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ICIS 2017 Panel Report: Break Your Shackles! Emancipating Information Systems from the Tyranny of Peer Review

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ICIS 2017 Panel Report: Break Your Shackles! Emancipating Information Systems from the Tyranny of Peer Review

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Abstract:

The paper presents the report of a panel that debated the review process in the information systems (IS) discipline at ICIS 2017 in Seoul, Korea. The panel asked the fundamental question of whether we need to rethink the way we review papers in the discipline. The panelists partnered with the audience to explore some reviewing limitations in IS today and the ways that reviewing in the discipline might change to address some of its difficulties. We first report key concerns with modern reviewing. We then present arguments for and against three proposals (i.e., paying for reviews, mandatory reviews, and open reviews) and a panel audience vote on the issues. We neither advocate for nor condemn these solutions but rather use them to illustrate what we believe represent the core underlying issues with reviewing in the IS discipline. Specifically, we believe the key stumbling blocks to effectively improving our review process include 1) a lack of empirical data on actual practice, 2) a lack of clear goals, and 3) an ignorance of the possible solutions to the review dilemma that the wider literature articulates.

Keywords: Double-blind Reviews, Open Reviews, IS Discipline, Paying, Conscription.

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1 Introduction

Modern peer review was not holistically designed but rather evolved into its current state (much like the Internet). As a result, peer review was never designed for the rapid pace of change in a distributed, global, technology-focused, and electronically enabled discipline, and it is not clear that incremental changes to peer review have kept pace with changes in its environment. Prior to World War II, particular individuals owned and ran journals as fiefdoms and solicited manuscripts for them from authors at conferences. Conference submissions were often at best "lightly reviewed", which means that only the editor and perhaps an associate read them (Fyfe, 2015). Simply put, prior to World War II, the various academic societies that existed at the time had no need for peer review due to their small size. Indeed, the concept was so alien that Einstein, for example, expressed anger when an editor sent his papers for review in 1935 (Lipscombe, 2016; Spicer & Roulet, 2014).

Modern peer review became a consistent practice only after World War II when increased government funding led to an increase in research such that so many individuals began to generate ideas and write papers that a single editor could not attend all the relevant conferences nor manage the volume of submissions to a journal (Fyfe, 2015). *Nature*, perhaps one of the top two journals in the sciences, only implemented peer review in 1967 (Spicer & Roulet, 2014) and consistent peer review (i.e., across all papers) in 1973 (Baldwin, 2015). In other words, peer review, an institution many researchers associate with quality, emerged partly due to practicality: it evolved as a mechanism to offload and outsource editorial workload. Today, reviewers face the same pressures editors faced in the 1950s. Simply put, submissions to journals and conferences have grown at a rate faster than the existing pool of reviewers can manage (Arns, 2014).

In this paper, we report on a panel that debated the review process in the information systems (IS) discipline at ICIS 2017 in Seoul, Korea. The panel asked the fundamental question of whether we need to rethink the way we review papers in the discipline. The panelists partnered with the audience to explore some reviewing limitations in information systems today and the ways that reviewing in the discipline might change to address some of its difficulties.

Approximately 70 people attended the panel session. In addition to panelist presentations and discussion, audience members voted on a wide range of issues. This panel report comprises the insights from the panelists and contributions from the audience and details of the estimated audience vote.

This paper proceeds as follows. In Section 2, we report on the key issues various panelists raised and the audience feedback on those issues. In Section 3, 4, and 5, we present three ideas panelists debated as ways to move forward with improving the IS reviewing process. We discuss each idea's benefits and demonstrate how each one addresses key reviewing issues. We also highlight each idea's potential drawbacks. We close each section with audience feedback. The summaries here reflect the diversity of ideas among panelists and audience members rather than a consensus on particular critiques or possible solutions. Based on panel feedback, in Section 6, we present some suggestions that the IS discipline can implement to move forward. Specifically, we argue the discipline needs to 1) expose more data about actual reviewing practices, 2) have clear goals for reviewing, and 3) inform itself on the wider reviewing literature. Finally, in Section 7, we conclude by advocating for both the necessity of being open to (possibly radical) change and the need to be cautious about change that might sacrifice the valuable aspects of today's reviewing process.

2 Key Issues

In the IS discipline, most journals and conferences use peer reviewing to assess the quality and contribution of manuscripts submitted for journal or conference publication. An editor-in-chief or conference track/mini-track chair(s) manages the review process (e.g., invite peer reviewers). In a double-blind process, the reviewers do not know the authors' identity and the authors do not know the reviewers' identity. Editors or track chairs by necessity know both the authors and reviewers to avoid conflicts of interest, which includes the possibility of inviting authors to review their own work. Academia has viewed blinded peer reviewing as the gold standard for academic publication for several reasons: 1) a discipline's own members have the contextual and detailed disciplinary knowledge to better assess quality and contributions and uphold the discipline's standards versus professional editors, 2) reviewing becomes an opportunity to help authors further develop and enhance their work through the editorial process, 3) participating as peer reviewers can initiate new scholars into the process of writing and publishing, and 4)

a blinded review process minimizes positive and negative biases between editors, reviewers, and authors. However, the peer-review system today also has notable problems that inhibit researchers from producing and disseminating scholarly IS work. The panel focused on identifying and debating possible "fixes" or alternatives to current reviewing practices.

In the introductory comments, panelists raised four key issues: 1) inequity in review allocations, 2) content validity of reviewing, 3) general scarcity of reviewers, and 4) rich/poor divide in reviewer selection.

2.1 Inequity in Review Allocations

Like all social systems, the current review system can be unfair (Clair, 2015; Ho et al., 2013; Marx & Engels, 1867). Studies in other academic disciplines provide empirical evidence for this critique. The structures and institutions of the review system can promote and strengthen certain groups but marginalize others (Armstrong, 1997; DeVries, Marschall, & Stein, 2009). At the most basic level, blinded peer review does not overcome biases in favor of work from more prestigious institutions (Blank, 1991; Peters & Ceci, 1982; Tomkins, Zhang, & Heavlin, 2017). To a lesser extent, research documents geographical biases in that journals are more likely to accept papers from authors of particular countries (frequently their own) (Godlee & Dickersin; Lee, Boyd, Holroyd-Leduc, Bacchetti, & Bero, 2006). For some journals, these biases weigh heavily in favor of North Americans (Ernst & Kienbacher, 1991; Timmer, Hilsden, & Sutherland, 2001). Further, English-language journals favor native English speakers over non-native English speakers (Herrera, 1999), and people who submit to a conference are more likely to reject other papers submitted to the conference (Blackburn & Hakel, 2006).

One cannot easily overcome these biases. For example, many journals use double-blind reviews (in which both reviewers and authors do not know each other's identity) to eliminate bias and favoritism in peer review. While some work has found it effective (Tomkins et al., 2017), other work has found that it worsens the bias (Blank, 1991). Anecdotal evidence suggests author identity bias can impact reviewing in all kinds of ways. Robert Rosenthal, a well-respected psychologist, noted that he had trouble getting papers accepted while at the University of North Dakota, but observed that his acceptances increased while at Harvard (Rosenthal, 1982). Further, researchers have reported anecdotal accounts of reviewers' sabotaging papers in their research areas that are better than their own (D'Andrea & O'Dwyer, 2017; Scully, 2016). Conversely, researchers have documented that double-blind peer review can create "academic road rage" in which reviewers assume the worst of other authors due to anonymity (Ghaemi, 2009).

