August 1999

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Ron Weber

University of Queensland, weber@commerce.uq.edu.au

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Recommended Citation

Weber, Ron (1999) "The Journal Review Process: A Manifesto for Change," *Communications of the Association for Information Systems*: Vol. 2 , Article 12.

DOI: 10.17705/1CAIS.00212

Available at: <https://aisel.aisnet.org/cais/vol2/iss1/12>

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THE JOURNAL REVIEW PROCESS: A MANIFESTO FOR CHANGE

Ron Weber
Department of Commerce
The University of Queensland
weber@commerce.uq.edu.au

OPINION

NOTE: A LETTER TO THE EDITOR FOLLOWS PAGE 22

[Click here for hyperlink to the Letters Page](#)

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Ron Weber
Department of Commerce
The University of Queensland
weber@commerce.uq.edu.au

ABSTRACT

For many academics within the information systems discipline, the merits of the current processes we use to review papers to evaluate whether they should be published in journals is a contentious issue. Allegations are often made that reviews are not timely, that their quality is low, that they are not supportive and affirming of authors, and that they reflect the prejudices of an “elite” who control the journals. Whether we believe these allegations have substance will depend on our own experiences with journals and our knowledge of the experiences of colleagues. Based on my own experiences, I believe the allegations have some foundation. Accordingly, in this paper I present a manifesto for changing and hopefully improving the journal review process. My manifesto has four major recommendations:

(1) remove the blind review process and make the names of authors, reviewers, and associate editors public;

(2) indicate the names of reviewers and editors on the published paper, along with their final recommendations, and the number of review cycles that the paper has undergone;

(3) maintain a public Web archive of papers under review to enable colleagues other than the reviewers to comment on papers;

(4) maintain a public Web archive of rejected papers along with the reviewers' and editors' reports.

My goals are to make the review process more transparent, to make stakeholders more accountable for their actions, to mitigate the effect of biases and prejudices, to make the review process more affirming and supportive, and to reduce the likelihood of high-quality papers being wrongly rejected and low-quality papers being accepted.

Keywords: IS journals, reviewing, publication process, accountability

I. INTRODUCTION

Within academe, few issues evoke as much heated discourse as the journal review process. Witness the exchange that occurred among information systems (IS) scholars on the ISWorld listserver during April-May 1999 [ISWorld 1999]. The initial messages that dealt with journal review times quickly elicited much broader comments on other major failings that various IS scholars perceive exist with the journal review process – failings that seemingly need urgent rectification.

It is easy to understand why emotions run high when the journal review process is discussed. In a nutshell, the stakes are high. Publications are a primary means of establishing scholarly reputation. Reputation, in turn, impacts decisions such as promotion and tenure and the compensation and resources that a scholar can command. Especially among younger scholars, careers (and sometimes lives) can be made or broken by the acceptance or rejection of a single paper, for a tenure decision may hinge on the outcome. Reputation also affects the power that scholars can exercise within their discipline, and for some,

power gives a giddy charge. Furthermore, egos are at risk. For young scholars, constant rejection leaves them disillusioned and disheartened, especially if they perceive the review process is erratic and destructive. Some leave the academic game after investing much of their lives in equipping themselves to play it. Even for senior scholars, the vagaries of the review process take their toll. For them, research is often done when a few precious hours can be salvaged from a deluge of other responsibilities. Rejection hits hard, therefore, when the opportunity costs associated with crafting the paper are high.

In Section III of this paper, I present a manifesto for changing the journal review process, at least as it is practiced in the IS discipline. Let me be forthright as to my motivation. As it currently stands, I believe the review process is flawed. The outcome is often unpredictable and harmful, and the costs are surely high. Thus, I do not subscribe to the view that complaints about the journal review process reflect inappropriate and self-interested outcries by a minority of scholars who have been disadvantaged by the process (sometimes rightly, it is alleged!). True, some criticisms are misplaced. Nonetheless, I believe that many of the current concerns are substantive.

My manifesto is based on three primary types of experience.

- First, over a fair number of years I had review and, sometimes, senior editorial responsibilities for major journals in both the accounting and IS disciplines.
- Second, like most scholars, I had my share of frustrating, heartbreaking rejections.
- Third, during recent years, I spent a fair amount of time counseling young colleagues in light of unfavorable reviews they received. Sometimes the reasons for the rejection are clear and well-justified. Often, however, I can provide no insights. The only consolation I can offer my colleagues is that the review process and the editorial process for their paper reflect a shoddy job and they should proceed, therefore, to submit their paper elsewhere.

