

Publishing in accounting journals: A fair game?

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Abstract

Publication in the social sciences appears to have evolved into a game, played by four parties: the author, the reviewers, the editor and the bureaucrats using the simple criterion that a quality researcher publishes in quality journals. Acceptance rates for top quality journals now hover around the 10% mark. Something cannot be right with a system which creates so much apparent waste. Either too many authors are submitting substandard articles or too many reviewers are setting unrealistically high hurdles over which authors have to jump. Most of the literature has focussed on the unrealistically high hurdle rate explanation and also on the fallibility of reviewers and editors. The aim of this paper is to explore the issues of low acceptance rates as well as an increasingly lengthy publication process. The paper considers what is the purpose of publishing in academic journals and what are the motivations of authors, reviewers and editors. In order to inform both authors and reviewers of best practice, a summary of the extensive literature is given in the Appendix. The paper concludes with a survey of the suggestions that have been made to improve the publishing process in order to link back to the original purpose of publishing, that of communicating important results to inform public debate on major issues.

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“It is because we put up with bad things that hotel keepers continue to give them to us.”
[Anthony Trollope, *Orley Farm*, Chapter 22,
Oxford Dictionary of Quotations]

Introduction

The genesis of this paper was an invited contribution to a conference of PhD students on the subject of publishing articles in academic journals. One reason for the invitation was that I had just stepped down as a joint editor of a top 10 accounting journal as judged by rankings of academics throughout the world. At first, I thought that the presentation would be simple; a few “does and don’ts” for authors anx-

ious to please both reviewers and editors. However, on reflection and reading the literature on the subject, I became increasingly conscious of the way in which publication in the social sciences has evolved into a game, played by four parties: the author, the reviewers, the editor and the bureaucrats using the simple criterion that a quality researcher publishes in quality journals. Somewhere in this game, the original purpose of publishing to advance knowledge in one’s discipline seems to have got lost. A review of the literature showed three main concerns: very low acceptance rates, too much time spent on the reviewing process and too much focus on the technical quality of a manuscript rather than its inherent contribution to the academic discipline.

Of the three, the problem that seemed most significant was the low rates of acceptance, which are com-

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monly less than 20% and even as low as 10% for the major accounting journals. This implies a rejection rate of 90%. This is in strong contrast to the acceptance rate in science journals. In a pioneering study, Zuckerman and Merton (1971) found substantial variation in rejection rates ranging from 20–40% in the physical sciences to 70–90% in the social sciences and humanities. In a later study, Hargens (1998) found a similar range of rejection rates from 15% to 90% and Schminke (2002) reports that 90% of all manuscripts are rejected by the *Academy of Management Journal*. Something cannot be right with a system which creates so much apparent waste. Either too many authors are submitting substandard articles or too many reviewers are setting unrealistically high hurdles over which authors have to jump. From my experience as author, reviewer and editor, my suspicion was that both explanations had some merit. Clearly as an author, I favoured the unrealistically high hurdle rate explanation and as a reviewer and editor, the large number of substandard articles!

The objective of the paper is to improve the process and by two main approaches:

- to produce advice on how both authors and reviewers can best cope with the current situation, and
- considering what changes can be made in the process to make the publication process better able to fulfil its original purpose of improving the discipline.

The structure of the paper is the following. Firstly, the avowed purpose behind publishing is, i.e. the publicly acceptable version of the process for consumption by bureaucrats and funding bodies. There then follows a discussion of the academic publishing process and in particular what motivates the three main players: authors, reviewers and editors. The paper then considers the issue of the time it takes to get published and includes a discussion of the implications of Glenn Ellison's *q* and *r* theory. The paper concludes with some radical suggestions as to how the situation can be improved and how academics might be able to free themselves from the tyranny to which they have subjected themselves.

Purpose of publishing in academic accounting journals

The academic spin that is put on the publishing process can be seen from the UK's Competition

Commission's report on a proposed merger between two publishers of academic journals (Harcourt and Reed Elsevier, *Competition Commission*, 2001). The Commission suggested that the primary function of a journal, from the viewpoint of the contributing author, and the research institution to which he or she belongs, is to communicate the results of their research as widely as possible, with the segment of the academic community to which it is of interest. The Commission did also notice that the career of an individual academic depends on his or her record of published research. The phrase "publish or perish" was used to describe the position of academics (*Competition Commission*, 2001, p. 59).

The Competition Commission's focus was primarily on publishing in medical, scientific and technical journals. The challenge is to produce a similar statement for the purpose of publishing in accounting. One of my seminal experiences was hearing the late Professor Eddie Stamp¹ of Lancaster University answer exactly this question when it was put to him by a mechanical engineer in a pub in deepest Lancashire. The engineer asked Eddie what was the good of doing research in accounting. Eddie thought for a moment and then said that if you imagined that accounting was the oil of British industry, then the role of the academic researcher was to improve the quality of the oil. At the heart of this response is the utilitarian view that accounting research has as its objective the production of something of value to society. That this is the view of non-academics is apparent at civil receptions for the Annual Congress of the European Accounting Association, where local dignitaries will inevitably wish participants well in their important scientific work. Similarly, there is often a rhetorical assertion by regulators that they would like to make policy decisions based on academic research as this gives legitimacy to their recommendations. However, there is little indication that the publishing process also has this objective.

Most studies looking at the impact of accounting research on practice, suggest that in general accounting research does not have much impact

¹ It was Eddie Stamp's column in *The Times* newspaper in 1969 criticizing the lack of definition and firmness of accounting principles that led the Institute of Chartered Accountants in England and Wales to introduce a new program for approving accounting standards (Zeff, 1990). This set in train the process that was to lead eventually to the creation of the International Accounting Standards Board.

on the activity of accounting. For example, Bedford (1978, p. 2) came to the conclusion that a survey of the accounting literature over the past 10 years provided little reason to believe that a priori accounting research has caused changes in accounting practice. Dyckman, Gibbins, and Swieringa (1978, p. 87) conclude that the impact of behavioural research on accounting practice has been almost non-existent. Kaplan (1978, p. 168) concluded that he was pessimistic about the ability of empirical research to give many insights into the many and varied questions confronting the accounting profession. He argued that while it is typical, when confronted with a difficult policy issue, to call for more research on the question, he could not see how empirical research in particular can provide much guidance in debates over issues which involve costly disclosure. Similarly, Sullivan (1993) expressed the view of most practitioners that their work was unaffected by academic research, although he did argue that focused and practical academic audit research could affect the auditing standard setting process at both the firm level and the Auditing Standards Board level. Hopwood (2007, p. 1371) explains why this should be:

“The accounting research community has become ever more internally focussed and self-referential, and thereby, less subject to a diversity of pressures and interests that would be created if there were more active consumers of accounting knowledge”.

If accounting research has so little obvious practical impact, the question remains as to what is its purpose. An answer might be expected to be found in the published objectives of the journals which exist to publish such research. Appendix A details the published objectives of the major international journals publishing research in accounting and auditing. The *Journal of Accounting Research* and the *Journal of Accounting, Auditing and Finance* are not included because neither journal includes anything that sets out the journal's objectives. It can be seen that the primary objective of most journal editors is to publish the results of academic studies. Few of the editors set out to determine why publishing the results of academic studies is itself a worthwhile activity; that seems to be taken as a given. When framing the objectives of the journal, some editors seek to delineate the areas of research in which they seek to publish academic research articles, but the central purpose of the publishing exercise is not

mentioned. Accounting Horizons could be seen as the exception as its mission does include the notion that there will be some net benefit from publishing its articles. However, for the rest, the unspoken assumption would seem to be that journals exist to allow academics to publish the results of their work, subject to some set of criteria involving notions of research excellence. These are rarely spelt out, except to say that manuscripts will be reviewed by researchers usually of international standing who are presumed to be able to differentiate between “good” and “bad” research. One conclusion that could be drawn from this is that the purpose of academic journals is simply to exist. The purpose of publishing an academic journal is to publish the work of academics. Within this Kafkaesque closed system, roles are continually being interchanged with academics one minute writing articles, another reviewing them and another editing journals that publish them. Even in the field of scientific research, doubts have been raised about the purpose of publication with 80% of scientific articles never having been cited more than once (Hamilton, 1990, p. 1, 331).

