Journal of Economic Perspectives—Volume 31, Number 1—Winter 2017—Pages 231–244

How to Write an Effective Referee Report and Improve the Scientific Review Process

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The review process for academic journals in economics has grown vastly more extensive over time. Journals demand more revisions, and papers have become bloated with numerous robustness checks and extensions (Ellison 2002b; McAfee 2010; Spiegel 2012; Harvey 2014). For example, Card and DellaVigna (2013) document that recent published papers are on average three times longer than in the 1970s—a total which excludes the many robustness checks now contained in online appendices. Even if the extra resulting revisions do on average lead to improved papers—a claim that is debatable—the cost is enormous. We argue that much of the time involved in these revisions is a waste of research effort.

The leading explanation for the expansion of the review process, as modeled by Ellison (2002a), is that social norms have evolved toward increased demands for revisions. We believe that part of the explanation for why the profession has evolved to this equilibrium is that referees feel the need to demonstrate their intelligence or industriousness to editors by identifying problems in papers. The result is that in many cases reviewers inflate minor blemishes to the status of major flaws.

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† For supplementary materials such as appendices, datasets, and author disclosure statements, see the article page at
https://doi.org/10.1257/jep.31.1.231

doi=10.1257/jep.31.1.231

Another cause for concern is the level of disagreement amongst referees. As Welch (2014) documents in a study of eight prominent journals in economics and finance, in cases with multiple referees on the same paper, the probability that one referee recommends inviting a revision rather than rejecting, conditional on another referee doing so, is only marginally higher than the unconditional probability. Furthermore, the correlation of referee recommendations at a major finance conference was only 0.28. This pattern of disagreement suggests a high level of arbitrariness in the review process.[1]

To identify and highlight what is going right and what is going wrong in the reviewing process, we wrote to a sample of former editors of the *American Economic Review*, the *Journal of Political Economy*, the *Quarterly Journal of Economics, Econometrica*, the *Review of Economic Studies* and the *Journal of Financial Economics*, and asked them for their thoughts about what might improve the process. We found a rough consensus that referees for top journals in economics tend to make similar, correctable, mistakes. The italicized quotations throughout this paper are drawn from our correspondence with these editors and our own experience. Their insights are consistent with our own experiences as editors at the *Journal of Finance* and the *Review of Financial Studies*. Our objective is to highlight these mistakes and provide a roadmap for how to avoid them.

This article is not a comprehensive "how-to guide" for refereeing. Readers interested in such a guide can consult Berk, Harvey, and Hirshleifer (2015), or the essays in this journal by Hamermesh (1992, 1994). Instead our objective is to draw attention to specific shortcomings in the process that we believe can and should be improved. We begin by discussing what we see as three central current problems of refereeing in economics: 1) giving appropriate weight to the importance and innovativeness of a paper (and not being distracted that such papers may have minor flaws); 2) drawing a clear and explicit separation between comments that must be dealt with to make the paper publishable and suggestions that do not affect the publication decision and are therefore optional; and 3) living up to the implicit contract involved in a revise-and-resubmit process. We then offer thoughts on some other topics involved in refereeing: conflicts of interest, cover letters, politeness, and acting ethically.

Importance: The Hardest Decision

Perhaps the hardest part of a referee's job is assessing the *importance* of the paper, which involves judgments about whether a paper is of sufficiently broad interest and offers a sufficiently original contribution to be appropriate for the journal to which is has been submitted. Submissions exceed the available space in all A-level and most B-level journals; there are plenty of "correct" papers that do

[1] Similar findings are reported in a study of 823 submissions to the *Journal of Applied Psychology* by Gilliland and Cortina (1997). In an earlier study, Laband (1990) tries to measure the value added by referees by analyzing the relation between reviewer comments and citations.

not make a significant enough contribution to existing knowledge to appear in a top-tier journal. The editor needs to assess the importance of the contribution. Thus, a referee report should contain a scientifically based argument that explains the referee's assessment of the importance of the work and details the considerations that bear upon your judgment. Only an argument that is scientifically based is useful and informative to an editor in making the decision.

> *I quickly learned that there were three critical determinants of whether a paper was publishable in the Journal: (i) whether it addressed a question of sufficiently broad interest; (ii) whether it made a sufficient leap over existing literature; and (iii) whether the analysis was correct. Since most of the papers I received were "correct", my decision often boiled down to considering how well the paper fared according to the first two criteria. … I often felt that if referees more clearly understood the factors that make a paper publishable, they would organize the reports accordingly, making them more useful overall.*

The importance of a contribution can be undervalued in some cases by referees and editors. After all, papers that are more ambitious are often more likely to have loose ends, which gives referees and editors a reason to avoid taking a chance on them. Indeed, Arrow (1995) pointed out: "I think the publication selection procedure at the major journals has become methodologically more conservative, more given to preferring small wrinkles in existing analysis to genuinely new ideas. This conservative tendency also appears in the allocation of grants by government agencies and in faculty appointments and promotions."