We also do not know just how much of the double-blind review process actually is double blind. Studies have demonstrated that, 83 percent of the time, a reviewer can guess the author of a paper in double-blind review (Yankauer, 1991). Also, in many journal review processes, associate editors or senior editors know authors' identity.

Individuals who disproportionately tend to review and publish in IS journals and conferences may exacerbate the bias problem. Generally, reviewers tend to be junior faculty or PhD students and, thus, typically lack experience managing or publishing papers. Indeed, research has consistently found that age is inversely correlated with review quality (Callaham & Tercier, 2007; Kliewer, Freed, DeLong, Pickhardt, & Provenzale, 2005; Stossel, 1985).

In contrast, senior faculty often receive reviews since they collectively have more papers out for review than junior faculty. That is, those who reap most of the publication system's rewards do not always match those who do most of the work. One could argue that senior faculty are merely reaping the rewards from once having to review copiously themselves as junior faculty. Moreover, some senior faculty are deeply committed to the editorial process as editors and conference program chairs and, thus, may not be available as reviewers. However, these arguments do not consider that: 1) the volume of submissions to reviewing outlets (i.e., journals and conferences) has dramatically increased, 2) not all junior faculty become senior faculty (many do not receive tenure or otherwise leave academia), and 3) as a result, the bulk of reviewing falls on the shoulders of junior faculty.

2.2 Content Validity of Reviewing

Beyond systematic biases in reviewing, substantial evidence shows that reviewers often do not agree on a manuscript's quality or contributions or on whether it should be revised or accepted for publication. Studies in other academic disciplines again provide empirical evidence for this critique. At the most basic level, inter-rater reliability of reviewers falls short of the Cronbach's alpha (> 0.7), Cohen's kappa (> 0.7 or

r> 0.7) that we normally expect from research (DeVries et al., 2009; Kemp, 2005; Welch, 2014). Indeed, many studies find the inter-rater reliability of reviewing approaches that of random chance (Howard & Wilkinson, 1998; Onitilo, Engel, Salzman-Scott, Stankowski, & Doi, 2013; Rothwell & Martyn, 2000)[1].

Beyond inter-rater issues, evidence suggests that reviewers often fail to identify fatal flaws in the reported quantitative analyses or even misconduct in papers. Several studies report experiments that introduced deliberate errors into papers' reported results to determine whether reviewers would detect them. Generally, such studies have reported detection raters lower than 40 percent (Baxt, Waeckerle, Berlin, & Callahm, 1998; Godlee & Dickersin). Further, studies suggest that reviewers often fail to identify scientific misconduct such as plagiarism or falsified data (Resnik & Elmore, 2016; Smith, 2006).

Also, the review system primarily focuses on rejecting bad papers and has few quality-assurance mechanisms that ensure that reviewers accept good papers. Essentially, reviewing has four quality conditions: 1) reviewers incorrectly accept a bad paper ("type 1 error"), 2) reviewers correctly reject a bad paper, 3) reviewers incorrectly reject a good paper ("type 2 error"), and 4) reviewers correctly accept a good paper. Reviewing as it stands leans towards controlling for type 1 errors but neglects managing for type 2 errors. As a result of this bias, while journals successfully accept relatively few bad papers, they incorrectly reject many good ones (Gans & Shepherd, 1994; Siler, Lee, & Bero, 2014).

Perhaps unsurprisingly, increasing the number of reviewers who review a submitted manuscript increases the likelihood that the review process will reject good papers because the likelihood that the manuscript will receive at least one negative review also increases (Neff & Olden, 2006). Given that four or more people review most IS papers (if we include senior and associate editors), good papers face a high chance of rejection. More disturbingly, as the competition for limited journal or conference publication slots increases, the propensity for reviewers to reject a good paper also increases (Balietti, Goldstone, & Helbing, 2016; Blackburn & Hakel, 2006).

Furthermore, the review process tends to encourage the status quo: papers that challenge existing perspectives are substantially harder to publish than those that further existing lines of research (Siler & Strang, 2017). This fact is especially disturbing given research supposedly concerns falsifying existing ideas to promote new ones (Kuhn, 1996; Popper, 1959).

Of course, these findings about review process disparities come from other disciplines, and some predate the era of global communication that the Internet has enabled. Almost no research in information systems has critically analyzed the discipline's review process in terms of fairness, bias, and workload. We have some reason to believe the IS discipline differs at least modestly from these other disciplines in the fairness and reach of its review processes. For instance, as a relatively new discipline, IS "grew up" in the global arena. It embraced Internet-based review systems that can reach reviewers globally early on, and, through its professional organization, has promoted global reach and the inclusion of academic members from around the world. Nevertheless, research across disciplines suggests that peer reviewing as currently practiced has significant limitations as a guarantor of high-quality published content.

Given reviewing serves as our discipline's principal measure of academic quality, we need to ask whether we can do something to improve the content validity of reviewing. On this topic, research has again presented remarkably dismal findings (Bruce, Chauvin, Trinquart, Ravaud, & Boutron, 2016). It has found that the obvious intervention, training reviewers, does not improve review quality (Callaham & Tercier, 2007; Callaham, Wears, & Waeckerle, 1998; Houry, Green, & Callaham, 2012). Further, research that does demonstrate that training works has found only mild improvements in quality that fade over time (Schroter et al., 2004, 2008). Research shows that experienced (i.e., naturally trained) reviewers provide harsher feedback than untrained reviewers (Nylenna, Riis, & Karlsson, 1994). Similarly, research has demonstrated that single-blind or double-blind reviews (Blank, 1991; Resnik & Elmore, 2016; Tomkins et al., 2017) and an increase in the number of reviewers (Neff & Olden, 2006) do not improve review quality. Indeed, editorial intervention represents the only factor that research has found to consistently impact review quality (Kovanis, Porcher, Ravaud, & Trinquart, 2016; Neff & Olden, 2006). With that said, we do not advocate purely editorial reviews for papers since that has its own significant problems (Hilgartner, 1997).

[1] On the surface, this finding seems problematic, but it assumes that quality represents a fixed quality that reviewing focuses on discovering. On the other hand, one may view reviewing as an exercise in looking at a work from multiple perspectives to illuminate in a developmental sense the broadest range of issues to provide greater opportunity for correction and improvement.

2.3 General Scarcity of Reviewers

Reviewer scarcity exacerbates the review system's quality problem. Research has demonstrated that review scarcity increases the likelihood that reviewers will make incorrect editorial decisions (Fox, 2017). Given the problems with peer review that we discuss above (e.g., bias, poor content validity), scarcity further weakens the reviewing process.

As the IS discipline has grown and become more globally inclusive, authors have submitted more papers to our journals and conferences and more journals and conferences require peer reviewing—in other words, the demand for reviewers has skyrocketed. One panel participant reported that reviewers performed approximately 300 reviews on 75 submissions for a recent ICIS track but ultimately accepted only 18 papers (about 16.6 reviews per accepted paper). The relatively low acceptance rates in the IS discipline's premier outlets along with the discipline's focus on a subset of journals as the most notable for tenure and promotion decisions exacerbate this reviewer-demand problem (Dennis, Valacich, Fuller, & Schneider, 2006). For instance, *MIS Quarterly* (2-3 reviewers, AE, SE), *Information Systems Research* (2-3 reviewers, AE, SE), *Journal of the AIS* (2-3 reviewers, SE) and possibly other "basket" journals generally review papers sequentially. Since a paper typically goes through multiple review rounds, a paper can receive a dozen or more reviews before it finds a publication "home". Consider also that the paper may also have gone through multiple rounds of review at one or more of these (and other) venues.

