Mourning Edition*

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January 24, 2020

Abstract

Journal of Economic Literature Classification Numbers:.

*Prashant Bharadwaj and David Lagakos asked me to talk to third year graduate students at UCSD about the editorial process. This manuscript evolved from that presentation and similar talks prepared for audiences at other universities. I thank Romans Pancs and Andy Postlewaite for comments. This essay reflects my views and is not an official statement of the policies of any journal. Philip Neary supplied the T. S. Eliot quotation.

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1 Introduction

> 'Teach me your tricks,' I say to him; 'I want to be a clown like you.' He puts his hand on my shoulder. 'I can't teach you,' he says. 'This is my act, and to make it really mine I've been practicing it for decades. Every decent clown has his act. If you are patient, by the time you are an old man you'll perfect your own.' G. Konrad

I recently completed a term as editor of *Econometrica*. Prior to that, I was Co-Editor of *Econometrica* and *American Economic Review*. I have (electronically) signed about 2,000 decision letters in these roles and have written another 1,000 as an anonymous referee or Associate Editor. Other people have done more, and better, editorial work, but I have done a lot.[1]

This essay reflects on my experience as editor. I hope that it shines some light on the editorial process. I also reflect on the strengths and weaknesses of the current system. If you are hard at work on a research problem, stop reading now. If you are unable to focus on your research because you just received a rejection letter, you may wish to read on. You will still be upset when you finish, but you may be upset for different reasons.

Much of what follows is either obvious or incorrect, but there may be disagreement about which parts are which. Much has already been said by others.[2]

In this essay, I use first-person singular to express my view. Maybe my views represent the views of a larger set of "editors," but some of the observations will be personal and idiosyncratic. I address the reader as "you." Sometimes I speak to you in your role as author. Sometimes I speak to you as a referee. I hope that the context makes your role clear.

2 Process

You write a paper and upload it to a journal's webpage. What happens after you submit? A staff person at the journal mechanically checks whether

[1] More people have read – and in many cases read with great care – my decision letters than my research. These general thoughts on the review process presumably are of intermediate interest.

[2] Szenberg and Ramrattan [22] is a collection of essays from former editors about their experiences.

you have followed the rules. If the journal requires disclosure statements or submission fees and you have not provided them, then the journal will ask for this information. Different journal have different policies about the extent to which they enforce other posted rules (for example, page limitations) at this time. Failure to follow the rules delays the review, uses time of the journal's staff, but does not influence the editorial outcome. After the paper passes the administrative screening, the review process begins. At *AER* and *Econometrica*, the editor receives the paper. The editor will desk reject the paper or send it to a co-editor (who may be the editor), who will lead the review process. The co-editor desk rejects the paper or assigns it to referees. At *Econometrica* roughly one third of the submissions are desk rejected. Desk rejects typically are processed in less than two weeks.

2.1 Desk Rejects

If your paper is desk rejected, you may receive a form letter (that contains no evidence that anyone has read the paper) or a short letter that contains comments about your paper. Receiving this news is painful. You worked hard on the paper and thought it was a good match for the journal. The journal, with little apparent thought, informs you that it thinks you were mistaken. From my perspective, what happened is that you sent a paper that just does not fit the journal. This does not mean that the paper is bad. It means that the journal is really unlikely to publish the paper. Do not appeal this decision: I have the power to accept or reject the paper. If I am biased against the paper (even if it is due to bad taste or a narrow vision), I am unlikely to change my mind after an extended review. Once you realize that desk rejected publications are not going to get published, you realize that the desk rejection is not all bad. You do not receive detailed comments, but you do get the bad news quickly and can move on.

2.2 Extended Review

If I decide to initiate a detailed review, I select referees. Some referees refuse and need to be replaced. Time passes. I may cut short a review if I receive a decisive, convincing (usually negative) review quickly. If referees are late, then I write to referees and remind them of the assignment. When all of the reviews are in, I make a decision.

I rarely, if ever, overruled a panel of referees who all gave the same recommendation. In many cases, there will be at least one positive referee and at least one negative referee. In difficult cases, I may check back with referees or other experts prior to making a decision. I viewed any case in which there was a convincing argument for publication and a convincing argument against publication as marginal. In these cases, my own assessment of the paper entered into the final decision (but was not always pivotal).

I report the final decision to the author and the referees.

2.3 Selection of Referees

When it is time to select referees, I look at the paper. If the paper is close to my area, then I am likely to know people working on related topics. If there have been recent submissions on the same topic, the authors and referees of these related papers become candidate referees. I try to identify the primary references. Authors of these papers are candidate reviewers. I regularly use the journal's data base (to look up reviewers of related papers) and Google Scholar (to see who cites the author's related work). These searches typically turn up enough candidate referees. If they fail to do so, I can iterate the process – looking for people who cite papers who cite the author or referees of papers written by these people. Sometimes people who are unable to prepare reports suggest names. People I meet when I visit other universities or people I speak to when they give talks at my institution become part of my informal data base and become candidate referees.

2.4 Possible Decisions

My decision letter states either that I am rejecting the paper or requesting a revision. (Essentially no papers are accepted unconditionally after a single round.)

Rejection typically means that the review process at this journal is over. The decision letter should provide reasons for the negative decision. The editorial correspondence could provide guidance about what to do next with the project. *American Economic Review* and *Econometrica* now have close association with other journals: the American Economic Association publishes several specialized journals (*AEJ: Macro, Micro, Applied*) and the Econometric Society publishes *Quantitative Economics* and *Theoretical Economics*.

AER permits authors to share reports with *AEJ* journals; *Econometrica* permits authors to share reports with the other Econometric Society journals. This procedure can expedite the review process. Any author of a rejected paper can resubmit to a related journal (with or without sharing past reviews). I explicitly mentioned this option to authors in my decision letter only when I felt confident that a majority of the people who read the paper for *Econometrica* would support publication of a feasible revision in *QE* or *TE*.

An invitation to resubmit the paper is an extremely good sign. If you are fortunate enough to receive an invitation to resubmit, you should be optimistic about the changes of publishing the paper in the journal you targeted. Some requests to resubmit are speculative and virtually every invitation carries the risk that you will ultimately be rejected, but a request to resubmit is a signal that someone influential in the editorial process is on your side. When I invited a revision, I tried to outline sufficient conditions for publication in my decision letter. Further, I wanted these conditions to be unambiguous and feasible to satisfy. I did not expect authors to follow my sufficient conditions in every case, but I wanted to offer a clear path to publication when I invited a revision. In these cases, when an invitation to resubmit does not turn into a publication, the reason is typically either poor communication (I did not describe conditions carefully enough), underestimating the difficulty of satisfying the conditions (the feasible sufficient conditions were not feasible after all), shifting standards (the referees or I ask for something new in later rounds of review), or the author deciding not to follow the editorial recommendations. I view the first three reasons as editorial failures and I tried to minimize them (I did not lower them to zero). I was disappointed when authors decided not to follow editorial recommendations and that their choices led to a rejection, but this outcome is not necessarily a failure. If the reviewers and the author have different visions of the contribution of the paper, it is reasonable to imagine circumstances in which the best outcome is for the authors to publish the paper in their preferred form in a different journal. I encouraged authors to ignore the sufficient conditions provided in a decision letter if they were either not feasible or not (in the authors' view) appropriate, but that ignoring the conditions increased the risk that the paper would be rejected. Frequently, authors could provide convincing reasons for the revision that they submitted and could publish a revision even if it did not satisfy sufficient conditions for publication.

Occasionally, I could not specify a clear path to publication in a decision

letter even when a significant fraction of reviewers recommended that I invite a revision or if I felt that the paper was really promising, but had not realized its potential. Sometimes, usually due to bad writing, reviewers were unable to evaluate the contribution and I wanted to give the authors another chance to make the case for publication. One way to respond to papers in this category is to reject the paper but leave the door open for a resubmission. These "reject and resubmit" decisions distinguish between a definitive rejection and an unambiguously positive decision to encourage a revision. After some experience with open-door rejections, I decided to avoid the category of "reject and resubmit" entirely. Authors almost always treated "reject and resubmit" as an invitation to resubmit. When they resubmitted, they believe that they fully satisfied the concerns (or at least the legitimate concerns) raised in the previous review. They appeared to be confident that the revision would be accepted. Referees typically did not change their original assessment, however. Sometimes their position did not change out of stubbornness, sometimes because their negative recommendations were based on more than what appeared in their reports, and sometimes because authors failed to respond to the original concerns. In any event, I found that most "reject and resubmits" led to negative decisions, unhappy authors, and unhappy reviewers. I did not offer any "reject and resubmit" decisions during the final two years of my term. What I did instead is increase the number of speculative revise and resubmit decisions.

