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How to Improve the Quality of Peer Reviews? Three Suggestions for System-level Changes

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Abstract:

Peer reviewing is critical in the process of legitimizing new scientific knowledge. Yet, concerns about its quality exist, especially if one considers developmental reviewing as an ideal. In this essay, I suggest three ways to improve review quality: provide reviewers with systematic feedback about their performance, reward active and good reviewers, and make reviewers more accountable by revealing their identities to the authors in certain conditions.

Keywords: Scholarly Reviews, Peer Reviews, Developmental Reviewing, Feedback to Reviewers, Rewarding Reviewing, Reviewer Accountability, Reviewer Anonymity, Blinding.

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1 Introduction

Scholarly or scientific peer review is the evaluation of research findings for competence, significance and originality by qualified experts who do research in the same field (Brown, 2004; Benos et al., 2007). Peer review is critical in the process of legitimizing new scientific knowledge and assuring its quality. A piece of research that has not passed scholarly review and has not been published cannot be regarded as scientific since its findings have not been accepted by the scientific community in question and are not trustworthy in that sense.

However, even though peer reviewing is not perfect, nobody has found a better alternative. Benos et al. (2007) summarize its weaknesses such as various biases (status and gender biases and biases because of ideological differences, unconventional ideas, and conflicts of interest), its inability to identify major flaws and scientific misconduct, and delays in the publication process. Yet, scholarly reviewing provides authors with an opportunity to respond to criticism their peers raise before publishing and, consequently, to improve their papers. These reasons alone are sufficient to preserve peer reviewing (Benos et al., 2007).

In the information systems (IS) field, various scholars have raised concern that the quality of reviews has recently been deteriorating (see, e.g., discussion on the AISWorld forum from Fall 2013). Recently, there has also been lively AISWorld discussion on review cycle times in IS. Excessive delays in reviewing are naturally annoying to the authors, but, at the same time, we should keep in mind that thorough reviewing takes its time, especially in fields in which one cannot clearly circumscribe the research problem and intended contribution in a handful of pages (e.g., social sciences; in contrast, compare with the much shorter papers in natural sciences and psychology) (Hayashi & Fujigaki, 1999). Even though psychology is a significant reference field for many research topics in IS such as human-computer interaction and individual use of IT artifacts, IS as a whole is closer to social sciences. Furthermore, the IS field is still relatively young and IS research comprises multiple paradigms, applies a variety of research methods, and is inspired by multiple reference fields. As a consequence, many research papers in IS tend to include a lengthy front-end compared to their results section, which makers reviewing them laborious and, in many respects, challenging. Therefore, I believe that it is much more crucial that reviews are substantively and attitudinally of good quality rather than fast even though review cycle time is also a significant issue.

One reason for the quality problems in reviewing is the increasing number of journal submissions, which leads to a constant shortage of (good) reviewers. If reviews' quality decreases, journal editors may increasingly make type I errors (in which papers of low quality are accepted) or, still more alarmingly, type II errors (in which papers with great potential are rejected) (Straub, 2008).

Therefore scientific communities should always consider how to improve peer reviews. But what is a good review? Carpenter (2009) proposes that a great review "identifies weaknesses and identifies a path or paths to remedy those weaknesses" (p. 193); that is, they are developmental (Saunders, 2005a, 2005b; Lepak, 2009). A developmental review offers high quality content in its evaluating a paper's contribution, theoretical background, research method, argumentation, and presentation. It provides constructive, cordial, supportive critique and includes enough detail and concrete suggestions. Moreover, it is written to both the paper's editors *and* the authors. In this essay, I regard developmental reviews as the ideal review type.

Some authors such as Lee (1995) have published guidelines for reviewers about how to write good reviews. Others have discussed what a developmental review means in particular from reviewers' viewpoint (Saunders 2005a, 2005b; Lepak, 2009). Although definitely useful, they rely on individual reviewers' readiness to develop their reviewing skills and do not imply any system-level changes that might create conditions for better reviews and motivate reviewers to invest more on reviewing.

Other researchers have also made suggestions about how to use IT to support the review process (e.g., Mandaviwalla, Patnayakuni, & Schuff, 2008; Kane & Fichman, 2009; Hardaway & Scamell, 2012). However, when resorting to technology, one should consider the "business" rules of reviewing to be embedded in the technology and the role of technology in the larger system of publishing.