Furthermore, reviewing is an expensive activity. In some disciplines, estimates of cost vary from US$200 to US$2,500 to review one paper (DeVries et al., 2009; Smith, 2006). Most estimates anticipate two to four hours per review (Diamandis, 2015; Snell & Spencer, 2005; Walsh, Rooney, Appleby, & Wilkinson, 2000). The Research Information Network (2008) has estimated that peer review in the United Kingdom costs £1.9 billion annually. Cost here refers primarily to the voluntary labor of academics who conduct reviews as part of their academic job or on their own time. Business schools and, by extension, the IS discipline bear a disproportionate share of that cost in terms of voluntary labor. As a result, business school journals have the slowest review turnaround time among the academic disciplines (Björk & Solomon, 2013), and the review cycle represents the most significant component. In IS, a paper can undergo four to five review rounds (including cycles that end in rejections) at one or more journals before an outlet accepts it, which roughly represents 48-96 hours of voluntary reviewer and editorial time per paper. As an unfortunate side-effect, it can take years to publish peer-reviewed research in the IS discipline, which purportedly focuses on new developments and innovation.

One should not find willing reviewers' scarcity surprising given that scant market incentives for supply to increase to meet rising demand exist. Panel members noted that they can ask 10 people to review to obtain two who agree but who may then fail to submit their review on time (Hazen et al., 2016). IS scholars' responsibilities include developing new curricula, teaching students, performing administrative duties, and conducting and publishing their own research. Thus, they need to carve out time to review work out of the "research/teaching/service" pie, which leaves little time for sleep, family members, and so on. Further, scholars—particularly junior ones—receive only qualitative and uncertain compensation for reviewing. For instance, from reviewing, junior scholars can build their knowledge of the publishing and reviewing process and build their social network with senior scholars/editors in the discipline who might serve as mentors or tenure case reviewers in future. Tenure or contract renewal decisions never rest on a scholar's reviewing history, nor do decisions about raises or additional compensation. Reviewers seldom receive any direct compensation except the occasional "best reviewer" award and possibly a token monetary gift.

2.4 Rich/Poor Divide

Among the business disciplines, the IS discipline uniquely focuses on ranking journals and authors. While many business schools (where many IS academics work) maintain lists of preferred or highly ranked journals, to the best of our knowledge, the IS discipline represents the only professional academic body in business research that endorses a list of high-quality journals—the AIS Senior Scholars' basket. Moreover, only the IS discipline publishes an annual ranking list of authors based on publications in this list of journals (i.e., the AIS research ranking service) and papers that rank order journal quality (see, e.g., Lowry et al., 2014).

The panel speculated that the introduction of ranking lists that categorize some journals as more prestigious than others has exacerbated the reviewer-scarcity problem. Such lists include AIS-sanctioned lists such as the Senior Scholars' basket and non-professional organization-based lists such as the FT-50

that rank *MIS Quarterly* or *Information Systems Research* as the two best journals. Prior to the introduction and widespread acceptance of such lists, some journals and conferences formed communities, and members of these communities would participate more willingly by reviewing for these outlets.

The introduction of ranking lists, however, has meant reviewing for prestigious journals provides more social capital and status than reviewing for less well-recognized journals. With limited time available to do reviews, potential reviewers, and particularly junior scholars working towards permanent status or tenure, make choices regarding where to focus their efforts, and senior colleagues in the discipline may advise them to select only "high-profile" service. Simply put, a scholar's curriculum vitae looks better if it shows that the scholar reviews for, for example, the *European Journal of Information Systems* or *MIS Quarterly* because these journals appear in the Senior Scholars' basket. One panelist recounted that he might request five reviews for a basket journal before obtaining enough reviewers but would need to ask for 15 reviews before obtaining a sufficient number for a non-basket journal. In addition, after finally agreeing to provide a review, a reviewer might provide "quick" or "suboptimal" feedback for a less prestigious journal.

2.5 Strategies for Moving Forward

After discussing each problem, panelists presented three broad strategies for overcoming them:

- **Payment for peer review:** reviewers receive compensation (either monetary or otherwise) for reviewing
- **Mandatory peer review:** reviewers must review. Often, some non-review activity such as submitting to a journal triggers the obligation to review.
- **Transparent, crowdsourced, post-publication review**: a review model that the hard science journals have successfully trialed. A single reviewer and an editor make accept/reject decisions, and the real assessment occurs after an outlet publishes papers.

Of course, one could apply other strategies as well. For instance, the panel did not discuss editorial review or paper invitations, the primary model for journals prior to World War II. However, the panel felt the three options presented the most viable ways forward for the IS discipline. In Sections 3 to 6, we present each strategy in depth.

3 Payment for Peer Review

The following paragraphs summarize the viewpoints that various panelists presented and the opinions that audience members offered in discussing whether the IS discipline should use monetary or other extrinsic incentives in its review systems. Paying reviewers presents a method that would more fairly compensate them for their time, provide leverage for editors to insist on following defined procedures or quality standards, and, ultimately, increase the supply of reviewers. Payment for reviews can take multiple forms.

Cash payments: various journals in other disciplines have already implemented cash payments. As of this writing, journals associated with the American Economic Association, for example, pay reviewers US$100 per review (see, e.g., https://www.aeaweb.org/journals/aer/reviewers). *The Lancet*, a renowned medical journal, pays its "statistical reviewers" (McNamee, 2000). Some journals do not pay reviewers but do pay editors. The *Project Management Journal*, for example, provides funding for editors to travel to a finite number of conferences a year. This latter practice may help motivate editors and enhance editorially reviewed processes but does not address the issues with obtaining high-quality and timely peer reviews.

While some journals report no problems with paying for reviews, other journals have faced controversy. Several journals, including the well-respected open-access journal *Scientific Reports*, attempted to link such payment schemes to review fast-tracking (Rinaldi, 2015). Essentially, these journals had a two-tier review system in which authors could opt to pay reviewers for expedited reviews. In most such journals, the editors became upset and threatened to resign due to the proposed changes. Some journals, such as the *New England Journal of Medicine*, found paying too little was worse than not paying at all. Reviewers complained that the US$5 they received for reviews insulted them (Campanario, 1998; Ingelfinger, 1974). As of this writing, the general consensus seems to be that US$50 per hour represents a "fair wage" for peer review (Chetty, Saez, & Sándor, 2014; Diamandis, 2015; Gasparyan, Gerasimov, Voronov, & Kitas, 2015).

Journal/publisher credit: journals or publishers can also offer non-monetary payments. Elsevier, for example, has a reviewer-recognition program that offers discounts on Elsevier products to their reviewers (see https://www.reviewerrecognition.elsevier.com/). Wiley provides reviewers with free access to some journals and credits towards certification (https://authorservices.wiley.com/Reviewers/journal-reviewers/recognition-for-reviewers/index.html).

The boldest suggestions for journal credit systems argue for a universal artificial credit system (Fox & Petchey, 2010; Spearpoint, 2017). In such proposals, reviewers and editors receive compensation for each paper they manage. Journals charge a submission cost in the artificial currency. Everyone would initially have a small sum of the currency, which would allow them to submit papers to one or two journals. Thus, after a small initial number of submissions, one could only submit papers after performing reviews. These proposals come in multiple variations. For example, one question concerns whether the artificial credit market should be centrally controlled (Fox & Petchey, 2010) or left to market forces (Ott & Hebenstreit, 2014).

