Prose, psychopaths and persistence: Personal perspectives on publishing

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Abstract

The process of attempting to publish a paper in a refereed journal can be rather stressful. This paper presents a number of personal reflections on the publishing process, with the aim of helping aspiring journal authors to appreciate the nature of the challenge, and some of the requisites for success. The challenges in dealing with referees include the element of luck involved in securing sympathetic referees, the poor quality of the reports prepared by some referees, and the slowness of the review and editorial process. A number of examples from my experiences in agricultural economics journals are presented. These reveal that one of the most important characteristics which a journal author needs is persistence.

Introduction

Last week I received a document sized letter from a journal. It was from a journal to which I had submitted a research paper for publication about six months previously, so I expected (correctly) that the envelope contained news about the fate of that paper. I opened the envelope with the usual mix of anticipation and foreboding, hoping for the best, but fearing the worst. A quick scan of the editor's letter revealed that the news was bad. He suggested other journals to which I should consider sending the paper, and made it clear that he would not welcome a resubmission of the paper to his journal. When I looked at the two referees' reports, I found that one had a reasonably positive attitude to the paper, but the other was very negative indeed.

As an experienced publisher of journal articles, I believe I can judge the relative quality of my papers fairly well. They are not all great, but some are better than others, I would definitely say that this rejected paper is among the best I have written. However, it is a little unusual is some respects, and its conclusions are at odds with what some previous researchers have found. Perhaps I have trodden on the toes of the second reviewer. In any case, I now have to begin again, and the eventual publication of the paper has been significantly delayed. Worse than that in the short term is that I've been stewing on some of the comments of the second reviewer. I really cannot believe that he or she has read the paper properly. I feel frustrated, a bit disheartened and, at times, angry.

This is all perfectly normal. Publishing in reviewed journals is an important aim for all serious researchers. It provides scrutiny, recognition, rewards and a vehicle for wide communication, but it does not come easily. The process is slow, heartless and somewhat random. Given the importance of the process, and its obvious weaknesses, it is remarkable how little writing and reflection there has been about it.

My aims in this paper are (a) to provide encouragement and advice to researchers who are inexperienced with the publishing process, or to more experienced researchers who have experienced difficulties with it in the past, and (b) to share some true stories of amazing things that have happened to me in the course of trying to publish. My pursuit of aim (b) should, I believe, reinforce aim (a) by showing readers that appalling ill-luck and/or ill-treatment is a completely normal and unavoidable aspect of publishing, and not necessarily a reflection of one's own inadequacies.

The paper includes three main parts. The briefest section, "Prose", is about the process of writing. There is plenty already written about that, so not much is needed from me. Then there is "Psychopaths", which is about the process of reviewing or refereeing papers. Finally I will emphasise the important of "Persistence", which can be required for even the best papers (some would say, especially for the best papers).

Prose

I have either co-written or supervised the writing of papers, reports, and theses with over 100 different people. One prominent observation from this experience is that most people do not find writing easy. Some people suffer agonies just getting things down on paper. Others are able to produce volumes of writing reasonably easily, but have to work very hard (or require considerable help) to bring it up to a publishable standard. Of the 100 odd people whose writing process I have experienced directly, I would say that somewhat fewer than 10 were able to produce good, clear, readable text without a struggle of one sort or another.

So if you are among the majority, take heart in that knowledge. Take heart, also, from another observation: writing is a skill which improves with practice. For some people I have worked with, the improvement over time has been remarkably dramatic.

For those at the beginning of that process of improvement, writing as part of a team is an excellent way of acquiring experience and honing your skills. In my experience, smaller teams are more enjoyable and more productive. Large teams can get bogged down with the process of coordination and communication and keeping everybody happy. Nevertheless, it is certainly essential that everyone who has contributed sufficiently to the research should be acknowledged with co-authorship. In some sciences, this can result in some extraordinarily long list of authors. The longest I have seen is the following:

Nature Contents: 13 Jul 2000 Volume 406 No. 6792, p.151
The genome sequence of the plant pathogen Xylella fastidiosa
A. J. G. Simpson, F.C. Reinach, P. Arruda, F. A. Abreu, M. Acencio, R. Alvarenga, L. M. C. Alves, J. E. Araya, G. S. Baia, C. S. Baptista, M. H. Barros, E. D. Bonaccorsi, S. Bordin, J. M. Bové, M. R. S. Briones, M. R. P. Bueno, A. A. Camargo, L. E. A. Camargo, D. M. Carraro, H. Carrer, N. B. Colauto, C. Colombo, F. F. Costa, M. C. R. Costa, C. M. Costa-Neto, L. L. Coutinho, M. Cristofani, E. Dias-Neto, C. Docena, H. El-Dorry, A. P. Facincani, A. J. S. Ferreira, V. C. A. Ferreira, J. A. Ferro, J. S. Fraga, S. C. França, M. C. Franco, M. Frohme, L. R. Furlan, M. Garnier, G. H. Goldman, M. H. S. Goldman, S. L. Gomes, A. Gruber, P. L. Ho, J. D.

Hoheisel, M. L. Junqueira, E. L. Kemper, J. P. Kitajima, J. E. Krieger, E. E. Kuramae, F. Laigret, M. R. Lambais, L. C. C. Leite, E. G. M. Lemos, M. V. F. Lemos, S. A. Lopes, C. R. Lopes, J. A. Machado, M. A. Machado, A. M. B. N. Madeira, H. M. F. Madeira, C. L. Marino, M. V. Marques, E. A. L. Martins, E. M. F. Martins, A. Y. Matsukuma, C. F. M. Menck, E. C. Miracca, C. Y. Miyaki, C. B. Monteiro-Vitorello, D. H. Moon, M. A. Nagai, A. L. T. O. Nascimento, L. E. S. Netto, A. Nhani, F. G. Nobrega, L. R. Nunes, M. A. Oliveira, M. C. De Oliveira, R. C. De Oliveira, D. A. Palmieri, A. Paris, B. R. Peixoto, G. A. G. Pereira, H. A. Pereira, J. B. Pesquero, R. B. Quaggio, P. G. Roberto, V. Rodrigues, A. J. De M. Rosa, V. E. De Rosa, R. G. De Sá, R. V. Santelli, H. E. Sawasaki, A. C. R. Da Silva, A. M. Da Silva, F. R. Da Silva, W. A. Silva, J. F. Da Silveira, M. L. Z. Silvestri, W. J. Siqueira, A. A. De Souza, A. P. De Souza, M. F. Terenzi, D. Truffi, S. M. Tsai, M. H. Tsuhako, H. Vallada, M. A. Van Sluys, S. Verjovski-Almeida, A. L. Vettore, M. A. Zago, M. Zatz, J. Meidanis and J. C. Setubal

It is amusing to notice an embedded system of hierarchy in that list of authors. One might guess that Simpson and Reinach did most of the real work, that the next 112 authors made some direct or indirect contribution, and that Meidanis and Setubal would have been left off the list entirely if Simpson and Reinach had thought they could get away with it.

Further advice on writing aspects are provided by Fischer and Lawrence (1997) and in more general texts such as Lindsay (1994), Van Leunen (1992), Booth et al. (1995), and Becker (1986).

Psychopaths

Most of the pain and suffering in publishing arises in the process of refereeing. Indeed I would venture to say that this would even be true for people who find the writing itself difficult. From an author's perspective, the problems with refereeing include the following.

- The process seems to involve a high degree of randomness, so that acceptance of rejection of your paper depends on good or bad luck, to an unfortunate extent.
- Some referees do a poor job of it.
- Refereeing can be very slow.

Some reflections on these points follow, illustrated with examples from my own experience.

Randomness

The sheer unpredictability of what referees will say still manages to take my breath away at times. It can be particularly entertaining when referees for the same paper are diametrically opposed in their views.

> Pannell, D.J. and Falconer, D.A. (1988). The relative contributions to profit of fixed and applied nitrogen in a crop-livestock farm system, *Agricultural Systems* 26(1): 1-17.

This paper was originally submitted to the *Australian Journal of Agricultural Research*. On 10 October 1986 I received two reviews from that journal which included the following criticisms.

Referee 1: 'This paper falls into the class of papers that can be denominated as "measurement without theory"'.

Referee 2: 'This paper, as it stands, is purely theoretical'.

The paper was rejected.

Sometimes, the entertainment comes from comparing the attitudes of referees for one journal with those of a subsequent journal.

Pannell, D.J. (1994). The value of information in herbicide decision making for weed control in Australian wheat crops. *Journal of Agricultural and Resource Economics* 19(2): 366-381.