Most of the difference between "reject and resubmit" and "speculative revise and resubmit" is semantic. Both categories refer to papers that are promising, but for which there is no clear path to publication. Operationally there are three differences: when authors of reject and resubmits resubmitted their papers, they frequently failed to prepare detailed responses to the editorial comments. Failure to do so increased the chances of reviewers arguing that authors did not respond adequately to first-round comments. The second difference is that frequently co-editors would leave the door open for a resubmission, but would rotate off the board by the time the authors resubmitted. The former co-editors would be reluctant to handle the revision. This meant that the authors would need to satisfy a new co editor and the standards of the review could change. The third difference is that I tended to make letters inviting a speculative revise and resubmit more detailed than rejection letters. That is, although I could not specify a clear path towards publication, I invested the time to describe what I thought the paper needed to do. I tried to provide sufficient conditions, but I did not guarantee that

these conditions were easy to satisfy. All three of these differences could be eliminated with clearer communication. I could make it clear in reject-and-resubmit decision letters that authors prepare detailed responses to editorial comments when they resubmit. I could demand (as editor) than any co-editor leaving the door open for a resubmission must agree to handle a resubmission (at least if the resubmission arrives in a short enough time). I could make reject-and-resubmit decision letters clearer about what would constitute a successful revision. I decided that it was easier to eliminate the reject-and-resubmit category, but I understand why other editors may prefer to leave the option open.

2.5 Further Rounds

If you are asked to revise your paper a second time, you can be confident that you will be able to publish the paper. Exceptions arise. In my experience they are due to unusual initial conditions (the first review is actually the review of a paper that had been rejected; the second revision involved a new set of referees; the editor points out a specific, necessary condition and frames the invitation to revise as a demand). I rejected papers after inviting two revisions, but I view these as cases where I failed to do my job effectively.[3]

The basic process will be the same as the first revision: Supplement your conscientious response with detailed comments that describe the ways in which you have modified the paper. If you failed to follow recommendations, explain why not.

Typically later stages will be negotiating about housekeeping details, but most authors have stories about second-round requests that have nothing to do with (or even contradict) first-round requests. Changing the conditions for publication is frustrating and unfair, but it is understandable. Referees may miss something in the first round (and a new referee may add a different perspective). New second round recommendations may not improve the paper, but their value is not significantly lower than first-round comments.

[3]I am not admitting that the final editorial decision was incorrect (although that may be true), nor am I accepting all of the blame for the outcome, but either by being more decisive at an earlier stage or providing clearer instructions I could have avoided disappointing authors, frustrating referees, and inducing inefficient effort.

2.6 Handling Appeals

Journals should provide a transparent appeal process. Here is my policy. If an author complains, I always ask the author if I have permission to send the complaint to the relevant referee. When I get permission, I send the comments to the referee and tell the referee that responding is optional, but they must tell me whether they will respond and that I will press them to respond within two weeks. I aggregate the responses, if any, and write back to the author. (The author always grants permission to forward something to the referees, but often not the initial letter to me; about half of the referees make serious replies. I respond to the author within a month. I rarely reopen the review, but I have done so.)

3 Advice to Authors

3.1 Submission Strategy

Before you submit to a journal, you should try to get a sense for the kinds of papers that the journal publishes. If none of the papers published in the journal "look like" your paper and if you do not cite any papers published in the journal, it is likely that you should try another journal.

I generally advise authors to submit their paper to the journal where they want to see it published, assuming no time constraints and that someone knowledgable believes that the chances of acceptance at the journal are positive. Politeness norms suggest that the reactions you get from advisers, colleagues, and seminar audiences will be more positive than what you can expect from anonymous reviewers. This means that if no one encourages you to submit to your target journal, you are likely overreaching. If you face time constraints, do research on the response time and acceptance probabilities at different journals before you decide where to submit. Time constraints most likely to be an important consideration for people nearing a tenure review. If you are in this situation, get specific advice from senior colleagues in your department to approach the difficult problem of balancing departmental deadlines and standards with variable response times at journals.

It is not the job of referees to turn a rough draft into a polished contribution. It is possible that the journal's comments will lead to dramatic improvements, but you should not count on this. Do not submit to Journal A with the sole goal of obtaining comments that will make the paper a

better candidate for publication in Journal B. If you do not have colleagues at your home institution who can provide critical comments on your work prior to submission, try to make connections elsewhere. You may be able to create these connections through gift exchange: make a habit of providing comments on working papers in your area.

Submitting prematurely is costly. In the best case, you may receive a speculative request for a revision. This request will likely remove some of your ability to frame the paper. Instead a sympathetic editor or reviewer will impose a frame on you. The author is typically better informed and probably has a better notion of what the paper should be, so I see no value in giving extra discretion to reviewers. In the typical case, you will be rejected. Almost everyone gets rejected sometimes and your record does not have a direct effect on the review of future submissions, but there are two real costs. First, it is likely that one of your referees will be asked to review the paper when you submit it to another journal. A referee will remember a poor submission and may not take the time to check for improvements. Second, editors may eventually draw the inference that you submit papers that are weak and take subsequent submissions less seriously. This would increase the probability of desk rejections or reviews that end after one negative report.

3.2 Responding to Rejection

> There are so few people who do not sooner or later take advantage of you if you admit to having made a mistake; they try to turn you into a mule and load their own mistakes on you. ... And then, when the mule suddenly kicks out, they are genuinely astonished. But only for a moment; once they get their breath back they remind you, completely unabashed, of the mistakes you yourself once admitted to – as proof that it is you, not they, who make mistakes. M. Frisch

No means no. Journals make mistakes, but it is necessary to have a definite end to the review process. If your paper is rejected, then you should not negotiate with the editor to reopen the review or consider a revision. When can you write back to the editor? You should certainly contact the editor if you think that there are serious logical errors in the reports. No editor wants to base a decision on a demonstrably false assertion from a

referee.[4] Moreover, the editor can note the mistake. Future editors will be less likely to consult the referee who erred (or would be more vigilant about following this reviewer's recommendation). Bad taste is not a logical error. If the referee's primary mistake is underestimating the importance of your seminal work, move on to the next journal and hope that you will find more enlightened readers. Be aware that sometimes there will be mistakes in the reports but these mistakes need not play a decisive role in the review. The editor's decision letter should indicate which arguments are the basis for the decision. That is, pointing out a logical error need not trigger a new review.

You can also write back to the editor if you genuinely want clarification. If Referee 3 suggests an attractive, alternative argument, but you do not understand aspects of the suggestion or if Referee 4 states a "well-known" theorem that you cannot find in the literature, you can request more information. State that you do not wish to re-open the review, but cannot take advantage of the reviewers' comments without more information.[5]

If you do resubmit a revision of a paper that was previously rejected, mention the history of the paper in a cover letter. Otherwise, it is almost certain that the journal will discover the previous submission and be upset that you violated journal rules.

After a rejection, you go through stages that may parallel Kübler-Ross's [18] stages of dying (denial, anger, bargaining, depression, and acceptance).[6] The denial phase may involve the hope that the rejection letter was sent by mistake, followed by the realization that the referees' comments were fatally flawed and that a sensible process would reverse the decision on appeal. Anger begins when you realize that the incompetent reviewers control the situation and you are unlikely to change the decision. Bargaining arises if you appeal the decision. You become depressed when you realize that the editorial decision is final and you wonder whether all of your work will be

[4]No one likes to make mistakes. Few people admit mistakes with grace. Often referees will replace their original argument justifying rejection with a new one. This reaction is frustrating for an author. But an editor needs (at most) one good reason to justify a rejection.

[5]Many people appeal for other reasons. Sometimes these appeals cause the review to be reopened and lead to reversals of the original editorial decision. Hence it is possible that the expected value of appealing a decision is positive for authors even if, in my opinion, there is no basis for appeal.

[6]I view these stages as a suggestive description of the grieving process. There is considerable controversy about whether people pass through stages of grieving and, if they do, whether they pass through these stages and in this order.

poorly received, unfairly reviewed, or (perhaps) is not as good as you once thought. The depression may turn into negative reflections on your status in the profession. When you get to the acceptance[7] stage, you are able to look at the paper again and, in most cases, prepare it for submission to another journal. How long it takes to pass through these stages (and the pain associated with each one) depends on you, on the quality of the project, on the quality of the review, and on the broader context (it helps to have a support network and job security). Take some consolation at the end of the five stages you, unlike Kübler-Ross's subjects, will probably still be alive. You go back and improve your research article. Some journal will publish it.