In the sections below, I first characterize the journal review process as a decision-making problem. Next, I describe my change manifesto and some of its implications. Finally, I give some summary comments and conclusions.

At the outset I want to underscore four matters. First, I have had the pleasure and privilege of working with many outstanding editors, associate editors, and reviewers – colleagues who are role models in terms of their selfless, anonymous contributions to other colleagues and the IS discipline as a whole. In no sense is my manifesto a criticism or a condemnation of all review work with which I have been associated. Indeed, I have learned much from observing the work of outstanding colleagues as they engaged in the review process. Moreover, with shame, I also recognize that at times I have been guilty of some of the failings that I outline below. Second, in my opinion the quality of the review process within the IS discipline improved considerably over the last 25 years (the period during which I have been both an observer and a participant). We are not in crisis, but we can still do better [see also Zmud 1998]. Third, while I focus on the IS discipline, my comments are not confined to the review processes used within it. I mix with colleagues from many different disciplines. The matters I canvass below appear to be generic. Fourth, I wrote this commentary as a polemic. My purpose is to engender discussion and debate. I hope that any offence I might give as a result of my comments will be tempered by the recognition that I seek to articulate matters that will provide a basis for any discernment that occurs on the merits of the current journal review process. Ultimately my hope is that the outcome of evaluating the manifesto and perhaps implementing the recommended changes will be positive and enriching rather than hurtful and destructive.

II. DECISION RULES, DECISION ERRORS, AND LOSS ASYMMETRIES

The journal review process is sometimes characterized as a problem of classic hypothesis testing. This characterization is useful because it helps to

structure the decision process surrounding the evaluation of manuscripts submitted for publication. More importantly, however, I believe it is useful as a way of teasing out some of the assumptions, values, and biases that editors and reviewers bring to the review process.

In the context of a hypothesis testing problem, we can frame the review process in two ways. The first is to adopt a world view (Weltanschauung) whose null hypothesis is in the following form:

H_0 : manuscript is *not* publishable

To be more precise, we should adopt a Bayesian perspective whereby initially we ascribe some probability to the state that a submitted manuscript is *not* publishable. This probability could be based, for example, on the average over some period of time of the number of published papers to submitted papers. Assuming this number is less than 0.5, members of the review team primarily will hold a prior expectation that a submitted manuscript is not publishable. We should then see the review process as undertaking belief revision to arrive at a posterior probability, which in turn leads ultimately to a final acceptance or rejection decision. Belief revision sometimes occurs over multiple review cycles as reviewers suggest changes to a manuscript and then evaluate how well the author implemented their suggestions. In the interests of simplicity, however, for the most part I disregarded Bayesian considerations in my analysis below.

Table 1 shows the outcome table that the editors and reviewers face under the first world view.

Table 1: Outcome Table Under World View That Manuscript is Not Publishable.

Choice	“True” State of the Paper	
	Null is True: Paper Should Not Be Published	Null is False: Paper Should be Published
Rejection Decision	Correct Decision	Type-II Error
Acceptance Decision	Type-I Error	Correct Decision

For many of us, our education in statistics leaves us with a predilection to control Type-I errors primarily and Type-II errors secondarily. As a result, if we frame the world as one where we assume the manuscripts a journal receives are not publishable, our focus primarily will be to avoid accepting papers where the null is true. We will be concerned to a lesser extent about mistakes whereby we reject papers that should be published.

The second way we can frame the review process is to adopt a world view whose null hypothesis is in the following form:

H_0 : manuscript is publishable

Table 2 shows the outcome table that the editors and reviewers face under the second world view.

Table 2. Outcome Table Under World View That Manuscript is Publishable.

Choice	“True” State of the Paper	
	Null is True: Paper Should Be Published	Null is False: Paper Should Not Be Published
Acceptance Decision	Correct Decision	Type-II Error
Rejection Decision	Type-I Error	Correct Decision

Note, now, the change in the nature of Type-I and Type-II errors that occurred. If our primary focus remains the control of Type-I errors, under this world view we seek first to avoid the rejection of publishable papers. Our secondary concern is the mistake of accepting papers that should not be published.