It can be argued that the need to fund those research projects that require the gathering of data will introduce a set of ‘real world’ priorities, since funding bodies have their own agenda, which is primarily the need to show that they are using their monies wisely to produce some economic benefit. Hence, the Economic and Social Funding Council in the UK often seeks to influence the type of research it funds by creating particular initiatives, for example the Functioning of Markets Initiative, in order to justify to government that the funds given to it are being put to good use. Funding bodies, therefore, require some form of output that is likely to produce tangible benefit for the greater good. Funding by professional firms and professional bodies is also usually judged in relation to the production of some benefit for the firm or professional body. There is then an inevitable tension between the desires of the funding organisation for output that will either have policy implications or lead to better ways of operating and the desires of journals to publish what are judged to be high quality manuscripts by academicians. One obvious example of such tension is in the area of descriptive research. Such work may receive funding because the funding body believes that it is useful to find out what is happening as described by neutral, unbiased academic researchers. The descriptive results

will then have utility to policy makers as it gives a firmer ground on which to make policy judgements. However, from the perspective of an academic journal, such descriptive studies are less likely to find favour as they will not conform to the prevailing model of a developed theory leading to testable hypotheses which are then statistically investigated by using the relevant data or surrogates.

The process of academic publishing

The Competition Commission's report described the production process as follows:

“Once academic researchers have completed a piece of original research, they will try to get it published. To do this, they send the paper to a journal. The academic transfers the copyright to the publisher, generally without payment. The publisher employs an expert in the field to act as editor. The editor sends the paper to referees (other academics in the field) and asks for verification that the paper is of sufficient quality to publish. Authors, members of the editorial boards and peer-reviewers receive no financial remuneration.” (Competition Commission, 2001, p. 56).

The Commission's report describes the role of referees as academics “who report to the editorial boards with an assessment of the originality of the research, whether it is soundly based and whether the conditions under which it was conducted were sufficiently rigorous. They may also suggest ways in which an article can be improved.” (Competition Commission, 2001, p. 11). This positive view of the process is echoed by Thornton (2004, p. 51) who states that “Editors and referees evaluate manuscripts for professional competence, quality of presentation, relevance of subject matter, and the significance of the contribution to the literature.”

Looking at the cash basis of the academic publication process shows an interesting market. Journals receive academic articles free of charge and, in many cases, charge fees just for examining the manuscripts. Very rarely do authors receive compensation from journals. Leading journals usually employ at least two reviewers, who occasionally get a small fee, but more usually receive nothing. Hence, reviewers give their time for free and as the process is usually blind, their identity is kept secret, with only the editor knowing who has reviewed what. Journals do sometimes at the end of the year, publish the list of those who have reviewed from

them, but there is no link to the papers that were reviewed. For their part, editors, receive payments of variable size from publishers for their role in the process. There would appear to be a sufficient supply of academics willing to create the articles to be published and to act as reviewers and editors. It is therefore interesting to reflect on the motives of those involved in the academic publishing process. The next three subsections will look at the motivations of the three groups: authors, referees and editors.

Authors

Why academics wish to publish relates to the nature of their employment. In many countries, individual academic researchers have economic incentives to publish in journals that are deemed to be of high standing. In the US for example, promotion and tenure decisions as well as various “faculty support decisions” such as salary increases, teaching loads, research funding, summer support and research awards are usually based on research publication records (Swanson, 2004). Jönsson (2006) quotes the editors of the *Journal of Accounting and Economics* as having calculated that an article published in their journal was worth US\$ 30,000 to the author in terms of increased life income. A similar point has been made by Starbuck (2005) who notes that deans of US business schools announced new criteria for tenure and promotion that placed extreme emphasis on publications in so-called A journals. To gain tenure or promotion to full professor, a professor must have published at least *N* publications in A journals, since publications in B or C journals would be judged as insignificant. A European colleague of mine had a similar experience when a submission to an “A grade” journal was rejected after a first revision, which meant that promotion to a full professorship at a European university was lost. A corollary of this imperative for young researchers to publish is that there is less incentive for the older tenured professors to do so (Oster & Hammermesh, 1998). For example, Dunn (2005) shows that the publications in the *American Economic Review* are dominated by the 30–44 age group, with a modest rising trend in the role of the 45–54 age group as they work with younger colleagues who have stronger technical skills.

In the UK, researchers are judged by the five or six yearly Research Assessment Exercise (RAE)

when four of their publications are judged to be of international standing, etc. To be judged a researcher of international standing can result in promotion and an increased salary. Otley (2002) describes the 2001 RAE as it applied to UK accounting and finance academics. UK universities submitted names of those researchers whose work they wished to be evaluated, with the main emphasis being placed on an evaluation of the quality of a maximum of four pieces of published work cited for each researcher in the 5-year period 1996–2000. The researchers' publications were judged by a panel of 10 members, one of whom was not a full time academic. Each individual submitted piece of work was graded into three categories of excellence: international, national and sub-national and then aggregated directly into a unit of assessment table. On the basis of this aggregation, individual university departments were then graded on a seven point scale. Otley (2002, p. 412) sets out how the three grades of excellence were defined.

“International excellence is interpreted as being ‘as good as the leading research in those countries where there is a significant body of work in the field’. The Panel does not in general confine the international standard of comparison to any particular geographical area. The Panel confirms that the terms ‘international’ and ‘national’ refer to a quality standard, and not to the nature or scope of any particular research. International excellence equates to work of high quality with which researchers in the specialist field ought to engage. National excellence equates to work of sound quality with which other researchers in the specialist field should be expected to be familiar”.

The Panel had to evaluate the publications of over 400 researchers and did so by reading “over five hundred pieces of cited work”. In general the work examined tended to be concentrated on those items which had no external ‘imprimatur’ of quality such as books, research reports and working papers (Otley, 2002, p. 391). The clear impression given is that the Panel took the view that if an article appeared in a journal judged to be of international standing, then that should be the standard attributed to the article. Similarly, Easton and Easton (2003) found that marketing publication in the UK was highly concentrated in two major UK journals and they raised the question of whether the RAE process should involve greater efforts to assess articles and

not journals. The RAE clearly therefore reinforces the economic incentive on researchers to publish articles in journals deemed to be of international standing (Bence & Oppenheim, 2004; Humphrey, Moizer, & Owen, 1995). In their evidence to the Competition Commission, members of the research assessment exercise explained that whilst all research is treated equally regardless of where or how it is published, there is a widespread belief amongst higher education institutions that prestigious journals count most highly in the RAE and this produces an inevitable pressure to be published in them (Competition Commission, 2001, p. 59).

Not surprisingly given the importance of publishing for the careers of academics, there have been a considerable number of publications with the aim of showing inexperienced faculty how to do it. Appendix B contains a distillation of this sage advice, so that the present manuscript will have utility to those who want to take the plunge into the cold waters of the academic publishing sea. The advice is primarily of the form that one has to put up with the system, because it will be worth it in the long run. The aim for an aspiring academic is therefore to “play the game”, since that is the road to success.