The biggest problem that you face when revising a paper that has been rejected is that you know that someone does not like it, but you cannot communicate directly with that person. As an author, I treat the revision process of a rejected paper exactly as if I am resubmitting an invited revision: I prepare detailed responses to the editorial comments and explain, if I can, why some remarks are off target. Because no one but you will read these comments, you can insult the referees for being lazy and stupid and the editor for having bad judgment. Having carried out the exercise, try to make visible adjustments to the text of the revision that show sensitivity to the most important criticisms provided by the reviewers. Why? There is a good chance that one of the people who reviewed the paper at the first journal will be asked again. If this person is conscientious, she'll check to see if you responded to the earlier comments. If you do not, the old reviewer will be unlikely to change her opinion of the paper and may lower her opinion of you. And she is likely to review one of your next papers. Maybe you think that the reviewer's objection was crazy or wrong. Treat this as a problem in communication. Make an attempt to clarify your position in the text. Typically, this strategy will not change the referee's mind, but it has two benefits. It might change the referee's opinion of you (you took the trouble to respond to her insightful comments). If may be that a fresh referee would misunderstand you in the same way that the first referee did. Referees can be lazy, insensitive, and stupid. But so can any reader of your paper. Treat negative comments – even ones that are objectively incorrect – as suggestions that you are demanding reviewers to do too much. Sometimes if readers understand what you are trying to do, they will be sympathetic.

[7]Alas, this is acceptance of the negative editorial decision, not acceptance of your article.

Better writing can reduce the chance of misunderstanding.

Most submissions are rejected and in most of these cases the reason for the rejection is a subjective evaluation of the marginal contribution of the paper rather than a logical error. These decisions are painful to make and much more painful to receive. But they are inevitable. Compared to other social sciences, there is remarkable agreement in economics about what is a good question, what constitutes an appropriate methodology, and what qualifies as a good answer. But there are disagreements about all three of these things. Added to flexible notions of which results are appropriate for the target journal, editorial decisions must turn inconclusive information into a binary decision. A rejection is devastating and the justification for rejection is often not informative. I have faith that the profession eventually gets things right: good papers eventually will be published and recognized; bad papers, even if well published, will be criticized, discounted, and eventually ignored. The problem is that the profession does not get things right immediately. This problem has grave consequences for young scholars who need evidence that their work has been recognized fairly quickly. It has broader consequences for the profession, which cannot trust the venue of publication as a definitive signal of quality.

3.3 Responding to Revise and Resubmit

In almost all cases, you should plan to resubmit the paper and make returning the paper to the journal a high priority, especially if you are untenured. Preparing a revision quickly benefits you because referees and editors are more likely to remember the paper (and the pivotal reviewer was sympathetic). Securing a publication is much more valuable than having an invitation to resubmit, so the more urgent it is for you to improve your CV, the stronger the argument for concentrating on a revision. This recommendation is not a law of nature. Your time is valuable and you may be burdened by other responsibilities or more interested in other projects. You may need to coordinate with co-authors. Sometimes it requires a new idea to respond to reviewers. This cannot be rushed.

Ideally, the editor's letter to you outlines necessary and sufficient conditions for publication. Sometimes the necessary conditions are not demands. If you cannot meet them (or you believe meeting them harms the paper), then you can resubmit the paper without meeting these conditions, but you must explain why you could not or chose not to do so.

Sufficient conditions provide a guideline for authors. These conditions are negotiable. You want a publication, but you know your contribution better than reviewers. Do not let the revision process degrade your work and fight for aspects of the paper that you believe important. You should be prepared to lose some of these fights. Depending on the circumstances, that means you should indicate a willingness to modify the paper according to reviewers' wishes or to try another journal. If you face no time pressure and possibly in general, I recommend that you try to publish what you want in a different journal rather than let the review process degrade your work.

When you resubmit the paper, include detailed responses to all reviewers' comment and, especially if these comments are lengthy, an honest overview of which comments you dealt with fully and which you did not. These documents are the place to justify why you chose not to comply with suggestions and to respond to critical comments.

Referee reports will often contain contradictions. Referee 1 will praise Theorem 1 and ask you to remove Theorem 2. Referee 2 will say that Theorem 1 is well known and Theorem 2 is the main result. All of the referees will ask for new material. The editor will ask you to shorten the paper. The editor's cover letter should help you decided how to handle incompatible requests. If your goal is to maximize the chance of getting published, you try to satisfy the decisive reviewer (ultimately this is the editor, but the editor may suggest that publication relies on convincing a particular reviewer). If the editor does not provide guidance, you should view incompatible requests as an opportunity rather than a challenge. Most reviewers understand that you cannot satisfy incompatible requests. Pick the direction that you believe is correct and defend the choice in a cover letter.

3.4 Gaming the System

> If they can get you asking the wrong questions they don't have to worry about answers. T. Pynchon

Spoiler: This section doesn't tell you how to game the system, but rather argues that it is not worth the effort.

I explained factors that influence the choice of referee. Using that information, you may be able to influence the choice of referees. All other things equal, R is more likely to be asked to review a paper the more that the paper cites papers written by R. This opens up the possibility of an author

manipulating the pool of referees by citing people likely to be favorable or not citing people likely to be unfavorable. The strategy is costly. If your paper doesn't cite the most related papers or has a set of curiously irrelevant citations, someone is likely to notice and it will not reflect well on your scholarship.

Some authors will make pre-submission inquiries about a paper's suitability for publication.[8] I can see the use for these if the question is general ("Does *Econometrica* publish papers on finance?" "Must a paper make a significant methodological contribution for publication?"[9]), but authors should not expect answers to questions of the form "If I submit this paper, will it get accepted?" These inquiries do not increase the probability of a favorable outcome or generate an expedited review.

Of course, there is a more direct way to influence the selection of reviewers. The author can include a cover letter that nominates referees (or a co-editor). There is nothing wrong with nominating a co-editor (and some journals request recommendations). At *Econometrica* these nominations are unlikely to make a difference. Very rarely, an author would nominate a co-editor different from the one I would have selected. In those cases, I was somewhat skeptical of the recommendation and usually consulted with both the co-editor I thought most appropriate and the one nominated by the author. It is not customary for authors to suggest referees in economics. Authors rarely do so and I was skeptical of suggestions when they were offered. Naming a referee increases the probability that the nominee would be asked to write a report, but decreases the weight placed on the reviewer's evaluation.[10] I see nothing wrong with authors identifying reviewers, but these recommendations will always create the suspicion that the author is putting forward the name of someone biased in favor of the paper.

There are some situations in which it makes sense for an author to recommend that certain people not take part in the review process. I did not always follow the authors' wishes, but I noted that there was a reason to discount some opinions. Often I did follow an author's recommendation to

[8]It is possible that the motivation for these inquiries is that one must be a member of the Econometric Society to submit to a society journal. Perhaps authors were trying to decide whether the chances of publication were high enough to justify paying the membership fee.

[9]The answers are "yes" and "no."

[10]I did consider the possibility that a strategic author would nominate a referee in order to lower the probability that the person gets asked.

avoid a reviewer. In those cases, I looked for a close substitute to the reviewer the author named.

I have heard conflicting stories about whether including someone in the acknowledgments raises or lowers the probably that the person thanked would be asked to review the paper. I made a habit of identifying referees without looking at the acknowledgments. Sometimes I found that a potential referee received an effusive acknowledgment. Normally I would still invite the person to write a report (often mentioning in the invitation that the referee may decline if she/he had already provided detailed comments), but the acknowledgment might influence the weight I placed on the referee's comments.

Econometrica recently adopted the policy of identifying the co-editor who handles published papers in a neutral way while prohibiting authors from thanking the co-editor. The first part of the policy does two things. It permits readers to see whether the journal has made a minimal effort to avoid conflicts. For example, an *Econometrica* reader will see that no accepted paper is handled by a colleague, adviser, or recent co-author of one of the authors of the publication. It also suggests which co-editor to blame or credit for an editorial decision. The second part of the policy reflects the notion that there is no need to thank editors for just doing their job. It eliminates the temptation authors may have to praise editors to influence an editorial decision or for readers to speculate on the meaning of the absence of an acknowledgment.

4 Advice to Referees

Referees can get advice from other sources, including Berk, Harvey, and Hirshleifer [4] and Thomson [Chapter 3][23].

