Communications of the Association for Information Systems

Volume 38 Article 19

1-2016

Iivari’s Response to the Rejoinders on How to Improve Peer Reviewing

Juhani Iivari
University of Oulu, juhani.iivari@oulu.fi

Communications of the Association for Information Systems

Essay ISSN: 1529-3181

Iivari's Response to the Rejoinders on How to Improve Peer Reviewing

Juhani Iivari
Department of Information Processing Sciences
University of Oulu
Juhani.iivari@oulu.fi

Abstract:

In this paper, I respond to the six rejoinders to my original paper. I categorize the rejoinders into three groups depending on whether they largely disagree with my suggestions, whether they mostly agree with them, or whether they suggest largely complementary ideas to my own. In line with my own paper (Iivari, 2016), I specifically focus on the concrete proposals how to improve the situation.

Keywords: Scholarly Reviews, Peer Reviews, Reviewer Accountability.

This manuscript was solicited by the Department Editor for Debates, Karlheinz Kautz.

1 Introduction

I thank all the six authors of the rejoinders for the variety of views and additional references that help provide a more complete picture on scholarly reviewing than I provide in my paper (Iivari, 2016). The rejoinders naturally share some concerns. I categorize them into three groups depending on whether they largely conflict with my suggestions, whether they mostly agree with them, or whether they suggest largely complementary ideas to my own to address the reviewing problem. In line with my own paper (Iivari, 2016), I specifically focus on the concrete proposals to improve the current situation.

I start from the most conflictual and radical view (Ralph, 2016), the spirit of which—according to my reading—is to overhaul the whole publication system, including removing the scholarly peer reviewing as a gatekeeper of what is considered scientific knowledge. As I show below, I am not ready to adopt such a radical view. After discussing Ralph's rejoinder, I move to more moderate views that Mora (2016) and Saunders (2016) express. I interpret that they largely accept my suggestions but possibly with some modifications.

Three of the rejoinders (Jennex, 2016; Recker, 2016; Stafford, 2016) address issues that I left outside of my paper and, therefore, are largely complementary in that sense. Most distinctively, Jennex (2016) suggests that faculty should be allocated time for reviewing and that reviewing service should be accounted for in the promotion and tenure decisions. Recker (2016), on the other hand, focuses on author responsibilities in improving the peer review process, and Stafford (2016) discusses the problem from the viewpoint of editors and reviewers and puts forward that their service is free and not rewarded in any way.

2 A Conflicting View

Ralph's (2016) assertions that peer reviewing is generally unscientific and practically random and that developmental reviewing is counterproductive are refreshing and thought-provoking to read. However, I cannot agree with these claims on several grounds. I think that he interprets peer reviewing's purpose too narrowly. Most of his arguments assume that reviewing is concerned with only the correctness of the research methodology. In my view, reviewing's purpose is much broader: to evaluate manuscripts' contribution (which suggests the logical possibility of having a methodologically excellent paper that does not contribute anything), to assess their literature reviews' comprehensiveness and faithfulness, to assess their argumentation more generally, and so on.

Referring to his attention on methodology, I wonder how methodologically valid the evidence that Ralph (2016) uses is. I do not have energy to consult the references he cites, but, as I attempted to make clear in my paper, one should be careful in generalizing the findings from fields such as medicine where one can usually describe the research problem and the intended contribution in a handful of pages. I think that most IS papers do not confirm to this norm.

Although my own experience is limited, I must say some of my papers have greatly benefitted from developmental reviews. I do not know to what extent others have perceived my own reviews to be developmental, but, being a stubborn man, I naturally have psychological resistance to accept that they have been purely random.

I think that I recognize in Iivari (2016) many of peer reviews' six problems that Ralph (2016) lists. However, I have difficulties in accepting his point of inter-rater reliability. In the case of a decent manuscript addressing a rich, multifaceted issue, I—as an editor—would be worried if the reviewers fully agreed since it might suggest that the whole team is biased. As I note in Iivari (2016), it may also be wrong.