Of course to play the game, one has first to understand its rules (Loeb, 1994). Borkowski and Welsh (2000) found that the majority of authors thought that codes of publishing ethics were needed, whereas editors did not. In terms of what should constitute such a set of rules, there are obvious prohibitions such as the deliberate falsification of data and plagiarism (Borkowski & Welsh, 1998), but more subtle and less accepted rules can also be gleaned from the literature:

- (i) identical manuscripts should not be sent simultaneously to two journals (Calabrese & Roberts, 2004; Von Glinow & Novell, 1982);
- (ii) publishing two papers which have considerable overlap is acceptable to UK accounting academics provided there are different audiences (e.g. professional and academic), but not acceptable where the audiences are the same (Brinn, Jones, & Pendlebury, 1998);
- (iii) deriving multiple publications from a single set of data should be allowed only (a) if it is not possible to write a single integrative article that is clear, digestible and meaningful and (b) the multiple articles have distinct purposes (Fine & Kurdek, 1994);

- (iv) colleagues should only be shown as authors if they have contributed to the research (Sherrell, Hair, & Griffin, 1989);
- (v) papers should not replicate other researchers' methodology without giving due credit (Brinn et al., 1998).

Another element of the game is the issue of replication and extension research, where work of one researcher is replicated or extended by another. In social science there is evidence that such work is seen as being of inferior status and therefore would-be authors are advised to avoid it (Hubbard & Armstrong, 1994). Such a situation is in contrast to that in the sciences and medicine, where replications and extensions are commonplace and are seen as a powerful way of ensuring that empirical findings are robust from one group of researchers to another. The value of replication and extension research is that such research can help ensure the integrity of a discipline's empirical results by serving to guard against Type I errors (erroneous rejection of the null hypothesis) and more generally by assessing the robustness and empirical generalisability of results (Hubbard & Vetter, 1996, p. 154). The lack of this type of research in social science based subjects is especially odd given that social science theories are much less powerful than in science. For example, a review of accounting research published in the period 1970–1991 showed that of 373 empirical studies, only 32 (8.6%) were replication and/or extension studies (Hubbard & Vetter, 1996, p. 158). Of the 32 replication/extension studies, six supported the results of the original studies, 10 provided partial support and 16 (50%) had results which contradicted the original findings (Hubbard & Vetter, 1996, p. 160). This result has two possible explanations, the first being original empirical findings in accounting research are not very robust to replication or the second that authors, reviewers and editors are biased towards publishing those replication studies whose findings contradict those of the earlier research. The problem may also be related to the framing of the null hypothesis to be tested. Burgstahler (1987) argues that accounting researchers perceive that significant results are much more likely to be published than non-significant ones and so they will choose to report those results that appear to contradict the null hypothesis and not to report those results that do not contradict the null hypothesis.

“The proportion of incorrect rejections in published research is also increased by the behaviour of journal editors and reviewers. If, as casual empiricism suggests, stricter editorial standards are applied to completed research with non-significant results, the proportion of published incorrect rejections of the null hypothesis will be higher than implied by the nominal level of the tests. In fact, if only rejections of null hypotheses were published, then, even for a correct null hypothesis, the probability of observing a published study which rejects the hypothesis would increase rapidly as the number of tests of the hypothesis increased.” (Burgstahler, 1987, p. 209).

All games need umpires and an important aspect of playing the academic game is the concept of “dodgy umpires” who fail to recognize the true merit of a manuscript. A number of studies have focussed on the fallibility of reviewers and editors. There is not space to detail all the findings, but most research on the journal review process has found little consistency among reviewers' evaluations of manuscripts (e.g. Campanario, 1996; Gilliland & Cortina, 1997; Starbuck, 2003). Probably the most discussed and controversial study of the reviewing process was the one by Peters and Ceci (1982) quoted in Starbuck (2003). In this study, 12 highly regarded articles by authors from prestigious departments were resubmitted to the journals that had already published them just 18–36 months earlier. The resubmissions had disguised names and institutions and only three were spotted by either editors or reviewers. Of the remaining nine, eight were rejected by the editors, generally on the grounds of “serious methodological flaws”, including inappropriate statistical analysis and faulty study design. Such results help support the “publishing is a lottery” perspective or serendipity as one colleague expressed it to me. In this view, a submission to a journal is a gamble with the chance variable being the quality of the review. If one gets a reviewer who has clearly not bothered to understand the paper, this perspective suggests that the best approach is to try another journal and hope that the serendipity process is kinder.

As well as the element of unreliability in the reviewing process, the author has to learn to adapt to the demands of the reviewers, leading Bruno Frey to claim that this amounted to a form of prostitution Frey (2003), Frey (2005). These two papers are notable for two reasons. The first is that they

are very similar with similar blocks of text and exactly the same abstracts, with neither paper referring to the existence of the other.² What makes this double publication so wonderfully post-modern is the contention of both papers that authors have to prostitute themselves in order to get published. What could be a better metaphor for prostituting one's talents than publishing substantially the same paper in two different journals! The essence of the argument in the two papers relates to what happens once an author's papers have come back from the refereeing process. As Frey points out, an author knows that, normally, he or she will be lucky if, after something like a year, he or she gets an invitation to resubmit the paper according to the demands exactly spelled out by the referees and the editor. For most scholars, this is a proposal that cannot be refused, because their survival in academia crucially depends on publications in refereed journals. Authors believe that they have a chance of getting their paper accepted, only if they slavishly follow the demands formulated by the reviewers and the editor. Frey characterises this as academics becoming prostitutes, selling their soul to conform to the will of others, in order to gain publication (Frey, 2003, p. 206 or Frey, 2005, p. 175). If the author revises according to the demands of the referees, the editor makes the final decision of whether to reject or accept the paper for publication with a given probability. In the case of rejection, the author has expended a considerable amount of work to please the referees, has lost more time (Frey estimates that something like 2 years is not unrealistic) and has to carry the moral cost of prostituting him or herself. The effort, time and moral costs also apply in the case of acceptance, but then they are counterbalanced by the benefits of having the article published, with all the resultant benefits. The point made by Frey is that in most cases referees want to see substantial changes almost to the point of requiring a new paper.

² Both papers start with the same challenge to potential reviewers. The *Public Choice* paper begins "When writing this paper, I never expected that it would be published in a (refereed) economics journal because it would not be able to pass the refereeing process within a reasonable period of time, if at all" (Frey, 2003, p. 205). The *European Journal of Law and Economics* paper begins "This paper will never be published in a (refereed) economics journal, nor will it be submitted to a scholarly journal. The author knows that if he submitted it, he would be lucky if, after something like a year or so, he got an invitation to resubmit the paper (Frey, 2005, p. 173).

Reviewers

The motivations of reviewers are the most problematic element within the academic publishing process. Monetary reward is hardly an issue, since when payments are made, the amounts are so low as not to create any significant incentive. Reviewing receives virtually no weight in individual performance evaluations and good reviewing means almost nothing in the job market (Harrison, 2002). There is no public recognition of the work that the referee does, except as a member of an editorial board or having one's name published as an *ad hoc* reviewer in the end of year edition of a journal. The name of the referee is known only to the editor and so there is an element of social control involved, with a referee wishing to be seen as a good citizen, particularly if the editor is responsible for a prestigious journal in which the referee might hope to publish. However, this does not explain fully the motivations of referees, since they also referee for journals in which they may never publish. The issue is further clouded by the fact that not all academics will act as a referee. They may simply refuse with apologies, they may never reply or they may take so long to produce the review that it is of no value. Brief (2004) noted that too often those asked to serve as reviewers decline. As an editor, one soon learns that there are a group of reliable referees who will produce thoughtful and helpful reviews, whereas there are others who cannot be relied upon.