Thus, we offer this advice to referees: Do not dismiss papers that attack larger issues merely because flaws can be found. The important question that you need to assess is whether the flaws actually invalidate the contribution. If the flaws do not rise to this level and you judge the contribution to be important enough to warrant publication, then you should recommend publication. All papers have flaws, and no amount of revision removes all uncertainties. There is always need for further research to provide deeper perspectives. Try to ask yourself the following question: Flaws and all, would I have been pleased to have written such a paper? If yes, that gives a strong hint that it should be strongly considered for publication, flaws and all.

Separating the Essential and the Suggested: The Highest-Cost Mistake

All papers have weaknesses, and catching a serious problem can be an indicator of referee quality. As a result, referees have a perverse incentive to persuade the editor that their own intellectual work is of high quality and that they are smart by pointing to minor blemishes and trumpeting them as serious problems. The distinction between these two categories may not be obvious to an editor who is not a specialist in the area of the paper: for discussion of this dynamic, see the signal-jamming model of Hirshleifer (2015). So by pointing out these supposedly

critical problems and requiring authors to address them, referees believe they are improving their reputation with the editor.

> *Some younger referees feel that they need to be overwhelmingly negative about everything in a paper in the report to the author to prove their own mettle and critical insights. They don't appreciate the need to at least demonstrate that they have read the paper and to provide some kind comments indicating some understanding of what the author is trying to do.*

Often, the ultimate outcome of such signal-jamming is an unwieldy and lengthy paper. This raises the question of why signal-jamming would be more important now than in the past. One possible reason is that growth in the profession makes it hard for an editor to have an independent assessment from personal contact of the quality of referees—even those at top schools. This increases perceived incentives to boost reputation by other means. Also, growth in the body of knowledge needed to evaluate papers makes it harder for editors to see through signal-jamming efforts (though in our experience, signal-jamming referees are often not nearly as subtle as they think they are).

Perhaps more importantly, signal-jamming pressure may cause social norms about standards for publishability to evolve over time toward increased demands for revisions, in the spirit of the model of Ellison (2002a). When referees engage in signal-jamming, more ambitious and potentially more important papers tend to lose in competition with routine papers in the review process because of a standard that effectively demands, as a prerequisite for publication, the virtual absence of minor shortcomings. The emphasis on superficial perfection over substantive importance may have the disastrous side effect of reducing the incentive to engage in innovative research.

> *I handled a paper in which the reviewer offered micromanagement of how the paper should be written, and made a specific demand for how the paper should be repositioned—in a way that highlighted a paper of the reviewer's. As for cases in which reviewers demanded numerous unimportant extensions or robustness checks—this was so common that specific examples are not especially noteworthy.*

One key step to reduce dysfunctional signal-jamming is that a referee report should divide comments into two clearly demarcated sections: 1) problems that make the paper unpublishable, which (if revision is invited) must be addressed before the paper is publishable; and 2) problems that are not essential for the publishability of the paper, which should labeled as "suggestions." From our perspective, perhaps the most common and most crucial mistake most referees make when providing a revise and resubmit recommendation is not clearly distinguishing between suggestions that are required for publication and ones that are not.

From the referee's perspective, the key difference between these two categories is the amount of justification that is required in the report. Statements about

problems that must be addressed before publication require rigorous justification; others do not. More specifically, must-address statements require a *scientifically convincing* argument for why these problems currently render the paper unpublishable. Of course, the same requirement may lead to a recommendation that the paper be rejected. A referee is *obligated* to provide a scientifically based argument for why a perceived problem renders the paper unpublishable. The argument needs to be clear and understandable to the editor (and authors). After all, the goal is for this argument to be used in the editor's decision.

It is surprising how many reviewers have a highly refined olfactory sense; several editors write about handling empirical papers in which a referee comments on the "smell" of the results. A referee hunch that theoretical or empirical results are wrong is not a scholarly argument against a paper's conclusions. Instead, as referee you must provide scientifically based reasons *why* you suspect the results are not correct.