Donations: instead of paying reviewers directly, some journals donate money to worthy causes on their behalf. The journal *Environmental Earth Sciences*, for example, donates one water filter to the charity Filters of Hope for every review done for the journal (Springer, 2016). The journal *Collabra* has a scheme whereby reviewers can obtain credit to reduce various costs for either their own institutions or for anonymous others (Kulkarni, 2015).

Reviewers as co-authors: one particular radical proposal posits that accepted papers should list their reviewers as co-authors (Bernstein, 2013). After all, the reviewers provided useful comments that improved the paper prior to publication.

Most proposals associated with paying reviewers have links to the idea of author-processing fees. Authors must pay money to submit papers, which reviewers then receive (in part). While the IS discipline does not feature such fees, many other disciplines do. For example, the *Proceedings of the National Academy of Sciences*, a premier science journal, charges US$1700 per paper (see http://www.pnas.org/page/authors/fees for details).

As a strategy, payment for peer review principally addresses the first three review problems (inequality in review allocation, content validity, and review scarcity). However, it likely aggravates the rich/poor divide.

3.1 Addressing Review Problems

Inequality in review allocation: on the surface, payment for reviews equalizes review allocations because those who review more become richer. The reality, however, is likely more complex because introducing payments changes peoples' cognitive processes (Gneezy, Meier, & Rey-Biel, 2011). At the moment, reviewers have non-obvious incentives to review for journals and conferences. Some reviewers could altruistically review as a civic duty. Others could review with the expectation that they will receive an eventual reward by becoming an editor of a journal. Still others could do so as a quid pro quo (e.g., I review for you if you will review for me). Once one introduces an explicit payment system, these non-obvious incentives for reviewing disappear because reviewing has an explicit incentive (Gneezy & Rustichini, 2000). As a result, reviewing will likely witness a redistribution towards people incentivized by the specific form of payment given for reviews.

As long as the form of payment lacks bias (e.g., based on nationality, ethnicity, gender, or sexual orientation), all IS academics have equal access to the market and payment can sufficiently ensure "market clearing", payments for reviews could address the review inequality problem. Of course, the devil is in the details. If one demographic wants the rewards for reviewing and cannot do so for reasons other than incompetence, inequality would remain.

Author fees would also help reduce the power imbalance between authors and reviewers. At the moment, authors bear all the risks of failures in the review process (e.g., reviewers fail to submit reviews on time, editors make a mistake) (Clair, 2015). Under a fee-based system, authors may have the right to demand compensation or remedy under breach of contract. For example, in a situation where a senior editor accidentally reveals an author's identity to reviewers, the author may have a right to a new review process or some other mutually agreed remediation.

Content validity: free work does not always equal good work (Pasternak & Glina, 2014). The generally poor quality of reviews (e.g., Howard & Wilkinson, 1998; Onitilo et al., 2013; Rothwell & Martyn, 2000) may be associated with the poor remuneration that reviewers currently receive. Correctly implemented,

payment for reviewing would partly ameliorate the content validity issue of reviewing. It would become possible to sanction reviewers for late or poor-quality reviews by rejecting the reviewer's review and not paying the reviewer. Alternately, the editor could refuse to use the reviewer in the future and, thereby, deny the reviewer a future revenue stream.

Review scarcity: most research on reviewing suggests the system would become more efficient if reviewers received pay for doing so (Chang & Lai, 2001; Cotton, 2013). Perhaps the strongest piece of evidence comes from Chetty et al.'s (2014) quasi-experiment: the authors found paying reviewers US$100 to review resulted in a faster review at no reduction in review quality as compared to all other treatments (no intervention, reviewer response times made publicly available, shortening due date).

Linking paid reviewers to an author fee also would reduce review scarcity. For one, increasing the submission cost to a journal would reduce the number of submissions. Authors would also be more careful to submit to the appropriate journal rather than submitting to a journal to try their luck or get feedback because gaming the system in such a way would be costly. Many studies have argued that a code of ethics binds IS academics as professionals. However, given the high number of low-quality unfinished submissions the journals regularly receive, professional ethics may not be sufficient to manage the reviewing process.

Indeed, studies on improving research generally (House of Commons Science and Technology Committee, 2004) and improving research in the IS discipline in particular (Lee, Srinivasan, Valacich, & Weber, 2007) have recommended paying for reviews as part of an overall strategy to reduce review scarcity.

Additional benefits and issues: beyond addressing many of the pressing review issues in the discipline, paying reviewers would provide additional benefits and address other issues, such as the imbalance between individuals who review for journals and conferences and individuals who review for other academic products. Most academic publishers treat journals as for-profit enterprises. Reviewing takes up substantial amounts of time, and time has a monetary value. Furthermore, most academic employment contracts do not include reviewing as a duty; one cannot argue reviewers' regular salary includes compensation to conduct reviews. Most Western countries protect labor by insisting that individuals can charge for the onerous tasks they perform for others, and, other industries would consider similar practices of making people do onerous work for free illegal (Lewis, 2011). Indeed, in academia, individuals commonly accept that a grant, book, or thesis reviewer will receive pay (Matthews, 2016). Thus, the fact that journal reviewers do not receive pay seems strange.

In addition, while journal publishers often seek a profit, the individual journals often have limited budgets to host workshops or perform other community-building activities. Author fees would provide a small pool of money for the journals to use.

Finally, existing review systems such as ScholarOne's Manuscript Central cost money to use—ScholarOne itself costs AIS approximately US$65,000 per year. The AIS pays these fees on a per-submission basis. At the conference level, attendees rather than authors bear these fees. Having authors pay these fees would do much to reduce the subsidy that authors with accepted papers and other attendees at conferences currently pay to support authors with rejected papers.

3.2 Drawbacks

While a pay-for-review system has many apparent benefits, it also has numerous drawbacks. First, reviewing has many inherent benefits. Notably, reviewers uncover new research and learn what their community's prevailing standards are (Cressey, 2015). While a pay-for-review system might improve a reviewer's extrinsic motivation to review, it interferes with the reviewer's intrinsic motivation (Lepper, Greene, & Nisbett, 1973), which leads to all kinds of potential negative consequences, which we list below.

The rich/poor divide: the Matthew effect refers to the unequal distribution of resources. It pervades science and research in particular in that those who have resources can acquire more while those who do not can find it difficult to access more (Merton, 1968). Pay-for-review systems aggravate the Matthew effect because specific groups monopolize the review system and acquire resources as a result. Potentially, the Matthew effect can lead to a new class system in which prestigious journals pay more and reserve an exclusive community of reviewers and less prestigious journals pay less and have a different community of reviewers (Cressey, 2015).

Nature of reviews: research on motivation has demonstrated that paying for tasks changes the way the brain engages with tasks. For example, receiving pay for tasks can reduce individuals' performance by narrowing their focus and reducing their creativity (Ariely, Gneezy, Loewenstein, & Mazar, 2009). At the moment, academics view reviewing as a professional (i.e., an ethical) duty. Public service (e.g., reviewing) is part and parcel of a professional identity (Dinger, Stepina, Thatcher, Breland, & Treadway, 2015). Paying reviewers could have negative impacts on reviewers' professional identity with concomitant negative effects on the IS academic profession as a whole (Underhill, 2016).