20 June 1991: Submitted to the *American Journal of Agricultural Economics*
13 September 1991: Rejected, following very negative reviews, citing lack of substance and lack of originality

30 September 1993: Submitted in unaltered form to the *Journal of Agricultural and Resource Economics*
4 February 1994: Accepted with considerable enthusiasm and with no revisions (the only time I have had that pleasure).

To compound the injury of their rejection of this paper, the *American Journal of Agricultural Economics*, soon after published a paper by Swinton and King (1994) on a strikingly similar topic: "The value of pest information in a dynamic setting: the case of weed control". This is a perfectly good paper, but, I sincerely believe, one with no more originality than my paper which was rejected for lack of originality. Perhaps their paper was better written than mine, or perhaps they were just luckier with their referees.

An even more remarkable example of different responses to the same paper by different journals was the following.

Vercammen, J. and Pannell, D.J. (2000). The economics of crop hail insurance. *Canadian Journal of Agriculural Economics* 48:87-98.

Originally submitted to the *American Journal of Agricultural Economics.* Rejected with no option to resubmit following highly critical responses by the reviewers.

Slightly modified and submitted to the *Canadian Journal of Agriculural Economics.* Accepted with minimal changes. Subsequently received the award of the Canadian Agricultural Economics Society for best journal article of the year.

My delight at this wonderful example of the capriciousness of the publishing game was perhaps a little moderated by a half-joking comment from my friend Julian Alston who said that, "there isn't necessarily any inconsistency in that" (referring to the greater prestige of the first journal).

On the other hand, I can take heart in the experiences of far more eminent economists than myself, as documented by Shephard (1994), with selected highlights presented by Gans and Shepherd (1994).

For example, "In 1923, Bertil Ohlin submitted to the *Economic Journal* a paper that introduced the factor proportions theorem to international economics. The theorem eventually earned Ohlin a Nobel Prize. [Editor John Maynard] Keynes returned the manuscript with a blunt rejection note: 'This amounts to nothing and should be refused'" (Gans and Shepherd, 1994, pp. 174-175).

Bad or unreasonable referees

If a referee says your paper is insufficiently interesting or original to deserve a place among the pantheon in Journal X, it is usually hard to argue. Such judgements are subjective and personal. Sometimes, however, referees write what is obviously nonsense.

> Pannell, D.J. and Falconer, D.A. (1988). The relative contributions to profit of fixed and applied nitrogen in a crop-livestock farm system, *Agricultural Systems* 26(1): 1-17.
>
> Submitted to *Agricultural Systems*.
>
> Referee 2 (20 February 1987): 'The conclusion that "yield boost" was more important than nitrogen fixation seems to be built into the model. It may or may not be correct but it is not really a product of the model. Similarly, the conclusion that lupins are a more important source of nitrogen than pastures [is] better demonstrated by experimental evidence than models.'
>
> My response to the editor (27 March 1987): 'The criticisms that conclusions are "built into the model" and are "not really a product of the model" are nonsensical. Of course, results of the study depend on the data used. How could it be otherwise? But, conclusions are a product of the model in as much as it provides an economic interpretation of the technical and biological data. In the statement that the conclusions reached are "better demonstrated by empirical evidence than by models", the referee seems to deny the importance of placing an economic interpretation on experimental results. It should hardly need saying that no amount of experimental work can, of itself, provide the answers to economic questions.'
>
> The editor accepted the paper without requiring further revision, 15 May 1987.

The following example did not have such a happy ending, until I changed journals.

> Pannell, D.J. (1995). Optimal herbicide strategies for weed control under risk aversion. *Review of Agricultural Economics* 17: 337-350.
>
> 2 October 1990, submitted to the *American Journal of Agricultural Economics.*

26 December 1990, Referee 1 made some constructive suggestions. Referees 2 obviously hates the paper, primarily because "I don't think [its main] point is sufficiently worth making to be the primary focus of a paper, particularly one which has some technical problems." Although the paper's point seemed obvious to Referee 2, it clearly was not so obvious to numerous previous writers on pest and weed economics, whose results it directly contradicted. Despite the referee's comment, no genuine technical problems were identified.

31 May 1991, revised paper resubmitted

3 September 1991, Referee 1 is happy. Referee 2 still hates it. Makes no comment about the so-called technical problems he or she identified last time (which I've shown were not errors at all), but introduces a brand new set of technical problems, which again have no foundation. Most amazing is the claim that one conclusion depends on an assumption that a covariance is positive and that this assumption "is flat wrong, because the covariance between terms is necessarily negative." The two terms in question were basically $k_1.W$ and $k_2.W^2$ where k_1 and k_2 are constants and W is a stochastic weed density (which can only be positive). The fact that these terms would have a positive relationship is so obvious that it hurts.