> *A theory paper is submitted to the Journal and a senior referee recommends rejection because they "do not believe the proof". This is insufficient. While it is expected that the referee should be skeptical, the referee needs to provide a solid foundation as to why the proof is incorrect. Hunch is not sufficient.*

If there is a mistake in the proof, the appropriate recommendation is often rejection, though in some cases a mistake is fixable. A mistake can also be identified by finding a counterexample to a claim. However, if the authors demonstrate (correctly) an error in your counterexample, then their proposition stands. It is not grounds for rejection to retreat into generalized skepticism. A revise-and-resubmit for a theoretical paper can also ask the authors to explain how they reconcile their results with existing findings, or the extent to which their result may depend on nonstandard assumptions. But remember that it is possible that the results in the paper are right and it is the previous literature that got it wrong.

When it comes to empirical papers, a Bayesian should be skeptical of a conclusion that has a low prior probability—more evidence needs to be brought to bear, and greater validation of the evidence is required, to be persuasive. But a referee cannot dismiss evidence simply because he or she finds the results to be surprising. Obviously, surprising results have a greater probability of being wrong, so the appropriate response is to request confirming evidence. You can ask the authors to explain how they reconcile counterintuitive or unexpected empirical results with existing findings. You can also evaluate the robustness checks that the authors have done and consider whether it might be important to perform a limited number of other robustness checks. But in making such comments, the explicit line between what is necessary and what is suggested should be preserved. If the author satisfies these additional requirements, then the referee should leave it to subsequent research to evaluate whether existing preconceptions need to be updated based on the paper's results.

An empirical paper is submitted to the journal and the referee recommends rejection because "the empirical results do not pass the smell test". While there are plenty of reasons that the result may not be solid, the referee needs to ask questions or request specific robustness tests. "Smell" is an insufficient reason for rejection.

The review process for academic journal articles should sharply focus on what is essential. This focus allows authors greater freedom to write papers in the way that they think is best. All else equal, scholars should be able to develop their ideas as they see them. On issues that are not essential for publishability, there is no presumption that authors and referees need to see eye to eye. As a referee, it is important to keep in mind that unless comments about secondary issues are carefully labeled as such, such comments are inherently coercive. Few authors have the luxury of risking rejection of a project in a top journal for the sake of skipping the n^{th} robustness check mentioned by a referee, or of expressing their ideas in the way that the author thinks is most effective but the referee does not

The Implicit Bargain in a Revise-and-Resubmit

As a referee, it is important to keep in mind the implicit deal you are making with the author when you recommend a revise and resubmit: if the author satisfactorily addresses the issues that you have raised, you will recommend publication. When you make a revise-and-resubmit recommendation, you are actually making three statements: 1) the paper is of sufficient importance in terms of scope and findings that you believe it is suitable for the journal at hand; 2) there are problems with the paper that currently make it unpublishable in its current form; and 3) these problems are correctable.

In short, you are helping the editor provide a road map to publication. Keep in mind that the editor's road map will require heavy investment of time by the authors and usually relies heavily on referee advice. It is also an implicit contract. If the authors address these requests in a satisfactory way, then the editor will likely accept the paper. This should be the case even if other good ideas for improvement occur to the referees in the next round. Avoid putting the editor in the very unfortunate position of discovering that there are serious problems with the paper that were not noticed in the first round. It is very important that your own ideas for improvement be as refined as you can make them in the *first* round, so that the editor is positioned to offer a useful and reliable road map.

In a revise-and-resubmit recommendation, there will likely be other problems with the paper that are not severe enough to render the paper unpublishable. For such other problems, you do not as a referee need to provide detailed reasons (or a scientifically based argument) for your opinion. However, if the author chooses not to address these problems, you cannot use that lack of reaction as a basis for recommending rejection of the revised draft. In many cases, reasonable people can disagree about what should and should not go into a given paper. Ultimately, the

author's name goes on the paper, not the referee's name, so the decision on how best to write the paper is the author's. Do not hold a submission hostage because, in your (undoubtedly wise) opinion, the paper could be better written.

I handled a paper where the referee provided a thoughtful report which led to a revise and resubmit. The author addressed the comments and resubmitted. On the revision, the referee produced a new, long list of additional comments—each of which could have been detailed in the initial report.

In making requests of authors, weigh the costs of the request. It is not enough that a particular request will improve the paper. The benefits must exceed the costs, so that the improvement has positive net present value. Since the author bears the costs, it is easy for a referee to make absurd demands thoughtlessly. Don't.