Although at the general level I disagree with Ralph's (2016) assertions, some of his suggestions are worth considering for a revisionist like me. One is not to allow reviewers to make the accept/reject recommendations so that the editor making the acceptance/rejection decision cannot just follow the majority. I also agree with Ralph and other rejoinders (Jennex, 2016; Recker, 2016) that submitting to an outlet implicitly means that the authors are ready to review for that outlet on reasonable conditions, but I would not regard it as a categorical rule without any exceptions. However, if this rule is adopted, I feel that the suspension time would need to be much longer to be effective.

As for Ralph's (2016) suggestions for author rebuttals and ombudsmans, I am afraid that making them a norm means additional bureaucracy. In principle, I support possible author rebuttals, but I would see them as exceptions and relatively rare incidents rather than a norm for each review process to follow. When the author feels that the reviews include clear flaws, the author should have a socially accepted right to rebut. To make this work requires a change in editors’ attitudes. Instead of automatically viewing the rebutting author as a trouble maker, the editor should be ready to critically evaluate the reviews and not automatically defend reviewers’ work. The downside is that handling a serious rebuttal requires considerable work for an editor since an editor cannot just trust the review team’s reports. If rebutted, it may be difficult to an editor to make an informed decision without consulting other experts on whether the author’s responses are justified. In the case of an ombudsmans, I am worried that they may also lead to unnecessary fighting. The research community likely includes people who are always ready to “go to the court” in the case of minimal disagreement.

When considering the possible end of journals, I have difficulties in following Ralph’s (2016, p. 274) logic stated in his abstract: “Ultimately, therefore, we can only fix peer review in conjunction with replacing journals with repositories”. Personally, I do not see any reasonable alternative to scholarly peer reviews in legitimizing new scientific knowledge and assuring its quality. Without such a filter, one runs the danger that junk research could corrupt any repository whether a traditional journal or some alternative. There are amble examples of pseudosciences that attempt to enter scientific communities by emulating the scientific research process and its publication practices to get power and promote their business or their ideologies. However, as a detail, I totally join in Ralph’s concern with the file drawer effect. But the question is about journals’ editorial policy rather than journals as an institution. Fortunately, AIS started a new journal “AIS Transactions on Replication Research” in 2014 (http://aisel.aisnet.org/trr/).

To summarize, it seems that Ralph (2016) and I share with many other people the view that peer reviewing has its problems. However, our conclusions differ. While Ralph sees that the glass as half-empty and wants to totally overhaul the system, I see that it is half-full and want to reform the system. I am pleased to have this rare opportunity to be more optimistic than one of my colleagues.

3 Sympathetic Views

I interpret that Manuel Mora (2016) is sympathetic towards my three proposals even though he considers them insufficient for a behavior change in the peer-based review system. Before going to his counter-arguments, let me correct some potential misunderstandings. First, when reading Mora’s text I, was musing with the sentence “The author also suggests implicitly that due to the community service policy on peer-based review (e.g. no paid work), high-quality and secondary-level journals’ EICs must totally accept the recommendations from them”. I do not think that I state so, but, normally, the EIC (or the SE deciding whether to accept or reject a manuscript) must have strong reasons to override the review team’s recommendations. In that, sense Mora is right. On the other hand, I still believe that EICs and AEs have the option to override. I have exerted that power once as an SE of one manuscript when I was unhappy with the quality of work of the whole review team. Actually, it was one of the most frustrating experiences during my academic career, much more so than my own paper’s rejection by the EIC against the reviewers’ positive reviews and recommendations. Fortunately, *EJIS* later published this paper (Iivari, 1991).

As a second comment, I do not think that the correlation between my perceptions—as an author—of reviews’ quality and journals’ decision to accept or reject them are as strong as Mora (2016) interprets even though one can easily explain the correlation by the fact that we all are humans. To take an example, ICIS rejected (in 1989 or 1990, I guess) an earlier version of my paper on the organizational fit of information systems (Iivari, 1992), but, in my view, the quality of reviews was excellent, and they greatly helped to get the paper accepted later in *ISJ*. I also had several papers accepted into decent journals even though the quality of reviews has been (in my view) just modest.

