple of questions, however. One such question is whether the perceived inefficiencies in the review process, which are usually related in anecdotal fashion to the time taken between submission and final decision, are, in fact, a *bad* thing. After all, one rationalizes that the only ones complaining are the authors who are anxious for quick publication results, and, on that point, I made a point in the AISWorld interchange that is well worth repeating:

> *On the side of constituent pressure for quicker queues to acceptance for articles, there is the equally important and increasingly popular issue of what I will call "publication bounties". When scholars are paid interestingly large amounts of money as a bonus for successful publication in premier journals—well, of course they get anxious to find out what has happened with a given submission as quickly as possible. Big money is riding on that decision. Yet, it remains that the folks upon whom they rely and become increasingly impatient with for results are not given bounties for reviewing the work that will be subsequently rewarded when published. This is a business scholarship issue to think about; we appear to be handsomely incentivizing the production of scholarly research, but not its review. Marx would have field day with that economic equation.* (Stafford, 2015)

To be sure, most participants in the debate over journal cycle time problems were focused on the time it takes to generate a completed review. Yet, at the same time, the research on the matter clearly identifies the *objectivity* of the review process as the key and most troubling issue (Stamps, 1997). We have seen relatively less introspection on the reason why long review cycles are distasteful to the parties of the debate (why (some of) the authors are complaining so loudly in other words).

This introspection takes place behind the scenes, where editors and their boards are asking themselves why authors might be unhappy with review process cycles that have heretofore been deemed acceptable if not exemplary? To be blunt, many editors are increasingly concerned about "pay for publication" schemes that are abounding in both Western and Eastern academies. We all hear about these in part of the popular research mythos. Some few of us may already benefit from such schemes; I know that my academic employer offers appreciable bounties for premier-level publications.

This raises the important question about author discontent over cycle times in the face of the research indicating that issues of objectivity are more predominant: are authors who envision such publication bonuses *unreasonably impatient* for the most immediate service obtainable in the review process because they anticipate outstandingly large pecuniary rewards for success? We Americans of the country persuasion typically resort to folksy metaphors of ursine excretory activities in forest habitats to answer such obvious questions.

With big money in play (anecdotal accounts of USD$5,000 to $10,000 per paper abound), there could be serious pressure for fast results. Mind you, on the editorial side of things, there are differing concerns about publication bounties that largely stem from the potential temptations arising from such huge bonuses to promote unethical publication practices. Editors are rightly concerned about practices such as gaming the review system: that is, rigging it so that reviewers and evaluators are people that submitting authors can count on for affirmative outcomes. These are the issues that one worries about in the editorial suite. One can even stretch it to the point of hasty research practices that result in papers that are, after the fact, questionable in provenance and quality. We've all heard recent anecdotes of public paper retractions at highly placed journals, and one suspects that these occurrences have some provenance in this issue of "pay for play" publishing.

To this point, I have couched my argument in the "get what you pay for" metaphor, but one cuts two ways: on the one hand, fast reviews only transpire in the presence of pay, but, on the other hand, loud complaints about the review process may well also transpire in the presence of pay on the other side of the equation. It's something to think about.

7   Editorial Ambitions and Motivations

The bears in the woods metaphor equally applies to the answer one would get about the desirability of improving review system efficiencies and cycle times were one to ask editors. Editors aren't a bad sort; they work hard and for free to provide the primary service of publication to our colleagues by managing

the review process, and anyone worthy of the title "editor" bears pride in the process such that they'd be happy to improve the process if possible and practical.

Here is the primary issue that haunts me about publishing in the contemporary world of scientific business inquiry: a basic imbalance between those who want to get and those who are willing to give as regarding the peer review process exists, and it all relates to the recognition and reward structure in our particular branch of science. We are paid to publish, plain and simple. We are not paid to review or edit. Base economics dictates the eventual outcome: a high degree of demand (and, lately, performance discernment) on the part of those paid for their research submissions juxtaposed against a hardworking core of altruistic and unpaid service colleagues who do the work absent any specific compensation, and often, little useful recognition.

The whole issue of reviewing's nature and value is a different one when framed in those terms—the idea of "giving to get"—but I suspect most folks don't think of it that way. Indeed, if recent experience as an editor is any guide, there are a host of bright, motivated, and capable researchers out there who all want fast, excellent, and actionable peer reviewers to guide the affirmative shepherding of their research submissions through to eventual publication but are, themselves, reluctant and resistive to provide the same service in return to others. But, Heaven forbid that the reviewers on their papers be in any way late or lacking in providing extensive, objective, helpful feedback (e.g., Weber, 1990).