At first glance, this change of world view might seem trivial. I believe, however, that it has some profound and subtle implications for how we undertake the review process. If our base position is that of Table 1, that is, papers submitted to a journal are unlikely to be acceptable for publication, we will require evidence to shift us from this position. Given the presence of psychological phenomena such as anchoring and adjustment, we can predict with some confidence that eliciting a shift is difficult. Moreover, given the high level of

skepticism with which many academics view any form of evidence, belief revision is especially difficult to evoke. The outcome is that Type-II errors (rejecting publishable papers) will occur more frequently than Type-I errors (accepting papers that are not publishable). My experience is that many editors and reviewers are most concerned about this latter type of error. The reason, I suspect, is that Type-I errors become public knowledge (the paper is published), and they fear the consequences for their reputations or the journal's reputation. Type-II errors, however, remain hidden. The author, and more generally the discipline, bear the costs when Type-II errors are made, but the editors, reviewers, and journal are protected.

On the other hand, if our base position is that of Table 2, that is, papers submitted to a journal are likely to be acceptable for publication, we require evidence to convince us otherwise. Once more, psychological phenomena such as anchoring and adjustment take effect, and again Type-II errors are likely to occur more frequently than Type-I errors. Under this world view, however, Type-II errors involve accepting papers that should not be published. The editors, reviewers, and journal bear the costs in terms of damage to reputation. The authors and discipline benefit, however, because good papers are less likely to be rejected.

Whatever our world view, we might argue that ideally editors and reviewers should control differentially for Type-I and Type-II errors as a function of the full costs associated with making each type of error. In this regard, some scholars argue that the costs of publishing low-quality papers are higher than the costs of not publishing high-quality papers. They point to such factors as (a) the possibility of scholars, practitioners, politicians, etc. relying on ill-founded results and therefore making poor resource-allocation decisions, and (b) the costs associated with information overload and the need therefore to publish only the highest-quality work. On the other hand, some scholars argue that the costs of not publishing high-quality papers are higher than the costs of publishing low-quality papers. They point to such factors as (a) the inhibiting effects on progress within the discipline when good papers are not published or their

publication is delayed until the authors find a “home” for their paper (often with a smaller audience), and (b) the difficulties in sometimes predicting *a priori* the worth of a piece of research.

In the IS discipline, for three reasons I believe the review process is dominated by a concern about publishing low-quality papers. First, in my view, we have a “rejection culture,” which may reflect, among other things, that the discipline is still young and paradigmatically immature. Whatever the reason, papers are more likely to be rejected than accepted when they are submitted to journals. As a result, young scholars are acculturated into a discipline where rejection is the norm. Second, scholars who control the journals are often risk-averse. Whatever the reason, they do not want to be associated with papers that ultimately are judged to be low-quality. Third, journals are constrained by economic factors. Page space and review time are scarce resources, and they must be rationed stringently. A rejection mindset emerges naturally as a means of accommodating these economic constraints.

The change manifesto I present below is intended to balance controls over Type-I and Type-II errors. While it guards against low-quality papers being accepted, it also puts in place measures that reduce the likelihood of good papers being rejected. As I argued above, in my opinion, current review processes are weighted too heavily in favor of reducing the likelihood of accepting low-quality papers. I see no reason to sustain this position unless we are confident that the costs associated with publishing low-quality papers exceed the costs of not publishing high-quality papers. In this regard, we should be mindful of the fact that most published papers are read by only a small number of scholars, and few are cited more than once [Denning 1997]. Are the costs of publishing a low-quality paper likely to be high, therefore?

Moreover, many scholars I know prefer that journals take risks and that authors be given the benefit of the doubt when the decision on whether to publish their paper is not clear-cut. They accept that high-risk, controversial papers occasionally provide the foundation for significant breakthroughs in a discipline. They accept also, however, that many ultimately are judged as insignificant,

misguided, or low-quality research. Indeed, in one discipline with which I am familiar, the most cited paper in the entire history of the discipline was *rejected* by one of the major journals in the discipline. Ironically, the journal is often remembered not for the high-quality papers it published nor for the low-quality papers it published. Instead, it is remembered for the fundamental mistake its editors and reviewers made in assessing this paper.

Economic constraints are also diminishing to some extent. For example, electronic journals mitigate page-space difficulties. Editors can choose to publish more papers and longer papers. Some journals are also taking steps to make more effective use of reviewer time by giving editors the prerogative to make decisions about the disposition of a paper before it enters the review process. For example, editors may return a paper quickly to an author if it clearly fails to meet the quality standards of the journal.

Again, my manifesto below is based on my belief that, among other things, we need to re-consider the relative costs of accepting low-quality papers versus rejecting high-quality papers as a basis for restructuring our review processes.