The puzzle is therefore why some referees are willing to perform their task without any economic reward, given that reviewing consumes the academic's most precious resource, time? Harrison (2002) argues that sustained good reviewing builds a positive reputation and a social network of grateful editors and colleagues and indeed, Hamermesh (1994) found that nearly one third of referees published recently in the journal, whilst others had articles under review or forthcoming. Woodruff (2003) explained his reasons for acting as reviewer as follows: "For me, the most important motivation for devoting time and energy to journal reviewing lies in the opportunity to meet the marketing discipline's needs for growth in and usefulness of the body of marketing knowledge." (Woodruff, 2003, p. 327). Singh expressed his motivations in a similar way:

"Reviewing, like an essential food group, feeds the intellectual health of a scholar. Reading and

critiquing scholarly outputs, listening and responding to alternative viewpoints (of authors and of other reviewers), and learning and growing this discourse are unsubstitutable goods for this reviewer's mind. That is the reviewer's gold." (Singh, 2003, p. 335).

In their model of the reviewing process, Engers and Gans (1998) assumed that referees are motivated by a concern for the quality of research journals in a self-interested way, although they did find that monetary incentives could play a role by eliciting speedier review or a greater review rate. Engers and Gans also comment on the similarity of their model to voluntary work more generally, citing problems in using monetary incentives to elicit more blood donations. In this context, it is useful to note that in the UK, blood donors do not receive any financial inducement, only the feeling that they are maintaining the quality of the UK's blood supply.

Azar (2006) notes that the speed that a referee takes to review a paper will be determined by the social norm of how long a referee should take based on his or her views of how long others take. This view will be conditioned by feedback from the journal when both reviews have been received as well as the referee's personal experience as an author of the length of time to receive reviews back from a journal.

Given the ambiguous incentives facing referees, it is worth analysing what would be their economic rational behaviour, since a rational economic person would not necessarily be motivated to act in the general social interest. Anonymous referees have no property rights to the journal they advise and so they are not concerned with the effect of their advice on the journal. For a rational economic person, such a lack of property rights should lead to shirking, since the interests of the journal and the referee are not aligned. Any advice offered by a referee has little or no consequences for them, since it is rare for a referee to be confronted by an angry author. Occasionally, journal editors might forward an irate letter from an author to the referee, but there is no obligation on the part of the referee to enter any dialogue. Therefore, the main task of a referee is to produce a report that follows the largely unwritten code of reviewing, which emphasizes the formality of the process. However, once the formalities are observed, the referee is free to indulge him or herself. They might consider the common good, but personal interests must inevitably also play a part.

For example, a referee might take exception if he or she feels that his or her own contributions to the area having been insufficiently appreciated by the author(s). Referees are also in possession of a "veto" power, since it is rare for an editor to go against a referee who is strongly opposed to the publication of a particular manuscript. There is thus created a "censuring" system, making it difficult to have unconventional ideas accepted. Referees could exercise the power of veto for three reasons: (i) the idea is new and therefore more difficult to grasp and appreciate for the referees than are more conventional contributions; (ii) the referees are normally leading researchers in the topic who fear the loss of some of their reputation if a new idea is introduced and (iii) the new idea is less well formulated than are the well-established ideas and therefore is rejected for lack of rigour (Frey, 2003, p. 212).

Editors

The motivations of editors are potentially similar to those of reviewers with the principal difference being that the identity of an editor is not a secret and therefore the editor's reputation is enhanced by the quality of the journal. Hence, editors are interested in the influence and quality of their journals as that reflects directly on them. Editors can also receive substantial payments for acting as editors depending on the deal that they can make with the publishers. They are also asked to speak at conferences on how to get published as well as being popular at convention buffets! In terms of the best practice for editors, the literature is small. Ben-Yashar and Nitzan (2001) use an economic model to argue that editors should inform reviewers as to how many reviewers will be employed to judge a paper and what kind of decision rule the editor will then employ to determine whether the paper should be rejected or not.

Delays in publishing and q-r theory

Having looked at the motivations of the three parties to the process, it is now appropriate to look at one of the vexed questions of publishing in the social sciences, namely the length of time it takes from submission to final publication of an article. The importance for academics is clear, for as Leslie (2005) notes, the major submission cost to an author is the long and unpredictable spent waiting

for an editorial decision. As result of the incentives operating on authors, the best strategy according to Leslie in the absence of time delays is for an author to start at the most prestigious journal and work down until the article is accepted. However, Azar (2005) took the result one step further by arguing that the effect of editorial delay is to create a significant submission cost to authors and therefore he argued that if editorial delay was reduced then that would induce many additional submissions of low-quality papers to good journals, increasing significantly the workload of editors and referees. This would also lead to an increase in the rejection rate and cause papers to be rejected more times before publication, offsetting at least some of the shorter first response times. In an earlier paper, Azar (2004) estimated that most manuscripts are submitted between three and six times prior to publication if all submissions to journals are included. He suggests that this implies that the first response time (between submission and the first editorial decision) is much more important than other parts of the publication delay.

In order to investigate the phenomenon of the increasing time taken by an article from submission to publication, Ellison (2002a) developed a model of quality standards in academic publishing in terms of “**q-r** theory”, the central premise of which is that academic papers can usefully be regarded as varying along two quality dimensions: **q** and **r**. The dimension **q** is intended to reflect the contribution inherent in the main ideas of the paper. Ellison characterises **q** as “a measure of what I would take out of a paper if I were to teach it in a graduate course”. The **r** dimension is “intended to reflect additional aspects of quality that may be improved when referees ask authors to generalize theoretical results, to check the robustness of empirical findings, to extend the analysis to consider related questions, to improve and tighten a paper’s exposition, to make clear relationships to other papers in the literature and so forth”. The model features a continuum of academics. They allocate their time between working on **q**-quality and **r**-quality in trying to write one paper to submit to the one journal in the profession. How the profession weights **q**-quality and **r**-quality in selecting papers for publication is a commonly understood social norm. Referees evaluate submitted papers using this norm and propose improvements that would bring a paper up to the publication threshold. After revisions are made, the editor fills the journal’s slots by accepting the fraction **r** of

papers with the highest quality. A crucial assumption of the model is that the initial work on a paper determines its **q**-quality and subsequent revisions improve only **r**-quality. In the model, the most important actors are the journal’s referees. They attempt to learn from the prevailing social norm from two sources: seeing what revisions they are asked to make on their own papers and seeing whether editors accept or reject papers they refereed. When an additional assumption is made that academics are biased and think that their work is slightly better than it is, referees would try to hold authors to the higher standards they mistakenly feel is being applied to their own papers. The result is the gradual evolution of social norms to increasingly weight **r**-quality. The dynamics are slow and steady and are used to justify the findings in Ellison (2002b) in relation to publishing in economics where he finds that there has been a gradual slowdown of the economics publishing process that cannot be attributed to observable changes in the profession. The important implication of Ellison’s work is the notion that all parties to the publishing process become unwitting accomplices to the pursuit of **r**-quality leading to either a reduction in the number of articles being published or a reduction in **q**-quality. Ellison quotes the example of many young economists who report spending as much time revising old papers as working on new ones. Guiding larger and more frequent revisions is an additional burden on referees and editors. The role of editors also becomes crucial because they have the power to intervene in the process assuming that they recognise what is happening. The problem with a slow change is that participants do not recognize the trend, which they themselves are helping to create.