Repeating my friend Dennis Gonier's pragmatic managerial wisdom: fast, cheap or good: you pick which two you want (and for which you are willing to bear the consequent costs). Whenever we are minded to complain about the review process's duration, we should remember that highly dedicated volunteer labor generally provides the editorial process's heavy lifting, which comes at the cost of the provider's own research productivity. It is one thing to have a free lunch; it is another to complain about its quality.

Lest one be tempted to treat this as a rhetorical issue, if *nobody* wanted to edit or review, who, then, would see to improving the processes that permitted those who are highly rewarded for publishing to actually *get* published? And, lest one overlook the importance of editors as the champions of the poor uncompensated reviewers so essential to the research publication process (Gray et al., 2006), don't overlook that editors, too, are largely uncompensated. To be sure, I like to brag about my time in the editors' suite, but it did not get me a raise at work and has pragmatically resulted in a situation that I have come to characterize as "working two jobs for one paycheck" since running a journal is a 40-hour weekly duty in its own right. Teaching classes, performing campus service, and, if there's ever any time, conducting my own research still account for another 40 hours or so.

8   Conclusion

I am facetiously making a point here that I consider critical for the colleagues to follow: that peer reviewing and the importance of colleagues who perform reviewing selflessly as a generally unrecognized and unrewarded service to their discipline of scientific practice are absolutely essential. We all know the witticisms about "having to give some to get some", but, increasingly, it's a whole lot of "get" in the face of not so much "give" in my informed view as an editor. This "get" and not "give" occurrence in a world where review times are pretty much as they have been since I was first a doctoral student back in the dawn of time tells me something else has changed. In marketing, we called it "buying market share". That, I think, is at the base of what troubles most colleagues about the review process today. Or, in any event, *should*.

Every dean that incents their faculty to "hit" premier journals with a generous publication bounty per paper to "buy" market share is unintentionally contributing to the problem we face: highly impatient authors wishing for incredibly tight cycle times because serious money is on the line. This is a generality and strictly for purposes of dramatic impact; some authors have tenure dossiers pending and every paper counts. That, at least, one understands and forebears because it is at the heart of our career trajectory: gaining tenure on the strength of our publications. And, I say that most of the inquiries I have seen over time from authors are simply polite questions wondering when they might know about their paper except for a few from certain quarters. And, there, my friends, is the rub: "certain quarters" are most certainly in the pecuniary publication business and do so for profit. The journal "business" is a charity, however, and editors have few pecuniary tools with which to improve process or quality outside of the good offices and fine intentions of a few prized colleagues who support the review process.

I raise an obvious point, but, in view of the increasingly prevalent culture of "get" in the absence of "give", I'm here to say that there never has been a free lunch since the dawn of economic transactions. Costs accrue, visibly, immediately, and notably or otherwise, and one doesn't get help in the publication process for "free". One has to "give" back.

I insist. It's only right.

As a student of motivation theory, what could possibly be the reason for which a highly skilled, extremely busy researchers would drop the work that pays them well in recognition, promotion, remuneration and, instead, give that intense and effective scholarly energy to the fair and objective evaluation of somebody else's research in support of somebody else's recognition, promotion, and remuneration? What reward is there for being a "diamond cutter?". If there were any reason at all, it would be the altruistic expectation that, by giving, one also gets. Few individuals, such as the late *CAIS* founder Paul Gray, give purely for the love of science and collegiality. The price of receiving reviews for publication is providing them to others. Not everybody understands this point; editors everywhere daily have to cajole, beg, and entreat colleagues to take on review assignments, just as they are also sure to get stern inquiries as to "the lateness of my manuscript's review".

At the same time, I remain constantly amazed by the quality and generosity of a group of famous researchers, many who are chaired professors and people who matter, names who are named, and who give their time willingly to support research publication at major journals. How do they ever get any work of their own done, these important luminary review board members, when they are busy doing the work for the rest of us wishing to get our papers published??

These are the folks who are rarely heard to say no: the board. My board. Your board. Any board, really. Slackers don't get appointed to boards; they get noticed because they review more than the average mortal, and, at the same time, while doing more research than the rest of us, still take time to review and turn in accurate, detailed, and useful guidance for authors, even when rejecting papers.