It hurt even more when, after resubmission, the referee again made no acknowledgment that the so-called errors in the paper were actually non-errors, conveniently ignoring the whole issue, while re-asserting disdain for the paper in very general terms. A third reviewer brought in to adjudicate was, unfortunately, not impressed by the paper and on 27 December 1993 it was finally rejected for good.

Subsequently it was published in the *Review of Agricultural Economics* with little trouble.

That was a particularly upsetting experience. I put in an enormous amount of work in the two rounds of revisions to the *American Journal of Agricultural Economics* to try to placate Referee 2, but could only feel that the referee was being extremely unreasonable and almost dishonest. Usually revising a paper does actually improve it and increase its chances of subsequent publication, but in this case the comments were so unreasonable that the revisions did nothing to improve the paper, in my view. The other feeling I had was one of powerlessness. Even though I was so obviously right and the referee so obviously wrong, I felt like my punches were landing on smoke. I had to be accountable for every tiny issue raised by the referee, but the referee could blithely ignore his or her past erroneous comments as if they had never happened, and still win the battle.

It did at least teach me a lot about the refereeing process, and thicken my hide for subsequent encounters. One certainly does need a thick hide. I have found that many referees are needlessly harsh and heartless. Too many seem to take the view that their role is to demolish a paper if possible. Even where referees are trying to be constructive, it is very easy for an author to become disheartened at the poor reception their work has received. I believe it is very important for referees to consciously highlight positive aspects of papers, not just negative. If you are writing a referee's report and you have something particularly cutting to say, it is far better to put it in a separate letter to the editor, rather than in the report which will be read by the author. More lengthly advice about preparing referees reports is available at http://www.ag.iastate.edu/journals/rie/howr.htm.

A sequence of particularly brutal referees at one point prompted the following outpouring.

I'm The Referee

David J. Pannell

You've posted in your paper
To a journal of repute
And you're hoping that the referees
Won’t send you down the chute

You'd better not build up a sense of
False security
I've just received your manuscript and
I'm the referee

This power's a revelation
I'm so glad it's come to me
I can be a total bastard with
Complete impunity

You might have won a Nobel Prize
It matters not to me
My savage wit will not remit when
I'm the referee

I used to be a psychopath
But never more will be
I can deal with my frustrations now that
I'm a referee

My final example for this section relates to a very unusual experience.

Xxxx. (19XX). Xxxx xxxx, *Journal of X*, vol X, pp. XXX-XXX.

I submitted the paper to a journal and, in due course, received mixed referees’ reports and a rejection.

I submitted it to a different journal and waited for the reviews. Not long after, a colleague who knew of my paper told me that he thought he had received it to review (it was anonymous, so he wasn’t sure). It had come from the very journal to which I had re-submitted. I was surprised that the editor would send it to someone so closely connected with me, and asked to see the paper to check if it really was mine. It was not, but it included a significant degree of overlap with my paper. I considered this an amazing coincidence, and wrote to the editor suggesting that the authors of the two submitted papers should combine efforts and produce a single paper. He put us in touch, and we completed the paper, which was subsequently accepted for publication. A happy ending.

However, my coauthor later casually commented to me that the idea to write the second paper was a result of refereeing my paper at the first journal. I was upset, and said so. The response I received, however, was that the original paper would not have been publishable, and that the additional ideas that had been included were decisive in making it a publishable paper. This is obviously a subjective judgement.

I felt most unhappy about this experience, but also somewhat lucky. It was only through the most amazing luck that I learned of the other paper in time to claim at least a share of the glory of publication. Without that luck, I would almost certainly have been unable to publish my paper, which was less advanced in the review process when I discovered the other one. I feel that the reviewer should have approached the editor of the original journal (as I did to the second journal) proposing a joint paper.

I have meditated on what the lessons from this experience might be. As an author, I think that the lessons are (a) that you should make sure that any truly original ideas you have are immediately documented in a form which will allow you to prove their source and timing in case of a later dispute, and (b) that you should get on and publish your work as soon as possible. As a referee, I believe the story highlights the importance of scrupulous honesty. I believe it is also beholden on referees to be at least a little generous in the advice they give.