Swanson (2004) sets out to compare publications by the different disciplines of accounting, finance, management and marketing using Ellison’s theory as a way of predicting that different disciplines will arrive at differing balances between **q** and **r**. He found that there had been a decline over time in the number of major articles being published, a result which is consistent with Ellison’s model of an increasing standard of **r**-quality. The implication of this result is that accounting academics spend too much time on the rigour of the analysis and insufficient time on ideas. In practice, what this can mean is that referees increasingly judge the quality of papers by reviewing how well the research method has been applied and how sophisticated are the analytical methods, rather than the inherent importance

of the paper's conclusions. Hence, a theoretical manuscript based on some game theoretic analysis would be judged on the extent to which the analysis had been competently performed, rather than the extent to which the resulting findings have some inherent value. Reviewers of statistically based empirical work would concentrate on the appropriateness and application of the underlying theory, the representativeness of the sample, the quality of the statistical analysis, rather than whether the results added in some way to what is already known. The criteria for the reviewers of case studies was well summed up by Hopwood (2000, p. iii) when he summed up the conclusions emerging from a special conference to celebrate the 25th anniversary of *Accounting Organizations and Society*.

“Another very clear message that emerged from the discussions at the conference was the continuing need for very careful structured observations of whatever was the focus of interest. Rather than writing case studies per se or getting involved in rather open-ended descriptive research, the discussions repeatedly emphasised the role of theoretically informed inquiries that moved forward in a considered and structured manner, comparing contrasting instances and striving to learn from the result.”

In the language of the Ellison model, this is an appeal for increasing *r*-quality.

The process modelled by Ellison would suggest a reinforcement mechanism where authors whose papers are accepted will seek to produce similar work in order to maximise their chances of publication. When such authors act as reviewers, they will use this acquired frame of reference in order to decide whether or not to recommend publication. Authors, reviewers and editors thus become involved in a joint learning process, which reinforces certain patterns of behaviour and discourages others. The system becomes internally referenced without any necessary link to “real world” issues. Hence, there is nothing in the process that will naturally produce work that has utility to those outside the academic sphere, since the only criterion is whether the work has utility for the academics involved in its production.

Discussion and conclusions

The above analysis shows that there is a problem in the world of social science publication and in aca-

demical accounting more particularly. The problem is that acceptance rates at journals have fallen to a very low level (10%) and the time from submission to publication has increased inexorably. The effect of this is that all three players in the process (authors, reviewers and editors) expect that an article is likely to be rejected. According to the model of Ellison, the process is inexorable as the feedback loop between authors who become reviewers and vice versa is that the reviewing hurdle continues to be raised. Ellison's model indicates that matters will not improve without some form of concerted external intervention. The low acceptance rates cause rejected articles to reappear as submissions to other journals until the author gives up on the manuscript. This process means that there is an ever increasing workload for reviewers and editors, which eats into the prime resource of academics, namely time. More time is spent on submission, more time is spent reviewing and more time is spent by editors.

The important question is how this vicious cycle can be changed if at all. However, before this can happen, there has to be a consensus as to the causes of the problem. Logically, there can be only two reasons for the low acceptance rates and longer publication times; either there are an increasing number of substandard manuscripts being submitted or the criteria for acceptance used by reviewers and editors are becoming more demanding. From the literature and personal experience, it would appear that both explanations are tenable, although the exact effects of each are hard to predict. The causes of both problems are not too difficult to identify, but suggesting solutions is rather more difficult. The obvious candidates for causes are: the doctrine of “you are where you publish”, the large number of conference papers in existence (often of low quality), a lack of training for would-be authors on how to succeed in the journal publishing process, a lack of training for reviewers on how best practice and a lack of a clear statement about a journal's *raison d'être*. Each of these possible causes will be discussed together with suggestions as to how they can be remedied. Finally, the section concludes with other suggestions for improving the academic publishing process, primarily from the viewpoint of authors.

The increasing importance being placed on the decision rule of “you are where you publish”

Those who make hiring, tenure and promotion decisions gauge an academic's research quality and

productivity by the number of articles published in journals of sufficient standing. This then creates a pressure on academics to submit to the perceived top journals, which in turn increases the number of submissions to those journals. If the rejection rate is as high as 90% and the process is not perceived to be fair then the obvious strategy is for would-be academics to submit as many articles as possible in the hope that one might be allowed into the journal. The alternative strategy of spending many weeks crafting an article in the hope of making it sufficiently attractive is not very appealing given that the high rejection rate and the perceived serendipity in the system. The dispiriting conclusion of this analysis is that as long as those in power to influence the careers of academics use the lazy route of simply looking at publications rather than reading the articles, then the process has to continue.

This fallacy of relying on only one kind of indicator became known as Goodhart's Law after the distinguished Chief Adviser to the Bank of England, who noted that as soon as the government attempts to regulate any particular set of financial issues, these become unreliable as indicators of economic trends – "any observed statistical regularity will tend to collapse once pressure is placed upon it for control purposes." (Chrystal & Mizen, 2001, p. 4). Marilyn Strathern's pithy version of Goodhart's Law is even more relevant to the present paper since it has generally applicability: "When a measure becomes a target, it ceases to be a good measure." (Strathern, 1997). Focussing on only one measure of an academic's research prowess, changes the behaviour of researchers in ways that are not necessarily in the best interests of the discipline. For example, researchers are no longer being asked to research those areas where their input is most needed to help policy choices by standard setters and other regulators. Instead they must choose to research those topic areas that are likely to be viewed favourably by those journals in which the researchers have to publish in order to further their careers. Hence, the focus of research becomes what can be published, rather than what is of high inherent value to the long term future of the discipline.

The large number of poor quality conference papers in existence

A common feature of most university funding of attendance at an overseas conference is that the applicant for funding must present a paper at the

conference; no presentation, no funding. Clearly this creates an unwelcome set of imperatives. Organisers of conferences know that in order for the conference to be a financial success, they must attract a lot of academics. However, in order for an academic to be able to afford the conference fees and travel and accommodation costs, the academic has to be allowed to present his or her research in order to receive funding and therefore to come at all. Hence, the large conferences attracting a multi-national attendance have to have large number of parallel sessions with each author allowed only a modest amount of time to present the results. There is an acceptance process but the hurdle is much lower than for journal acceptance. Is this perhaps the origins of the many substandard manuscripts which will never be accepted, but which will clog up the publishing process. If conferences were choosier about what papers were accepted, then perhaps the academic side of such conferences might be more rewarding intellectually. At the moment, there are too many poor quality papers that attendees have either to sit or through or ignore. Is this a sensible use of quality time? Maybe the first step is for conference organizers to be bolder in their acceptance criteria and to limit the number of papers. If this means that less people attend then that should be to the good. In time, the funders of conference travel will realize that presenting a paper is not necessarily the best measure of the funding decision.

Lack of training for would-be authors on how to succeed in the journal publishing process

Most PhD programs concentrate on giving the students expertise in the art of conducting research (ability to read journals critically, ability to apply the appropriate theoretical paradigms, ability to undertake appropriate research techniques, ability to apply and interpret appropriate statistical techniques and ability to conduct a sustained piece of research and write up the findings in a thesis). However, there is often little preparation given for the student as a potential publisher of academic work. There is also a problem in that there is an expectation that a PhD thesis can be mined for research articles, although the thesis is not constructed on that basis. As a result, the resulting submissions to journals do not fit the requirements of the journal as they tend to be taken directly from the thesis without sufficient thought as to what is required

for an academic article. In my experience, this is another source of submissions that ultimately fail. Part of the reason for writing this paper is to offer the distilled wisdom for would-be publishers that is included in [Appendix B](#).