Mora (2016) assesses that my three suggestions to improve reviewing are worthy individually but that they do not change the system’s behavior and, thus, that their final impact maintains the status quo of the peer-based review system. I think that this conclusion is partially based on misunderstanding. Referring to my suggestion to provide feedback to reviewers, I also believe that it is generally more important to junior people than to senior ones even though it may be useful for the latter group, too. Yet, I do not think that my suggestion excludes junior faculty as Mora assumes. According to my experience as an AE/SE in *JAIS* and *MISQ*, the editorial board members do not normally serve as reviewers. At least I do not remember that I had invited other SEs or AEs to serve as reviewers (except possibly by accident) since they are presumably more than fully occupied by their SE/AE duties. It may be that in most cases I invited senior people who I knew best, but often these busy seniors instructed me to invite their former PhD students, many of whom were assistant professors or post doctorate researchers at that time. But it may may depend on journals whether they use their editors as reviewers. Actually, the only journal that has regularly requested me—as a member of the editorial board—to review is *Information Systems and e-Business Management* (*ISeB*).

The intention of my suggestion to reward active and good reviewers was not that their "reviewer forum" papers would be accepted without reviewing. As I remark in Iivari (2016), they can be subjected to peer review just as other papers but possibly with more modest quality expectations than full research papers. Saunders (2016) proposes an idea that the best reviewers may review each other's papers.

As to my third suggestion to reveal reviewers' identities, Mora (2016) is right that that I suggest that it can be adopted only partially. The reason is that we as the scientific community do not live in an ideal world. Some of our members who feel that their papers are not reviewed fairly (even when developmentally) may take it as a personal attack and attempt to take revenge on reviewers if revealed. This risk of this behavior is naturally greater if there is a considerable imbalance between the author and the reviewer in their seniority as researchers. On the other hand, revealing reviewers' identities eliminates the possible risk of revenging to people whom were actually not reviewers.

Even though Carol Saunders (2016) is sympathetic to my suggestions, it also reminds me that there may be subtle resistance to them, especially the last two. As clarification to my comment that it may not a big loss if the reviewers' identities are not revealed in the case of accepted papers (considering that they are not revealed in the case of rejected papers), I was focusing on the impact of not blinding the reviewers' identities on the quality of reviews. I did not consider explicitly recognizing identified reviewers' contribution instead of the (normal) anonymous acknowledgements to the review team in the end of the paper. Since both *JAIS* and *MISQ* have abandoned the practice of optionally revealing reviewers in the case of accepted papers, it seems that our community does not consider this explicit recognition so important.

Saunders also mentions an interesting practice of AOM meetings to collect feedback from authors about reviews and about using it in selecting reviewer awards. This practice partially addresses the reviewer feedback problem but not fully since reviewers do not directly get that feedback.

4 Complementary Views

Tom Stafford (2016) in his vivid and eloquent rejoinder describes the cruel reality of reviewers and editors—both providing free services to the research community—without whose contribution the scholarly publication activity could not run. He starts his text with four quotations, including one from my paper (Iivari, 2016). A common theme of the other three comments is review cycle times. It seems that these comments are based on quite a different experience than mine. I still remember almost as nightmares occasions, when I received 50-70 pages-long manuscripts to review or to edit. Perhaps I could have formed the first impression of the text in a few hours. Yet, if a decent paper, I guess that it took much more time to read it properly, which I normally did in short intervals. And I also needed some time to digest the paper before writing my own review while again consulting the text. So, it took its time.

I do not have much to add to Stafford's (2016) text except that I feel almost guilty that I have criticized reviews' quality. My only excuse is that it, as a central process of the whole scientific communication system, is a more important issue than the feelings of relatively few reviewers or editors who do not do their "work" properly.