It's usually not as bad as it seems

In reading over a large number of old referee's reports in the course of preparing this paper, I found myself becoming more and more depressed. Partly, I was re-opening old wounds, and partly it was the consistently negative tone and sentiments I read about my work. It is common to feel that the task of revising a paper sufficiently to satisfy a reviewer will be enormous. Nevertheless, I have found that the level of time and effort required to revise a paper in response to a referee's comments is almost always much less than I expect it to be.

It is certainly also the case that responding to referees' reports usually improves the paper. In particular, I have found that it improves the clarity and readability of the paper, and removes sources of confusion or misunderstanding. Often the most annoying and frustrating referee's comments end up revealing to you that you have not expressed yourself clearly enough, and a better paper results.

Slowness

The process of publishing in a journal is slow, and lags in the refereeing process contribute significantly to that slowness. Hameresh (1994) tracked the refereeing process in a number of economics journals. Among those referees who did eventually provide a report, the median lag length was six weeks, and 75 per cent had responded by the tenth week. However 10 percent took more than four months to respond, and a few took more than six months. However, the process is generally slower than these numbers might suggest, because it takes as long as the *slowest* referee. About 17 per cent of potential referees refuse to provide a report. Although they generally do this fairly quickly, it does slow down the process. More seriously, about 5 per cent do not refuse, but never actually provide a report. Most of these repeatedly promised a report, but repeatedly failed to meet that promise.

In 1994 I started to keep records of when my papers were submitted, reviewed, resubmitted and rejected or accepted. I use these records below to illustrate the lag lengths which are common in agricultural and resource economics (and some related journals). The sample of papers included in the different tables and figures differs because I did not keep complete data for all of them.

The results are for 22 papers, all (finally) published between 1998 and 2001. Each observation in the data set is a submission, which ends either in acceptance or rejection. Three of the papers had previously been rejected once, and one (perhaps the best of them) had been rejected twice prior to eventual publication. Hence the full data set includes 27 observations. The journals included in the data set were Australian Journal of Agricultural and Resource Economics (6 observations), American Journal of Agricultural Economics (2), Agricultural Economics (2), Agricultural Systems (2), Australian Journal of Experimental Agriculture (2), Journal of Agricultural Education and Extension (2), Journal of Agricultural Economics, Review of Agricultural Economics, Canadian Journal of Agricultural Economics, Ecological Economics, Australasian Agribusiness Review, Agricultural Water Management, Journal of Sustainable Agriculture, Experimental Agriculture, Rural Society, Australian Journal of Soil Research, Agroforestry Systems, and the Review of Marketing and Agricultural Economics.

Figure 1 shows the total time lag between submission and the receipt of the editor's final decision. It varies from 94 days to 893 days. Only seven of the 27 submissions were accepted or rejected within 30 weeks, and the average for all 27 was 53 weeks.

Figure 1. Total time (days) between submission of an article and the receipt of editor's decision

