

Some thoughts on reviewing for
***Information Systems Research* and other leading IS journals**

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Introduction

Peer review of research before publication is both an essential and an integral part of scientific knowledge production. For reputable journals, the peer review process distinguishes knowledge claims in journal articles from those in sources with unknown or varying veracity. The peer review process assures readers that the published work is credible (i.e., conducted in line with prescribed norms of research) and meets a certain threshold with respect to contributions and potential impact. Leading journals are perceived as such not only because the best research is submitted to them but also because of the efforts of the best reviewers and editors in evaluating and, when applicable, developing the initially submitted manuscripts² to publishable form.

The sustained quality of reviews is critical for journals such as *Information Systems Research* (ISR). With the number of submissions to ISR growing each year, as well as an explicit policy of encouraging and celebrating inclusive excellence (Sarker 2023), there is a need for more reviewers for the journal (and the discipline, more generally) who have the necessary expertise to evaluate submitted papers, who understand and are attuned to the norms of the different traditions and genres of work submitted, and who know how to produce reviews that ensure the review process supports effective knowledge production.

In this editorial, we draw on the expertise of some of the experienced Associate Editors (AEs) at ISR³ who represent different research traditions to provide guidance on how ISR reviewers can contribute reviews that AEs and authors are likely to find valuable. The primary audience of this editorial is Ph.D. students and early career scholars who occasionally review for, or seek to review for, ISR and similar journals. While experienced reviewers likely know most of what we will say in the next few pages, we are hopeful that the editorial can provide a useful recapitulation of characteristics of reviews that are appreciated by ISR AEs, irrespective of the reviewers' experience. Finally, revisiting what reviewers look for in manuscripts can prove helpful for authors submitting papers to journals such as ISR.

² In this editorial, we use the terms "manuscripts" and papers interchangeable to academic articles that are in various stages of preparation and consideration at the journal.

³ The EIC invited 9 experienced AEs representing different research traditions from among the outstanding set of ISR AEs to contribute to this initiative. In addition, ERB member, Edgar Whitley, who has organized and run many reviewing workshops for the IS community over the years, was invited to help the EIC in developing the editorial..

Before proceeding, we would like to acknowledge the efforts of editors and editorial board members from various journals who have organized reviewer development workshops (e.g. Rai 2019, Whitley 2023), and reflections on the review process and effective reviews by notable scholars in our discipline (Davison 2015, Kohli and Straub 2011, Lee 1995, Leidner et al. 2022, Rai 2016, Saunders 2005a, b, Straub 2009), see Table 1. Our editorial does not seek to supplant this accumulated wisdom but seeks to add nuances to the various guidelines that have been offered in the past.

Lee AS (1995) Reviewing a manuscript for publication. <i>Journal of Operations Management</i> 13(1):87–92.
Saunders C (2005) Editor’s Comments: From the Trenches: Thoughts on Developmental Reviewing. <i>MIS Quarterly</i> 29(2):iii–xii.
Saunders C (2005) Editor’s Comments: Looking for Diamond Cutters. <i>MIS Quarterly</i> 29(1):iii–viii.
Straub D (2009) Editor’s Comments: Diamond Mining or Coal Mining? Which Reviewing Industry Are We in? <i>MIS Quarterly</i> 33(2):iii–viii.
Kohli R, Straub D (2011) Editor’s Comments: How Reviews Shape “MIS Quarterly”: A Primer for Reviewers and Editors. <i>MIS Quarterly</i> 35(3):iii–vii.
Davison RM (2015) The Art of Constructive Reviewing. <i>Info Systems J</i> 25(5):429–432.
Rai A (2016) Writing a virtuous review. <i>MIS Quarterly</i> 40(3):iii–x.
Leidner DE, Carte T, Chatterjee S, Chen D, Jones M, Preston D (2022) On Civil Critique: Reviewing for JAIS. <i>Journal of AIS</i> 23(1):1–12.

Table 1 Selected articles providing reviewing guidelines in the IS literature

Why does academia have peer review?

Peer review is the process by which academia ensures the quality of work published in research journals such as ISR. The *quality of scholarly work* is not easy to pin down (see, for example, Agarwal 2012), given the many research traditions and associated standards across and even within research traditions (see, for example, Rai 2017, Sarker et al. 2018).

When peer review is minimal or missing, the reader cannot be confident about the published findings, and the ideas are often poorly developed and presented. Moreover, a minimal review process does not contribute to advancing scholarship. In extreme cases of so-called predatory journals, the only activity that takes place before an article is published is payment of a publication fee (Safi 2014).

One way to evaluate academic quality is offered by Gupta (2018), who builds on Ellison’s (2002) distinction of two dimensions of the quality of an academic paper. The *q dimension* relates to the quality of the idea or underlying message of the paper while

the *r dimension* reflects the quality of the execution of the paper. The peer review process can perform a number of key roles along both dimensions.

The review process can improve the *q dimension* of the paper by helping to articulate the novelty of the contribution, although there are limits to how much the review process can do with respect to the fundamental ideas behind the study. Reviewers often need to make a judgment call regarding the adequacy of a manuscript on the *q dimension*.

With respect to the *r dimension*, the review process ensures that the published research has no fundamental flaws that affect its contribution to knowledge. Thus, the review process might result in the authors undertaking additional analysis or gathering additional evidence to ensure that the findings accurately reflect the phenomena being examined. The *r dimension* may also include a cohesive presentation of the findings. This can range from clarifying the form of argument being followed, to contextualizing the work in the broader historical literature more effectively, to helping the authors articulate their contribution more clearly and persuasively (Sarker 2023, p. 2).

When selecting reviewers for a manuscript, editors typically look for a mixture of expertise. Some reviewers may be specifically invited to focus on the *q dimension*: to assess the big picture and the profound implications the paper might have. Other reviewers might be chosen to assess and buttress the *r dimension* because of their familiarity with the specific techniques used in the paper or the phenomena under investigation. Doctoral students and early career faculty, having recently completed state-of-the-art methodological training, tend to have expertise that helps in assessing and strengthening the *r dimension*. Their reviews also tend to place a larger emphasis on the *r dimension*, sometimes without considering the *q dimension*.

A variety of images have been used in relation to the objective of the review process. A traditional one is that of the review process playing a “gatekeeper” role, but those of “diamond cutter” and “champion” are also often mentioned (Sarker et al. 2015). For example, Saunders (2005b) suggests that rather than acting as gatekeepers to publication in a journal, the review team should play a diamond cutter’s role, working with authors “in polishing manuscripts so that the gem can surface and shine” (2005b, p. iii).

The AEs who co-authored this editorial echoed many of these general points. For example, one AE saw his role as similar to the “coach” of a football team who manages and mentors a team of budding superstars to win games. Another saw their role as being a “coordinator, facilitator, helper, tutor, and advisor”. Yet another saw AEs as

“developmental gatekeepers” while a fourth saw editors and reviewers as “stewards” with responsibility for helping the research become the best it can be, thus creating value for a broad range of stakeholders: society, scholars, practitioners, and other consumers of research as well as authors, journals, and the academic community.

Notwithstanding the positive imagery, Rai (2016) encourages reviewers to differentiate “between issues that are fatal, showstoppers” and those that are “not necessarily fatal”. Along similar lines, Saunders reminds us that “expert diamond cutters can camouflage some errors in the diamond” but also reassures us that most “fatal flaws are impossible to conceal” (2005b, p. iii). *The key point is that reviewing involves a thoughtful balancing act between being constructive and charitable on the one hand, and not being oblivious to fatal flaws and offering incisive critique based on relevant criteria on the other hand.* This balancing act, when done effectively by the editorial team guided by the SE and the AE, ensures that Type I errors (accepting papers that should have been rejected) and Type II errors (rejecting papers that should have been accepted) in the review process are minimized (Kohli and Straub 2011).

In short, the peer review process for leading journals is designed to provide assurance to the reader that articles published in the journal adequately address both *q* and *r dimensions*. This ensures the high standards of the journal and each article published in it. In disciplines such as information systems (IS), the peer review process also helps the authors construct and present their work in the most consumable and potentially impactful manner. The entire process needs to be undertaken in a professional manner, where objective, even critical, assessments are welcome and are generally desirable. However, the feedback has to be provided in a considerate and constructive manner so that, even if the manuscript is rejected, the authors can learn from the process and can improve the manuscript for submission elsewhere (Kohli and Straub 2011).