III. THE CHANGE MANIFESTO

Here, then, is my change manifesto. It contains four fundamental recommendations. Each is designed to reduce the level of opacity that currently surrounds the review process and instead to make it more transparent. Each is also designed to guard against errors in the review process that lead to wrong acceptance of low-quality papers or wrong rejection of high-quality papers. Note, in making my recommendations I did not consider the question of whether the recommendations are likely to lead to more papers being published or a change in the type of paper being published (assuming the number of papers published remains constant). The answer to this question is clearly important because some consequences may be substantive – e.g., scholars face increased levels of information overload. Instead, my focus is to improve the quality of the review

process and, in particular, to mitigate the effects of certain factors that I believe stultify innovation within the IS discipline.

CHANGE 1: NO BLIND REVIEWS

All journals with which I am familiar use a blind review process. When the reviewers of a paper know the authors but the authors do not know the reviewers, a single-blind review process is in force. When the reviewers do not know the authors, and *vice versa*, a double-blind review process is in force.

The first and most-telling change that I would make to the review process is to terminate blind reviews. Blind reviews are an anachronism. Elsewhere within many societies, we increasingly accept the idea that individuals have a right to access full information about themselves, such as the identity of individuals who make evaluative comments about them. The journal review process, however, remains wedded to the past.

When I suggested the abolition of the single-blind and double-blind review processes to my colleagues on editorial boards, some argued vociferously that undesirable outcomes will occur. On the one hand, if reviewers know the author, either consciously or subconsciously they may let personal prejudices for or against the author or his/her work color their reviews. Moreover, in the case of well-known authors, reviewers may fall victim to a halo effect. In other words, they will allow their reviews to be shaped more by the author's reputation than the quality of the paper they are evaluating.

On the other hand, if authors know the reviewers of their papers, my colleagues argue that reviewers are exposed to possible retribution (even physical harm) from authors who dislike the outcome of a review process. If the author is a powerful figure within the discipline, editors and associate editors might even have difficulty finding a colleague who is willing to review the author's paper. The potential costs associated with recommending rejection for a paper written by a high-status colleague may be perceived to be too high.

If reviewer identities are known, my colleagues also argue that reviewers may be unwilling to be forthright in their comments because they fear the impact

on their current or future relationships with the author. Perhaps worse, reviewers and authors might begin to trade favors. Reviewers might explicitly or implicitly convey to authors that they expect favorable treatment of their own papers when the authors in turn become reviewers. When journals ask authors to suggest the names of possible reviewers, the problem may be that authors might deliberately select reviewers who are likely to be positively disposed to their papers because of their own past treatment of the reviewers' papers.

I am not persuaded by these concerns. They might have been valid in the past when information flows were costly and sparse and events within the discipline, therefore, were somewhat opaque. Individuals might have been better placed, therefore, to take advantage of power asymmetries or to engage in tit-for-tat trading of favors. In an information age, however, few within a discipline can escape careful scrutiny by their colleagues. With the Internet, for example, news about one's actions can disperse quickly and widely. Similarly, electronic trails of one's actions can be subjected to computer-based pattern analysis by independent parties. Improprieties relating to review decisions may have a high risk of detection (see my comments below). The costs arising from unprofessional behavior, therefore, can be high.

In addition, in the case of authors, my experience is that the blind-review process often does not work. It fails to protect because identity can be guessed with high probability based on the nature of the research or the references in the paper or the bibliography, or because the author presented the paper previously at workshops or conferences. Indeed, some authors deliberately try to signal their identity in these ways, perhaps in the belief that they have sufficient status within the discipline to influence the review outcome favorably.

My experience is that the blind review process sometimes evokes a number of undesirable behaviors on the part of reviewers and associate editors – behaviors that undermine long-run progress within the IS discipline. These behaviors arise because reviewers' and associate editors' accountability for their actions is limited – in the case of reviewers to the associate editor and the editor, and in the case of associate editors to the reviewers and the editor. Both

reviewers and associate editors work in the knowledge that they are unlikely to have to face the author at some stage and be called upon to justify and defend their decision. True, authors might appeal a review outcome, but editors have incentives *not* to overturn reviewers' and associate editors' decisions, except perhaps where they perceive gross errors of judgement have occurred. Otherwise, they will undermine their own relationships with reviewers and associate editors. The matter finishes, therefore, if the appeal is unsuccessful. In addition, for both ethical reasons and perhaps legal reasons, editors, associate editors, and reviewers are unlikely to speak publicly about one another's performance. In the case of a problematical reviewer, the likely result is that the editor and associate editor will cease to use the reviewer for review work, which some reviewers will see as a positive outcome anyway. Similarly, in the case of problematical associate editors, the likely result is that an editor will cease to call upon them for assistance.