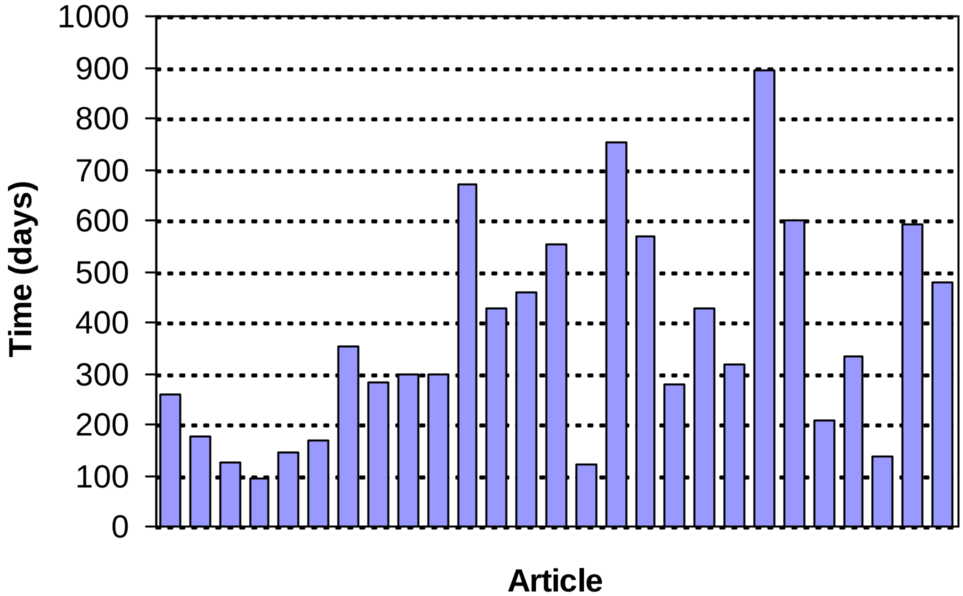
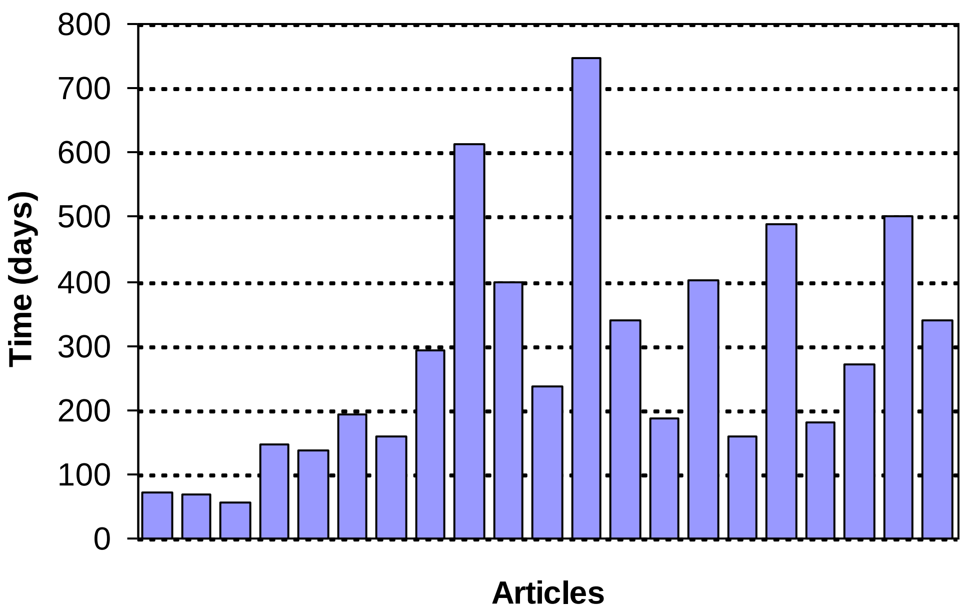


Figure 2. Time (days) between submission of an article and the receipt of editor's decision, excluding time spent under revision before resubmission



For 21 of the submissions, available data allowed exclusion of the time when the paper was under revision (between receipt of referees reports and resubmission of the revised paper). With this element excluded, Figure 2 shows the time lags which are out of the control of the author. They range from 55 days to 756 days (over two years), with an average of 41 weeks.

Figure 3. Break down of lags in the publication process for sample of 17 papers.

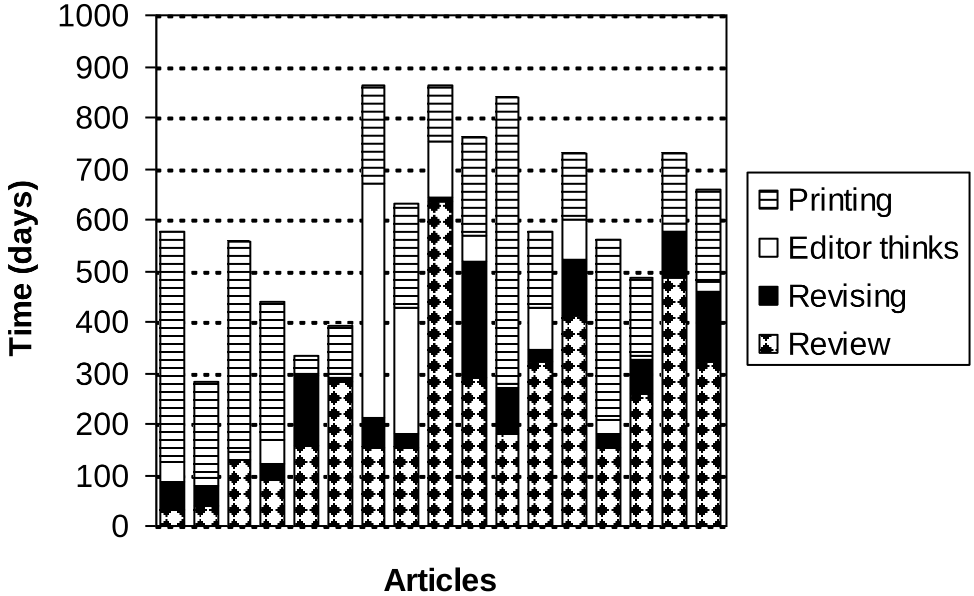


Table 1. Break down of lags in the publication process for sample of 17 papers.