This emphasizes the importance of being civil in the review process (Leidner et al. 2022). This is particularly important because ours is a relatively small discipline, and we see ourselves as members of a close-knit global community. Lee (1995) suggests that academia is at its best when “reviewers rise to the occasion and give extensive help, even though the anonymous reviewing process promises them nothing in return for their efforts” (1995, p. 87). This is in marked contrast to the negative behavior where reviewers offer “negative remarks that they would not have the courage to voice in public” (1995, p. 87) because they can hide behind the anonymous review process.

Why review for ISR and other prominent journals?

Although the collective benefits of peer review are apparent to the authors of published papers and the readers of the journals, the incentives for reviewing are often not obvious. This can be problematic as reviewers need to expend significant effort on reviewing.

For example, Bannister and Janssen (2019) note rather bluntly that “reviewing can sometimes seem to be one of the least valued of tasks. Reviews are not published. They will never be cited. Usually, they will neither earn you promotion nor any recognition beyond that of a small circle of grateful editors and associate editors (AEs) and, occasionally, your co-reviewers” (2019, p. 1). According to Kohli and Straub (2011, p. iii), reviewers are the “Good Samaritans who remain anonymous ...”.

Goes (2014, p. v) suggests that the act of reviewing manuscripts achieves a twofold objective, combining facets of “love and glory” (2014, p. v). The element of “love” is manifested in the form of appreciation (often not visible) from authors who benefit from insightful and constructive feedback, as well as from AEs and SEs, who rely on these evaluations to make informed decisions on the manuscript’s suitability for publication. The “glory” aspect provides a venue to build and solidify academic standing within the scholarly community. This recognition can manifest itself in several ways: it can elevate one’s stature among senior colleagues in the discipline, making one a prime candidate for accolades such as Best Reviewer Awards, and even pave the way for invitations to join prestigious editorial boards or highly visible conference program committees. Or as Lee (1995) notes, the professional relationships that arise from working closely with journal editors can result in leading scholars in the field writing letters of support to the reviewers’ promotion and tenure committees (see also Rai 2016, p. iv).

Lee (1995) outlines a number of additional benefits of being a reviewer. First, he notes that “Doing a review ... confers an insider’s view of the reviewing process. The reactions of the other reviewers and the editor all contain potential lessons for one’s own manuscripts to be submitted for publication. In reviewing manuscripts, one also gains access to invaluable bibliographies”. Lee also sees reviewing as a “socially significant gesture” that reciprocates “some of the help” received from colleagues who have been supporters, job contacts, or external letter writers. Finally, Lee believes that participation in the review process allows for the opportunity to champion work, especially those related to marginalized research traditions, and inform those involved

in the review / editorial process about the merits and appropriate standards -- especially those who may be “hostile to and ignorant of the research traditions” (1995, p. 92).

For researchers aspiring to benefit from participating in the review management process for papers aligned with their areas of expertise, the easiest way to do so is to create an account in the review management system that the journal uses (<https://mc.manuscriptcentral.com/isr> for ISR). These user accounts are commonly created when a researcher submits a paper to the journal but it is also feasible to create an account before submitting any papers, thus setting up oneself as a prospective reviewer for the journal. Beyond providing basic demographic and contact information, the researcher can specify their areas of expertise. These expertise keywords are often used by the AEs when they are looking for specialist reviewers for particular papers. Proactive engagement with relevant AEs—by informing them of one’s presence in the system and willingness to undertake review assignments—is another option. If a researcher is not registered in the review management system, they may still be invited to review a paper (and added to the system) by an AE who is already aware of their expertise.

While setting up an account is a good first step, and offering services to AEs with similar research interests can help, we should mention that, owing to the stature of the journal, *the AEs are very selective in who they invite to serve as reviewers*. Their invitations often hinge on the publication records of the potential reviewer in similar-quality journals, as well as past interactions at conferences and previous review processes at ISR and other journals.

The review process – an overview

Each manuscript undergoes initial screening at the Editor-in-Chief (EIC) and the Managing Editor (ME) level and, if it passes the screening, is then assigned to a Senior Editor (SE). The SE then checks the manuscript and identifies a suitable AE. The AE examines the manuscript and determines, in consultation with the SE, whether the manuscript should go out for review. If it is deemed suitable for review, the AE typically invites 2-3 reviewers for the manuscript.

For ISR and many IS journals, the review process is double-blind. Reviewers typically do not know who the authors of the paper are, and the authors do not know who the reviewers are. This anonymity should be maintained throughout the review process to avoid perceptions of undue influence. The double-blind review process is intended to

help ensure that the identities of the authors and reviewers do not influence it (positively or negatively).

Reviews and decisions

ISR operates a tiered review management process with SEs making the final decisions on a submission based on their own reading of the manuscript and the recommendations made by AEs. The AEs typically base their recommendations on their independent reading of the manuscript along with the recommendations by the reviewers. Once the SE makes the decision, a decision letter will be generated for the authors with the SE report, the AE report, and the reviews included. Reviewers will normally receive a blinded copy of the decision letter sent to the authors. They will also be thanked for their service as a reviewer, *sometimes* personally by the AE and SE, particularly if the review is truly outstanding.

To learn from the review process, especially if you are relatively early in your career, we suggest you go over the entire package when a decision is made on the paper that you were a reviewer for. What did the other reviewers say? Did they have concerns and observations similar to those you had? Did they provide guidance or solutions to problems that you identified in your review? Did the AE use or refer to your comments in their report? Did the SE use or refer to your comments in their report? Did they agree or disagree with what you had to say?

Please do not be disappointed if your recommendation (say “reject”) does not match the final decision of the SE. As a reviewer, you are *one part* of the process that decides the fate of the paper, and your review will typically have been very important for shaping the editors’ assessment of the paper, even if the editorial decision did not fully align with your recommendation.

The SE and AE, who are typically more experienced and have the responsibility of publishing papers suitable for the journal, are *informed* by the reviews – *they are not bound by reviewer recommendations*. In particular, it is important to recognize that *the editorial decision-making is not a “voting” process*. As an example, two “reject” recommendations and one “revise and resubmit” from the three reviewers may or may not result in the AE recommending “reject” and/or the SE deciding to “reject” the manuscript.

Additional rounds of review

If the paper is invited for resubmission after revisions, you (a reviewer in the earlier round) may be invited to review the revised version of the manuscript. At this point, you will need to read the reviews (including your own) from the previous round, the AE and SE reports, the “response document” that authors submit showing how they have addressed all the comments, and the revised manuscript. Then you will write reviews for this version of the paper.

You will find that some of the issues that were pointed out in the earlier round have likely been resolved, some have been addressed but not to your satisfaction, some may have been countered by the authors, and, finally, new issues may have emerged. As a reviewer, while you don’t have to necessarily agree with the AE’s and SE’s views regarding the (lack of) importance of some concerns that you had raised in the earlier round, it is important to consider their views carefully. If you feel very strongly about your concern, you may politely explain why you feel the issue is (still) critical. You may choose a confidential communication channel in manuscriptcentral with the AE / SE for this purpose, or reach out to the AE through email. This can also help to ensure there is no misunderstanding about these issues before you submit your report.

Several AEs highlighted the importance of *consistency across rounds of reviews*. For example, one AE mentioned that reviews offering contradictory or changing suggestions in different rounds (they referred to this as “oscillations”) were a source of frustration for not only the authors but also the AEs and SEs.

Collectively, the multiple rounds of review enable the manuscript to evolve and mature, and often times, gradually reaching closure. Thus, AEs urge reviewers to be conscious about not adding new layers of onerous concerns in each round, or ask for new studies to be conducted during the revision rounds unless they are critical for the work to stand. Here is an example where the reviewer acknowledges the progress of the manuscript, but seeks additional clarifications regarding some of the material added in the revised version (which is altogether appropriate):

In this revision... the additional literature review, clarifying the constructs and hypotheses in the theoretical model, and the description of the empirical validation are well-received... The earlier reviewer issues I had for this paper in areas of practical implications, empirical validation, and limitations of the empirical validation were all addressed in this round of revision. The hypotheses and the practical implications of this paper are clearer. That said, I have two more comments... (1) The most interesting finding is... The authors are suggested

to elaborate on this finding and provide concrete cases or examples to illustrate... (2) on page..., the authors discuss the first theoretical contribution is... I cannot see where this theoretical contribution comes from. Please clarify the linkage between this conclusion and the supporting empirical findings. Also, what do you mean by... Do you mean...? Or...? These are missing in the discussion section.