In this essay, I suggest three technology-independent system-level means to improve reviewing's quality: 1) provide systematic feedback to reviewers, 2) reward good reviews, and 3) make reviewers more accountable by revealing their identity to authors in certain conditions. Before discussing these proposals, I introduce some of my personal experiences of reviews as motivation.

2 My Personal Experiences as a Reviewer and as an Author

Since most reviewing is voluntary and not directly rewarded in any way, it may be next to arrogant to criticize its quality. Journal editors in particular often must be happy if they manage to recruit a sufficient number of reasonably qualified reviewers who are ready to do the job. Yet, one should always keep in mind that reviewers act as significant gatekeepers in science (Lyytinen, Baskerville, Iivari, & Te'eni, 2007). Their reports essentially affect not only editorial decisions on the submitted papers but, sometimes, the authors' future and, in the longer run, the direction of the journal will take and the respective field as a whole. Therefore, research communities should not disregard the issue of review quality just for discreetness. Instead, they should subject it to critical scrutiny.

I have not kept any record of my reviewing appointments, but I guess that I have reviewed a few hundreds journal manuscripts during my academic life. I have also served in editorial positions in journals such as *European Journal of Information Systems* (2003-2007), *Information Systems Journal* (1997-2012), *Journal of the Association for Information Systems* (2005-2008), and *MIS Quarterly* (2007-2011) and, in those positions, handled the review processes of many manuscripts. Furthermore, I estimate that I have personal experience of 60-70 journal review processes as an author.

As one might expect, the quality of the reviews I have seen is close to the normal distribution. Most of them have been average: not particularly good, not particularly bad but reasonable; some have been excellent and some weak. It is hard to change this distribution since it is a kind of human invariant typical to the quality of any human activity. Yet, journals could attempt to move the peak of the distribution towards better reviews and reduce its variance especially by eliminating bad reviews.

Bad reviews are reviews that are substantively low quality, indicate excessive bias in their evaluation, exhibit a negative (non-constructive, non-developmental) attitude toward the manuscript, and/or do not have any respect for the author(s).

One reason for bad reviews is reviewers' being incorrectly assigned to a paper. Assigned reviewers may not be knowledgeable enough about the topic of a paper or the research method applied in it, may be positively or negatively biased because of their paradigm and worldview, may have a conflict of interest that compromises their ability to review objectively, or simply may not have interest in the submission or sufficient time to review properly. Journal editors are constantly challenged to avoid such assignment errors.

A second reason is more attitudinal. Some reviewers, possibly in some circumstances, are non-constructive rather than constructive in their reviews. According to my experience, such cases are really rare since I have personally encountered only two. The first one was when I submitted a paper to a premier IS journal in the late 1990s. The paper attempted to understand IS development in terms of the number of inherent dilemmas preliminarily discussed in Iivari (1996). I do not have the review report of this submission archived anymore, but the most memorable remark in it was the comment that the paper looked like it was written after having had a few drinks. I did not find this remark particularly friendly or even funny.

The second case is more recent. I submitted a paper on two strategies of design science research (DSR) (Iivari, 2012) to another top IS journal in 2011. I was inspired to write the paper from Sein, Henfridsson, Purao, Rossi, and Lindgren's (2011) review process in which I served as an associate editor. During this process, I was forced to contemplate the relationship between action design research (Sein et al., 2011) and DSR (Hevner, March, Park, & Ram, 2004), which gradually led me to distinguish the two DSR strategies. The first strategy corresponds to mainstream DSR in which a researcher constructs or builds an IT artifact as a general solution concept to address a class of problem and, possibly, instantiates it to address a specific problem. In the case of the second strategy, a researcher attempts to solve a specific client's problem by building a concrete IT artifact in that specific context and distills and generalizes prescriptive knowledge from that experience so that one can package it into a general solution concept. I used Markus, Majchrzak, and Gasser (2002) and Sein et al. (2011) to exemplify this second DSR strategy.

The paper's first review cycle took almost a year and a half. One of the reviewers did not find any contribution in the distinction between the two DSR strategies and commented that "I really doubt that you are 'introducing' these choice, they have been described previously in papers that you cite, e.g., Markus et al, Sein et al, and Peffers et al". According to my reading, none of these references identify or still less contrast the two DSR strategies. As such, I interpreted that the reviewer was suggesting that any analysis of schools of thought in philosophy, sociology, economics and so on makes no contribution since those schools have already been identified by researchers classified to represent those schools. This reviewer continued:

> *In the major part of the paper you use Markus et al and Sein et al. (2011) as examples to contrast the two "strategies". This is a rather thin basis for you to make a contribution. What does the reader learn from reading your paper, rather than just those two papers - the only ones on which your analysis is based?*

To me, this comment is equally friendly as feedback to a student after an exam that, even though the student was able to write their name correctly, it was not enough to pass the exam.