Stafford (2016) raises several interesting points. One that has puzzled and amazed me is the relationship between research productivity and reviewing/editorial activity. As Stafford notes, some people excel in both. Yet, a point raised in many rejoinders is that some researchers active in publishing are not as active editors and reviewers and may also be loudest to complain about reviewing's quality. On the other hand, to my knowledge, some people are much better as reviewers than as authors. Unfortunately, their contribution to the field is barely (if at all) recognized since it largely remains hidden (if they are not full members of research groups that publish together). I also imagine that there are excellent researchers who for some reason may be poor reviewers, even though I do not have any names in mind. Therefore, even though I also consider "not publishing without reviewing"—proposed in several rejoinders to my paper—an important moral principle, I am skeptical about interpreting it as a categorical rule. We should accept that researchers are different.

In my view, Stafford's (2016) rejoinder is more a problem analysis of this complex issue of peer review, while Jennex (2016) suggests several concrete solutions to overcome the related problems. He expresses quite bluntly that "I don't agree with Iivari (2016) on the proposed approaches to solving these issues. In this paper, I propose alternate solutions". That is interesting, since I see his six proposals complementary with mine.

His first proposal is to "increase the number of reviewers by requiring that all authors who make a submission perform at least, for example, three reviews and that this ratio be maintained on a yearly basis". As I note above, I do not believe in categorical rules. Furthermore, I do not think that forcing authors to review addresses the quality problem in reviewing, which is my primary concern. In my view, learning "not publishing without reviewing" as a moral principle and reviewing in practice should be part of the doctoral studies and continue throughout one's career. My suggestion to provide systematic feedback on the quality of one's reviews is naturally instrumental in this life-long learning process.

His second suggestion is link to conference and journal reviewing, which has been quite widely applied to solicit more papers to conferences and to invite reasonably good submissions to journals. I am not confident that it is a measure to reduce the number of reviewers in the case of later journal submissions, but, if successful, it should reduce the number of review cycles.

His fourth proposal is to reduce the time for reviews by making better paper submissions and have the reviews themselves focus on the results and not the writing, the literature review, or the methodology. Jan Recker (2016) (see below) specifically addresses the issue of better submissions or eliminating poor ones. As a detail, let me remind the reader that, according to Bunge (1967), (empirical) science is based the scientific methodology[1]. So, I think that it is critical that we review it.

Jennex's (2016) other three proposals are largely beyond the control of our community and, therefore, difficult to implement. His third suggestion is to make reviewing a meaningful part of tenure and promotion. More concretely, he suggests adding standards for reviewing so that 10 reviews (or so) might be equivalent to a journal paper. Although he suggests that tenure/promotion packets include examples of actual reviews performed, this does not address the quality problem (it is totally unrealistic to expect that the tenure/promotion committee will read those reviews and evaluate them). Actually, purely quantitative measures may comprise quality also in this context. But, based on my experience, the most difficult issue is to get this third proposal accepted in universities.

Jennex's (2016) fifth suggestion is to support faculty in attending conferences if they review for them and not just because they have a paper in them. I think that this is a good suggestion, although it does not address the quality issue and likely cannot be followed categorically since the budgets of the majority of departments and schools are tight and getting tighter. So, perhaps department heads could consider reviewing when considering funding conference trips. I also wonder if conferences could give a small discount for their reviewers' participation.

Jennex's (2016) last proposal is to perform a task analysis on faculty positions and allocate time for reviewing. I am afraid that this idea is quite difficult to get through. It reminds me about my experience of the reform of the salary system in Finnish universities in 2005 when a flexible system was substituted for the earlier fixed salaries. According to the new system, one's salary was determined by how demanding their work was and one's performance in it. In 2005, my most active time in editorial positions started, which meant much intellectually demanding work. When I attempted to communicate in those salary-related negotiations that I do such service to the scientific community, I do not think that it was understood and still less counted since it did not contribute directly to the department's, school's, or the university's performance—the performance being measured only in terms of degrees awarded at each level. But it was in Finland.

Even though I would support Jennex's (2016) last three proposals that I comment on above, as a dammed realist, I am afraid that they are difficult, if not impossible, to implement in practice worldwide. Unfortunately, we do not live in the ideal world. My proposal to publish "reviewer forum" papers is a more modest attempt to alleviate the situation, which is up to our community to implement.