A referee's primary job is to answer the question: Is this paper suitable for publication in the targeted journal? If the answer is yes, the report should explain why and offer suggestions on how to improve the presentation or results. If the answer is no, the report should explain why not. Editors really want to know whether a modest revision of the paper would be suitable for publication in the targeted journal. I aspired to provide authors with a feasible set of sufficient conditions when I invited a revision. What I hoped to see in a referee report with a positive recommendation was a description of sufficient conditions for the recommendation and an explanation for why one could hope that the authors could satisfy these conditions.

The referee should identify arguments that are not clear or convincing, suggest literature that is relevant and incompletely discussed, and express doubts about the power or generality of the message. At some point, comments in these categories stop being constructive. The editorial process sometimes demands authors to provide detailed analyses of model variations because it can, not because they extra analysis is essential. Referees and editors hold up the publication process (and create cluttered articles) when they make (probably well intentioned) requests to relax one assumption, run another treatment, or to perform a different estimation. Conscientious authors should carry out these exercises prior to publication and make judgments about what material is insufficiently interesting to include in a publication. They may fail to conduct the choices due to lack of imagination, lack of time, or anticipation that the review process will make requests later. To the extent that authors have done the robustness checks prior to submission, inquiries from referees are easy to handle. If authors failed to think about important extensions, then it is important for the referees to bring them up. If authors deliberately decide not to think about extensions prior to initial submission because they expect the review process to make requests, the review process is failing. I urge referees to recommend against publication and editors to reject papers that fail to point out that important results require convenient but ultimately indefensible assumptions. This policy would motivate more careful preparation of initial submissions. Distinguishing between essential elaborations and tangential elaborations is subjective. It is primarily the job of the author and the editor (and not the referee) to identify which requests should be incorporated into the paper. It is valuable for referees to contribute their opinions, but the process should not assume that the author must include a discussion simply because a referee requests it.

If you recommend in favor of publication, you (sometimes implicitly) state that the results are correct. Referees do not have the ability to reproduce experiments, usually lack time to replicate computations, and cannot be expected to read every line of every proof. But referees ought to be experienced enough to suspect problems and conscientious enough to check to make sure that the author's work handles obvious troublesome cases. If you have not read technical arguments carefully, you should state that in your report or cover letter.

The referee's job is to help the editor make an informed decision about whether to publish the paper. In most cases, the report should contain a recommendation. Referees should understand that the editor will not always

follow a recommendation, but the editor does want your opinion. You should state it clearly and consistently. This does not mean that you must be an advocate. Your report typically will describe both strengths and weaknesses. It does mean that your summary recommendation should be supported by the detailed report and that the cover letter should not subvert the evaluation in the report. Ambivalence is fine; inconsistency is not. Some referees do not make explicit recommendations in referee reports to give editors more discretion. This is not necessary. Editors already have enough discretion.

It is not a referee's job to request the authors to write a different paper. Instead, a small benefit of being asked to write reviews is the chance to get new ideas. If a referee likes the general topic, but disagrees with essential details of the analysis, the referee could suggest that the authors follow a different approach in a different paper or the referee can try to write his own paper on the topic.

It is not a referee's job to show off for the editor. The editor wants a convincing answer to "Should the journal publish the paper?" Carrying out this task will win the approval of the editor. Save the brilliance for your own research.[11]

Referee reports vary in length. Ideas about how to improve a paper, lists of typos, or concerns about notation are rarely relevant for an editorial judgment on a paper that will be rejected. They could be valuable to authors and often do not require much extra work to assemble. Including these items in a report is a gift to the author that I see no reason to discourage. These "housekeeping" details are more important for papers that are strong candidates for publication.

It is not a referee's job to turn a badly written paper into a gem, but reviewers often can make suggestions that lead to significant improvement in the structure of the paper.[12]

Something has gone wrong if the review process leads to papers that have footnotes with a list of papers (but not discussion of why the papers are relevant). You should supply important references missed by the authors, but avoid listing vaguely related papers that would bloat the references.

A good referee responds quickly to an invitation to write a report. If you

[11]If you happen to figure out a way to make the paper better, either because the idea comes to you or the paper was interesting enough to stimulate serious reflection, by all means place the idea in your report. I have examples of cases in which particularly helpful referees became co-authors. This kind of outcome is wonderful, but unexpected.

[12]The profession should find better ways to reward good exposition. See Section 9.

decline the invitation, do so quickly. This means shortly after you receive a request (certainly within a week), look at the paper to see whether it is a good match to your abilities and interest and that you have time available to prepare a conscientious report by the deadline.

Decline invitations if you have a conflict or if you are overcommitted. If you are uncertain whether you have a conflict, you may describe the situation to the editor and follow the editor's recommendation, although if you have doubts, I recommend that you recuse yourself. If you know that you will be unable to meet the deadline, you may conditionally accept the invitation and propose to provide the report by a later date. If you do propose a later deadline, try hard to meet it. From the editor's perspective, it is much better for a referee to immediately decline a request than to hear three months into the review that the referee is unable to provide useful comments. If you decline, you are welcome to suggest alternative referees. If you receive inquiries directly from the editor (not automated reminders from the editorial system) at any time in the review process, you should reply to them as soon as possible.

Sometimes you will receive invitations to review manuscripts that you have previously reviewed. Immediately inform the editor and send your previous report. If you have the time to examine the submission to see if it to your earlier comments in a way that changes your evaluation, do this. Editors deserve to receive the advice of expert reviewers, but authors deserve fresh evaluations.

4.1 Cover Letter

Usually referees provide three pieces of information: a summary recommendation (selected from a menu that ranges from "accept" to "definitely reject"); a cover letter; and a detailed report. Ideally, the detailed report will justify the summary recommendation and the cover letter will communicate anything that the referee views as relevant to the decision, but not appropriate for the authors to see. If you use a cover letter, it should contain the same recommendation as the report.

Cover letters are unnecessary in most circumstances, because it is rarely the case that the referee needs to say something to the editor that the authors should not see. Sometimes there are special circumstances, the most common one is that the referee wants to provide information to the editor, but still maintain anonymity. For example, the referee may want to know that the

paper did not cite relevant articles by the referee, but feared that a long list of self-citations in a report would reveal his identity. Or the referee might repeat comments that she communicated to the authors during a seminar presentation or private correspondence. Some referees reveal strong negative opinions in cover letters and state that these did not enter the report to be polite. Accusations of academic dishonesty – plagiarism, deliberate falsification of data, or fundamental misrepresentations of results – are extremely serious. Fully substantiated assertions belong in a referee report. There may be situations in which the editor is better able to investigate claims than the referee. In these cases, there is an argument for discussing the possibility of academic misconduct in a cover letter.[13]

I was often tempted to quote cover letters when I thought that they provided a clearer summary of the reviewer's opinion than the report. At times I thought I could quote the cover letter without asking permission from the referee. Other times I asked permission. In the absence of a clear norm, if you do not wish the editor to refer to your cover letter in a decision letter, you should say so.

4.2 Why Referee?

Conscientious refereeing is essential to the publication process as it is currently organized. You will submit papers; they will be refereed. It is possible that you will be asked to review papers that are in your area, but you have not yet seen. Writing reports is an opportunity to figure out papers. (Of course, you can figure out papers even if you are not asked to do so by a journal.) Writing good reports builds up good will with editors. This probably does not improve the chances that your papers will be published, but writing good reports might translate into some kind words in a letter of reference written by the editor (these words have minimal value because your promotion primarily depends on an assessment of your teaching and research); they might lead to seminar or conference invitations; and they will probably lead to more invitations to write reports.

The profession demands that reliable assessments of scholarly work. The

[13]In this case, the reviewer is saying "I think that there may be a serious ethical lapse, but I do not have the time or interest to support my concern." Fortunately, I lack experience with cases like this, but typically the referee will be more able to confirm academic dishonesty than the editor. It would be the editor's job to decide how to act on the evidence.

assessments need not come from journals, but our work is sufficiently specialized that we cannot depend on departmental colleagues and certainly not campus review committees to evaluate the research. I believe that the profession eventually figures out which articles make lasting contributions and which do not, but the process works slowly. Peer review is essential for the evaluation of junior scholars and linking peer review to the publication process (and generating visible, noisy signals through the reputation of journals that publish your work) has been important historically. So referees are essential. This observation may be enough to convince a sociologist to referee articles (because the sociologist is more likely to accept that community interests may influence an individual decision), but it may not be enough to convince an economist with a narrower version of self interest.