Furthermore, this same reviewer had difficulties in understanding the distinction between DSR and behavioral science research, which is fairly standard in the DSR literature (e.g., March & Smith, 1995; Hevner et al., 2004). The reviewer questioned "why contrast DSR with behavioral research only? What about economic based research, interpretive research, other kinds of qualitative research, formal proofs, grounded theory, socio-technical, gender-based and any of a number of different paradigms used in IS research?". Yet, the associate editor (AE) handling my submission assured me in their AE report that the reviewer in question was a seasoned expert. Assuming that the AE was right, it seems to me that this reviewer had serious attitudinal problems with my submission.

In conclusion, I think that the two examples illustrate that really bad reviews, which are disrespectful, unfair, and non-constructive, do exist. Even though rare, they may be catastrophic—especially to junior authors. They may weaken their chances to be promoted, destroy their academic careers, and even ruin their lives. In Sections 3, 4, and 5, I suggest three system-level means to improve the reviewing process, means that hopefully are able to reduce weak reviews and eliminate really bad ones or, at least, prevent the same reviewers from repeatedly producing them.

3 Provide Systematic Feedback to Reviewers

I have never encountered a system in which a reviewer was provided with genuine feedback about their performance. Some journals may collect information about the cycle time of reviews in the case of each reviewer for editorial purposes, but I have never received such feedback as a reviewer (e.g., about how I have performed when compared with my peers).

As with any piece of text, the author is not the best person to assess it. I think that it is so also in the case reviews. At least I have found it difficult to evaluate the quality of the reviews I have authored. In particular, it is not easy to know how the review's readers (i.e., the authors of the manuscript and the editor) perceive the review. Therefore, it would be good for reviewers to receive systematic feedback about their reviews.

Currently, the only way to get some feedback about one's review is to compare it with those of other reviewers and with the AE's report[1] if the whole review package is delivered to all reviewers as it is usually done. If one's review is in line with those of other reviewers and if the AE picks up some of your points in their report, one is safe in a way, but it does not necessarily indicate that one is right. Sometimes, the whole review team may be wrong in its evaluation.

Assuming that an ideal review process is developmental, reviewers should also receive genuine feedback from the authors about whether they experienced that the review was substantively of high quality, detailed enough, actionable, cordially expressed, and so on. A short questionnaire could collect such information. Such questionnaires could occur after each review cycle so that it would be as authentic as possible but delivered to the reviewer—if the authors permitted it—only after the case is closed (i.e., when the paper is finally accepted or rejected so that it does not affect the ongoing review process). If preferred so, the feedback could be delivered to reviewers in an aggregated form when they have a certain number of reviews completed (e.g., five). In the case of rejected papers, the feedback could be anonymous.

When the paper is finally accepted or rejected, the AE can also evaluate each reviewer based on their own assessment and on the author's feedback. In particular, if the AE's assessment of a reviewer considerably differs from the author's, the AE should justify the difference. It may well be that the AE's

[1] Although all journals do not distinguish between senior editors and associate editors, for simplicity, I use the term "associate editor" in this essay to refer to those editors who usually recruit reviewers and write the first editorial report based on those reviews and their reading the manuscripts.

evaluations of individual reviewers cannot be delivered at this stage since it might weaken the AE's opportunities to recruit reviewers in the future. However, once reviewers have five or so reviews, the feedback could be delivered to them in an aggregated form, especially if the reviewer has served several AEs.

The system I describe above would make it possible to identify good reviewers—those whose reviews are consistently of high quality and developmental—and to identify those whose reviews have consistently been non-developmental or otherwise of low quality. The former can be rewarded for their good performance as explained below. In the case of latter reviewers, good reason would exist to critically look at their reviews and consider measures to correct their reviewing performance.

4 Reward Reviewers

Scholarly reviewing is voluntary work that is not directly paid for or compensated in any way. It is absolutely necessary for the scientific community to work, but, according to my experience, active and great reviewing is not much counted when one's academic performance is assessed. I cannot imagine this situation's changing in the near future even in the case of open access journals, although they may change the business logic of scholarly publishing.

Of course, reviewing helps junior researches to understand the publication game and to enter the scientific community (Lyytinen et al., 2007). A reviewer may also receive some valuable intellectual capital earlier than others, but the reviewer must be cautious with it so that, for example, someone cannot claim that they stole an idea from some rejected manuscript.