Jan Recker (2016) discusses authors' responsibilities in the publication game. Before proceeding to more substantive issues, let me make a few remarks. First, he has an interpretation that much of the debate on

[1] I do not mean by "scientific method" a positivist method, but any research method that the research community regards valid in its field.

reviewing, including Iivari (2016), explicitly or implicitly starts with the assumption that every manuscript is worth reviewing. I am not totally sure about this, at least in my case. My position is "yes": once a manuscript has been submitted and if reviewing covers desk rejects, I feel that even that decision should be justified preferably in a polite way. But "no" if Recker means a full review. I suppose that every *MISQ* senior editor, for example, has made several desk rejects. As a second remark, Recker seems to have a much more positive view of reviewing's quality control than I have. For example, he claims that "reviewers are monitored as well as evaluated (implicitly or explicitly) in regards to timeliness or quality of their reviews", which does not correspond to my experience.

I agree with the four problems that Recker (2016) raises about authors' entitlements. Naturally, I have as a reviewer encountered some of them, but I do not know how common such problems are from reviewing's viewpoint. He also makes five concrete suggestions to improve the situation. The first three are modifications and extensions of my ideas, while the last two are more provocative proposals. I focus on these latter two.

I find it easier to comment on them based on Recker's (2016) "what-if" analysis. Referring to his first (i.e., "one-shot option for reviewers"), even though I do not think that Recker's analogy with the limitation that an author can submit a paper to one outlet at same time works in the case of reviewers, the suggestion that a reviewer has only one commitment at a time may make the life easier to potential reviewers who cannot say "no". Since each reviewer normally decides about their reviewing workload, I wonder how wide this problem is.

The second suggestion (i.e., "reviewers can choose which papers to review") is ideal, of course, and journals could implement it at least in principle. Yet, I am afraid that, if reviewers are too picky, it makes the life of editors quite difficult. I at least have attempted to review papers about which I have not been so excited or knowledgeable while wishing that the other members of the review team can complement my limitations. I do not know to what extent technical solutions could help in the context of this suggestion. I think that I have tried it in the case of only one conference (XP 2015) when I was given an opportunity to express my preference for the papers I would be ready to review. The major problem was that picking up such papers from a set of 100 or so also took time.

I have essentially commented on Recker's (2016) third proposal (i.e., "authorship only after review commitment") earlier. Jan's fourth "what-if" idea is to monitor author's paper commitments at the community level, which raises at least three questions. First, are there are some really prolific authors who overload the review system without providing a corresponding service to the community? Second, even though parallel paper projects may lengthen revision cycles, I wonder if it is the main cause. At least in my case (not having been a prolific author), my limited opportunities to meet co-authors was the major reason for many of my papers' long revision times. Third, I wonder how the (prolific) authors would perceive this monitoring.

Perhaps, the last of Recker's (2016) "what-if" ideas is the most provoking: to penalize authors who (continue to) submit poor manuscripts. I guess that this partially happens in the case of desk rejects. If an author happens to have such a reputation, editors will more carefully screen the manuscript and evaluate whether to submit it for a full review. "Black lists" of authors seem a quite extreme solution to me. If there were such lists, I guess that they would include authors who do not have any probability to get any papers accepted. So, if that probability is zero, "statistically speaking" does not work in their case if the reviews and acceptance/rejection decisions are not next to random as Ralph (2016) claims.

5 Final Thoughts

When talking about publishing and reviewing, we naturally view the phenomenon in terms of authors, reviewers, editors, and so on and easily forget the ultimate purpose of our collective research effort is to advance our field (i.e., to cumulate well-justified and relevant knowledge related to information systems). Many, if not most, of us are or have been members of at least two of these three groups. So, one could expect that we have some understanding of and sympathy for each of the roles. On other hand, we as individuals constantly face the dilemmas of authoring and reviewing. As a consequence, our collective response to the dilemmas is an outcome of thousands of individual decisions. The question is if this aggregate outcome is near to the best for the whole field.