The effect of this lacking of training on how to publish in refereed journals is that there is a lot of “learning on the job”, where would-be authors learn the hard way about the review process by receiving heavily critical comments from referees. This can result in a potential researcher becoming discouraged by the process and hence leaving the profession. It is not obvious that a Darwinian law of natural selection will produce the best researchers. Survival of the fittest does not lead to the best researchers, merely those who have thick skins and have learned not to take “No” for an answer. The article by [De Lange \(2005\)](#) is a good example of how prospective authors could be introduced to the world of publishing. I once conducted a session for the British Accounting Association on the publishing process, by giving a group of new lecturers a copy of a paper to read before the session. In the session, they were then given the referees’ comments, which we then discussed. The authors’ revised manuscript was then given out for the audience to consider and then come back and discuss what they thought. The second set of reviewers’ comments was then given out. Such an event was very well received and it seemed to benefit the participants in appreciating what was involved in the process.

Lack of training for reviewers on best practice

The vast majority of reviewers learn their craft by doing it in the real world of manuscript evaluation. There is little formal instruction in how to be a good reviewer. [Appendix C](#) is a summary of what experienced reviewers and editors have said on the review process. This sort of information should be made generally available. It is not enough to expect new lecturers to pick up the review process as if by osmosis. One suggestion by [Miner \(2003\)](#) is to conduct workshops at professional meetings to train reviewers. This training should involve readings from the literature on the subject matter, designed to make the trainees sensitive to what constitutes effective reviewing and what does not.

Such review sessions at an annual meeting would allow different views on the academic publication process to be debated. For example, one key issue is the extent to which reviewers have the mind set that the paper sent to them is likely to be rejected

and therefore their objective is find reasons to fail it. It would be much more beneficial to regard the review process as a two stage one. In the first stage, the reviewer is answering the question of whether the manuscript could conceivably be published. If the answer is “No”, there is then the issue of what is the role of the reviewer. Should it be the job of the reviewer to train future researchers, by offering detailed suggestions as to how the authors can improve their performance? Or is simply the job of the reviewer to say that the paper is not capable of being improved sufficiently to make it of publishable quality? Having decided whether the paper has potential, the reviewer can then set about what is arguably the most important part of the review process, namely that of offering suggestions as to how the paper can be improved sufficiently to warrant publication.

Another point of discussion could be the extent to which the reviewer should adopt a forensic role to determine if the author is attempting to conceal something. As a reviewer, one sometimes gets the feeling that an author is trying to conceal some fundamental flaw in the paper by deliberating writing up the results in a way that hides what has happened, rather than reveal all. A reviewer almost needs to be mindful of the investigative approaches of Sherlock Holmes: “You mentioned your name as if I should recognize it, but I assure you that, beyond obvious facts that you are a bachelor, a solicitor, a Freemason, and an asthmatic, I know nothing whatever about you.” ([Doyle, 1905, pp. 31/32](#)).

There is also little feedback to a reviewer on the quality of his or her review. Only once have I received any comments back on one of my reviews. In most cases, the best that a reviewer can hope to receive is a copy of the editor’s letter to the author and a copy of the other person’s review. There is rarely any communication between the editor and the reviewer on how useful the referee’s review was. There is also little communication from an editor on the reasons for his or her own decision. The most dramatic illustration of this, was once when after having favourably reviewed a paper, I received a note from the editor, saying “thank you for your review, I have rejected the paper”.

A lack of a clear statement about a journal’s perceived raison d’être

As in the section ‘Purpose of publishing in academic accounting journals’ indicated, there is no

clear statement in most journals of the editor's perceived purpose in publishing articles in the journal. As [Appendix A](#) indicates, most statements are simply descriptions of the field in which the journal intends to publish articles. There is no clear statement of the journal's purpose. Using the Eddie Stamp logic from the section 'Purpose of publishing in academic accounting journals', one way of framing the purpose of the journal would be firstly to describe what the role of accounting is, then to describe the role of academic research in relation to its subject matter and finally to indicate how publishing research studies can be expected to benefit the discipline. This lack of a sense of purpose in academic accounting publishing is one of the elements that is most dispiriting. There is a sense in which it has no purpose other than furthering the academic careers of authors. This may be one of the reasons why reviewers focus so much on *r* quality, since they have no clear idea of what *q* is. All reviewers can do is determine whether the research has been properly conducted and analysed. The purposes of enhancing public debate or increasing understanding of an area seem to have got lost. A clearer statement of a journal's purpose might help reviewers and also authors contemplating submitting a manuscript. The issue was highlighted for me as an editor in relation to the publication of articles dealing with the more arcane elements of statistical sampling in auditing. The papers had high *r* content, but as the models were always based on unrealistic error distributions, there was no obvious link to improving the practice of obtaining audit evidence. Belatedly, I now realize that a much clearer statement of what the journal saw as its rationale for existence would have helped both authors and reviewers. There is also the possible improvement in the motivations of researchers to act as reviewers. At the moment, the principal motive seems to be to keep in with the editor so that he or she will view future submissions more kindly, which is a perpetuation of the publishing game. It would be so much more fulfilling if reviewers thought that they were helping improve the discipline and that the journal was a force for public good.

Additional suggestions for improving the process

Several suggestions have been as to how the system of the review process could be improved as well as simply improving the quality of the approach

taken by reviewers. The first suggestion that has been made is that the anonymity of reviewers should be removed ([Epstein, 1995](#)). This could take two forms. Either the names of the reviewers are revealed to the author when the first reviews are sent back or the names of the reviewers appear at the bottom of the published article, but are not disclosed in the event of the article not being published. The argument for the first alternative is that it would prevent reviewers from hiding behind the cloak of anonymity and make them more accountable for their views. The obvious disadvantage is that some academics might be put off reviewing altogether, but then maybe if they are afraid to defend their opinions, they should not be allowed to review. The other potential disadvantage is that it could lead to anodyne reviews, rather like personal references when the individual giving the reference knows that it will be made available to the person on whom he or she is giving the reference. The argument for the second alternative is that it would allow the final published article to be judged more as a joint product between authors, reviewers and editor. Hence, once the reviewer knew that the paper was heading for publication, he or she would be more co-operative in the search for the final publishable article and possibly less negative in their comments. This last suggestion would appear to have some merit and again would benefit from being debated in some forum where the role of reviewers was considered. There is nothing to stop reviewers from implementing this change themselves. It simply requires them to sign the review or indicate to the editor that they are happy for their name to be made known to the author. This could also facilitate direct dialogue between author and reviewer which could speed up the review process and result in a publishable article within a much quicker time frame.

Another possibility is that the journal editors would require submitting authors to suggest at least one reviewer for the paper ([Epstein, 1995](#)). The editor would not necessarily have to select the reviewer and whether or not the reviewer's name was revealed would depend on the policy of the journal. The advantage of this proposal is that it would concentrate authors' minds on identifying the potential merits of the paper and its contribution to the literature. The danger is that authors and nominated reviewers could cluster into groups, with one member nominating another as reviewer on some rotational basis.

Another possibility is raising the costs of submission to a journal and using the money to pay referees for completing their reviews within a certain time-scale. The advantage of this proposal is that it should cut down the length of time that a reviewer takes to send a review to the editor (Engers & Gans, 1998). The disadvantage is that by increasing the costs of journal submission, some academics will be priced out of the publishing market. This would be especially true of academics from poorer countries.