In short, I argue the blind review system reduces the costs to reviewers and associate editors associated with their undertaking shoddy work. As a result, my experience is that the following types of outcomes sometimes occur:

1. Reviewers and associate editors become risk averse. They perceive that the costs of their making the mistake of accepting a poor-quality paper are higher than the costs of their making the mistake of rejecting a high-quality paper. In the former case, the mistake becomes public because the journal's readers perceive that a poor-quality paper was published. As editors become aware of this fact, they are likely to return to the review process and to evaluate the work of the reviewers and associate editor who recommended the paper be accepted. The "damage" is more salient for the editor because the mistake is public, and therefore I argue they are likely to mark down the reviewers' and associate editor's reputations more than the case where they perceive the reviewers and associate editor have been harsh with a potentially publishable paper. The reviewers' and associate editor's reputation will be damaged, therefore, if only in the eyes of the editor.

2. Reviews are not positive, affirming, and constructive. Instead, they are harsh and sometimes punctuated by cavalier, gratuitous comments. Because the reviewers' and associate editor's identities have been protected, they can make comments in the knowledge that the author has little recourse. Indeed, with some reviewers and associate editors, they make comments in their reports that I know they would not be prepared to make face-to-face with the author.

3. Reviews are tardy. Because authors do not know the identity of reviewers and associate editors, they have no means of exerting direct pressure on reviewers when reviews are late. They can complain to editors, but some authors, especially junior scholars, fear their complaints might prejudice the outcome with their paper. Reviewers answer only to the editor and associate editor, and associate editors answer only to the editor. Neither reviewers nor associate editors feel the keen frustration of authors who suffer from tardy reviews. Ironically, these same tardy reviewers and associate editors often are the first to complain when their own papers are not treated expeditiously.

4. Reviewers and associate editors overcommit themselves. Many of us find it difficult to say "no" when asked to join an editorial board or to undertake yet another review. In the former case, membership of the editorial board enhances our vitae and our standing within the discipline. In the latter case, we may be reluctant to turn down a request from a senior colleague. The blind-review process enables non-performance by those who overcommit themselves to be masked. Reputation enhancement by some is paid for by authors who suffer from tardy, hurried, or dismissive reviews. Moreover, progress in the discipline is undermined because review work tends to be concentrated among a relatively small cohort of scholars. Scholars outside the cohort often find it difficult to avail themselves of the developmental opportunities that come from undertaking review work, in spite of their offers to assist.

By removing the double-blind review process, all stakeholders in the review process are exposed and accountable. It is less likely that errors will occur, and it will be more difficult to perpetrate injustices. Review work will become an even more serious affair.

CHANGE 2: REVIEWER NAMES, EDITOR NAMES, AND REVIEW ITERATION TIMES APPEAR ON PUBLISHED MANUSCRIPTS

The second change in my manifesto is to publish the names of the editor (if a journal has more than one), the associate editor, and the reviewers with their final review recommendations when a manuscript is published in a journal. I would also publish (a) the number of review iterations associated with the paper and the timing of these iterations, and (b) a URL for a Web page containing the reports of the editors and reviewers so that interested colleagues could examine the contents of the reviews.

My motivation for this change is twofold. First, it recognizes and affirms the substantial amount of work that editors and reviewers often perform in assisting authors to improve their papers to the point where they are publishable. If editors and reviewers frequently have their names associated with high-quality published papers, their reputations will grow. Moreover, if the reports frequently manifest that they contribute significantly in a selfless way, their reputations will grow.

Second, this change provides accountability in a public way for published manuscripts. All stakeholders in the review process know that the journal readership will hold them responsible to varying degrees for the quality of published papers. Thus, some of the concerns that arise with the abolition of the blind-review process should be mitigated. For example, recall that one concern is that reviewers and associate editors would be reluctant to recommend rejection of papers authored by senior colleagues. If reviewers and associate editors compromise their evaluations and recommend publication of a poor-

quality paper by a senior colleague, however, they will undermine their own reputations because their names become associated with the paper.

Similarly, to the extent that tit-for-tat trading of favors begins to occur among a cohort of colleagues, their reputations will suffer. When the review process is transparent, potentially these sorts of patterns can be detected. Indeed, I predict that some scholars will have sufficient incentives to use their data mining skills to study various types of associations between authors, reviewers, and associate editors (akin to the citation analysis studies that are now undertaken). One of their goals will be to determine whether regularities can be detected that manifest potential improprieties in the review process. To the extent “pathological” regularities can be articulated by such research, high-quality journals might even put in place monitoring mechanisms to detect them.