Unintended effects: Merton (1936) highlights how changes in institutional structures often have unintended effects. While paying for reviews rewards reviewers, it also applies pressure to journals. The journal *Scientific Reports* became embroiled in a scandal when it began outsourcing reviews to Rubriq, a commercial review organization (Bohannon, 2015). A similar scandal could arise in our discipline as journals find it cheaper to outsource reviews rather than recruit reviewers themselves. Another potential unintended consequence relates to the relative value a reviewer fee has across countries. A fee that appears a pittance in North America might seem like a large amount in a less developed country. As a result, a few less developed countries could come to disproportionally dominate reviewing.

These represent but a handful of potential consequences. We cannot predict how the reviewing system will actually evolve once we introduce market forces into journal reviews.

Beyond the potential drawbacks, little evidence shows that paying for reviews actually improves reviewing. To our knowledge, Chetty et al. (2014) have conducted the only true empirical study (in which they conducted an experiment on economists, a demographic with particular economic behaviors). Most other work (e.g., Chang & Lai, 2001; Cotton, 2013) is based on simulations and models. Tite and Schroter's survey of Biomedical researchers (2007) suggests that monetary incentives would not influence science peer reviews as much as it would influence economists' reviews.

However, we do have evidence of community pushback. Researchers have proposed fees for conference submissions at several IS conferences, but they have always experienced rejection at many levels (including the AIS council).

Also, while some have made proposals for journal/publisher credit, no existing platform for such a system exists. Furthermore, we lack details about how these platforms would actually work. For example, how much credit should one earn for reviewing and editing? How much free credit should everyone receive? Is credit transferrable? How much credit should an author spend on each submission?

In addition to the problems associated with paying peer reviewers, author fees have problems as well. A fee that might be tolerable to a North American author in a business school may prevent authors in less endowed schools or from other parts of the world from submitting and publishing their work. To manage this issue, some journals employ a tiered fee system in which authors pay an amount based on where they live. However, such systems are imperfect and subject to gaming. For example, what fees should a four-author paper attract with one author from North America but others from a less developed country? Fee-based systems naturally encourage the Matthew effect.

Author-pays systems can also potentially create conflicts of interest. Research has evidenced such conflicts of interests, when the client pays for its own evaluation, across a multitude of industries, such as accounting (with audits) (Ronen, 2010; Sinha & Hunt, 2013) and politics (with political lobbyists) (Duso, 2005; Yu & Yu, 2011). Simply put, as journals begin to rely on authors for revenue, they face the danger that they will accept more low-quality papers. In a tiered system in which people from wealthy nations pay more than people from less wealthy nations in particular, a situation could arise where individuals from wealthy nations unduly influence the publication of their own papers.

3.3 Audience Response

We polled the audience on several issues, which evoked interesting conversations and perhaps surprising outcomes.

1. Should journals charge authors a fee to pay reviewers? Roughly half the audience agreed and roughly half disagreed. Audience members expressed some discomfort with the idea.
2. Should we employ a journal/publisher credit system where reviewers earn artificial money that they use for submissions? The audience favored an artificial money system. Audience members favored the discipline's experimenting with this model at a workshop or conference.

3. Should authors pay a fee just to cover the cost of the reviewing system (e.g., Manuscript Central)? The audience slightly favored covering the cost than not. However, a scan of faces suggested visible discomfort with the idea.

Beyond the issues we discuss above, the audience also raised the issue of inconsistencies in review practices across business disciplines. For example, some disciplines (e.g., finance and economics) regularly charge fees, while others such as information systems do not. This heterogeneity of practice makes it difficult for business school deans to explain research funding allocation decisions to higher levels in the university hierarchy.

4 Mandatory Peer Review

In this section, we summarize the viewpoints that various panelists presented and the opinions that audience members offered in the discussion about requiring authors who receive the benefits of the review system to contribute in some proportional measure to that system.

Paying for peer review in a sense adopts a "market" approach to peer review. An alternate proposal involves adopting a "central planning" or "community" approach where people who submit to journals are required to do peer reviews. Mandatory peer review has three principal forms: 1) localized to the outlet (i.e., those who publish in the journal/conference must review for the journal/conference), 2) discipline wide (i.e., a person who publishes in one outlet in the discipline must review for some other outlet in the discipline), and 3) universal (e.g., anyone in the community—every AIS member—must review).

Interestingly, practical implementations for mandatory peer review often employ market mechanisms. Many such proposals revolve around universal credit systems (Fox & Petchey, 2010; Spearpoint, 2017). Under such systems, authors cannot submit papers until they accumulate a certain number of reviews. Mandatory peer review addresses the inequality in review allocation, review scarcity, and the rich/poor divide, but aggravates the content validity problem in reviewing.

4.1 Addressing Review Problems

Inequality in review allocation: voluntary peer review does not allocate reviews fairly. In most voluntary systems, the greatest submitters often perform the fewest reviews (Graur, 2014). Indeed, several studies have reported the 80/20 rule (20% of people do 80% of the reviews) is a reality (Kovanis et al., 2016; Petchey, Fox, & Haddon, 2014).

Most disciplines also require scholars to review. However, voluntary reviewing unnecessarily penalizes those who uphold disciplinary tradition. In principle, by requiring everyone in the IS discipline to review, review-allocation equality can be achieved.

Review scarcity: mandatory "conscription" into the review system principally addresses practicality: the volume of submissions to every discipline is growing at an exponential rate (Arns, 2014). The system of voluntary peer review cannot cope with this kind of growth, and we need a system in which reviews scale with submissions. Mandatory peer review scales since every author becomes a reviewer.

Further, the IS discipline is a fragmented adhocracy (Banville & Landry, 1989). Unlike many disciplines, the IS discipline is actually a collection of highly specialized subdisciplines that each have a limited pool of expertise. As a result, a small number of refusals can exhaust the reviewer pool for that specialized area while possibly leaving a surplus in another area. Mandatory reviewing would reduce or eliminate this scarcity assuming that submitters in an area would be adding to the specialty area pool.

Rich/poor divide: a mandatory review system would cause the rich/poor divide to all but disappear because every submitter to a journal would have to review.

Additional benefits and issues: many journals experience a high rate of submissions by people who do not understand the standards of the journal's community. Reviewing for such journals and receiving feedback on the reviews provides a way of educating reviewers and, therefore, authors, which would hopefully reduce the number of poorly written papers.

4.2 Drawbacks

Because everyone would need to review, both good and bad reviewers would review under a mandatory system. Furthermore, researchers have also argued that mandatory reviewing leads to poorer-quality

reviews than voluntary reviewing (Poutvaara & Wagener, 2006; Sandler & Hartley, 1995). This lower quality arises because conscripted individuals tend to be less motivated and skilled at assigned tasks than those who volunteer and because those in charge may misallocate resources based on their own (rather than the real) costs. For example, at the moment, reviewing is a costly service and editors make desk reject decisions partly based on this cost (Fox, 2017). In a mandatory review system, reviewers would be effectively free to editors. As a result, editors would likely over-allocate reviewers to reviews.

Reviewing also involves expertise. Especially for novel works, the expertise to perform a review may not come from a journal or even in the discipline. An author may submit a paper to a journal that leverages theory from other disciplines. Under such circumstances, an editor may need to draw reviews from outside the disciplinary pool. Both of these issues aggravate the content-validity issues in reviewing.