Another suggestion is that journals should create a meaningful appeals procedure for authors who believe that their rejected article has been improperly evaluated (Miner, 2003). Ideally someone other than the original editor should determine whether a request for a re-review should be allowed (Epstein, 1995). Once an appeals procedure has been established, records should be kept of the number of appeals generated by different reviewers. If there is consistency in the nature of complaints about the work of a particular reviewer, the reviewer should be provided with appropriate feedback from the editorial team. In the event that the feedback does not lead to an improvement, the services of the reviewer would be discontinued.

Another idea is to allow authors to provide feedback on the quality of the service that they have received from the review process. This could take consist of standard forms asking questions such as whether the reviews were conscientious, constructive, unbiased, informed and balanced with respect to considering positive as well as negative aspects of the manuscript (Epstein, 1995). The advantage of this proposal is that it would provide reviewers with some feedback and therefore to help them improve their reviewing and reduce the incidence of errors. At present, reviewers can make any sort of statement with impunity and the author is not in a position to challenge it. I remember one paper that I as editor thought had potential and would interest readers, but which had received poor reviews. I wrote an encouraging editorial letter and the author responded that he would resubmit provided I agreed “to protect him from the reviewers”. This suggestion does not require any sort of editorial change since in principle any author can create a response to the points made by the reviewers and ask the editor to forward them to the reviewers. Obviously, such letters will have to be carefully structured to acknowledge the authors appreciation of the time given by the reviewer, but also to make informative and constructive com-

ments about areas where the author disagrees with the comments of the reviewer.

Clearly there is a lot that can be done to improve the present state of publishing in accounting journals. The issue is the extent to which members of the accounting academic community wish to engage in a debate about the nature of the problem and its possible solutions. One suggestion might be to get representatives of the major academic accounting associations across the world to come together and debate these issues. As Ellison (2002a, p. 1025) concludes: “If many social norms are indeed possible, then academic communities may be able to achieve dramatic welfare improvements by simply discussing what standards members would like to have and agreeing on a change”.

Appendix A. Aims/objectives of main academic accounting and auditing journals

Abacus

“ABACUS has as its objective the publication of exploratory constructive and critical articles on all aspects of accounting and on those phases of the theory and administration of organizations and of economic behaviour generally which are related to accounting, finance and business studies.”

Accounting, Auditing and Accountability Journal

“The journal *Accounting, Auditing and Accountability* is dedicated to the advancement of accounting knowledge and provides a forum for the publication of high quality manuscripts concerning the interaction between accounting/auditing and their socio-economic and political environments.”

Accounting and Business Research

“The objective of *Accounting and Business Research* is the publication of substantial and original contributions to knowledge in the areas of accounting broadly defined.”

Accounting and Finance

“The Journal seeks to publish work that develops, tests, or advances accounting and finance theory, research and practice, etc. The primary criterion for publication in *Accounting and Finance*

is the significance of the contribution an article makes to the literature.”

Accounting Horizons

“*Accounting Horizons*’ primary objective is to publish papers focusing on the scholarship of integration and application. Building on the work of the Carnegie Foundation, we can define integration scholarship and application scholarship as follows:

- The *scholarship of integration* asks – What do research results mean to those who might benefit from those discoveries? Is it possible to interpret new discoveries in ways that provide a larger, more comprehensive understanding?
- The *scholarship of application* focuses on engagement and asks – How can knowledge be responsibly applied to consequential problems? Application is not a one-way street, as new intellectual understandings can arise out (of) the very act of application...theory and practice vitally interact and one renews the other.”

Accounting, Organizations and Society

“The aims of the journal are:

1. To provide a specialized forum for the publication of research on the behavioural, organizational and social aspects of accounting.
2. To foster new thinking, research and action on the social and behavioural aspects of accounting.
3. To report on experiments on the behavioural and social aspects of accounting in a way that explains how the experiment was developed; the process by which it was implemented and its consequences, both planned and unplanned.”

Accounting Review

“The *Accounting Review* should be viewed as the premier journal for publishing articles reporting the results of accounting research and explaining and illustrating related research methodology. The scope of acceptable articles should embrace any research methodology and any accounting-related subject, as long as the articles meet the standards established for publication in the journal... The primary, but

not exclusive, audience should be...academicians, graduate students and others interested in accounting research.”

Auditing: a Journal of Practice and Theory

“The purpose of this journal is to contribute to the theory and practice of auditing.”

British Accounting Review

“The Journal seeks to serve the aims of the *British Accounting Association* and the broader international accounting community by publishing research of the highest quality possible and of interest to an international audience.”

Critical Perspectives on Accounting

“*Critical Perspectives on Accounting* aims to provide a forum for a growing number of accounting researchers and practitioners who realize that conventional theory and practice is ill-suited to the challenges of the modern environment, and that accounting practices and corporate behaviour are inextricably connected with many allocative, distributive, social and ecological problems of our era. From such concerns, a new literature is emerging that seeks to reformulate corporate, social and political activity, and the theoretical and practical means by which we apprehend and affect that activity.”

European Accounting Review

“The major aim of the journal is to publish scholarly articles, which fulfil the highest quality standards of academic accounting research... The journal acknowledges its European origins and the distinctive variety of the European accounting research community.”

International Journal of Accounting

“The *International Journal of Accounting (IJA)* aims at publishing accounting research that contributes to the analyses and understanding of international business conditions and transactions. The editors of IJA encourage authors to submit high quality research that studies accounting as a subsystem of the cultural, economic and institutional factors of different nations.”

International Journal of Auditing

“Its primary aim is to communicate clearly to an international readership the results of original auditing research conducted in practice and in research institutions.”

Journal of Accounting and Economics

“The *Journal of Accounting and Economics* encourages the application of economic theory to the explanation of accounting phenomena. The theories of the firm, public choice, government regulation and agency theory, in addition to financial economics can contribute significantly to increasing our understanding of accounting. The JAE provides a forum for the publication of the highest quality manuscripts which employ economic analyses of accounting problems.”

Journal of Accounting Literature

“The objective of the Journal is two-fold: (1) to provide a concise topical synthesis of research to assist academics and others in gaining knowledge and appreciation of accounting research areas outside their own expertise, and (2) to publish papers presented at University of Florida research conferences on selected accounting and auditing issues.”

Journal of Accounting and Public Policy

“The relationship and mutual effects on each other of accounting and public policy is an increasingly important area of research. *Journal of Accounting and Public Policy* serves researchers, educators, policymakers, and practitioners in both disciplines by publishing research results and serving as a forum for the exchange of ideas.”

Journal of Business Finance and Accounting

“The Journal exists to publish high quality research papers in Finance and economic aspects of Accounting... A distinctive feature of the Journal is that it recognises that adverse selection and moral hazard issues are pervasive in financial markets and business organisations, and that Accounting (both financial and managerial) plays a part in ameliorating the problems arising from such informational problems. Thus, the editors see Accounting and Finance as being conceptually interlinked.”