Some people agree to write referee reports because editors have power and if you fail to do a favor for them, it may have negative consequences. This is not a good reason to referee papers. The negative consequences may exist, but declining an invitation to review would not lead to a less sympathetic assessment of your paper. (It might lead to a longer time to decision.) An editor might come to view you as a poor citizen, but there are many ways to serve the profession (mentoring colleagues, service on departmental and campus committees, organizing conferences, ...) and it makes sense for at least some of us to specialize in different things. You can decline some invitations to write reviews and maintain a good professional reputation. The consequences of providing bad reports or breaking promises to provide reports are greater larger – both for the smooth functioning of the review system and for your reputation.

The arguments against refereeing are strong. Refereeing is time consuming. It is fundamentally thankless. The chances are that your recommendation will not be decisive and your comments will not have a serious impact on the final version of the paper. Referees might receive thank you notes from grateful editors and (more rarely) from grateful authors, but these notes hardly compensate for the time needed to prepare a report. Good referees will receive more offers to write referee reports and, eventually, invitations to become associate editors and perhaps editors.[14] There is a small chance that the work you put into writing a report will influence your own research.

Refereeing comes with some rewards, but it is not in your narrow self interest to be a good referee. I was surprised and grateful that so many

[14]Bad referees will sometimes receive these invitations too.

people contributed so much to the review process. These contributions are essential to the process as it is currently organized. I hope that people continue to prepare conscientious reports.

5 Editing

> Some editors are failed writers, but so are most writers. T.S. Eliot

I have already discussed what an editor does. This section considers a few unanswered questions.

5.1 What Does an Editor Optimize?

I tried to select the highest quality work in a process that was as fair, transparent and efficient as possible. None of the arguments in my utility function are easy to measure. The views in this subsection are certainly personal. I do not claim to represent the motivation of other editors. It may be that I do not accurately describe my own behavior. I refer the read to Card and DellaVigna [8] for an empirical study that attempts to study editorial decision making systematically.

Independent of correctly identifying the quality of papers, an editor should be fair (consistent across reviews; decisions independent of irrelevant characteristics); transparent (authors and reviewers should understand the process and decision criteria); efficient (deciding quickly and not without using too many referees); and professional (treating all participants with respect and concentrating on scientific issues).

I often wished that I have a machine that linearly ordered submissions leaving me with just the decision of which papers were above a cutoff. Indeed, if there was an uncontroversial linear ranking of quality, the decision of what to publish would be completely straightforward.[15] Instead the profession, despite having remarkable agreement on methods and research questions (at least relative to other social sciences), will disagree. You might mention in

[15]Romans Pancs offers to arguments why there are drawbacks to a deterministic, "objective," review system. First, Morgan, Tumlinson, and Vardy [21] point out that there adding chance to decisions may have positive incentive effects. Second, he suggested that the lottery nature of reviews makes the submission process more entertaining.

your Nobel Prize biography that your seminal work took extra time to be recognized, largely because a stupid *Econometrica* editor rejected the paper. But if you get to shake the hand of the king of Sweden, then my decision didn't do lasting damage to you or to the profession. I worried more about passing on promising papers that other journals would reject, making errors in process that had serious impact on tenure cases, and (to a lesser degree) encouraging lines of research that seemed popular but not of lasting value. I did not consciously attempt to select papers that would maximize citations, partially because I lack confidence in my ability to predict citations and partially because citations are an extremely noisy indicator of quality. To the extent that citations are valuable, I wanted to publish papers that would be cited ten or more years after publication.

I have opinions about what makes an article suitable for publication and these opinions were decisive in marginal cases. I am not sure whether I had the opportunity to exercise my preferences enough to identify them in an empirical study.

Somewhat surprising, whatever the objective of the editor, I was never explicitly constrained by space. No editor has instructed me to accept fewer papers because the journal had limited space. I was aware that different co-editors accepted papers at different rates (in most cases the differences were not statistically significant), but I did not intervene as editor to try to equalize these rates. *Econometrica* co-editors shared the goal of increasing the number of published papers while maintaining high standards, but the fraction of submissions that were accepted did not increase.

5.2 How to Get the Job

The best way to get asked to referee papers is to write professional reports and deliver them on time. The second best way is to be considered an expert in your field. If you do these two things, you will be asked to write more reports. Eventually, you will be asked to serve on editorial boards. The usual suspects for editorial positions are people who have served on editorial boards already.

It is easy to measure conscientiousness (number of reports completed and time needed to complete the report) than the quality of advice. Generally search committees for open editorial positions have some idea about the quality of advice offered by candidates for editorial positions, but information about speed and willingness to review is easy to access and interpret. I

was asked to be an editor primarily because I was a conscientious referee. Conscientiousness may not be correlated with good judgment.

5.3 Why Become an Editor?

Editors get paid. I never asked how much I would get paid as an editor[16] and the level of compensation did not influence my decision to become an editor.

Editors have residual authority. Their decision is pivotal in a significant fraction of cases.[17] I imagine some people get intrinsic pleasure from having authority. More people value the opportunity to use their authority to move the profession in a direction they prefer.

Editors may be motivated to maintain institutions that are essential to the functioning of their profession. We are part of a community with closely aligned interests. Peer review of publications is an essential part of the profession (until something better comes along). As an author, I have benefitted (and been hurt) by referees' comments. We have an obligation to contribute to the functioning of the profession.[18]

Editors have little choice but to be informed about what people are working on. I may not be smart enough or dedicated enough to understand everything, but I assigned almost 4,000 submissions to co-editors over my term and I am much more aware of the kinds of topics people are working on in subfields far from my own. Even within game theory, I have a much more nuanced view of what the best researchers are doing, what are the challenging problems, and what is in fashion.

Editors get to read referee reports. This means that in addition to seeing a large sample of current research, they get to read thoughtful comments from informed experts. This experience is somewhat akin to listening to questions in a seminar audience, but referees devote more thought to their summaries and their questions than members of seminar audiences. Editors must take the time to understand these comments. I did not anticipate this aspect, but I learned a great deal from reading confidential comments.

[16] When the first check arrived, I dreamed that the payments came monthly. When a second check didn't arrive the next month, I feared that the payments were annual. They were quarterly.

[17] I would say between 5 and 10 percent, but it is not clear what pivotal means.

[18] Professional service is part of our job, but this does not means that editorial service is for everyone.

I am insecure. I crave respect and admiration, but I do not deserve it. I received respect on the basis of my title and hoped that some of glory of others who have held similar positions transferred to me. I am aware of other people who have held top editorial positions. They are impressive. Even though I know better, I like the idea of being comparable to them. I know that my research contributions are not a match for those of other editors of *Econometrica*, but I'm hoping that you don't and will mistakenly confuse me with Frank Fisher or Ragner Frisch.[19]

The immediate external rewards for reviewing are having a voice in editorial decision making and an opportunity to study new work in your area.

An editorial position can serve as a "get out of jail free" card with respect to other editorial responsibilities. I essentially refused to write reports for other journals (and I believe that editors did not expect me to write reports for them). You do not need an editorial position to turn down reviewing invitations, but editors tend to be the kind of people who cannot say no requests, but have no trouble rejecting manuscripts.

I reflected on whether people directed speaking invitations or compliments to me or my editorial position. I pretended it was the former and assumed it was the later.

5.4 Why Not?

There are plenty of reasons not to become an editor. It takes time. It may be wonderful to see the full range of top submissions, but it is more wonderful to choose freely what to read. You may feel uncomfortable making decisions that will have significant impact on people's careers. You are likely to make more new enemies than new friends.[20] One burden that I did not expect, is that I was asked to write a large number of letters evaluating candidates for promotion. In my university, campus reviewers treat it as a negative signal if someone is asked to write an appraisal letter refuses to do so. Consequently

[19]When first asked to be co-editor, I requested a list of people who have been offered the job and refused, thinking that I could refuse the job but still have place myself in a distinguished set. No one gave a direct response to my inquiry, but over the years I have learned about many people who have been asked to and refused to serve in top editorial positions. The honor comes from being asked. It is a responsibility, but a burden, to accept the job.

[20]I am already prepared to attribute any rejection of a paper I try to publish from now on as an act of reciprocity.

I agreed to write almost all of these letters even though in many cases the candidate's research was far from my specialty.

6 Behavioral Economics and the Editorial Experience

> The persuasion that one's own infallibility is a myth leads by easy stages and with somewhat greater satisfaction to a refusal to ascribe infallibility to others. B. Cardozo.

Economists have a standard set of tools for studying how people behave. Traditionally we assume that agents act to maximize well defined preferences given accurate expectations about their environment and respect for constraints on their choices. This view is both powerful and flexible. Editors see a lot of evidence that this approach misses important aspects of the review process.