In addition, one of the AEs indicated that it is generally inappropriate to raise concerns of a fundamental nature, such as issues with the selection of cases or data samples, identification strategies, improper matching of treatment and control groups, or potential endogeneity issues, in later stages of the review process if these issues were already evident in earlier submissions. If evident, they really need to be brought up as soon as they are spotted. Obviously, in some cases, the significance of these factors only becomes apparent as the overall argument made by the authors becomes clearer. In such cases, these (emergent) concerns should be brought to the attention of the AE and SE through the review. However, wherever possible, flagging these issues in the initial rather than advanced rounds makes the whole process smoother, and fairer to the authors.

Your involvement in the review process

Having briefly described the overall review process, we now outline some good practices to follow as a reviewer.

You have been invited to review, what is the first thing to do?

We recommend that *you acknowledge and accept (or decline) the invitation as soon as possible*. If the manuscript is completely outside your area of expertise⁴, you perceive a conflict of interest (e.g., you know who the authors are, and feel that your participation in the process may weaken the integrity of the review process⁵), or your schedule simply does

⁴ It is understandable that your expertise may not cover all aspects of the paper. In your review, you can specify what aspects of the paper your expertise does or does not allow you to comment on authoritatively. This might be apparent from the paper's abstract included as part of the invitation. Sometimes, however, it may only become apparent on seeing the full paper. It is therefore good practice to quickly skim through the paper after you have accepted the invitation to review to make sure you will be able to review it.

⁵ ISR's guidelines for ethical behavior state that conflicts of interest might arise from "competitive, collaborative, or other relationships or connections with any of the authors, companies, or institutions" (ISR 2023b). Ultimately the objective is to avoid any perception that the peer review process might be compromised by relationships or connections between the authors and the reviewers.

In some cases, the conflict is clear and you should not act as a reviewer, for example, if the authors are from the same institution as you, or have had a close collaborative relationship with you (e.g., supervisor and student). Other cases are more nuanced. Perhaps you heard an earlier version of the paper at a conference or as a job talk and so you have a strong sense of who the authors are. In such cases, it is a good idea to flag the issue to the AE

not permit you to take on the responsibility of submitting the review by the deadline, please write to the AE to explain the situation and, as applicable, ask to be excused from the review or request an extension (a week or so is usually fine). If you are unable to review due to any reason, you may recommend a colleague who you believe has suitable qualifications and will do a good job. Please note that if you do not respond to the AE's invitation promptly, you hold up the review process which may result in the authors hearing back from the journal late, thereby losing precious time which may have impact on their career progression.

As well as accepting invitations to review in a timely manner it is, of course, important to submit your review within the period indicated in the invitation to review that you accepted. Occasionally, external events will affect your ability to deliver the review on time – in which case you should inform the AE as soon as possible, and renegotiate a new deadline. In some cases, the AE will need to move ahead without your review or will need to recruit an alternate reviewer.

Practicalities around writing and submitting your review

We recommend that you write your review in a word processor (or other program), and upload or paste the review text into the review management system. This can prevent the loss of the entire review that you may have typed if the website or browser experiences difficulty and shuts down. Alternatively, you can also submit your review as a Word or PDF document, especially if the review report contains mathematical equations or formulae that require specific formatting.

Alongside the main review, you may also (optionally) provide brief comments to the editors which will not be shared with the authors. In these confidential comments, you may mention your overall impression about the manuscript, express serious reservations that you have (if any), and so on.

However, *your comments to authors should be consistent with the comments to the editors*. Sometimes, AEs are left facing a perplexing situation where the review to be shared with the authors is positive, whereas the confidential comments to the editors are extremely negative (or vice versa), making it difficult to ascertain the reviewer's overall assessment of the paper.

and let them, in consultation with the SE, make the final determination. For example, the AE might prefer to take the fact that you know who the authors are into consideration when evaluating your review rather than to replace you with a less expert reviewer.

Some Attributes of a Helpful Review from the AEs' Perspective

The IS community has produced a number of excellent guidelines on the review process (see Table 1). We urge you to look at them. In this section, we complement these existing guidelines with some of the key issues -- Ethicality, Approach, Structure, and Technique (E.A.S.T.) -- highlighted by the AEs we invited to be part of the editorial. Among other inputs, they provided examples of suitably disguised reviewer comments from reviews of papers that they had managed in recent years. Excerpts from some of these sample reviews are included, with suitable modifications.

Ethicality: Uphold highest standards of ethics in the review process

" We may regard our own behaviors, as reviewers of manuscripts in the 'double-blind' reviewing process, to be a manifestation of the values that we hold as members of the community of scholars" (Lee 1995, p. 87)

Given the role that peer review plays in ensuring that the published research is of the highest quality possible, ensuring that you behave ethically in the review process is of paramount importance. The kinds of unethical behavior that can affect peer review range from the conflation of personal bias with legitimate critique to deliberate attempts to subvert the integrity of the whole peer review process. Another concern arises when reviewers perceive the journal as favoring specific methodological or theoretical orientations and use this as a basis to reject papers. Such actions can often stem from reviewers not being clear about the journal's editorial objectives.

While such biases cannot be eliminated from the review process, they must be actively managed to not unduly skew the process. As a reviewer, you must reflect on your own biases and strive to transcend them when writing a review.

Sometimes, as a reviewer, you may disagree with the authors' norms and values that underpin their research. This is to be expected, indeed, as one AE noted:

The academic community would not be as interesting and valuable as it is today if we all agreed on everything.

Such differences in norms and values will likely influence your assessment of the quality of the research (or open up additional areas for improvement of the work).

However, it is unethical to disguise this intellectual conflict by wrongly attributing a reject recommendation to other broad-brush reasons, such as a “lack of theoretical contribution”, or a “sample size” issue. Rather, we advise that you disclose your own perspective and respectfully argue your point, while also respecting the authors’ perspective as they write their paper, even though it differs from your own.

The kinds of unethical behaviors most threatening to peer review integrity are those that are hidden, including attempts to “game” the review process, for example, through *quid pro quo* arrangements where reviewers will be more supportive of particular authors’ papers in the expectation that they will reciprocate this support when reviewing the reviewers’ own papers. If you suspect attempts at gaming the review process, you should raise this with the AE immediately.

We mentioned above the need to disclose conflicts of interest to the AE upon receiving a review request, as well as during any point of the review process when realizing that a conflict exists. This is an ethical duty of reviewers and one AE gave a compelling illustration of a response to an invitation to review where the reviewer took a strong ethical stance:

I declined the review because recently I've discovered who the authors were from a copy of the paper on SSRN. I know one of the authors. As a consequence, I don't think it is appropriate for me to review this paper because I may have some positivity bias towards the work.

Upholding high standards of ethics also includes being transparent about your limitations as a reviewer. For example, in the (common) situation where you do not have the expertise to authoritatively comment on every aspect of a manuscript that you have been invited to review, it is helpful to state what aspects of the manuscript you feel qualified to comment on. This may allow the AE and SE to decide whether they need to invite an additional reviewer to assess those aspects of the paper that you are unable to evaluate or how much weight your comments should carry regarding a particular aspect of the paper. For example, one AE suggests that you might say something like the following:

Before commenting on the merits of the paper, I should discuss my qualifications that are relevant to this research. I have experience in researching ..., as well as in deploying and evaluating the performance of ... within the context of a variety of ... issues. I have some experience with the... methodology, but I have no experience in the specific realm of...

Some AEs prefer to have comments regarding your expertise as a reviewer in the form of *confidential* comments to the AE, because they feel that reviewers might lose credibility in the eyes of the authors.

Another case of ethical transparency is when a reviewer discovers that they have already reviewed a previous version of the paper, such as in this case:

Now that I see the full paper, I need to let you know that I already reviewed it for another IS journal ... I compared the previous submission and this one. Although some of the suggested changes have been made, key problems I identified previously remain, such as ... With this in mind, I am not an appropriate reviewer for this paper

The most pressing ethical concern is if a paper exhibits indications of academic misconduct (plagiarism, fabrication of data, etc.). Your responsibility as a reviewer to clearly communicate your concerns about potential misconduct, including data manipulation, misrepresentation of results, and other ethical lapses, cannot be overemphasized. This is not the place to “not want to cause any trouble.”