Further, the law in many nations would not allow mandatory peer review. The 1957 ILO Convention (No. 105) Concerning the Abolition of Forced Labor requires governments to prohibit "any form of forced or compulsory labor". Article 6 of the 1969 Organization of American States (OAS) American Convention on Human Rights states "No one shall be subject to slavery or to involuntary servitude, which are prohibited in all their forms" and "No one shall be required to perform forced or compulsory labor". We cannot force people to review just because they are members of our discipline, which makes the third form of mandatory peer review untenable. In principle, an outlet could withhold publishing a researcher's paper if the researcher does not provide an adequate number of reviews, but one might view such an act as a kind of coercion given that researchers often need to publish academic work to maintain their job and receive promotions.

Further still, peer review does not constitute the only form of disciplinary service. People in the discipline serve as deans or belong to governance bodies either in their universities or in the discipline. Often, editors submit to one journal because they feel a conflict of interest would arise if they submitted to the journal for which they serve as an editor. Mandatory review systems may not consider these other service obligations. However, some researchers have argued that these other forms of service have their own rewards (e.g., professional visibility, reputation, and influence). Reviewers do not receive the intangible rewards of deans, university leaders, and journal editors, and people who serve in these roles should not automatically receive reviewer benefits.

Also, while many often argue that mandatory conscription is fair in principle, actual system implementations may not be fair. Those with wealth or power can often manipulate the system to exploit those without wealth or power (Sacher, 2007). For example, one could imagine a system where a senior scholar pushes off mandatory reviews to junior faculty or doctoral students. We acknowledge that such things likely already occur. However, mandatory peer review could create a world that further aggravates review duty disparities where particular subclasses in information systems become exploited classes.

Mandatory conscription also requires an overarching administrative system to manage conscripts. Someone has to pay for it. Perhaps due to our structure of being a set of communities focused on a single research question (Banville & Landry, 1989) (e.g., how information technology positively impacts individuals, organizations, and societies), we lack the coherence to develop a structure to manage a discipline-wide review system.

Finally, the available pool of conscripted reviewers and actual review demand may not match, which would require a redistribution of work allocation. Higher-quality journals tend to accept fewer papers, which means they have a higher review workload to author ratio. Certain kinds of paper submissions have more volume than others. As a result, authors in particular domains who publish in top journals would likely perform more reviews than authors in other domains who only publish in lower-quality journals, which has certain implications such as:

- Particular domains have a greater influence in the discipline (those authors review far more papers in the top journals).
- Conscripted reviews exhibit the Matthew effect (Merton, 1968). Because researchers who have authored existing papers in a journal serve as its primary gatekeepers, the journal preserves the status quo and is less likely to accept new ideas or ways of doing things.

4.3 Audience Response

1. Should the IS discipline require a journal's authors to review for that specific journal? About 90 percent of the audience favored the idea.

2. Should the IS discipline require authors who have published in one journal to review for the community and not necessarily for that specific journal? About 98 percent of the audience favored the idea.

5 Transparent, Crowdsourced, Post-publication Reviews

The final proposal the panel considered involved instituting a specific kind of open review: a transparent, crowdsourced, post-publication review. Open reviews are a generic concept associated with making the review process more transparent (Ford, 2013). In the current review system, only editors see the entirety of the review process from author submission to final manuscript acceptance. When errors and problems arise, authors and/or reviewers lack sufficient information to raise concerns or remedy the problem.

The specific form of open review that the panel presented operates as follows:

1. Authors submit a paper to the outlet.
2. An editor and/or a single reviewer prescreens the paper. The reviewer has a restricted mandate in the review—the reviewer only evaluates the methodology to see it is passable. The reviewer does not evaluate the topic, theory base, findings, or any other part of the paper.
3. If the paper survives prescreening, the journal publishes the paper.
4. The community at large then reads the paper and comments on it.
5. Authors must respond to negative comments and revise the paper. The paper is effectively always alive.

One can find various forms of this publication model in various open access journals, such as *Scientific Reports*, the Public Library of Science journals (including its flagship journal *PLoS One*), and *IEEE Access*. The models these journals adopt differ from the one we describe mainly in that, while they allow commentary, once an editor has accepted a paper, the journals consider it published and its authors have no obligation to revise it. Conferences have also trialed this model of publication (How, 2017).

5.1 Addressing Review Problems

Content validity: the model directly addresses the content validity problem. Recall that type 2 errors (rejecting good papers) constitutes the principal content validity issue in reviewing. Reducing the number of reviews reduces the rejection rate (Neff & Olden, 2006). Admittedly, it also increases the false acceptance rate. However, also recall that editorial intervention constitutes the principal mechanism that research has shown to best manage content validity (Kovanis et al., 2016; Neff & Olden, 2006). The model essentially returns journals to their roots as editorially reviewed publications but with a reviewer included to provide a check on editorial error.

The proposed open review model also encourages journals to publish different types of papers, such as data sets, replication studies, and studies with non-significant findings (Ross-Hellauer, 2017).

Review scarcity: most information systems papers have multiple authors, and the model requires exactly two individuals (an editor and a reviewer) to manage the review process. Thus, the model scales with growth of submissions.

Rich/poor divide: because the model scales with submissions, it radically reduces the review workload in "high-status" journals and, thereby, frees resources for other journals.

Additional benefits and issues: other disciplines have successfully trialed the model (Shaffer, 2014), and many open review journals are credible. The 2016 impact factors of *Scientific Reports*, *PLoS One* and *IEEE Access* were 4.25, 2.81, and 3.24, respectively.

Open review also has a quick review turnaround. Because fewer people view the paper before publication, ideas disseminate more quickly—typically in a matter of weeks rather than the years required in more traditional journals (Ford, 2013).

Finally, this model encourages a type of review that IS essentially lacks today: the post-publication review (Erren, 2007). A post-publication review continues the editorial process after a paper is published. It recognizes that traditional peer review with a limited number of reviewers who are typically socially linked to the editor necessarily involves imperfections (Herron, 2012). The model judges a paper's merits based not on the ideas of a small select set of people but on the community's commentary at large. As but one

example of effective post-publication review, readers caught problems with the data in one paper in *Science* one day after publication. The paper had previously gone through two rounds of peer review in which reviewers missed the issue (McCook, 2017).

5.2 Drawbacks

Open review models have one principal drawback: they aggravate the inequality in review allocations. With fewer resources required to do a review, editors can become more circumspect in selecting reviewers. Reviewers who in our current system do not do reviews will not likely do reviews in an open review model.

Also, while proponents of open reviews and post-publication reviews highlight their rare successes, on average, no one actually comments (Schriger & Altman, 2010). Indeed, empirical studies have demonstrated post-publication review tends to be a poor signal of quality (Eyre-Walker & Stoletzki, 2013). In principle, a Wikipedia-type system in which one author (or group) proposes papers that others improve on could address this issue. However, such a system can still suffer from a feedback loop in which groups keep changing one another's views or where no one invests the time to correct small or even large errors perhaps because they think that they will be "obvious" to any reader.

Furthermore, open review exposes authors to a potentially political environment in which their friends/enemies deliberately make positive/hostile comments on their papers (DeCoursey, 2016; Vidal & Leaver, 2015). Admittedly, the existing closed review system also suffers from this problem in that biased reviewers can reject a paper (D'Andrea & O'Dwyer, 2017; Scully, 2016).