CHANGE 3: JOURNALS MAINTAIN A PUBLIC WEB ARCHIVE OF MANUSCRIPTS UNDER REVIEW

The third change in my manifesto is to have journals maintain a public Web archive of manuscripts under review. A condition of submitting to the journal would be that authors agree to their manuscripts being posted to this archive immediately upon entering the review process. Furthermore, the journal should establish a facility to allow anyone to comment on the manuscript. These other “review” comments should be stored publicly with the manuscript. The names of those who made the comments also must be publicly available. Once more, everyone must be accountable for their comments.

My motivation for this change is twofold. First, potentially it enhances the quality of the review process. The editor, associate editor, and reviewers who have formal responsibility for reviewing the manuscript can access any other comments made on the manuscript. They can then take these comments into account as they evaluate the manuscript to reach their own judgements on the quality of the manuscript. With additional information, the risks of accepting low-quality manuscripts or rejecting high-quality manuscripts might be reduced.

Nonetheless, two types of problems arise when these “other comments” are publicly available to the formal review team. The first is that the comments may not have been made by colleagues who are independent of the author. The review team may be swayed unduly by comments that are biased purposely either toward or against the author. Thus, the independence of those who comment on the paper will need to be evaluated carefully.

The second problem is that the identity of those who comment on the paper may influence the formal review team’s judgements inappropriately. For example, a reviewer may be averse to reaching a judgement that is contrary to that given by a high-profile scholar who comments on the paper. Editors will also have to be prepared for a greater level of disputation with authors when the formal review team’s judgement is at odds with the comments posted by other colleagues to the Web site.

The second motivation for having a public archive of papers under review is that it may reduce the problem of authors submitting their papers so they are under review by multiple journals at the same time. Within the IS discipline, journals usually have a policy of asking authors upon submission of their papers for an assurance that their papers are not currently under review elsewhere. The review process is too costly for most journals to be willing to embark upon it unless they have first right of publication in the event the paper is accepted.

Moreover, the review process should not be used as a means of refining working papers (see my comments below), which is likely to occur if authors can submit to multiple journals at the same time. Given the vagaries of the review process, they might have an expectation that at least one journal is likely to accept their paper eventually. In the event their paper is rejected by all journals to which they have submitted it, authors will still have substantial feedback to enable them to refine and submit their paper elsewhere.

In spite of the policy of not allowing submitted papers to be under review elsewhere, unfortunately a small number of authors still seek to compromise it. Without wishing to condone such actions, nonetheless we might understand the incentives some authors face when they are under acute pressures to publish

and they perceive the journal review process is lengthy and erratic. With a public archive of papers under review, journals will be better able to monitor violations of the policy. In effect, collectively the members of the discipline will be monitoring the archives to detect any breaches that occur.

CHANGE 4: JOURNALS MAINTAIN A PUBLIC WEB ARCHIVE OF REJECTED MANUSCRIPTS

The fourth change in my manifesto is to have journals maintain a Web archive of rejected papers, at least for a period (e.g., three years). Rejected papers should be indexed in various ways to assist scholars to search the archive according to different criteria (e.g., author name, subject, reason for rejection). In addition, the editors' and reviewers' reports, together with their names, should be available for scrutiny. Author responses to editors' and reviewers' comments should also be accessible. Again, a condition of authors submitting to a journal is that their manuscripts can be retained in an archive in the event their manuscript is rejected.

My motivation for this change is threefold. First, the journal review process will never be flawless. Mistakes sometimes will be made, and high-quality papers will be rejected. In the accounting discipline, for example, I am familiar with several highly cited papers that remain in working paper form. These papers clearly are exceedingly helpful to accounting scholars. For some reason, however, the authors never managed to have them published. Indeed, in some cases, I believe the authors allow their papers to stand as a reminder to the journals that rejected them that their review processes sometimes are seriously flawed. In any event, rejected papers may still be useful to scholars within an area. A Web archive is one way to support scholarly activities that turn to a search of working papers and rejected papers.

Second, the archive provides a public record of the work undertaken by editors and reviewers. It enables the members of a discipline to scrutinize and evaluate this work. In this regard, the archive may be a good candidate for data mining. For example, the following sorts of questions can be asked: Which

editors and reviewers are fair, positive, and constructive? Which editors and reviewers provide timely reports? Which colleagues contribute significantly to the review processes within the discipline? Is there any evidence of bias or tit-for-tat trading of favors within the discipline? What are the sorts of factors that lead to rejection of papers?