When great reviewers advance to an editorial position, they obtain visibility, recognition, and influence in their research community and may be rewarded in that way. Yet, there are relatively few who are "promoted" to such positions, at least in prestigious journals. Furthermore, since journals do not systematically evaluate reviewers' performance, one usually selects new editors based on other merits such as their number of publications in elite journals and personal contacts rather than on great reviewing.

As a whole, reviewing is invisible work conducted largely for altruistic reasons. At the same time, most reviewers work under an increased pressure to publish, which leads to a dilemma between the altruistic motivation to serve the community and the egoistic motivation to publish more and to satisfy (or exceed) the expectations of their employer. I am afraid of that the "publish or perish" push forces most potential reviewers to emphasize the egoistic desire to advance their career rather than to altruistically serve the research community.

In this situation, it is becoming increasingly important that good reviewing is concretely rewarded so that the egoistic motivation to publish and the altruistic desire to serve the community can be better aligned. One option for journals to concretely reward active and good reviewers is to introduce a new paper category ("reviewer forum") to provide great reviewers with a "special" opportunity to publish. Of course, this should be complemented with a fair system in choosing the great reviewers. To underline it, such forum papers could also be supplemented with a summary of the author's contribution as a reviewer in the journal.

These reviewer forum contributions do not necessarily compromise a journal's scientific quality. They can be subjected to peer review just as other paper but with more modest quality expectations than full research papers. Most IS journals already publish editorials, research notes, opinion papers, and so on with their own quality requirements, so why not reviewer forum papers?

I suppose that the two system-level changes I discuss above are fairly easy to adopt and implement since I cannot see any reason why reviewers, editors, or authors would resist them. The third proposal (see Section 5), on the contrary, is more controversial, although researchers have questioned reviewer anonymity for at least 30 years (Armstrong, 1982).

5 Make Reviewers Accountable by Revealing their Identity to Authors

Everybody who has followed reader comments in the context of online newspapers, for example, has evidenced that the author anonymity is a significant reason for uncivilized and offensive comments: if the commentators are anonymous, the quality of comments may be quite low; when commentators use their real names, comments are often more carefully considered and more constructive. Scholarly research has also confirmed this observation (Santana, 2014). Anonymity leads to lower self-control and lower discourse quality (Ruesch & Märker, 2012).

Following that analogy, one can seriously question whether reviewer anonymity is the major reason for low-quality reviews and, in particular, for non-developmental reviews. Yet, most premier journals regard double-blind reviewing in which reviewers do not know the authors and the authors do not know the reviewers as the preferred form of peer reviewing. Many believe the two-way anonymity to increase objectivity of the process and reduce biases (Hillman & Rynes, 2007).

But is it really so? Regehr and Bordage (2006) identify four alternatives to double-blind reviews: 1) single-blind in which the authors are revealed but reviewers concealed, 2) single-blind reversed in which the authors are concealed but reviewers revealed, 3) optional single-blind in which the authors are revealed and reviewers are free to sign their reviews, 4) and open review, which is entirely open. This classification misses 5) optional single-blind reversed review in which the authors are concealed but reviewers are free to sign their reviews.

Although the decision between single-blind review and double-blind review is not any big issue in top IS journals, it is interesting to note that the evidence supporting the latter is not so compelling. While Hillman and Rynes (2007) claim that existing research suggests that revealing authors' identity influences the acceptance so that papers from prestigious universities are more easily accepted (particularly if reviewers also are from prestigious universities), Benos et al. (2006) conclude that the empirical evidence in this respect is conflicting. Nevertheless, they note that the logic behind concealing authors' identity is sound. Thus, it seems that the decision to adopt double-blind review rather than single-double review has taken place based on the logic rather than on empirical evidence. So, the practice not to reveal authors' identity is just a precaution against a possible bias.

Of course, keeping authors' identity a secret with double-blind reviews is challenging if a reviewer really wants to know it. Assuming that submissions properly conceal their authors, I advise reviewers that they should not make any effort to find out who the authors are. It makes fair and objective reviewing easier.