Various permutations of open review have also been attempted unsuccessfully in the IS discipline (Hardaway & Scamell, 2012). For example, *MISQ Discovery*, the *MISQ* repository of living documents, received few submissions. Admittedly, in this example, people may have found it difficult to accept changes to an existing journal (*MIS Quarterly*) than to accept an entirely new journal. Open review has taken off in the sciences (e.g., in *Scientific Reports* and the PLoS series of journals) even though it failed in the journal *Nature* (Nature, 2006). Also, acceptance of open review could depend on the discipline in question. It is a much more widespread practice in the sciences than the social sciences or humanities (Mellon Foundation, 2016).

Finally, maintaining living documents is exhausting. Authors want to move on to new research rather than having to continuously revise old documents. Empirical examinations of existing open reviews highlight that authors often seldom bother to respond to post-publication review comments unless editors mandate that they do so (Eyre-Walker & Stoletzki, 2013).

5.3 Audience Response

1. Should the IS discipline adopt transparent, crowdsourced, post-publication reviews? About one third of the audience supported use of these types of reviews. Those who rejected mostly brought up the political risk to authors and problems with maintaining live documents.
2. Should the IS discipline adopt an open review of the form in which only an editor and one reviewer with a constrained reviewing mandate review papers? We asked the audience about the actual proposition of *Scientific Reports*, *PLoS One* and *IEEE Access* (i.e., that, after the editor and one reviewer accepts and publishes a paper, its authors have no obligation to revise it or respond to comments). However, given the constraints of a live session, we were unable to formulate this question clearly enough to get a reading of audience reaction.
3. Audience members also highlighted a number of variations to open review in existing open access journals such as the *Australasian Journal of Information Systems* and the *Asia Pacific Journal of Information Systems*[2]. The "letters" section in many journals represents one popular variation that approximates open review. Letters typically undergo only editorial review and journals can quickly publish them in their next issue. Some journals also have "debate" sections for which journals invite various letter submitters to extend their letters into paper form. The journals then publish the collection of letters as a special issue.

[2] Note that these journals differ from the AIS-sponsored journal called the *Pacific Asia Journal of the Association for Information Systems*.

6 Moving Forward

Beyond the specific issues and strategies that the panel presented, panelists learned a great deal about the state of reviewing in information systems. This section consolidates what panelists feel are the most salient questions and issues that the discipline must grapple with to determine how reviewing in the discipline should evolve: 1) ensuring review data's transparency, 2) clearly determining the goals of reviewing, and 3) understanding the reviewing body of knowledge.

6.1 Transparency of Review Data

In preparing for the panel, panelists were surprised to discover that while IS has traditionally been a discipline given to retrospection and reflection (Athey & Plotnicki, 2000; Avgerou, Siemer, & Bjorn-Andersen, 1999; Benbasat & Weber, 1996; Culnan, 1986, 1987; Gallivan & Benbunan-Fich, 2007; Keen, 1980; Orlikowski & Iacono, 2001), its reflection on the review process is substantively less than many other disciplines. Discussions about reviewing in disciplines including psychology, medicine, biology, and economics rest on not only anecdotal observation but also formal empirical study.

One cannot easily conduct such work in information systems because most IS scholars cannot access data on reviews in a way that they can easily analyze. We note the lack of availability of such public data sets for any single journal or for a set of journals such as the AIS Senior Scholars' basket. Private data sets exist, but one cannot easily access them. For example, ScholarOne can report on how many reviews a specific reviewer has done for a journal in the past year. However, it does not provide the data in a way that one can easily view and analyze at the journal level. We (i.e., this paper's authors) serve as editorial board members and have observed that editor-in-chiefs sometime manually extract journal-level data for the summary reports they present to their boards at annual meetings. To the best of our knowledge, no journal reports basic descriptive information such as how many reviews they have solicited per paper or the length of time between when editors send a review invitation and when reviewers accept it, nor do they report more insightful metrics such as reviewer-quality ratings or the number of papers that the top 10 percent and bottom 10 percent of reviewers handle. Without more sophisticated data or even basic descriptive data at the journal level, we cannot easily study the review process.

Review data's sensitivity compounds the difficult in extracting review data, which could harm reviewers if widely disseminated. The public should probably not be able to access individual reviewer acceptance and rejection rates or length of time since last review because one could use such information to game the review process or to criticize people for reviewing too much or too little. It might also have unanticipated influence on reviewers' future tendencies to perform additional reviews. More importantly, depending on the level of detail, one could use it to determine reviewers' identity. One could use descriptive data such as a reviewer's location, a paper's topic, and the decision on the paper to infer the identity of positive or negative reviewers. Nonetheless, one could make more aggregated information available both to the wider information systems academic community and restricted sets of that community (e.g., editors) than currently available.

The IS discipline needs such data to make rational decisions. For example, editors of all journals can attest to the difficulty they face in procuring reviewers, and reviewers at all journals can attest to their having too many reviews to do. But, how many reviews does the median information systems scholar perform and how wide is the variance? Knowing the answer to such questions constitutes the first step towards determining a "fair" review workload. Knowing the answer can also reveal viable and unviable solutions. For example, ICIS requires approximately 2,500 reviewers and editors every year (Urquhart, Carte, & Heinzl, 2017)—substantially more people than actually attend the conference. Clearly, the conference cannot sustain this review model, and, therefore, we need to address the problem at the institutional level and not through (for example) exhorting some scholars to do more reviews.

We suspect making such information available would also partly alleviate review problems because it allows the discipline to enact a social form of both reviewer payments and mandatory reviews. From the first perspective (reviewer payments), individuals obtain most editorial board positions via recommendation. Frequently, authorship rather than reviewership at a journal represents an important deciding factor. However, ironically, a good editor probably requires review skills more than authorial skills. If editorial boards consistently had statistics about reviewing at a journal, these boards could systematically consider high-quality, frequent reviewers for new board positions. From the second perspective (mandatory reviews), providing more review information allows the discipline to apply social pressure to those who avoid reviews.

6.2 Goals of Reviewing

Another fundamental issue the discipline must address concerns reviewing's purpose. Do reviews serve a developmental role to make papers better? Or do they principally serve a gatekeeping role? Answering these questions would allow the discipline to redesign the reviewing institution.

As one simple example, the organizing committees of many information systems conferences argue that these conferences adopt an inclusive and developmental approach. However, these conferences have an institutional structure that comprises multiple reviewers and editors. Recall that increasing the number of people involved in reviewing increases the rejection rate (Neff & Olden, 2006). In effect, the institution (reviewing) serves the opposite purpose from what the organizing committee claims.

Transparent, crowdsourced, post-publication reviews could provide a better review model for inclusive and developmental conferences. Before presentation, exactly two people (i.e., an editor and reviewer) would review every conference paper. The true "review" and development of the paper would then occur at the conference itself when authors present their papers to the audience (i.e., post-publication). The authors could receive feedback at the conference and in threaded conversations and comments associated with the proceedings. This model would require conference organizers and journals to provide a platform for commenting and a mental shift in readers on the importance of providing feedback to authors.

Revisiting the source and structure of our review processes could shed light on how to generate improvements. Consider that, in many cases, our institutional structures have imitated our reference disciplines and then evolved out of necessity. The elaborate structure of track chair, associate editor, and multiple reviewers found at ICIS, for example, arose in part due to the volume of submissions the conference attracts but also because we unthinkingly assume that papers at conferences should undergo peer review and that a good peer review requires multiple reviewers. In contrast, premier journals in mathematics such as the *Advances in Mathematics* can have a single peer reviewer (see https://www.elsevier.com/journals/advances-in-mathematics/0001-8708/guide-for-authors). Reviewing suffers because we unthinkingly follow the paths we have always followed rather than considering our actual goals.