As a reviewer, you need to communicate your concerns as clearly, precisely, and specifically as possible. However, as it is not your place to act as “jury, judge, and executioner”⁶, the most appropriate way to do this is to share your concerns (and the inferences that you, as an expert on the topic, draw from your observations of these concerns) confidentially to the AE and SE. Journals such as ISR have clear procedures to follow if allegations of misconduct are raised by reviewers.

Another important ethical consideration relates to the possible use of generative AI and large language models in the review process. Whilst the technological capabilities of such systems are rapidly developing, *at this time*, we feel that it is not appropriate to delegate the task allocated to you as a human expert to a software system. Generative AI systems are trained on large quantities of general texts and, as such, are unlikely to have a good fit with the current norms and expectations of a specialist area of academic knowledge such as IS and even less so about the evolving standards for review in a particular journal (Hosseini and Horbach 2023). Susarla et al. (2023) provide an example of how reviews by generative AI models can be *completely misguided*, and hallucination can even generate fake references.

Additionally, uncertainty about large language models’ use of submitted prompts and other input data to refine their systems has caused journals like the *Journal of the*

⁶ See (Kock 1999, Kock and Davison 2003) for an example of how complicated allegations of academic misconduct can become.

American Medical Association (JAMA) to remind reviewers about their confidentiality policy. JAMA “prohibits the entering of any part of the manuscript or your review into a chatbot, language model, or similar tool” (Flanagin et al. 2023).

The research you have access to as a reviewer is, by definition, not publicly available and you must not draw on it for your own research or otherwise benefit from the findings until the research has been published. Finally, you should not reveal to the authors that you are one of the reviewers nor reveal the identity of the anonymous AE.

Approach: Maintain a professional and constructive orientation

“In what ways may we ...”

Given the general proclivity toward a review process that seeks to serve the community, it is imperative that you enact a constructive, author-centric orientation as a reviewer. As Bannister and Janssen (2019) state, “It is important to approach any review with good will, i.e. an open mind and a willingness to change your own mind or position if the paper is convincing” (2019, p. 2).

It goes without saying that a significant proportion of papers submitted to ISR will not end up being accepted for publication in the journal. Reviewers are responsible for bringing flaws to the attention of the editorial team. Consequently, critical comments in reviews are sometimes unavoidable, but they should not appear dismissive and rude but instead be offered in a collegial spirit to move the work forward, so that it may be published in ISR after revisions or in other journals.

Starting a critical part of your review with the phrase “in what ways may we” forces a constructive orientation. Compare: “In what ways may we better locate the study in the recent ongoing discourse in the field” to “The study doesn’t seem to be aware of the ongoing discourse in the field”. In the words of an AE:

A recommendation I would have for reviewers is to please take the perspective of a reader, a peer, or a colleague in the review process, and treat the authors in the same way that you would like to be treated (not necessarily how you have been treated in the past).

This ties in with Lee’s advice (1995) to reviewers -- “Be kind.”

Undoubtedly, pointing out the flaws in a study is important; however, of even greater importance is the articulation of ways in which authors can leverage its strengths, rectify its flaws, and thereby enhance the overall quality of their work. In essence, adopting a mentality of collaboration - an us-with-them rather than an us-versus-them

perspective - can transform the review process into an intellectually stimulating experience and result in a higher-quality article.

It is also important not to be overly judgmental. In the words of an AE:

A common reviewer mistake is to recommend reject on the basis of weaknesses that, in their book, are egregious, but that might not be fatal flaws. For some reviewers, a certain weakness might appear particularly jarring based on their personal experience, and perhaps indicate that the authors are not experienced or well trained. We probably all have such hang-ups. But you are not there to judge their training, or their character, only to assess their submitted paper. If you are concerned about the quality of the paper, explain why in the review; if you are concerned about the authors' abilities or efforts, reserve that for the confidential comments to the editors, if relevant.

Neither is it professional to discredit authors' professional competency or focus of the manuscript in sweeping statements:

No one takes the log-log relation as literally as the authors. ... I applaud the authors' effort in taking this potential issue seriously. But the paper oversells. The current scope is narrow, and it offers little insight beyond Figure 2. ... Is accuracy SO important? The claim in the abstract that "We demonstrate that ..." is unnecessarily strong. ... Last but not the least, looking at the Figure on page 34 you obtained from [citation], we've known for two decades that the relation between log sales rank and log sales is nonlinear...

One of the AEs mentioned yet another unhelpful characteristic of some reviews:

I do not appreciate reviewers who take only the perspective of a critique and stop at the very first issue they identify that could be the ground for recommending a rejection ... One reviewer recommending rejecting a paper because they disagreed with one term used by the authors. This turned out to be a misunderstanding on the reviewer's part.

Interestingly, a number of AEs mentioned reviews that arise from a fundamental misunderstanding of the role of the reviewer in the process. Such reviews often come across as some reviewers trying to demonstrate their own research expertise (perhaps in the hope of being invited to become an editorial board member for the journal) rather than actually seeking to help the AE (and authors) assess the paper's potential and improve its quality. Some reviewers also feel the need to appear harsh so as not to look incompetent to the AE. For experienced AEs, such poor reviewing behaviors are very obvious. As one AE notes:

I think equating negative sentiments in reviews with reviewer competence is a grave misunderstanding that can be extremely detrimental to the wellbeing of members in our community.

Straub (2009, p. v) speculates on why reviewers might be unduly critical of the papers they have been asked to review. One possible reason is that they believe that publishing is a zero-sum game and see the submission they are reviewing as competition for their own papers. It could also be a defensive reaction to a study that uses techniques the reviewer is unfamiliar with, or a belief that it is the reviewer's duty to (singlehandedly) uphold the status of the journal.

A practical tip, particularly if you feel your review might be unduly harsh, is to wait 24 hours after completing your review before submitting it. This time gap gives you an opportunity to reflect on (and possibly edit) the presentation of your review so that it more closely aligns with the kind of review you would like to receive yourself.

Structure: From broad reaction to specific suggestions

"There is no such thing as: a perfect result, or a complete study of a phenomenon" (Edgerton 1987)⁷

A suggestion made by Lee (1995) and others, that we endorse, is to start the review with a brief overview of the paper as you see it. This gives the authors (as well as the AE and SE) get a sense of your perspective on the paper. Writing up the summary may also prove helpful in clarifying your own understanding of the work, remembering any paper will incrementally improve our understanding of a phenomena. This can be followed by a broad reaction to the paper and then more detailed critiques, suggestions, and a concluding summary.

The AEs provided a number of examples from reviews they appreciated:

There are a lot of aspects I like about this paper. To name a few, the research questions are well motivated, and the positioning of the contribution is well defined. I am inclined to agree that the proposed research questions and objectives can contribute to several key literature streams in information systems. Besides, the paper is well written and transparent with regard to its findings and limitations. Nevertheless, I also find a few critical limitations of

⁷ We thank Izak Benbasat for pointing us to this quotation.

the current manuscript that I would like to discuss in more detail, in order to help to improve this study.

The review continued:

In my view, the paper has the potential to address an important and underexplored question in the [online/mobile customer referral] literature. The very rich longitudinal variations in the panel dataset and the complementary natural experiment is promising and can enable the authors to tackle unanswered research questions in this stream of work. I am overall positive towards the study and see a lot of promise in it.

Another helpful review began with:

I read this paper with great interest as I believe that [CAI] is an underexplored topic that IS researchers should put more effort into. The paper included a comprehensive literature review, and the empirical estimation results seem to be robust. As mentioned by the authors, it is a great opportunity to perform a highstake field experiment concerning the role of [CAI]. Having said that, I find some flaws in the theoretical framework. I also find that the interpretations do not seem to match some of the findings. Below I provide detailed comments and suggestions.

AEs also felt that it is helpful to end reviews with a brief summary, as in the following example:

In conclusion, the paper is well-written and tackles an important topic. Yet, the weak motivation, inappropriate literature review, underdeveloped empirics, and the absence of a clear contribution represent problematic issues. But the directions to improving the work are straightforward and potentially very fruitful. I wish the authors the best of luck.

Technique: Engage deeply with the contents of the paper, and avoid opaque reviews

“Engaging deeply and sympathetically with the topic. This requires the reviewer to put her / himself in the author’s shoes and attempt to see the research topic through the author’s eyes” (Davison 2015, p. 430)

Inadequate engagement with the manuscript can result in reviews that reveal a lack of thorough understanding of the paper. Reviewers who skim through the paper without delving into the details may overlook crucial elements of the research, and provide feedback that is incomplete, inaccurate, and not actionable.