	Review	Revising	Editor thinks	Printing	Total
1	31	57	37	451	576
2	39	39	16	187	281
3	131	0	14	413	558
4	92	30	45	273	440
5	157	138	2	36	333
6	284	5	9	94	392
7	153	59	459	190	861
8	151	31	246	205	633
9	635	8	111	110	864
10	289	229	51	192	761
11	181	91	7	560	839
12	321	25	80	151	577
13	412	110	77	131	730
14	151	28	30	353	562
15	260	64	10	151	485
16	485	91	15	139	730
17	322	137	18	182	659
Average (days)	241	67	72	225	605

Figures 1 and 2 deal only with the review and editorial decision process. Once a paper is accepted, there can be a further lengthly delay until it actually appears in print. Figure 3 and Table 1 show the breakdown of time lags into four stages: under review, under revision, awaiting the editor's final decision, and awaiting printing. For these purposes, where papers were reviewed twice for the same journal (which occurred four times in this sample), the combined duration of both reviews is reported.

For this sample of papers, the average time awaiting printing (over seven months) was only a little shorter than the average time spent under review. The author(s) and the editor each contributed a little over two months to the process. The average total lag from initial submission to receipt of reprints was 605 days, or almost 20 months.

Persistence

As we've seen, different referees can hold remarkably different opinions about the same paper, so that the editor's selection of referees is crucial to your chances of success. Suppose there is a 1 in 3 probability that a referee chosen at random will consider your paper to be bad, OK or good. For many publishable papers, this is probably not unrealistic. Table 2 shows one randomly generated sequence of sets of referees reports for your paper (the second random sample that I generated), assuming that the journals to which you submit use three referees. The first two journals each received two negative reports about your article. At best, you would have to persist until the third or perhaps even the fourth journal to get this paper published. If the editors gave greater weight to negative reports than to positive ones (which often seems to be the case), you might have to persist until a tenth submission in order to experience the joy of receiving no negative reviews. Whether your constitution could withstand the trauma of journals 5 and 8 and persist until journal 10 is very doubtful.

The problem is sample bias. We recognise the possibility of sample bias in our design of experiments and surveys, and the problem is identical in refereeing. Even if your paper is truly a good one, there is every chance that such a small random sample of referees will consider it a poor one. There is nothing much we can do about this, other than recognising it as a reality, and being prepared to persist with the paper until it is accepted somewhere. According to Hamermesh (1994) the rejection rate at leading economics journals is about 90 percent, which is interestingly similar to the probability of getting at least one bad reviews in the above example.

Table 2. Illustrative hypothetical sequence of referees reports for a paper submitted to 10 journals in succession, based on equal probabilities of bad, OK and good reviews.

Journal	Referee 1	Referee 2	Referee 3
1	Good	Bad	Bad
2	Good	Bad	Bad
3	OK	Good	Bad
4	Good	OK	Bad
5	Bad	Bad	Bad
6	Bad	Good	OK
7	Good	OK	Bad
8	Bad	Bad	Bad
9	Bad	Good	OK
10	OK	Good	Good

Gans and Shepherd (1994) present an amazing and entertaining set of stories of rejection of papers by outstanding economists, all Nobel Laureates or Clark Medalists. Some of the most cited and celebrated economics papers of all time have only seen the light of day because their authors did not give up in the face of repeated rejections. George Akerlof's wonderful 1970 paper on the economics of information, "The market for 'lemons'", was rejected by

- the *American Economics Review* ("I got a reply from the editor which said that the article was interesting but the AER did not publish such trivial stuff");
- the *Journal of Political Economy* ("the JPE's referee's report asserted the opposite: that the paper was too general to be true"); and
- the *Review of Economic Studies* (again "it was rejected on the grounds that it was trivial".)

Finally the *Quarterly Journal of Economics* accepted the paper with some enthusiasm.