We do not prize them enough since they are doing the heavy work of journal editing. Editors are primarily caretakers and behind-the-scenes process managers who perform mundane operational work such as fixing and optimizing technology tools used for the work. These hard-working volunteer members of journal boards are the ones who serve you most and with the least reward.

I owe them, as do you.

And with that, I say from Memphis to hard-working board members and reviewers, anywhere: thank you; thank you, very much[9].

---

[9] Since I first became an Editor in Memphis, I have often emulated the humble Southern etiquette of Memphian Elvis Presley in appreciating his audience, who were *his* reviewers, in an effusive and folksy statement of thanks.

www.manaraa.com

References

Armstrong, J. S. (1997). Peer review for journals: Evidence on quality control, fairness, and innovation. *Science and Engineering Ethics, 3*(1), 63-84.

Fletcher, R. H., & Fletcher, S. W. (1997). Evidence for the effectiveness of peer review. *Science and Engineering Ethics*, *3*(1), 35-50.

Gray, P., Lyytinen K. J., Saunders, C. S., Willcocks, L. P., Watson, R. T., Zwass, V. (2006). How shall we manage our journals in the future? A discussion of Richard Watson's proposal at ICIS 2004. *Communications of the Association for Information Systems*, *18*, 2-41.

Iivari, J. (2016). How to improve the quality of peer reviews? Three suggestions for system-level changes. *Communications of the Association for Information Systems, 38,* 263-273.

Jennex, M. (2015). More on journal cycle times. *AISWorld.* Retrieved from http://lists.aisnet.org/pipermail/aisworld_lists.aisnet.org/20150401/141150.html

Kohli, R., & Straub, D. (2011). Editor's comments: How reviews shape MIS Quarterly: A primer for reviewers and editors. *MIS Quarterly, 35*(3), iii-viii.

Mora, M. (2015). More on journal cycle times. *AISWorld.* Retrieved from http://lists.aisnet.org/pipermail/aisworld_lists.aisnet.org/20150401/141150.html

Palvia, S. (2015). More on journal cycle times. *AISWorld.* Retrieved from http://lists.aisnet.org/pipermail/aisworld_lists.aisnet.org/20150401/141150.html

Pauleen, D. J., Evaristo, R., Dawson, R. M., Ang, S., Alanis, M., & Klein, S. (2006). Cultural bias in information systems research and practice: Are you from the same place I am? *Communications of the AIS*, *17*, 354-372.

Ralph, P. (2015). More on journal cycle times. *AISWorld.* Retrieved from http://lists.aisnet.org/pipermail/aisworld_lists.aisnet.org/20150401/141150.html

Saunders, C. (2005). Editor's comments: Looking for diamond cutters. *MIS Quarterly, 29*(1), iii-viii.

Saunders, C., & Benbasat, I. (2007). A camel going through the eye of a needle. *MIS Quarterly*, *31*(3), iii-vviii.

Smith, R. (2010). Classical peer review: An empty gun. *Breast Cancer Research*, *12*, S13.

Stafford, T. (2015). More on journal cycle times. *AISWorld*. Retrieved from http://lists.aisnet.org/pipermail/aisworld_lists.aisnet.org/20150401/141150.html

Stamps, A. E., III. (1997). Advances in peer review research: An introduction. *Science and Engineering Ethics*, *3*, 3-10.

Straub, D. W. (2009). Editor's comments: Diamond mining or coal mining? Which reviewing industry are we in? *MIS Quarterly, 33*(2), iii-vii.

Trauth, E. (2014). Cultural bias in reviews and mitigation options. *Information Systems Journal*, *24*(6), 475-477.

Weber, R. (1999). The journal review process: A manifesto for change. *Communications of the AIS*, *2*, 1-24.

About the Authors

Tom Stafford is Professor of Business Information Technology at the University of Memphis Fogelman College of Business and Economics. He earned a PhD in Marketing from University of Georgia, and earned a PhD in Information Systems from University of Texas—Arlington. In his time as a research scientist, he has edited 13 notable Journal special issues. He served two terms as Editor-in-Chief of the longest continually published Information Systems journal, *Data Base*, and also served as Editor-in-Chief of the operations journal *Decision Sciences*.