Appendix B. Recommended best practice for getting articles published “Work, finish, publish

The guidance to academic faculty on how to get published in the social sciences represents a considerable publication industry. There are books showing how it can be done, such as [Cameran \(1988\)](#) and [Huff \(1999\)](#) offering a step-by-step approach to the writing and publishing process; [Cummings and Frost \(1995\)](#) setting out the issues, challenges and emotions generated by the process of becoming an organizational researcher; and collections of essays such as [Frost and Taylor \(1996\)](#) organized around the metaphor of the rhythms of academic careers, reflecting a belief that there are multiple points during professional development. There are also many personal statements by experienced academics. These can appear in an edited book such as [Simon and Fyfe \(1994\)](#), which contains the recollections, interpretations and advice of 14 former and current editors of social science journals, special series publishers and university presses. Alternatively, editors and experienced researcher write journal articles on the subject of publication and rejection (e.g., [Armstrong, 2006](#); [Cady, 2001, 2002a, 2002b](#); [Clark, Floyd, & Wright, 2006](#); [Czyzewski & Dickinson, 1990](#); [De Lange, 2005](#); [Estrin, 1981](#); [Frankfurter, 2000](#); [Gilmore, Carson, & Perry, 2006](#); [Homburg, 2003](#); [Howard & Stout, 2006](#); [Jönsson, 2006](#); [Kacmar, 2002](#); [Locker, Reinsch, Dulek, & Flatley, 1994](#); [Neuhauser, McEachern, Zyzanski, Flocke, & Williams, 2000](#); [Parasuraman, 2003](#); [Perry, Carson, & Gilmore, 2003](#); [Rynes, 2002](#); [Starbuck, 2003](#); [Stewart, 2002](#); [Stout, Rebele, & Howard, 2006](#); [Summers, 2001](#); [Swartz, 1999](#); [Toppins, Henson, & Solezio, 1988](#); [Walker, 2004](#)).

The general advice that all this accumulated wisdom offers is remarkably similar and can be distilled down into the following general rules of publication:

1. Understand that a long-term planning horizon and constancy of purpose are essential. Included in this long-term planning is taking a view on whether the would-be author wants to maximise the quantity of publications by having a large number of publications in lesser-known journals (an *r*-strategist) or maximising quality by concentrating most of their publications in top journals (a *K*-strategist) ([Faria, 2003](#)).

2. Develop a broad set of methodological skills (e.g. qualitative research methods, survey research, experimental design, econometric analysis, etc.) which will provide long term benefits as one can rely on this same set of skills for many years.
3. Learn to be a critical reader of the literature looking for problems with the theory, data collection, analysis or conclusions.
4. Use the literature to stimulate one's own thinking beyond merely understanding what is covered in each of the individual articles reviewed.
5. Write down one's ideas to avoid losing them and also to test their logic as this is more apparent when they are summarised – "How can I know what I think until I see what I say?" (quoted in Jönsson (2006, p. 480).
6. Make academic writing a habit and construct a routine that suits one's own preferences.
7. Focus on an interesting problems to research, by considering if there are gaps in existing knowledge or new areas of research created by recent developments (e.g. changes in market conditions, new regulations, etc.). Voss (2003) suggests that innovation in research can be characterised in terms of increasing interest as: *incremental* (refining and extending an established design); *modular* (replacing one or more core concepts without changing the system's architecture); *architectural* (new interactions and linkages between core concepts are introduced); and *radical* (a dominant new design is created by incorporating links between new core concepts).
8. Focus on developing hypotheses to be tested. These should contribute to the literature and should lead to a feasible rigorous research design to test them.
9. Work with others, since the process of collaboration allows ideas to be shared and tested.
10. Work on several manuscripts at a time, since the publishing process is a long one.
11. Target journals to ensure that the manuscripts pass the test of being appropriate for the journal.
12. Make the effort to write well in English. For those for whom English is not their first language, it is better to write first in bad English which is then corrected than to write in the home language and then to translate it.
13. Learn how to present information clearly, including the appropriate use of tables and footnotes. Papers should follow the journal format and usually should consist of at least six parts: introduction (what is the paper about?), background/literature review, theoretical framework leading to a statement of research purpose (hypotheses), data creation, analysis of data, discussion and conclusions (why does the paper matter?).
14. Recognize that the bibliography in a paper is an indication to the editor of possible reviewers.
15. Learn how to deal with US journals, when reporting non-US results, since US reviewers will need convincing that the paper is relevant to a US audience. Homburg (2003) also makes the point that US reviewers may not understand A4 sized paper and he says that he always sends papers to US journals on US paper format.
16. Get colleagues to read and critique manuscripts. Brown (2005) shows the importance of circulating and presenting manuscripts by finding that the probability of acceptance in leading journals increases the more that the papers are presented at workshops.
17. Ensure that the paper conforms to the journal's house style, before sending it off. Follow the journal submission rules.
18. Do not send the same article to more than one journal.
19. Learn to absorb the slings and arrows of fortune that are the comments by reviewers. Try to incorporate as many of the reviewers' points as possible. If a small minority appear to be "off track" and they cannot be incorporated without seriously affecting the flow of the article, then a polite note is necessary for the reviewers. In general, a polite letter should be sent in which the author explains how each point made by the reviewer has been used to improve the paper.
20. Expect to act as a reviewer for the journal to which you have submitted articles as editors are continually searching for reviewers who will deliver insightful reviews within a short timescale. Potential authors have an incentive to act as good reviewers.
21. Learn to accept rejection, as appeals to an editor are rarely successful. The following are the types of reasons given for rejection: poor motivation, not consistent with the journal's objec-

tives, unreliable or inappropriate theory, measurement issues, statistical issues, not a meaningful contribution to the literature, poorly organized paper, badly written paper, failure to take account of the reviewers' concerns (for resubmissions) and the results already published elsewhere.

22. Analyse the reasons for rejection. Consider whether a new paper can be created from the reject or whether a revised version of the paper could be published elsewhere. It is not a good idea simply to send the original manuscript off to another journal.
23. Realize that a resubmission offer is no guarantee of acceptance. "Once I submitted a paper three times to a journal, based on the reviewer's encouragement. Each time, the review was less enthusiastic than the time before. After three submissions, the paper was rejected." (Frankfurter, 2000, p. 305).
24. Recognize that authors' motivation and belief in their work play large parts in determining whether their manuscripts make it into print. There is certain Darwinian process of "survival of the fittest" at work.

Appendix C. Recommended best practice for acting as a reviewer

A number of reviewers have written articles about the review process in an effort to improve it, such as Bedeian (2003), Brinn et al. (1998), Epstein (1995), Harrison (2002), Holbrook (1986), Miner (2003), Omer, Porter, Yetman, and Magro (2004), Singh (2003), Summers (2001) and Woodruff (2003). A summary of these points would suggest that a good reviewer should:

1. Inform the editor if he or she knows the identity of the author, but still review the paper.
2. Be professionally qualified to undertake the review, by having a sufficient knowledge of the subject of the paper, he or she has been asked to review. If this is not the case, then the good reviewer will send the paper back to the editor.
3. Set aside enough time to read the entire article in depth in one sitting.
4. Be aware of any paradigm preferences, which might influence judgement and lead to the rejection of a paper because a particular direction of inquiry is not interesting to the reviewer.

5. Struggle to find a way to make the review positive in tone. Even when finding fault, use adjectives like *interesting*, *imaginative* and *ambitious*.
6. Ask for additional material that will strengthen the paper, not for some self-interested reason.
7. Allow for the possibility of no revisions in those rare cases where the law of diminishing returns would render any further work of minimal incremental value.
8. When making extensive or detailed suggestions, try to create a list of necessary changes the first time to avoid discovering more and more new suggestions on each succeeding revision of the paper. However, a good reviewer will allow the authors some flexibility to write the article that they want to write.
9. Distinguish between correctable and uncorrectable flaws.
10. For correctable flaws, indicate what might be done to fix them.
11. For uncorrectable flaws, indicate which should be discussed in the Limitations section of the paper.
12. When recommending rejection of an article, specify the reasons (e.g. uncorrectable flaws) and provide a convincing argument as to why these flaws justify rejecting the manuscript.
13. When not too time consuming, direct the authors to articles or books that may be useful to them in revising their manuscripts.
14. When receiving a revised submission, pay attention to what the other reviewer and the editor has had to say.
15. Produce reviews within a reasonable span of time (i.e. within 30 days).

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