Third, the archive should provide incentives for authors *not* to submit their work prematurely to journals. Otherwise, their reputations are likely to suffer if journal archives contain too many instances of rejected manuscripts that they authored or co-authored. Unfortunately, some scholars use the journal review process as a way of testing their ideas and refining their working papers. They do not first present their research in other venues, such as workshops and conferences, to obtain feedback that would enable them to evaluate the quality of their ideas and refine their working papers. As a result, the burden of review work falls primarily on those who have formal editorial and review responsibilities associated with journals. A number of undesirable outcomes occur – for example, the quality of review work falls because editors and reviewers are overworked, too much power becomes concentrated in editors and reviewers, and junior colleagues receive insufficient exposure to the evaluative process.

One downside to this proposal is that review and editorial decisions at one journal may begin to have a greater impact on review and editorial decisions at another journal. Because the archives are public, editors and reviewers might initiate searches of other journal archives to determine whether papers they receive have been rejected elsewhere. This might discourage authors from submitting papers to journals that maintain public Web sites of rejected manuscripts (or manuscripts under review). Copyright issues also might have to be addressed if a paper rejected at one journal is published eventually in another journal. For example, sometimes authors retain copyright in their papers. Thus, journals would have to seek permission from an author before they posted the author's manuscript to their Web site.

IV. CONCLUSIONS

In presenting my manifesto, my goal is to provide a platform for renewed, vigorous discussion and debate on the journal review process. It is time! In my opinion, we have not yet properly discerned how new information technology and new organizational arrangements might be used to break some shackles of the past. Too much is vested in the current journal review process. It is hard to think laterally and to conceive how we might reengineer existing protocols.

In this regard, I too am “trapped” by my past and current experiences with the review process. Accordingly, my manifesto reflects just one view of some changes that might be trialled. During conversations with colleagues, I heard other creative proposals for change. I hope these colleagues, along with others who believe they have useful insights, might document and share them more widely so we are better placed to determine a way forward.

Furthermore, I hope that my focus on some of the problems with the current review process will not lead to narrow debate. Much about the current review process is laudable. If we are to have balanced discourse, we need to document its strengths. We especially need to understand more about the behavior of those colleagues whom we deem to be outstanding reviewers. We learn and grow as scholars by having role models to emulate.

We need, also, to be careful that we do not treat symptoms rather than the underlying problem. Perhaps some of the difficulties inherent in the current journal review process manifest broader problems associated with the complex social, psychological, political, and historical contexts within which scholars and universities must operate. Change is afoot within these contexts [see, e.g., Denning 1997, Tsichritzis 1999]. To try to improve the review process without due regard to this broader context may be foolhardy.

Ultimately, however, I am an empiricist. I believe we learn primarily through action and experimentation. We should not be rendered inert by the complexity of the bigger picture. My hope, therefore, is that some (courageous) editor will see sufficient merit in the manifesto to initiate and trial the proposed

changes. In a world of increasing competition among journals for good papers, the payoff may be that the editor's journal survives and prospers whereas other journals that remain rooted to existing review processes decline and fail.

Finally, I am mindful that I am seeking to bring about behavioral change that some will perceive as painful. For example, I recognize that the increased transparency of review work that will occur under my manifesto might lead to a smaller pool of willing reviewers. Cultural change among all stakeholders will occur. Accordingly, editors might be wise to follow the precepts of a well-accepted change model – for example, the Kolb-Frohman model. Those who seek to implement the manifesto will first *unfreeze* their journal's stakeholders. They will have to convince stakeholders that the changes incorporated in the manifesto have merit. Only if they can garner support for the changes should they then invoke them – the *moving* stage in the Kolb-Frohman model. Perhaps the moving stage initially should be confined to a small group of stakeholders who are willing to act as pioneers in the process. Once the changes are made, editors must then take care to engage in *refreezing* – in other words, supporting and affirming the stakeholders who had the courage to participate in the change. Based on the evidence that arises from our experimentation, we must then reflect carefully on the merits of the changes. We must have the will to embrace them if they are beneficial and to discard them if they are detrimental.

ACKNOWLEDGEMENTS

I am grateful to Peter Clarkson, Paul Gray, Allen Lee, Russell Lundholm, and Robert Zmud for their timely and constructive comments on earlier versions of this paper. In no way should they be held responsible for the views expressed in the paper. Moreover, in no way should the views expressed in the paper be construed as the official position of the journals with which I am currently associated or have previously been associated or of CAIS.