In the case of revealing reviewers' identities to the authors, the evidence is also conflicting. On the one hand, researchers have argued that, if reviewers' identities are revealed to authors, they will likely provide more objective, fair and developmental reviews since they must be prepared to defend their reviews publicly (Hillman & Rynes, 2007). On the other hand, Benos et al. (2006) conclude that revealing reviewers' identities has no beneficial effects in terms of substantive quality or strength of their reviews but that authors may feel that they receive more courteous and constructive feedback. So, if there is a risk of bad reviews because of reviewer anonymity as Armstrong (1982) and Hillman and Rynes (2007), for example, suggest, why don't we attempt to safeguard against it as a precaution?

Benos et al. (2006) point out that, if reviewers' identities are not concealed, reviewers more likely decline to review. They refer to Godlee, Gale, and Martyn's (1998) experimental study in which 50 percent of the reviewers declined to participate when asked to sign their reviews, while 46 percent declined in groups in which no signing was requested. These findings do not indicate any significant decrease in reviewers' readiness to review when requested to sign their names. Yet, van Rooyen, Godlee, Evan, Black, and Smith (1999) report a 12 percent decrease in the readiness to review when reviewers were requested to sign compared with the situation in which their identities were blinded to authors.

Regehr and Bordage (2006) surveyed reviewing preferences among 838 authors and reviewers of *Medical Education*. They found that about 50 percent of reviewers resisted the idea that their names be revealed to the authors. The remaining 50 percent either preferred that they were revealed (roughly 22%) or were indifferent (roughly 28%). Those who resisted mentioned several reasons for their position: to facilitate honest reviewing, to avoid "bad blood" and tensions among colleagues and friends, including the danger that critically reviewed authors may interfere with the reviewers' career development and grant applications,.

The risk that a critical report—even when developmental—may negatively influence a reviewer's future career development and grant applications may be a valid concern, especially among junior reviewers (e.g., who do not have tenure). Yet, I have problems understanding why a senior reviewer (e.g., with tenured professorship) would not have the courage to write honest criticism even when their name is revealed to authors. Even when highly critical, a review can be written in a polite and developmental way.

Personally, I have never had any problem with the idea that my name would be revealed when I have reviewed a paper. If I have attempted to be as honest, fair, objective, and constructive as possible as a reviewer, why should I conceal my identity from the authors? It may be partly because of my cultural background, which does not appreciate a person's telling one story in the front of someone and a totally different story behind their back.

Those scholars who agree should consider signing their reviews. Hillman and Rynes (2007) refer to McCook's (2006, p. 3) quote of the editor of *Journal of the American Medical Association* as saying, "I've always signed every review I've ever done, because I know if I sign something, I'm more accountable. Juries are not anonymous, neither are people who write letters to the editor, so why are peer reviewers?". I have never done it—except very recently—due to the double-blind review practice of most journals. When signing my reviews, I discovered that some journals prefer to maintain the double-blind process and do not wish reviewers to violate it.

In hindsight, not to sign my reviews may have been the biggest mistake during my academic career. Perhaps I would have more academic friends if I had done so. Actually, I have an example in which concealing reviewers' identities likely caused "bad blood": it concerns a highly cited paper with well above 1000 ISI Web of Science citations. For some reason, I have sensed that one of its authors has always been exceptionally reserved in my company when we have occasionally met in different roles during the years. Later, I got to know that this seminal paper had enormous difficulties to get accepted. When I heard about that, it occurred to me that this specific author may have assumed that I have served as a reviewer of the paper. This paper refers to a few of my publications. It may well be that one of its reviewers has suggested these papers in the reviewer's review report. If so, this reviewer must have been quite knowledgeable of my work at that time. But that reviewer was not me.

In conclusion, I am strongly in favor of single-blind reversed review in which the authors are concealed but reviewers are revealed at least optionally. This optional form allows junior reviewers to choose whether their names are revealed or not, but the names of more senior reviewers are revealed when the review process is closed.

Some IS journals such as *Journal of the AIS* and *MIS Quarterly* have applied a practice in which reviewers' name are optionally revealed in the accepted paper if the reviewers are ready for that. However, it seems that both journals have abandoned this practice. It may not be any big loss since revealing an accepted paper's reviewers is not usually a big problem if a reviewer has not been highly critical in its case or if the paper does not turn out to be a type I error. Rejected papers are much more problematic, especially in the case of type II errors when it later turns out that an excellent paper has been rejected.