Examples include statements such as “*Hypothesis 1 seems trivial*” without justifying the critique, and “*The authors should control for more variables discussed in extant studies outside IS,*” without suggesting possible control variables and explaining why they are needed and why they should come from outside IS.

An AE calls for “specific and actionable feedback”. In their words:

Detailed and specific feedback is highly appreciated by both authors and editors. Valuable reviews offer concrete suggestions for improvement, such as addressing research questions, expanding on analyses, or acknowledging potential limitations. By providing specific recommendations, reviewers assist authors in making targeted revisions and editors in assessing the viability of the work.

Indeed, vague assertions—for instance, merely stating that a finding is not novel or a contribution is insufficient without providing reasons or examples—are unhelpful. One approach you can use is to ask, “can this review be submitted for another similar manuscript without substantive changes?” If the answer is yes, the critique is not specific enough and is probably not actionable.

Reviews that engage deeply with the core problem of the paper, whether on the research question, motivation, or any other aspect of the paper, can be invaluable to the review process:

The authors claim that [platform research on international transferability] is limited, and typically focuses on network effects which are crucial for platforms to function. Where they define a space for contribution to the discussion of [platform internationalization] is in a focus on resources; that is, on other aspects beyond the traditional emphasis on network effects (platform partnerships, architecture, etc.) This premise / positioning is largely correct, I think. However, the analysis in the paper does not teach us too much about “platform resources” (given – it does talk about the IT architecture, but clients and commercial relationships are largely network issues). So, where I enjoyed reading the case study and learned from it, my main doubt is: is the contribution here really to the [platform literature / theory], or is it in a more general space on [internationalization of financial services —or any regulated industry] for that matter?

It is often very helpful to refer the authors to specific literature rather than just saying the literature is incomplete:

Below I provide some studies that conduct ... for your reference.

My major concern is that the authors are not attending to the recent developments in the IS and related literature on ... Many features used in the paper can be improved by borrowing approaches from recent studies. For instance, please see ...

Similarly, engaging with the methodological details and how data analysis is presented can help improve the presentation of the paper:

The summary of the outcomes of the coding was overwhelming (Table 2) and it was unclear how it related to the Gioia-based coding scheme in the Appendix. Are the “theoretical constructs” in Figure 2 the same as the “concepts” in Table 2? At least, there is a huge overlap. How are open codes and second-order themes in Figure 2 related to the “illustrative codes” in Table 2? For example, in Figure 2, there are three second-order themes for “Sources”, but only two seem to be reflected in Table 2. The authors should make these representations consistent.

Along similar lines, one of the AEs mentioned the case where a reviewer strongly disagreed with an assumption made in a manuscript. However, instead of simply rejecting the paper on the grounds of this disagreement, the reviewer took the time to understand the intended contributions and analyze how the contributions would change if the assumption in question were removed. Based on this analysis, the reviewer pointed out to the authors (and the editors) that the key contribution of the paper would indeed stay intact without the controversial assumption. Given that multiple reviewers had questioned the same assumption, it is likely that the paper would have been rejected had the reviewer not made diligent efforts to dig deeper and offer a viable avenue to the authors.

From the perspective of the AE, a minimalist review is of very little value. A recommendation to accept or reject a paper without clearly identifying the reasons behind the recommendation will not help the AE (or the SE) in their decision-making process. This means that the AE has to “read between the lines” of the review to determine whether the (negative) recommendation arises because of a mismatch in the philosophical paradigm or the genre of research of the authors and the reviewer, or from legitimate substantive and methodological considerations. Minimalist reviews, even with positive recommendations, can be unhelpful to the review process, as in a recent case, when a reviewer for ISR recommended “conditional acceptance” in the first round and offered sparse comments, primarily pointing to the importance of the topic.

Paper quality considerations to keep in mind while writing your review

In an editorial on how to get a paper *published* in ISR, Agarwal (2012) examined a large number of reviews of papers submitted to the journal and proposed five conceptual categories of criteria that successful papers met. Although her editorial is about the characteristics of successful papers, considering these aspects of the paper can be helpful in writing your review. The five considerations that Agarwal (2012) identifies are Fit, Interestingness, Rigor, Story, and Theory (F.I.R.S.T.).

As with all academic activities, the F.I.R.S.T. considerations do not form a template that all research must follow but instead provide a framework within which individual instances will fit to a greater or lesser degree. Also, understandably, the criteria are not mutually exclusive, and some of the examples provided below relate to multiple criteria.

Fit

The first consideration of quality papers relates to the Fit with the journal. The notion of Fit is complex and has multiple dimensions, but, at a fundamental level, *manuscripts published in ISR must be consistent with the mission of the journal* (ISR 2023a). In particular, questions of Fit relate to how well the work ties in with the ongoing discourse in the IS literature, and whether the paper can be considered IS research at all.

For example, reviews sometimes question the relevance of the work to the IS discipline, but without elaborating on why. This is not particularly helpful to the authors or to the editors, given the various conceptions scholars hold about the essence of IS research (e.g. Benbasat and Zmud 2003, Gupta 2018, Sarker et al. 2019 to list a few).

A review that argues about Fit in this way is unhelpful:

I am not convinced that one of the top outlets (ISR) in the IS discipline is the appropriate venue for this manuscript. It is not clear how the manuscript connects and contributes to IS.

A relatively more helpful review raises a similar issue more substantively:

Looking at the theoretical development and the stated hypotheses, there is very little IS relevance. Instead, in its current form, the manuscript fits more to judgement-and decision-making type of research. Although there is a section about the implications for IS design, the

specific suggestions are not verified in the study. Also, based on the findings, one could change that section to, say, implications for governments and non-profit employees. This implies that IS is easily detachable from the front-end and results of this research, which does not bode well for the study to be considered for an IS journal.

Questions of Fit can be raised based on assumptions (in the reviewer's mind) about what the journal values, which may or may not be accurate. An AE mentioned how a reviewer deemed a design science paper as not suitable / sufficient for ISR because, according to them, that type of work is more often published in journals of other disciplines, such as computer science.

The paper is a well-written technical paper. It may easily fit a technical journal as the outlet. But its contribution is minor and cannot make it to the standard of ISR ... When reading the paper, I can clearly feel the authors' desperation to connect the model with some kind of theory ... On the one hand, as a design science researcher, I also struggled with this kind of feeling in my daily life. On the other hand, I feel it is necessary to keep such a bar in journals like MISQ and ISR due to their different audience from technical journals.

The perceived fit with the journal or with a special issue can be an important issue, but *judgments must be made with caution and from a broad, inclusive perspective*. For example, the fact that a paper does not cite a lot of IS papers may not be a sufficient reason to conclude that it is not relevant to IS: it might be that a paper opens up a new research stream within IS by addressing a novel phenomenon or bringing a theory into IS. However, a lack of citations to *relevant* extant IS research indicates, at the very least, that the authors need to engage more fully with the ongoing IS discourse. Thus, when assessing fit with the IS discipline, as per the journal's editorial statement, it is expected that you not only ask "Is this a topic currently addressed within IS?" but also "Is this a topic that meaningfully *could* be addressed within IS?".

Another point you may consider is the unique signature of IS that is evident in the study of a phenomenon that is being investigated by different disciplines. This may be important because there are few topics today that are pure "IS topics"; for example, poverty alleviation is a topic that is studied by IS, sociology, political science, and different technology-related disciplines (e.g., Sarker et al. 2019). Similarly, disaster response is a topic that is studied by public administration, development management, geographical sciences, as well as IS (Zhang et al. 2023).

Since Fit is a multidimensional issue, as a reviewer, you should certainly feel free to make observations pertaining to Fit from your perspective in your review, but you

should not make rejection recommendations solely (or primarily) based on the perceived lack of Fit. In particular, as a reviewer, you should remember that if you are invited to review a paper, it is because the SE and AE both feel that the paper is *likely* to be within the scope of the journal. Of course, the editorial team may revisit the question of Fit in light of reservations expressed by you or the other reviewers.

Interestingness

This criterion is related to the novelty and the revelatory nature of the findings and contributions. This may be related to how well the study has been motivated and is related to Ellison's *q dimension* introduced earlier. As a reviewer, therefore, a key question for you to consider is: *Why should the research community and other stakeholders, such as practitioners, policy makers, and our students be interested in this research?*

In particular, there is growing reflection on the tension between identifying "research gaps" from the literature and problematizing of the "state of the art" of a research area (Alvesson and Sandberg 2011, Chatterjee and Davison 2021, Sandberg and Alvesson 2011).