I assert without proof:

1. Authors have systematically higher assessments of their work than reviewers.
2. Authors are overconfident about their ability to identify referees.
3. Referees overestimate the author's ability to identify the referee.
4. Referees underestimate the amount of time that they will have to complete a review.
5. Referees contribute valuable services in spite of the lack of high-powered rewards.

Most authors have a general idea of how selective journals are and senior scholars would give well calibrated forecasts about editorial outcomes for papers in their area. But authors are generally surprised by bad news about their own submission. Some of the surprise may be a public display for strategic purposes (to convince an editor to discount negative reviews, for example), but some may well be a sensible adaptation. If you wake up in the morning thinking that your work will be severely criticized, deemed unsuited for publication, or ignored upon publication, then you might be more inclined

to search for pictures of cute puppies (or work on an undergraduate lecture) than to fix a hole in a proof or test an alternative structural model. Overconfidence may help maintain socially beneficial investments in innovation, especially when there are external benefits to successful innovation.

It is natural to try to guess the identity of your referees. And it is not hard to come close. I described how I find referees and I did not reveal any secrets. I am confident that if I asked an author to name the four people who wrote the four reports, the author would get at least one name correct. But authors often tell me that they are certain that they know the identity of a particular referee. In my experience, more than half of the authors who attribute a particular report to a particular person are wrong. Perhaps an author's confident assertions are an attempt to learn the referee's identity by evaluating my reaction.[21] Maybe the announcement is just idle chatter. But I suspect that authors attribute comments to the wrong person. Blame the editor for failing to discount inappropriate criticism (you have my permission to blame me retroactively), but do not adjust the way you interact with a colleague because you conjecture that the colleague has reviewed your paper.

Referees express concern that their reports will reveal their identity and often take pains to conceal their identity. If my previous comment is correct, the irony is that referees are often associated with comments that they did not write. Referees worry that authors may treat them differently if authors knew who wrote the report. Referees do not seem to worry that they may be blamed for the comments written by another referee or that their comments may be attributed to someone else.

We know a lot about time inconsistency and it is not surprising that referees will agree in January to provide a report in May and then, in mid May, realize that they cannot meet the deadline. Somewhat more disconcerting is when the reason for the delay is the birth of the imminent (and apparently unanticipated) birth of the referee's child.

There are both rewards for participating in the review process and punishments for not doing so. The fact that many people write reports does not demonstrate that a standard economic assumption fails, but it suggests that many scholars place positive weight on making contributions that benefit the profession and that some referees overestimate the punishments associated with refusing to referee (and fail to realize that it is worse to deliver a poor

[21] I strived to provide non-informative reactions to the announcement, but I am a college professor and not a poker player.

report or a late report than to immediately decline an invitation).

7 There's Something Happening Here

Peer review is a strange institution. Our work is specialized and not many people are capable of evaluating it completely. But this leads to a situation where, inevitably, many reviewers will have a stake in the outcome of the review that might influence their evaluation. The interests may be professional and appropriate (identifying important contributions in the reviewer's area of expertise), professional and inappropriate (promoting the work of friends or ideas that enhance the reviewer's influence), or personal (permitting personal connections to influence professional judgments). Reviewers receive scant direct compensation. I cannot think of an alternative review system that can take advantage of expertise without concerns about professional or personal biases clouding decisions,[22] but I cannot think about the current system without worrying about the possibility of it being abused.

The current system tries to protect against the dangers of peer review. Editors do not review papers written by colleagues, co-authors, students, and advisers.[23] Inevitably an editor will have direct responsibility for a paper written by a close colleague. Even when the editor does not have a direct connection, it is impossible to eliminate the suspicion that the editor's close associates might receive preferential treatment. Assuming that the editor is objective, he will strive to identify and avoid referees who may not provide balanced recommendations or obtain enough recommendations to place individual recommendations in context. Still, authors' legitimate doubts will exist.

In this section, I assume that a select group of elites who are unsympa-

[22]Popular arts – books, movies, and musical performance – are often reviewed by professional reviewers who are not engaged directly in the creative process. There are inactive researchers and unproductive editors who participate in the review process, but I am convinced that it generally takes an active researcher working in a related field to evaluate specialized research.

[23]Different journals define conflicts differently. At *Econometrica* co-editors were blocked from even seeing correspondence on submissions by close associates. There are always grey areas, but I believe that the journal did an excellent job of preventing editors from being directly involved in editorial decision making of papers when there was a potential conflict. The journal reports the name of the co-editor who handles published papers to demonstrate that it avoids some conflict.

thetic or perhaps hostile to the needs and existence of others controls the publication process.

7.1 What Can You Do?

> Paranoids are not paranoids because they're paranoid, but because they keep putting themselves, fucking idiots, deliberately into paranoid situations. T. Pynchon

What can you do if you are not a member of the elite and are faced with repeated rejection from the editors motivated by advancing their friends, colleagues, students, and their own research agenda?

If you want an academic career in a top research department in economics, you need to publish and you probably need to publish in top journals. You cannot avoid the paranoid situations. For graduate students and junior faculty, the issue is how much energy to devote to breaking into the system. Hirschman [17] describes the options in a different (or, at least, more general) context. He describes the options of "exit," "voice," and "loyalty." In this context, exit means leaving academia or settling for positions available for people who do not publish in high status venues. Voice means identifying the problems and hoping to raise awareness and, by doing so, eliminate problems. Loyalty means making the best of the current system and, if you reach a position of authority, working to improve the system.

If you are invited to collaborate with an insider, how much should the added influence of the co-author compensate for working on a project that is not your highest priority or sharing some of the credit for the work? How much time should you spend supporting the current system by providing reports?

7.2 Who is in Charge?

> The innocence of the creatures is in inverse proportion to the immorality of the Master. T. Pynchon

Just as authors appear to overestimate the quality of their work (relative to the estimates of referees), authors (and editors) may underestimate their perceived stature. I understand that I am a successful member of the profession, but I do not think of myself as a (former) member of a select

ruling class. My terminal degree was in a different discipline (Mathematics, not Economics). I spent two years in an insecure temporary position before receiving a tenure-track appointment. I have never worked in one of the very best departments. I know most of the very best people in my subfield, but I do not consider myself their equal. Unhappy authors, in many cases people I viewed as more prominent than I, suggested that the editorial process was biased against them.

These observations led me to remind myself of the advantageous that I have had.[24] I also reached the conclusion that independent of objective circumstances, rejected authors often feel powerless.

8 Future of Journals

> A particularly invidious example of market power is the oligopoly in academic publishing. ... Advances in knowledge require the dissemination of ideas. But in our market-based economy, this has been entrusted largely to the market, and the form that has taken is a highly concentrated and highly profitable oligopoly, with some five publishers accounting for more than half of all papers published, and for 70 percent of those in the social sciences. The irony is that the publishers get the articles for free (in some cases, they even get paid to publish them), the research reported is typically funded by the government, the publishers get academics to do most of the editorial work (the review of the articles) for free, and educational institutions and libraries (largely government-funded) then pay the publishers. Their high prices and excess profits, of course, mean that there is less money to fund research.[25] J. Stiglitz

Publishing in refereed journals has four main purposes. It creates a permanent record of published papers. The review process certifies that published results are accurate. The review process strives to improve the quality

[24] I never directly experienced the problems that women or members of underrepresented groups may face. I have had the advantage of being able to publish in my native language. I work and was educated in the country that is the home of the most influential journals. My research area was fashionable when I was an assistant professor, probably making it easier to get established.

[25] Bergstrom [3] describes the problem within economics.

of published work. Journal placement provides information about the quality of the paper.

Publication is no longer necessary to make research broadly available. When I started my professional career, word-of-mouth, seminar presentations, and (snail mail) distribution were the only ways to learn about work that had not been published. Publication created a durable record of your work (although accessing publications required a trip to the library). The world has changed for the better. People post working papers on their own webpage or sites like SSRN. Search engines turn up papers. Publication makes research slightly more accessible, but is not necessary to make research available.[26]

Publication is not a guarantee that the proofs are correct, that an experiment has external validity, or that third parties can replicate the data analysis. But publication means that a journal has reviewed the paper and found that it meets the journal's standards. So publication does provide quality control. Further, authors are aware that the review process will examine arguments and that this leads to more careful preparation of manuscripts.

The review process certainly influences the form and content of published papers. Referees and editors have the power to ask for authors to alter their presentations. Even if you have faith that reviewers are wise and public spirited, it is not clear that revisions lead to better papers. The review process adds more value than it subtracts (especially taking into account that the existence of the review process changes how authors prepare papers), but I am skeptical about whether it is worth the cost (from authors and reviewers).