Our institutional structures have created several unanticipated consequences. For example, partly due to the intense human resource commitment that reviews attract, some conferences often secure review commitments before they even occur. Editors effectively draw reviewers from their social networks and assign them before anyone even knows about the papers' topics. Because reviewers come from editors' social networks, they are likely to share editors' institutional norms. Thus, at many IS conferences, reviewers serve as institutional gatekeepers rather than as developmental guides.

Further, our discipline's focus on rigor in the review process may unwittingly stifle innovative ideas. As an example, the International Conference on Information Systems has an acceptance rate of 25 to 30 percent (Urquhart et al., 2017), a rate comparable to many journals. Because ICIS publishes papers in its proceedings, publication of the same work becomes somewhat more complex for authors. Journal paper submissions that do not include a sufficient volume of new material may face rejection for not providing enough additional contribution (Zhang & Niederman, 2017). On the one hand, it would logically move scholars to withhold from submitting well-developed work that would need little additional material for journal consideration. On the other hand, it would encourage them to submit less developed work to conferences. These less developed papers have a higher risk of rejection, which prevents authors from sharing creative and novel ideas. A variety of conferences that authors can choose from exist, and they vary in the degree to which they support "finished" work or innovative ideas. Conferences that aim to publish near-journal-ready rigorous work have both an opportunity and challenge to create the kind of reviewing that aligns with the conference purpose.

Consider the following as a way to fancifully redesign existing institutions. In information systems, the word "proceedings" always refers to conferences. In other disciplines, the word can refer to journals (e.g., *Proceedings of the National Academy of Sciences*). One alternative institutional structure would be that competitive conferences such as ICIS simultaneously produce publications with varied purposes. ICIS would retain its existing track chair/associate editor/reviewer structure. However, it would also accept full-sized manuscript submissions (e.g., 40 pages). Authors present their accepted papers at the conference, and the conference publishes an abstracted version of such papers (e.g., 10 pages) in a preliminary proceedings. It then gives these papers a revise-and-resubmit recommendation for publication in a new journal called the *Proceedings of the Association for Information Systems*. Assuming the consent of the authors, the same review team at ICIS shepherds the paper through the journal. With this option, the

discipline as a whole would reduce the number of reviews because conference reviewers are journal reviewers. Furthermore, this option overcomes the institutional dilemma concerning the viability of conference submissions for later journal publication—the two become essentially identical.

Note that the model applies not only to ICIS but also to any competitive conference. The *Proceedings of the AIS* could have multiple issues per year, one for each represented conference.

6.3 Learning from the Literature

While little IS research has examined reviewing, research in other disciplines has examined it substantially. Nevertheless, we find editors make decisions to improve reviewing without considering this literature. For example, several of our journals have initiated reviewer-training programs. However, the literature overwhelmingly has suggested that one-time training interventions do little to nothing to improve review quality (Callaham & Tercier, 2007; Callaham et al., 1998; Houry et al., 2012; Schroter et al., 2004, 2008). Thus, while we applaud the intent behind such initiatives, we would suggest that we divert resources to mechanisms that research has shown to be effective or to ways of doing things that no one has trialed before.

For example, the literature has not discussed sustained training as an approach to improve reviews. Potentially, if reviewers receive feedback every time they do a review, their review quality could improve. Of course, this practice would accelerate or slow down with the quantity and quality of the feedback.

As another example, we often exhort our reviewers to review in a different way. For instance, Rai (2016) pleads with reviewers to "detect and advocate for ambitious and intellectually novel work that may be impactful" and laments the "culture of excessive reviewer negativity". Unfortunately, we downplay the role of institutional forces in shaping the way reviewers act. For example, Rai recognizes that "training in doctoral programs [that] orients students to critique work" in part drives the reviewer bias towards rejecting rather than developing reviewed manuscripts. Developmental reviewing can be quite difficult. The editor/reviewer needs to envision a pathway by which a finished study may provide significant value. They need to communicate this pathway clearly enough so that authors may follow it if they wish. Furthermore, reviewers have to be open to the revision deviating from their suggestions. Sometimes, suggestions move a paper in a direction authors do not intend, the suggestion is infeasible given authors' situation, the suggestions that separate reviewers provide contradict one another, or authors' good faith revision turns out to be less than expected or desired.

However, beyond training for developmental reviews, innovative work when first submitted and when finally published differs substantially. While the typical reviewer has seen numerous examples of published innovative work, such a reviewer often has not reviewed innovative work when newly submitted. As an analogy, we all generally know what diamonds set in jewelry look like, but we do not know what uncut, freshly mined (i.e., rough) diamonds look like. The latter tend to be ill-shapen, dirty, and translucent rather than pointed, brilliant, and transparent. Reviewers reject innovative work in part because they use the wrong standard of comparison—they expect submitted manuscripts to look like diamonds in jewelry.

Thus, increasing review transparency (i.e., open review) represents one way to improve the review process. If everyone can see what papers look like from inception to final publication, we can better judge and evaluate submitted manuscripts.

Much literature has discussed open reviews. In this paper, we present one stream of this literature (i.e., transparent, crowdsourced, post-publication reviews). Nevertheless, we painfully recognize that our discipline, including our editors, remain distressingly unaware of this literature. As evidence, we highlight that attendees could not vote on an open review topic at the panel because they did not understand it. Essentially, our reviewing remains in its current state partly because we do not recognize the alternatives. Better understanding what others have tried can potentially provide the IS discipline with ways to improve reviewing.

7 Conclusion

Anyone who has had any experience with the peer review process can attest that it suffers from various problems. However, many of those same individuals often cannot precisely articulate what the problems are, demonstrate their severity, and place those problems in the context of the academic environment. Furthermore, they cannot perceive how solutions to many of these problems may have both positive and negative cascade effects on the IS discipline's institutions.

This paper, a product of a panel held at ICIS 2017, exposes the information systems research audience to these problems and solutions. We present four problems with the existing review institution and the benefits and drawbacks of three potential solutions. We neither advocate for nor condemn these solutions but rather use them to illustrate what we believe represents the core underlying issues with reviewing in the IS discipline. Based on these underlying issues, we advocate that:

- The IS discipline needs to make more information about the review process available both to editors and to the wider community. Among other reasons, we cannot make good changes to the review process because we do not have enough information on it.
- Each peer-reviewed outlet in the IS discipline and the discipline as a whole needs to more clearly articulate what it really wants peer review to achieve. We demonstrate contradictions between the purported goals of conferences and conference peer review, and we show how institutional changes can overcome the conference/journal publishing dilemma.
- As a discipline, we need to be more informed about what peer review actually is. Much literature on the subject exists, and other disciplines have tackled the issues we grapple with. Curiously, the IS discipline does not really engage with this literature. We need to both contribute to and be informed by the literature on reviewing.

Changing our existing institutions, especially peer review, is a risky process. However, it is a necessary one. Given the exponential growth of journal and conference submissions, we cannot continue with our existing models (e.g., Saunders & Benbasat, 2007). We believe the best approach involves making decisions based on clear goals and on both empirical practice and the literature.

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