As one review noted:

The positioning of this work relative to existing literature is not compelling, for the following reasons ... Gap filling alone is simply not an effective strategy of articulating your contributions because not every gap is worth filling, and simply filling a gap may not generate any novel theoretical or practical insights.

While useful as a general prompt to the authors, the above comment may be considered to be generic, and, perhaps, borderline patronizing. Compare this to the more deeply argued feedback offered in the following example:

I think the authors can still significantly improve the paper along a few dimensions. The paper now has a broad set of descriptive findings in the current version but I believe the paper could be tighter and more cogent with one focused question, instead of covering many facts of the [referral decision, quality, and offer] that are dispersed and involve post-hoc explanations. Specifically, the authors can 1) better situate the study in the [customer referral literature], 2) sharpen the focus of the study towards a revised core question, and 3) further strengthen the empirical analysis and acknowledge other related challenges or limitations. A clearer positioning and a sharper focus of the study would help further highlight the theoretical interestingness and practical contributions.

Another helpful review wrote:

As it stands, I feel that this study currently provides only limited value in terms of theoretical novelty above and beyond previous (IS) research. Previous scholars in IS have intensively investigated the importance and effectiveness of [phishing detection systems]. As it is currently framed and presented, this study simply confirms that [phishing detection systems] help to increase trust in the tool, tool use, and continuance intention. It is also unsurprising to find that the tools are more efficient by allowing personalization of system elements. The main focus of the paper hides what I believe are the most interesting and novel insights. The more interesting question would be to break up monolithic conceptualizations of proposed [phishing detection tools] right from the start and theorize on their more specific and distinct attributes (e.g., psychological ownership, perceived controllability). This would also enable the authors to speak more clearly to the current conversation in the [IS phishing] literature and on how to advance this conversation with novel insights.

Additionally, an AE reported that particularly helpful reviews linked interestingness and innovation:

Consider the significance of the research question and the challenges associated with gathering data to answer that question instead of primarily zeroing in on empirical issues. Undoubtedly, readers need to have confidence in the empirical integrity to ensure the reliability of the results. However, it's imperative that as a field we also consider the innovativeness of the topic. If not, we risk amassing a plethora of studies offering robust analyses of clean datasets but containing results of negligible value to practitioners.

Similarly, the following review takes aim at a more “upstream” problem related to articulating the positioning of the study in relation to relevant literature, while also indicating potential contributions and (subtly) for problematization of assumptions in earlier studies:

There is a wealth of literature investigating this issue in social media in both information systems (IS) field and outside IS fields (e.g., marketing, management) documenting various related work. The authors should review extant research in more depth. One section should explain how those users' beliefs have been measured and examined in the IS literature. The authors should present a summary table with empirical studies. The authors need to explain how this research informs the present study and how their research extends this body of knowledge. It would be helpful if the authors could engage with the ongoing discourse in this area. Here are a few examples of relevant studies

Note that these examples address both how to strengthen the contributions (for example, by positioning the paper more effectively), and the practical implications.

When authors submit their paper, they set the scene in terms of the *q dimension* of their original submission; however, your insightful and developmental reviews can help them execute considerable improvements in this dimension by providing guidance for how a more effective framing and more compelling implications can be crafted.

Rigor

This area is probably where most reviewers direct much of their attention, and, thus, the AEs offered many examples of both unhelpful and helpful parts of reviews that relate to questions of Rigor, particularly in relation to quantitative studies, though similar issues apply to qualitative and mixed method studies as well.

Examples of relatively unhelpful review comments include:

The proposed [multi-armed bandit algorithm] was poorly written and lacked clarity.

From the authors' and editors' perspectives, this comment raises the question of what aspects of the methodology were unclear or poorly written.

A review that vaguely suggests a need for additional work on identification strategies to address endogeneity, such as utilizing valid instrumental variables, as in the example below, is less valuable than one that provides specific, actionable advice. A more effective review would identify (some of) additional relevant literature and offer constructive suggestions for enhancing the methodology and overall quality of the paper.

This paper suffers from endogeneity problems, and this issue is very serious in empirical research. I would urge the authors to look into various different types of empirical methods to comprehensively address this issue. I do not think the instrumental variables the authors used are valid, the authors may need to find additional instrumental variables.

Similarly, the following review excerpts offer limited value due to their unspecific comments. They lack both suggestions for alternative instrumental variables and rationale for why different identification approaches might be more suitable:

The study ... suffers from several important empirical issues. The instrumental variable analysis employed in the study suffers from potential weak instrument issue. I believe that a difference-in-differences and propensity score matching analysis will be more suitable for this study. In order to achieve higher methodological rigor, the authors should enhance empirical identification and provide a much stronger case for the causal claims.

It is important for your review to clearly point out how and why specific model or identification assumptions are problematic for ensuring the rigor of the study and to substantiate these claims with compelling justifications or elaborations. The following is an example of a helpful comment for the authors:

The authors maintained a linear specification for the main independent variables from the standpoint of the ease of interpretability. However, my concern is that the linear specification may make the magnitude of the effects unrealistically large because the negative binomial family (including Poisson) regression coefficient is an exponential multiplier, and the linear specification implies that the marginal effects exponentially grow as a user accumulates more rides.

Similarly, when commenting on data collection, it is not particularly helpful if your review simply says “*The three studies lack details*”; instead, specific feedback on key aspects is desirable, as in:

What is the purpose of the three studies? What do you want to achieve from three studies? The ‘overview of studies’ section provides too little information. How many respondents involved in the first round? How do the authors get in touch with the respondents in the second survey and what is the dropout rate? From my personal experience, the dropout rate for longitudinal surveys is extremely high. If this also happens in your study, is there any systematic bias?

Similarly, rather than simply stating that the sample frame is inappropriate for the experiment, or that the procedures are inadequate, a reviewer may elaborate on the issue, as in the following example:

The subjects had to imagine that they head a humanitarian organization. A question is how familiar the mTurk workers are with [the role of the head of an NGO] (which could affect their eventual decisions). Also, although mTurk workers are Internet-savvy, it is questionable whether they have knowledge or experience with technical subjects such as big data, AI, ML, etc. Hence, the authors should provide evidence that the subjects’ responses are credible.

AEs consider reviews that discuss where and / or why certain empirical analysis and evaluation approaches were deficient, less than robust, or perhaps incorrectly applied or implemented, and whether any potential fixes or improvements can be made in a feasible manner, as more useful.

For example, the following examples aim to showcase instances where shortcomings in empirical executions are effectively identified and discussed. In the context of network analysis, this review adeptly highlights the limitations in the authors' analysis intended to substantiate their central assumption while also offering suggestions for improving the analysis:

A community-detection analysis is provided to illustrate the network homophily assumption, which also has a few crucial shortcomings. First, the paper uses a partial sub-network constructed with only edges pointing to the selected influencers (i.e., followers to followees) and ignores the full network structure. Second, out of the 72 million edges, the authors use only about 180 thousand edges, which results in an extremely small percentage (0.25%) of their full dataset. The Louvain community detection method (Blondel et al. 2008) is quite scalable and has been shown to handle large-scale networks with more than millions of edges, which makes it hard to believe that computational cost is the main reason for using such a small sample size. Furthermore, there are many other network clustering algorithms available that can easily work on large-scale networks. At the minimum, multiple runs of the same analysis should be conducted on different random samples of influencers to show the results are consistent.

For analyses involving survey data, instead of deeming the analysis as invalid outright, a constructive review like the one below points out the challenges in assessing it, emphasizing the need for additional information regarding the instrument employed for data collection:

The validity of the data analysis was difficult to judge with the information available. First, it is good practice to provide a list of the wording of the surveyed items (at least in an Appendix), stating which sources items have been adapted. Second, how was the design prototype introduced to the participants so that they receive a clear picture of the characteristics of various applications. Third, demographic data on the sample would be highly valuable ... as control variables for the analysis.

In controlled experiments, the random assignment of subjects is pivotal. The following review appropriately raises concerns regarding the experiment's shortcomings, notably the potential for non-random subject assignment and the presence of self-selection bias:

In the empirical procedure, (a) many subjects were dropped for various reasons which were not clearly explained. As a result, random assignment is most likely violated, which substantially undermines the strength of controlled experiments or the ability to make causal inferences. A look at balance check in Table 8 revealed significant differences between the

treatment and control groups – invalidating your claim that “the two groups are comparable in terms of all demographic characteristics.” (b) If [displayed popularity and WOM rating] are not something of your interest, why did you vary them and control them in data analysis? Isn’t it easier and cleaner to just fix it in the study? (c) Only a subset of subjects in the treatment group were used, and you need to deal with self-selection bias. By doing this, the power of random assignment is further undermined, and the strengths of control experiments are no longer there.