Journals still play an essential role in the evaluation of quality of published work. Of the four purposes of publication, it is the only one for which there are not viable substitutes in place. We will need journals unless we can find alternative ways to evaluate the quality of research. I discuss some alternatives in the next section.

One could make an argument that there was a time when academic publishers played a vital role. They earned their rents coordinating the publication process and created a permanent record of scholarly work. The business model is less viable now. Even if the publication process continues to be the

[26]Ellison [12] documents a decline in publications from prominent departments in top field journals. He attributes this trend at least in part to a reduced need for these individuals to publish in order to publicize their work.

central way in which the profession signals quality, the business of publishing is likely to change. Technological changes have made the production and distribution of journals less costly. The profession understands that it can self organize and provide editorial services without most of the costs added by the publishers. University libraries understand that the power of the journals may be weakening and have been using new-found market power to negotiate better deals. Funding agencies, striving to make research that they fund widely available, have started to require authors of funded research publish only open-access articles. The commercial journals have responded by offering by moving to a business model that bundles together journals and by giving authors the option to purchase the right to publish their manuscripts with open access. Bundling worked for cable services for a while. Authors (and, indirectly, employers and funding agencies) are still willing to pay for open access rights. But it appears that the old business model of journals will become less profitable and commercial academic publishers will need to make dramatic changes in order to survive.[27]

I lack sympathy for the large commercial publishers, but I am fond of professional societies. Currently, the Econometric Society journals generate revenue (through the money earned from its agreement with the publish and through memberships in the society) to pay for the Society's staff and to sponsor a variety of professional activities. The revenue from the agreement with the publisher will decline. This means that the society will need to find alternative sources of revenue (higher membership fees, more donations, higher registration fees at conferences, or charges for submission or publication of manuscripts are leading ideas) or to cut back on services. The business model of the society is threatened to the same extent as that of large commercial publishers, but changes in the publication process are likely to have a significant indirect effect on how the society operates.[28] In the short term, this will mean increases in fees (the increases will come from a mixture of higher membership dues, submission fees, or publication fees). In the medium term, I expect that the profession will supplement journal publications with other methods to evaluate the quality of research. The professional societies

[27]I am not the first to make this prediction. Academic publishing continue to be quite profitable, in part because of increased concentration (see Buranyi [5] and Larivière, Haustein, and Mongeon [19]).

[28]The American Economic Association benefits because of its role as an intermediary in the job market matching process. Consequently, revenue from journals is a less important part of its operating budget.

can influence the norms of the profession in more ways than the journals that they sponsor. To the extent that you think that the societies do – or can – operate in the best interests of the profession, it is important to invest in their stability.

9 Making Things Better

> Once a new technology rolls over you, if you're not part of the steamroller, you're part of the road.

S.Brand

If we are going to make the editorial process better, we must think that there is something wrong with it in the first place.

The current system uses a lot of resources. Journals have staffs and editors. Refereeing takes time. The review process takes time. Authors work hard to satisfy the sometimes frivolous requests of editors. If we could obtain the same editorial outcomes without these costs, it would be an improvement.

The current system surely influences the quality of published papers. The direct effects are easy to enumerate. Referees and editors request that you make changes in your paper. You attempt to follow most reasonable requests because you value the publication. Some of the requests are cosmetic but beneficial. Referees catch typos, minor inaccuracies, infelicities, and push you to write more clearly. Some of the requests are substantive and idiosyncratic. Referees may request additional material or suggest that material be altered. These changes improve the paper slightly more often than they worsen the paper, but the costs may outweigh the benefits. The current system must influence the mix of papers written. The publication process has a conservative bias, suggesting that researchers may be motivated to make contributions to existing lines of research rather than pursuing riskier research. Editors may make bad decisions, either because they are biased or because they have imperfect judgment. There are relatively few leading publication venues, concentrating power in the hands of a small number of editors.

I cannot quantify the extent to which the existing system changes the quality of articles[29] and I am even less able to make statements about how different systems will influence choices. I am fearless enough to predict that

[29]Ellison's thoughtful work – evidence in Ellison [11] and theory in Ellison [10] has influenced my thinking.

replacing the current system with one in which people just post articles would lead to an increase in the number of articles and a decrease in quality. But this type of anarchy is a strawman. It is essential for experts to provide convenient signals about the quality of new research. In this section, I focus on how different institutions could provide useful signals of quality. I do not directly consider how different systems would influence the quality and quantity of research.

People with senior positions in research institutions managed to publish to survive. The stakes might be higher now, as larger numbers of junior scholars will perish if they do not manage to publish in one of "top-five" journals.[30] Heckman and Moktan [15] document the importance of top five publications.[31] The number of articles published in these journals has not kept pace with the number of scholars competing for senior faculty positions in strong research departments. Not all of the journals are sympathetic to submissions in all subfields, leading to biases in the opportunities available to scholars working in different subfields. Journals may impose space limitations that limit the kind of articles that they publish. Two of the journals are strongly associated with specific departments and the other three are associated with professional societies. So it is hard to ignore Heckman and Moktan's concern that the weight that the profession places on these publications concentrates power in the hands of a small number of journal editors and distorts the kinds of research that young scholars pursue.

The current process relies on contributions of interested parties and non-traditional incentives.[32] The system requires a lot of resources. It concentrates power is a relatively small group of people. It may distort the kinds of research that people do. It may be biased in favor of people who have strong connections. Is there a better way?

Using journal publications to provide evidence of research quality and

[30]If you cannot name these journals, let me know. I'd be interesting to know why you have read this much of the essay.

[31]This paper was presented in a round table discussion [1] at the January 2017 meetings of the American Economic Association on "The Curse of the Top Five." The other participants in the discussion were George Akerlof, Angus Deaton, Drew Fudenberg, and Lars Peter Hansen.

[32]Currently, the motivations for referees are a mixture of curiosity, an opportunity to influence the direction of their area of expertise, loyalty to the profession, desire to please influential people, and reciprocity. These motivations are not all positive, but many are. I would be reluctant to try to substitute high-powered direct rewards for the current compensation.

impact is an equilibrium phenomenon. It is hard to imagine an alternative replacing the status quo without an unlikely, dramatic shock (journals suddenly becoming economically impractical) or a transition period during which the profession alternative methods of signaling quality arise and gain credibility. I propose what may be relatively low cost variations of existing schemes.

The peer review process common in academic economics is not the only way to screen articles. Other disciplines use peer review in different ways. Publication in many scientific subfields is time sensitive and the review process is accelerated. Computer Science and other fields use refereed conference proceedings as a measure of quality. Humanities professors who write books may submit proposals to several publisher simultaneously. ExpressO and Scholastica are systems to enable authors to submit to hundreds of law reviews simultaneously. Editorial decisions come quickly and authors systematically use a conditional acceptance at one journal to increase the chances of publishing their article in another journal. None of these institutions dominate the practices within economics, but they illustrate that there are many ways to solve similar problems.

The profession could provide greater recognition for authors of review articles. There exist visible outlets for reviews (for example, *Journal of Economic Literature*, *Journal of Economic Perspectives*, *Annual Review of Economics*, and a variety of handbooks). The best of these articles are extremely influential. The authors of successful overviews are typically prominent contributors. They have a particular perspective on the literature they discuss. The fact that they write reviews suggests that they find writing such articles less costly than others and perhaps that the authors have a comparative advantage in writing clearly.

Reviews can do two things currently done by journals. First, to the extent that reviewers have strong expository skills, they can separate the labor of improving exposition from generating and developing novel ideas.[33] Second, the prominence and nature of citations in prominent reviews could become a measure of quality that is meaningful in career reviews.

Of course, people write review essays is to advance a particular research agenda. Naturally one would expect review essays to have more, and more

[33]Currently, one can approach this problem by finding the right coauthor. My proposal suggests the alternative of finding the right senior expositor who can package ideas in an accessible review essay.

favorable, references to articles written by the author or the author's associates. This problem is a transparent version of a problem that we fear about the current system, where referees and editors might tip the scales in favor of their own agenda. The solution to the problem is the same: Having a variety of different scholars provide reviews and keeping track of the reputation of their appraisals.

Receiving attention in review essays plays a small role in academic reviews, but its impact is tiny relative to the role of journal publications. This balance could change if there are more reviews published and more emphasis placed on recent contributions. The American Economic Association or the Econometric Society could offer regular summer schools in which prominent scholars give reviews of their subfield. The participants in these events could be instructed to focus attention on recent work by junior scholars. Funding for these activities would be available if there are fewer resources devoted to editing. I also expect that marginal increases in the visibility of these activities can lead to increased participation. The same kinds of indirect motivations for participating in the current review process will presumably attract participation in a substitute. Of course, to the extent that these motivations lead to biased evaluations, the possibility of bias will remain.