When redesigning the publication process, we should always attempt to assess whether it is good for the IS field. The idea of the developmental review is one example. I think that developmental reviews are a prime example of reviewers' altruistic readiness to help authors to improve their paper. The question is if the opportunity cost of such reviews—in terms of the time lost from one's own research with possible contribution to the field[2]—is greater than their benefits. This cost is, naturally, hard to know since it depends on two uncertain outcomes. Despite that, one could answer with yes in the short run if the reviewers are more competent to conduct research than the authors.

On the other hand, reviewing and especially developmental reviewing also have longer-run dynamic effects that affect scholars' (especially junior scholars') scholarly development, the entry into our research community (who will enter and who will not and how fast), and the continuity of our field. Our scholarly knowledge is also based on communication and dialogue. If co-authorship is excluded, I think that reviewing represents the closest to-point dialogue between colleagues. Therefore, partly contrary to Ralph (2016), I believe that developmental reviewing is good for advancing our field as a whole.

Recker (2016) raises in the end of his rejoinder an important point that we should have more empirical evidence about authoring, reviewing, and editing practices to dispel myths from truths. I totally agree with that. Let me suggest one research topic. We have a pool of papers published in leading IS journals that have been reviewed in a more or less developmental way. The question is whether developmental reviewing has really helped authors write better papers when one considers other influential factors such as the journal in which published, the time of publication, the topic of the paper, the number of authors in the paper, the authors' reputation (e.g., citations to their earlier work at the time of publication), number of review rounds, author's perceptions of to what extent the review was developmental, and so on. In addition to the authors' perceptions of improvement, perhaps one could use the number of citations—despite their limitations— as an objective measure for the papers' quality so that we could answer the question with more than perceptions.

[2] Not to mention other constituents of the opportunity cost such as time devoted to the normal family life.

References

Bunge, M. (1967). *Scientific research I. The search for system*. Berlin: Springer Verlag.

Iivari, J. (1991). A paradigmatic analysis of contemporary schools of IS development. *European Journal of Information Systems*, *1*(4), 249-272

Iivari, J. (1992). The organizational fit of information systems. *Journal of Information Systems*, *2*(1), 3-29

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

Jennex, M. E. (2016). No free lunch: Suggestions for improving the quality of the review process. *Communications of the Association for Information Systems, 38,* 307-313,

Mora, M. (2016). Rejoinder on Iivari's paper: How to improve the quality of peer reviews? Three suggestions for system-level changes *Communications of the Association for Information Systems, 38,* 284-291,

Ralph, P. (2016). Practical suggestions for improving scholarly peer reviewing quality and reducing cycle times. *Communications of the Association for Information Systems, 38,* 274-283.

Recker, J. (2016). Author responsibilities in improving the quality of peer reviews: A rejoinder to Iivari (2016). *Communications of the Association for Information Systems, 38,* 314-320.

Saunders, C. (2016). A rejoinder to Iivari. *Communications of the Association for Information Systems, 38,* 292-295.

Stafford, T. (2016). Optimizing the business side of science: Publication review cycles and process management considerations. *Communications of the Association for Information Systems, 38,* 296-306.

About the Authors

Juhani Iivari is Professor Emeritus at the Department of Information Processing Science, University of Oulu, Finland. During his career he has served as a professor at the University of Jyväskylä and at the University of Oulu. Before his retirement, he also worked for ten years as a part-time a scientific head of INFWEST/INFORTE programs, which are joint efforts of a number of Finnish universities to support doctoral studies in IT. Juhani has also served in various editorial positions in IS journal, including *Communications of the Association for Information Systems, European Journal of Information Systems*, *Information Systems, Information Systems and e-Business Management*, *Information Technology and People, Journal of the Association for Information Systems*, *MIS Quarterly, and Scandinavian Journal of Information Systems.* His research has broadly focused on the theoretical foundations of information systems, IS development methods and approaches, organizational analysis, implementation and acceptance of information systems, and design science research in IS. He has published in journals such as *Communications of the ACM, Communications of the AIS, Data Base, European Journal of Information Systems*, *Information & Management*, *Information & Software Technology*, *Information Systems, Information Systems Journal*, *Information Systems Research*, *International Journal of Information Management, Journal of MIS, Journal of the AIS, MIS Quarterly*, and *Omega*.