The next review clearly explains the potential deficiencies in a proposed method for topic analysis:

The motivation for [introducing each of the sub-methods (FOPE, CVAD, BSCL)] is not clear. The authors have neither explained their intuition nor cited related methods that they may be improving upon. For instance, [FOPE] is a novel method where [LDA] is first used to extract latent topics followed by constructing a square attribute matrix. Why should a [square matrix with custom-defined non-diagonal values] indicate [user expectation] is not clear. Why not use [a single dimension one-hot vector of the LDA topics]? The preference template while novel is a heuristic and can be different representations of the expressions in equations (1) and (2) (e.g., mod of difference, ratio, logistic function, etc.). Therefore, the authors should either cite similar work that uses a preference template or justify why the preference template matrix is related to user expectation more clearly.

Addressing the external validity of controlled experiments is often a challenge for researchers conducting laboratory studies. This review extract, about a paper on advanced driving systems (ADS), delves into the experiment’s limitations in that regard and offers alternative approaches that hold promise for enhancing validity:

The evaluation of the proposed system is, unfortunately, too simplistic in my opinion. There are several specific issues with the evaluation scheme: First, the authors did not provide any information on what was included in the driving videos shown to experiment participants. Was it normal [ADS-controlled driving] or [human drivers intervening in an ADS failure]? How was [ADS] performing in the videos? Without such information, the results of this paper (at best) amount to a confirmation that “providing more information about a complex system enhances people’s trust towards the system”. Second, participants in this experiment did not actively engage with the system ... the simulated environment may be too unrealistic to be informative ... there are established ways to test the proposed systems in a safe and more realistic manner – for example via vehicle simulators ...

In qualitative research, the process of theorizing from data might involve significant creativity and flexibility, oftentimes without clearly-defined “templates” from the literature. As a result, you could face the challenging task of striking the balance between affording the authors with sufficient creative freedom and prescribing concrete and actionable suggestions.

If the suggestions in the review are overly abstract and open-ended, it could be difficult for the editors and authors to develop the paper toward closure. For example, if a review only states “the paper’s theoretical findings are unclear” or “the theoretical model is too complex and has too many components” without further elaboration, the authors may find it difficult to narrow down the main issues in the manuscript.

It is worth noting that in the early stage of development of some manuscripts, such open-ended suggestions can be suitable. In other cases, though, it would be helpful for the reviewer to supplement such statements with more specific discussions of which portions of the findings are interesting but need to be further clarified or elaborated, and which portions of the findings could be downplayed or removed.

Conversely, if the suggestions in the review are overly specific, rigid, or even forced, the editorial process runs the risk of micromanaging at too early a stage in the paper’s development, therefore potentially limiting the novelty, creativity, and contribution the study. This balance can be difficult, but possible, to find. According to an AE, the following review extract provides an example of artfully striking such a balance:

The identification of the 3 types of IT use is well described in the data. The key problem here for me is that the transition between the stages is not clear – and seeing this is a process model, the transition is critical. The authors identify self-control as the mechanism that explains this transition – this is not that insightful (without doing any analysis one would think that the ability to control behaviour would be relevant to prevent addiction). What should / could be interesting is to understand what triggers this control mechanism. The data needs to be probed much deeper to understand how this self-control mechanism is activated. The implication (from discussions) is that this activation has to do with some individual characteristics (seeing that some individuals can continue with nominal use and other do not) – or, rather, with the interaction between features and individual characteristics (i.e. affordances). ...

While you must strive to offer constructive ways to move the work forward, significant (or even potentially fatal) flaws must be communicated in a well-reasoned and

transparent way. This allows the editors to appreciate the problems, and provides the authors with the opportunity to respond to the critique:

The [Yelp data] and study has multiple major problems. First, there is a severe self-selection issue in the data. Based on other users' average ratings, people first choose which restaurant to visit and then write a review for the restaurant. ... Second, only active users, i.e., people who wrote more than 10 reviews, were included in the data analysis. But on review platforms such as Yelp, most of the users either never create any review or just write a couple of reviews. ... Third, it is questionable whether the NLP tools used in this paper were able to accurately predict personality traits based on only review data (which tend to be very short and not reflective of one's typical writing styles). But the predictive accuracy of personality scores is impossible to evaluate due to the lack of ground truth. Because of the above reasons, the analysis results of Yelp data are not reliable. I recommend the authors to remove the Yelp study from the paper.

Another example of a helpful review comment is when a reviewer provides concrete feedback on the presentation of the manuscript's regression results:

The regression tables present a large amount of information simultaneously, including main effects, interactions, and controls, which can make interpretation challenging. It would be beneficial to provide three versions of each model, including (1) a version with only main effects, (2) one with controls, and (3) one with interactions, to facilitate better understanding of the results. This would allow readers to better grasp the impact of each factor and the role of controls and interactions in the overall model.

A broader point that we must reiterate is that standards of Rigor must be applied to the methodology for which the standards are applicable. Demanding tests of causality that are in vogue for machine learning and prediction studies or asking for secondary data for studies involving surveys may not be appropriate. Similarly, insisting on kernel theory for all genres of design science research or requiring open, axial, and selective coding or the "Gioia method" for every genre of qualitative research is not appropriate and would be considered instances of poor reviewing. Finally, we echo an AE's overall sentiment about reviews focusing on Rigor:

Be rigorous, and at the same time be reasonable and understand the manuscript's major contributions and methodological limitations.

Story

Agarwal (2012) reminds us that “The structure of narrative is a critical facet of successful publishing; manuscripts must be crafted with sufficient signposts and markers that remind readers where they are in the overall plot” (2012, p. 1088). In any paper, authors are trying to convey a compelling story about a particular phenomenon of interest to the community. Papers that are favorably seen by reviewers often tell the story in a manner that is imaginative, coherent, and well-composed.

While not all studies can have an imaginative storyline, it is expected that papers published in leading journals are coherent and well-composed, making it easy for the reader to comprehend the flow of the argument and how the various components of the paper contribute to the knowledge claims made by the research. You can use your review to help with the presentation and flow of the intellectual argument in the paper. For example, one reviewer expressed dissatisfaction with the disjointed nature of the research questions of the manuscript:

When I read the paper for the first time, I was wondering why the three research questions are proposed and what the link between them is. The current questions seem disconnected and an overarching theme of the questions is not very explicit. A better positioning of this study in the relevant literature might help. It would also be helpful to distill one theme or a key point, and use it to drive the story throughout the paper.

Another review highlighted the lack of alignment between the theoretical claims and the empirical analysis, which made the story less credible:

The authors refer to three mechanisms explaining how antecedents are related to the proposed technology beliefs, including autonomy, competence, and social support. I wonder why the authors did not measure and tests these mediating mechanisms if they are that important.

Some reviewers focus on the importance of the opening paragraphs in conveying the overall narrative of the paper, as in this example:

It should be very clear from the beginning what the research problem is within the context of the current literature. Again, without a well-articulated framing of the problem within the context of prior work, the contribution of the paper appears difficult to discern. I highly recommend writing a four-paragraph style introduction (What we know? What we need to know? What have we done? What is our contribution?).

When this articulation of the story is missing, it is common to find reviews like this:

The introduction does not develop a coherent and convincing narrative. The section contains bits and pieces, but an overarching story that brings these bits and pieces together is missing. Reading through the section multiple times, I still have difficulties guessing what the actual contribution is and how it matters for research and practice. An introduction should clearly identify a gap and position the paper as an attempt to fix the gap in the literature. Without a well-articulated gap, it is difficult to see how the study adds anything new and useful to the theoretical understanding of [ERP system adoption] decisions. The introduction does not address the objective or the contribution of this study within the context of the existing problem that the paper attempts to address. First, it is unclear how this study contributes to the overall literature. Why do we need to develop a framework? What is the current gap in what we currently have, and why is it insufficient? Second, the underlying framework of an [innovation ecosystem] is very underwhelming and poorly developed. It is uncertain why this lens is used as the underpinning of the [adoption decision of ERP system]. The authors need to justify why this lens is relevant within this context.