6 Conclusions

In this essay, I suggest three system-level means to promote better reviewing and to complement individual-level guidelines such as those in Lee (1995). These ideas are not necessarily new. When considering the keywords for this paper, I discovered that American Psychologist (Vol. 51, No. 11) had a number of comments on reviewing that were inspired by Epstein's (1995) earlier comments. He makes several recommendations to improve the review process, some of which are more radical than the suggestions in this paper (such as providing a standard appeal procedure for authors who believe that the review process has not been fair). He proposes two changes that can be implemented immediately: signing reviews and providing forms for authors to provide feedback to reviewers. These suggestions are in line with the first and third suggestions I outline in this essay even though they differ somewhat.

In the case of the first proposal, I would expect that most reviewers would welcome systematic feedback about their reviews. It does not require much additional work. If there is a short questionnaire for authors to collect that information after each review cycle, they do not have much choice but to answer. The challenge is to convince them that it is confidential during the ongoing review process so that it will not interfere with it and that they can decide how it is delivered (non-anonymously or anonymously immediately after the process is over or as a part of aggregated feedback package after the review has a sufficient number of reviews).

I would expect that rewarding active and good reviewers would also be well received at least by such reviewers. A critical question is how the editors-in-chief take this proposal. As I argue above, rewarding reviewers via forum papers does not necessarily lower journals' quality when compared with issues and opinions, research essays, research notes, and similar contributions. Furthermore, if the prospect of such a reward motivates reviewers to do better job, we can expect higher-quality accepted papers.

Generally, I suppose that the suggestion to reveal reviewers' identities is the most difficult to accept since reviewer anonymity may be some sort of taboo. I understand that there is resistance to the idea. Yet, it is encouraging that Regehr and Bordage (2004) found that about 50 percent of senior reviewers did not totally exclude it. Furthermore, I guess that part of the resistance is an outcome of the existing practice. For example, if the academic community regards revealing reviewers' identity as a sign of seniority and potential for editorial positions, the situation may change.

My suggestions are fairly independent of each other, so they could be adopted individually in any order or all together even though I believe that together they are most effective in improving the quality of peer reviews. As Fine (1996) notes, awareness among reviewers that their reviews will be formally evaluated by a manuscript authors and the respective editors may increase reviewers' sense of accountability, although—differing from Fine (1996)—I believe that reviewers' non-anonymity is key for this accountability. Concretely rewarding good reviewing performance obviously increases reviewers' motivation to perform well, and, in that case, the opportunity to hide behind anonymity would not be as significant as with sloppy reviewing.

These three suggestions are based on my own experiences as an author, reviewer, and editor and especially on two cases of quite bad reviews I have encountered. Yet, the two cases ended fairly happily from my viewpoint. Soon after *ISJ* rejected my submission, I was invited to join its editorial board. I have never asked David Avison and Guy Fitzgerald, the editors-in-chief of *ISJ* at that time, whether this unfortunate review was the reason for the invitation. I suppose so. In any case, it was a great honor: it was my first editorial board position in a premier IS journal, and it perhaps opened the door for additional ones. So, if one receives a really bad review, I suggest one not get depressed. Perhaps, as an apology, one will be invited to join the editorial board of the journal.

In the second case, I was provided an opportunity to revise. I did it just to provide straightforward feedback to the reviewer in question in my response letter. I did not expect anything from the second review after my response. I do not know if the reviewers remained the same, but I suppose that some of them were new. The quality of these reviews was acceptable even though my manuscript was rejected. Yet, I was provided an opportunity to submit a shortened version as an "issues and opinions" paper. I did so and the manuscript was eventually accepted (Iivari, 2015). I suppose that it was a sort of reward for my previous services to *EJIS*, a kind of "reviewer forum" paper even though not categorized in that way, which is fine by me.

During the former case, I was in the middle of my career and one paper's acceptance or rejection did not make any big difference to me. During the latter case, I had just retired and the rejection had still less impact on my career, although one always hopes that one's manages to publish on a good forum so that the ideas receive wider publicity. At that stage, I did not see any reason to silently tolerate a bad review. I made my perception clear in my response letter.

A sad conclusion of the latter story is that I sensed the editors involved were quite defensive when I criticized the quality of one particular review. Rather than admitting the situation, they simply denied it. The official view throughout the review process was that it was a review of normal quality. My criticism of the review's quality was against the implicit rules of conduct. Authors are not supposed to do so.

Generalizing from my experience, unfortunately, one cannot trust that the editors are always ready to eliminate really bad reviews. Therefore, journals should have a system that firstly encourages reviewers to write good and developmental reviews, makes it possible to identify those who repeatedly continue to write weak ones, and directs journals into corrective actions when a reviewer continuously keeps on writing such reviews. I believe that the three system-level suggestions that I outline in this paper can improve the situation.

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