The following case study from my own experience further illustrates the level of persistence which can be required.

> Pannell, D.J., Malcolm, L.R. and Kingwell, R.S. (2000). Are we risking too much? Perspectives on risk in farm modelling. *Agricultural Economics* 23(1): 69-78.
>
> First draft written in Jan 1995 for the AARES Conference in Perth. Submitted to *American Journal of Agricultural Economics* 7 August 1995. Returned 23 January 1996 with one positive, and two mixed reviews. Revised and resubmitted 4 December 1996. Second round of Referees' comments received 10 February 1997. Received one extremely positive review (by far the most positive review I've ever had or ever seen: "I strongly recommend publishing. I think this article is in the spirit (if not the substance) of Copernicus and the journal should be proud to publish it."). The other two reviewers got much more negative about the paper the second time around. They seemed to mistake the main point and as a result claimed it had already been made in the literature, which is not true. As a result it was rejected outright.
>
> Submitted to *Journal of Agricultural Economics* November 1997. Rejected March 1998 with pretty negative comments in the one report we were sent, and with the referee again missing (or perhaps ignoring) the main point of the paper.
>
> Submitted to *Agricultural Economics* March 1998. Asked for information about the refereeing progress in March 1999. Received advice that they were looking into it. Between April 1999 and August 1999 sent 11 emails to the editor or the editor's assistant asking about progress. Most of these emails were ignored by the editorial office. On 1 September 1999 I sent an email threatening to withdraw the paper if we did not receive a response within one month. After more than two months, no response was forthcoming. On 8 November 1999 I sent another email to the editor advising him that we withdraw the paper. On 9 November 1999 received an email from a new editorial assistant saying that the problem was the old editorial assistant and please give them one more chance. However, he said, they have lost both the paper and the one review that they had received (which was positive) so please send the paper again and we'll start from stratch. On 10 November 1999, after consulting with Bill and Ross, re-submitted the paper to them by email. This is called persistence.

On 30 November 1999 received an email advising that they had found the lost review and received a second one and sent me a letter saying "revise and resubmit", with strong positive signals about the likelihood of acceptance. Received the letter 10 December 1999 saying that it is accepted subject to very minor modification. Revised and resubmitted 18 December 1999. Received assignment of copyright form from the publisher on 24 March 2000, so it obviously had been accepted, even though the editor had not advised us of this. Received letter of acceptance from editor on 7 April 2000. Reprints received 26 July 2000, five and a half years after starting to write it.

Our persistence with this paper was rewarded, not just with eventual publication, but with it becoming one of the most requested articles in *Agricultural Economics* for 2000 (ranked sixth out of 54, with 252 requests, only five requests behind fourth).

Although persistence can at least ensure that good papers are eventually published, the authors of rejected papers can be harmed by means other than non-publication. Akerlof's understandable response to the trauma of publishing "The market for lemons" was to be discouraged about the topic and to underestimate the importance of the topic and its potential for further work. 'I do think its early rocky reception did have an effect on my own work. It was not until 1973 when I spent 6 months on sabbatical in England that I realised that quite a few people had read the paper and even liked it. I believe I would have done follow up work on the market for lemons sooner if I had not been made to feel lucky just to have it published at all. (I must say that I still feel very lucky that it was published.)' (Gans and Shepherd, 1994, p. 171)

For some papers, the delay in publication caused by rejection can put at risk the author's claim to being the original source of the ideas. Gans and Shepherd (1994) document several examples of this type. In my own experience, I was perhaps lucky that publication of Pannell (1994) was not nipped in the bud by the earlier publication of Swinton and King (1994) (even though its original submission date was later). In another example, the original conference version of Pannell, Malcolm and Kingwell (2000) had been cited for some years before it finally reached print in a journal. Most strikingly, Marshall, Jones and Wall (1997) undertook an empirical test of the propositions of our paper, concluding that we were correct, and succeeding in making it into print almost three years before the paper which inspired theirs.

Conclusion

The key message of this paper is that publishing in refereed journals requires fortitude, resilience and persistence. You should not be too discouraged about the negative responses of referees, because even the most celebrated researchers have suffered similar slings and arrow. You should not give up on a paper which you continue to believe has merit, despite it being rejected, because some of the most celebrated papers have suffered through several rounds of rejection before finally being accepted. Of course one should consider the possibility that you paper is, in fact, not worth persisting with, but do not be too quick to leap to this conclusion.

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