Theory

Theory and theoretical contributions are often seen as essential to get published in leading IS journals. Indeed, the preoccupation with Theory has resulted in many commentaries, including one that chides the IS discipline for having a “theory fetish” (Avison and Malaurent 2014). Furthermore, theory is employed in different ways in inductive and deductive studies, depending on the epistemological positions of the authors. Indeed, the literature highlights a wide variety of conceptions of theory, including, according to Sarker et al. (2018):

- “a set of generalizable, falsifiable propositions or laws (Doty and Glick 1994);
- a coherent framework with identified variables and relationships (Gregor 2006);
- a ‘conception or mental scheme’ (Gregor 2006), a ‘lens’ or a ‘scaffolding’ to support the iterative process between data collection and data analysis (Eisenhardt 1989, Van Maanen et al. 2007, Walsham 1995);
- a narrative, or ‘an account of a social process’ (DiMaggio 1995, Molnar et al. 2017);
- as fundamentally not true, and not objective (Mintzberg 2005) but as fiction, that is the product of ‘disciplined imagination’ (e.g. Weick 1995);
- an ‘enlightenment,’ or ‘artful and exciting insights’ (DiMaggio 1995, p. 391)”.

This suggests that, from your perspective as a reviewer, assessing the theoretical contribution of a paper is a complex undertaking and requires a nuanced understanding of the nature of the work, and the nature of the theory that can be used

or developed for that kind of study. While we do not feel that you should enact “theory fetish” compulsively, we also believe that a degree of elegant abstraction in a paper allows for greater memorability, transferability, and generalizability of knowledge compared to isolated relationships or findings found in empirical studies and particularistic descriptions of contexts in qualitative studies.

Frequently, reviewers offer comments highlighting the lack of theoretical contribution but do not go further to suggest how such theoretical contribution may be developed. An AE pointed to the following example of this kind of review:

The study did a great job in the empirical analysis. However, the theoretical building and contribution of the paper are weak ... Also, the current empirical analysis has still left the theoretical ambiguity regarding the underlying motivation of this phenomenon. Given these main issues, the current study appears to be largely data-driven ...

While there is never a perfect theory for examining or interpreting phenomenon or for designing artifacts, you should acknowledge the explanation provided in the paper for why a particular theoretical focus was chosen, and then comment on your assessment as to the suitability of this choice for the study:

The kernel theory, namely ..., is too broad to fit into the research context. While the theory is probably useful for understanding ... at a grand scale, it is far-reaching regarding the explanation of ... from a ... perspective. Because of that, the authors struggled to find formal constructs in the adopted theory and had to infer and define two high-level constructs by themselves. The inference significantly weakens the theoretical support of the subsequent design process.

A number of AEs noted that they find it particularly valuable when reviewers comment on the underlying mechanism of how the theory works in terms of both logical and empirical evidence. In their opinion, *why and how* elements are the essence of theoretical contribution and often the most fruitful yet challenging aspect of a manuscript:

What are the underlying mechanisms? I believe the paper would benefit from more explanation about the underlying mechanisms, which could come from previous theories and/or additional experimental analysis. When it goes deeper into the driving forces, the paper could make an important contribution to our understanding of human behavior of conforming to algorithmic suggestions under different conditions. This could also address the concern of the practical relevance.

Having accepted the particular theoretical framing of the paper, you also need to hold authors accountable for their implications of their theoretical claims, which are sometimes not supported by the empirical study:

Although the authors claim in their hypotheses that the mindset theory is a suitable theoretical lens, this application of the theory was never empirically checked to see if it is viable for the research at hand. The hypotheses assert that the mindsets of users change due to the design feature, but these changes have not been empirically tested, although several experiments were conducted that provided opportunities to measure these changes. Yet, these checks are absolutely crucial to justify that the selected theory truly fits the research at hand.

Good reviews can suggest relevant directions for how a predominantly empirical or data-driven paper can improve its theoretical background or framework relevant to the IS research literature:

This paper is largely descriptive now, i.e., reporting “what” happens when certain treatment takes place. But the paper does not explain “why” such a phenomenon happens. As currently written, there is no hypothesis in the paper. There is also no theoretical explanation with respect to why random numbers are expected to cause significant anchoring effects, nor why certain personality traits should or should not affect people’s susceptibility to the anchoring effects of online ratings. The paper needs to provide conceptual mechanisms that are logically convincing to achieve theoretical contributions that deepen our understanding of the relationship between anchoring effects and personality traits.

Concluding remarks

Academic publishing operates as a form of ‘two-sided’ market, with both authors and reviewers playing critical roles in sustaining our (i.e., IS) community and advancing our discipline. Transcending the broad benefits to the community and discipline, at the individual level, high-quality reviewing offers individuals an excellent opportunity to establish a solid reputation for being a responsible and engaged member of our profession. This attribute is often overlooked by scholars who do not necessarily not consider the long-term gains over short-term costs associated with reviewing papers.

In this editorial, we seek to provide nuanced guidelines for reviewers, by surveying a diverse set of AEs from ISR for their perspectives on the review process. We summarise these actionable guidelines in Table 2.

Table 2: Summary of Actionable Guidelines for Academic Peer Review

Guidelines	Elaboration
Ethicality: Maintain High Ethical Standards	
Manage Personal Biases	Be aware of your biases and actively strive to manage them.
Be Sensitive to Ethical Issues	Be sensitive to ethical issues in reviewing and consult with the AE / SE if you have concerns.
Be Transparent on Norms and Values	If disagreements are ideological, clarify that they are based on differing norms and values.
Avoid Unethical Behaviors as reviewers	Hidden quid pro quo or similar unethical behaviors are not acceptable.
Disclose Conflicts	Always be transparent about any conflicts of interest you may have. Be prepared to let the AE decide if the potential conflict should exclude you from the review process
State Your Expertise	Provide a brief overview of your expertise and what aspects of the paper you feel qualified to critique.
Report Misconduct	If you suspect academic misconduct, inform the AE confidentially rather than adding it to your review. The AE and SE can trigger the appropriate investigations.
Approach: Maintain a Professional and Constructive Orientation	
Respond promptly to Review Invitations	Acknowledge and accept or decline the invitation to review as soon as you can so as to avoid undue delays in the review process.
Approach doing the review with good will	Be open to the possibility that the paper's arguments could convince you.
Be Kind	Align your critique with established, constructive advice for reviewers. Think about how you would feel if you received the review.
Have a Collaborative Mindset	The review process is not adversarial, dismissive, or rude; rather your intention should be to enable the paper to become the best that it can be..
Avoid an overly Judgmental Tone in your review	The critique should focus on the paper's content and not your perception of the abilities or character of the authors.
Reread your review before Submitting	If your review seems harsh, wait 24 hours before submitting to allow you to reflect on how you have presented your review and possibly edit it
Structure: Follow a Clear and Organized Structure for the Review	
Provide an Initial Overview of the paper	Start with a brief summary of the paper to give the authors and editors an idea of your perspective.
Broad Reactions	Outline your initial impressions of the paper before getting into specifics.
Structure your reviews by Issue	Structure the reviews by issue (in decreasing order of importance) rather than simply going page by page.
Provide Detailed Critiques and Suggestions for how to address the concerns	Provide detailed and actionable advice for improving the paper.
Concluding Summary	Summarize your final thoughts, critiques, and suggestions.
Technique: Engage Thoroughly with the Manuscript	
Avoid Skimming	A thorough read is necessary to fully understand and assess the manuscript. Engage with the core elements of the paper before starting to critique the manuscript.
Be Specific and Actionable	Make your feedback concrete, specific, and actionable.
Avoid Unspecific Assertions	Any claims you make should be backed up with reasons or examples.
Avoid Opaque Reviews	Ensure the reasons underlying your reactions are evident to the authors and the editors.
Engage with Paradigm Clashes	Be conscious of making assumptions about the work based on a "paradigm clash."
Refer to Specific Literature	Suggest specific literature that the authors should consider adding or reviewing based on you expertise in the area
Critique Methodology	Take the time to understand and critique the research methods and data presentation from the perspective taken by the authors
Review Criteria	
Consider F.I.R.S.T. (Agarwal 2012) as applicable	As you write your review, consider the fit, interestingness, rigor, storytelling, and the theoretical elements of the work

As the submission volume and diversity of papers at ISR continue to grow, it is our hope that this editorial serves as a valuable resource, offering practical insights and actionable advice for both new and experienced reviewers, thereby contributing to the collective progress of our community.

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