Currently we have many journals. There is some consensus in the profession about the relative standing of the journals. It is natural for authors to aim to publish in the most respected journal (in order to maximize visibility and recognition), but with few exceptions, a rejection means starting the process over again. It need not be this way. The BE-Press journals allow authors to submit to a graded family of journals. Essentially, the editorial process does not only decide whether to publish an article, it also gives it one of four quality labels.[34] The American Economic Association and the Econometric Society now publish specialized journals and permit expedited review of articles rejected by flagship journals. One can imagine expanding either of these ideas. First, journals can offer to publish a paper with a grade.[35] It would take a while to reach a consensus on the meaning of different grades and it may not be possible for a review system that struggles with

[34]Itai Sher suggested in private communication the possibility of expanding the use of quality labeling within economics.

[35]My department asserts that "top-five" publications are neither necessary nor sufficient for tenure, but we talk internally about the need for one publication to be a "home run." Baseball suggests a grading system: single, double, triple, home run. I leave it to readers to consider variations from, say, international or American football or chess or go.

binary classifications to provide reliable, finer grades. Second, there could be broader agreement to share reports across journals.

Not far from the idea of grading published papers is the idea of creating more professional prizes. We have plenty of awards now. Career awards (Nobel Prizes, Distinguished Fellows of the American Economic Association, Fellow of the Econometric Society) recognize valuable contributions, but not at the stage that would be useful for tenure committees. Some awards, like the Clark Medal or the Yrjö Jahnsson Award, target younger scholars, but are too selective to replace journals as a way to identify quality. The European Economic Association gives young economist awards to scholars under 30; the Game Theory Society has a Shapley Lecture for scholars age 40 and under; and several field journals offer best paper prizes.

It is important to keep in mind that providing signals of quality is most important for junior scholars. This suggests that the alternatives that I suggest would be more useful if targeted towards young scholars (that is, directing authors of review articles to concentrate on important contributions made by untenured faculty members or by restricting awards to work done by younger researchers). A more radical approach would be to conscientiously devote more space in top-five journals to the work of junior faculty members.[36] I do not make this suggestion because the current system discriminates against young researchers (my experience suggests that it does not, although I lack objective data to support this assertion), but because information about the importance of the work of juniors is much more important than information about the work of seniors. We should be looking for ways to generate this information that are more efficient than the current system.[37]

[36]At this point, I make this proposal to be provocative. I cannot see how to make it operational. If the *AER* only published papers written by untenured faculty, would it maintain its reputation? How would it handle co-authored papers? Would untenured scholars send their best work to other outlets to avoid the stigma of publishing in a journal that favors youngsters?

[37]While searching, we should be aware of possible unintended consequences. Junior faculty implicitly are competing for a fixed supply of faculty positions. I have considered the problem of getting more accurate information about quality, but changing the review process will inevitably change the kind of research that is done and it will change the criteria for promotion. Presumably a change that uniformly decreases the time to decision at a journal would increase the number of publications needed for promotion. We must keep in mind that we would like to motivate good work and reward people doing this work.

10 Studying Editorial Outcomes

> You may never get to touch the Master, but you can tickle his creations. T. Pynchon

Editorial data bases store a large amount of useful information about the review process. Several scholars have taken advantage of this kind of information to conduct studies on trends in the publication system (Card and DellaVigna [6] and Hamermesh [14]), the effects of rule changes on submissions (Card and DellaVigna [7]), differences between journals (Guo [13]), potential bias in the review process (Card, DellaVigna, Funk, and Iriberri [9] and Hengel [16]), and what drives editorial decisions (Card and DellaVigna [8]).

I would like the profession to establish performance measures that made it easier to evaluate editorial quality. I doubt that there will be a consensus on what the constitutes ideal performance. There is often disagreement between experts about the value of a new paper. Although only a small fraction of papers receive attention ten years after publication, there will be persistent disagreement about what research has been the most valuable. I do not expect an empirical study to identify how many poor editorial decisions contributed to negative tenure decisions or what editorial practices contributed to these mistakes. But we can make objective information (about time to decision and the resources used in making decisions) more widely available; we can aspire to identify discrimination when it occurs; we can become a bit more confident in estimates of what causes changes in the selection of topics and the length of papers; and we probably can learn more about the effects of changes in journal policies on authors' behavior.

I would like to see the profession gather more information about the review process with the goal of identifying and eliminating bias when it exists, rewarding and supporting the best authors and reviewers, and informing the search for improvements to the current system. As the citations in the previous paragraph suggest, excellent empirical researchers are interesting in these questions. Data exist. But the data are not publicly available and journals are justifiably reluctant to make the data available to protect authors and reviewers. I am aware of delicate negotiations that have taken place to make some of this information available to researchers and careful efforts to use the data responsibly. I hope that journals will find resources to conduct more internal studies and that the profession words to find ways to make as

much data secure enough so that interested researchers can study it.[38]

I look forward to reading these studies and hope that they help future editors avoid my mistakes.

References

[1] AEA. The curse of the top five, January 2017.

[2] Ofer H. Azar. The review process in economics: is it too fast? *Southern Economic Journal*, pages 482–491, 2005.

[3] Theodore C. Bergstrom. Free labour for costly journals? *Journal of Economic Perspectives*, 15(4):183–198, 2001.

[4] Jonathan B. Berk, Campbell R. Harvey, and David Hirshleifer. How to write an effective referee report and improve the scientific review process. *Journal of Economic Perspectives*, 31(1):231–44, 2017.

[5] Stephen Buranyi. Is the staggeringly profitable business of scientific publishing bad for science?, 2017.

[6] David Card and Stefano DellaVigna. Nine facts about top journals in economics. *Journal of Economic Literature*, 51(1):144–61, 2013.

[7] David Card and Stefano DellaVigna. Page limits on economics articles: Evidence from two journals. *Journal of Economic Perspectives*, 28(3):149–68, 2014.

[8] David Card and Stefano DellaVigna. What do editors maximize? evidence from four leading economics journals. Technical report, University of California, 2017.

[9] David Card, Stefano DellaVigna, Patricia Funk, and Nagore Iriberri. Are referees and editors in economics gender neutral? Technical report, National Bureau of Economic Research, 2019.

[38] There is also room for more theoretical analyses. Here are three examples: Azar [2] points out that short review times may have the unintended consequence of increasing the number of submissions, increase workload, and increase delay. Ellison [10] investigates how review practice may influence the mix of publications. Leslie [20] argues that publication delays are a consequence of submission fees that are too low.

[10] Glenn Ellison. Evolving standards for academic publishing: A q-r theory. *Journal of Political economy*, 110(5):994–1034, 2002.

[11] Glenn Ellison. The slowdown of the economics publishing process. *Journal of Political Economy*, 110(5):947–993, 2002.

[12] Glenn Ellison. Is peer review in decline? *Economic Inquiry*, 49(3):635–657, 2011.

[13] Dongbo Guo. What drives paper citations. Technical report, Georgetown University, 2017.

[14] Daniel S Hamermesh. Six decades of top economics publishing: Who and how? *Journal of Economic Literature*, 51(1):162–72, 2013.

[15] James J. Heckman and Sidharth Moktan. Publishing and promotion in economics: The tyranny of the top five. Technical report, University of Chicago, 2019.

[16] Erin Hengel. Publishing while female. Are women held to higher standards? Evidence from peer review. Technical report, University of Liverpool, 2017.

[17] Albert O. Hirschman. *Exit, Voice, and Loyalty: Responses to the Decline in Firms, Organizations, and States*. Harvard University Press, 1970.

[18] Elisabeth Kübler Ross. *On Death and Dying*. Routledge, 1969.

[19] Vincent Larivière, Stefanie Haustein, and Philippe Mongeon. The oligopoly of academic publishers in the digital era. *PloS One*, 10(6):e0127502, 2015.

[20] Derek Leslie. Are delays in academic publishing necessary? *American Economic Review*, 95(1):407–413, 2005.

[21] John Morgan, Justin Tumlinson, and Felix J. Vardy. The limits of meritocracy. Technical report, International Monetary Fund, November 2018.

[22] Michael Szenberg and Lall Ramrattan, editors. *Secrets of Economics Editors*. MIT Press, 2014.

[23] William Thomson. *A Guide for the Young Economist*. MIT Press, 2001.