Editor's Notes: Readers are encouraged to write Letters to the Editor about this article. Selected letters will be published. This article was received on August 16, 1999. It was with the author for 1 week for 2 revisions. It was published on August 31, 1999.

REFERENCES

EDITOR'S NOTE: The following reference list contains hyperlinks to World Wide Web pages. Readers who have the ability to access the Web directly from their word processor or are reading the paper on the Web, can gain direct access to these linked references. Readers are warned, however, that

1. these links existed as of the date of publication but are not guaranteed to be working thereafter.
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ABOUT THE AUTHOR

[Ron Weber](#) is Professor of Information Systems in the [Department of Commerce at The University of Queensland](#). He is a Past President of the [Association for Information Systems](#) and a Past President of the [Accounting Association of Australia and New Zealand](#). He is also Co-Chair for the [International Conference on Information Systems](#) to be held in Brisbane, Queensland, Australia from 10-13 December 2000. Ron is author or co-author of over 80 publications. His research interests now focus on the ontological foundations of information systems and, in particular, the application of ontology to conceptual modeling.

LETTERS TO THE EDITOR

From:
Gert Jan Hofstede
Wageningen University, Netherlands
Applied Computer Science Group
gertjan.hofstede@users.info.wau.nl
To the Editor of CAIS:

AN ARTICLE DOES NOT EXIST UNTIL IT IS READ

Why journal article reviews?

The justification for reviewing journal articles is to keep up the academic standard of the journal, so that the journal can justify its existence. The journal itself exists in order for the discipline to justify its existence. Incidentally, the continued existence of the IS discipline is not to be taken for granted. If our work fails to be recognized as relevant to practice, surrounding disciplines will take our place. Critical keynote addresses at ECIS , ICIS, as well as recent discussions in, for example, MISQ and CACM, acknowledge this circumstance.

Keeping the above in mind, the reviewing process should ensure the rigor of articles, but especially their relevance to the journals' readership.

When does a journal contribute to the discipline?

A journal cannot contribute unless the articles published in it are actually read. Let me introduce the notion of reading/writing ratio. I define the reading/writing ratio as the number of times that a journal article is read. Each time the paper is fully read adds 1 to the numerator. Skimming through the paper counts for less than 1. The denominator is usually 1, assuming that each journal paper was written once for that paper. If it is published more than once, the denominator increases by 1 for each version. Ideally, the reading/writing ratio is in the range of hundreds to thousands for an academic article. In fact, it is frequently below 10 or even near zero. It is no wonder, then, that authors try to make the denominator of the fraction larger by publishing the same or more or less the same article in different channels. After all, it is publish or perish for many of them. But a paper

that is not read does not have any impact on practice. Its only effect is on the status and possibly the tenure of its authors.

Why are journal articles not being read?

Reading journal articles does not pay. It does not pay for the readership at large, because there are too many articles in too many journals, which leads to a very low return on investment of their time. It does not pay for researchers, because publishing is what counts for them. They only need to read cursorily to fill their lists of references and make sure their research is new enough.

These factors compromise the relevance of articles, of journals, and of the field as a whole. Of all fields, the field of information systems should know better than to impose information overload on its public. Worst of all, it is a vicious circle.

How can we reduce the bulk and improve the relevance?

Obviously, we need to put a premium on reading articles. What we could do for the outside public is to make the most relevant articles stand out clearly from the sea of information. We might, for instance, install a relevance award for IS journal articles. Or we might create a publication containing the best journal articles of the year. That publication would need to have few pages and vast publicity. It might just contain abstracts and pointers to the relevant journal articles.

For IS researchers themselves, the main stage at which papers are currently read - or skimmed - is during the review process. It is here that Ron Weber's ideas enter my argument. We should try them, because our discipline is immersing itself in publications of poor relevance. If we implement his ideas, the review process acquires the characteristics of a public reading and discussion. The quality of contributions to this discussion will be apparent for all to see. It will be easy, for instance, to make tenure decisions co-dependent on contributions to reviewing processes. Incidentally, I am skeptical of the possibilities of changing review processes at established journals. I rather think new journals should try it. They will then attract those researchers who value the approach. This might serve as a motivating example for other journals.

If reviewing articles is acknowledged and used as a performance evaluator for researchers, it will be less necessary to write "Contribute or perish", (where "contribute" might be "reading" or "writing") is better than just "Publish or perish". The numerator of the reading/writing ratio will go up, while the denominator will go down (less need for double-publishing), and the number of publications will decrease.

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