I receive a positive referee report on a paper that uses 19 years of hand-collected data. The earliest data was the most time consuming to load because the authors had to visit the archives and deal with paper documents. The referee insists that the data be expanded back to 20 years. This 20th year was not a special year but it was clearly going to be punitive for the authors to collect this extra year of data. In addition, the extra year would unlikely be influential for the results. This is an example of "make work" and as Editor it is often very difficult to separate the work that must be done from the frivolous work. In this case, it was easy and I instructed the authors to ignore the referee's comment. I never called on that referee again.

By following these suggestions, the revise-and-resubmit process should generally take just one round before reaching a final decision on publication, and that is how referees should view the process. Of course, sometimes a second round may be required for unavoidable reasons. For example, perhaps the authors only partially addressed the necessary changes specified in the first round. In such cases, referees and editors should pause and consider the wisdom of going another round rather than just recommending that the paper be rejected. A related possibility is that in an intelligent and honest effort by authors to address the comments of the first round, unforeseen issues have unavoidably arisen. Finally, cases may arise in which a request for a second revision is based on an issue that should have been apparent on the first round. In this case, the referee has made a mistake, and the referee should admit this oversight to the author and to the editor. Such requests present the editor with a hard choice, and should be rare.

I get a positive recommendation from a referee with a list of items for the authors to take care of in a revision. I issue a R&R [revise and resubmit] and the paper comes back with the authors making a serious effort to revise the paper and they address each of the referee's comments. The referee declines to provide a report on the resubmission but writes to me saying that he recommends rejection because of "insufficient incremental contribution". The rejection had nothing to do with the author responses. The referee changed his

mind with no reasoning. If there was insufficient incremental contribution, that case must be made in the initial submission.

In summary, if the author has appropriately satisfied the key requests of a previous revise-and-resubmit referee report, the paper should then be accepted. Do not invent a new set of requests—or even worse, reject capriciously.

Other Advice

Thus far we have highlighted the mistakes by referees and editors that impose the highest costs on authors. In this section, we will briefly cover a few other important lessons uncovered in our own experience as editors and in our correspondence with other editors.

Declining the Invitation

Upon receiving a referee request, decide immediately whether you will be able to complete the review within the allotted time. If not, respond immediately with suggestions for alternative reviewers. Often the editor is not a specialist in the specific area, and does not know exactly who the best match for a submission is, so thoughtful suggestions for other reviewers are valuable. Also, editors understand that peak loads can create the occasional need for a declined invitation or a request for an extended deadline—or at least greatly prefer this to having to deal with a severely late report, or a systematically derelict referee.

Agreeing to do a report at the requested deadline and then never doing the report on time and never responding to reminders or queries about the report me or the editorial office. In many of these cases, the individual did not have time to do the report but felt compelled to say they would do it and embarrassed to respond. It is much better for all involved to upfront decline a request immediately then to say yes when it won't be feasible to do the report in a timely manner. You lose a lot more face in my view from going into hiding and not responding or doing reports much later than promised, than from being upfront and saying you can't do the report at all.

Try to form an objective assessment about whether you are a good match for the submission. If you know little about the relevant literature, it might be wise to contact the editor to make sure that a mistake has not been made. The editor may have had a good reason for selecting you, and verifying this can make your job more useful and focused. For example, in a paper with both theory and empirical work, the editor might be seeking the views of the empiricist for the empirical sections and of a theorist for the theory sections.

If you have reviewed the paper for another journal, again immediately alert the editor. Some editors would prefer getting a fresh view. Let the editor make

that decision. Other editors may be satisfied that the referee can assess effectively whether the paper has improved.

You might also feel that you cannot complete the report anonymously. For example, you may have discussed the paper and feel your report would be so close to the discussion it will be obvious to the author that you are the referee. If you wish to maintain your anonymity, it is reasonable to decline to referee the paper. If you do not mind the loss of anonymity, you should still alert the editor, since nonanonymous reviewing can create agency problems. You may also provide advice to the editor in a letter without a report. Your views will be important even when they cannot be passed on to the author, though such service is not as useful to the editor as a formal report. In this case, the same rules apply. If you feel the paper is not publishable you must provide a scientifically based argument that will allow the editor to make an informed judgment.

If you are already working on a paper that overlaps substantially with the submission, you should notify the editor. As discussed further below, this is a potential conflict of interest and might be a reason to recuse yourself.

Finally, if a paper is obviously far below the bar at the journal where it has been submitted, a short (one-page) report is perfectly acceptable. It should be straightforward to provide a scientifically based justification. If it is not, you should reconsider whether the paper really is that far below the bar. In this case, impress the editor by returning your report within a week—not by spending time on a lengthy analysis of what, by hypothesis, is an obvious conclusion that would be shared by the overwhelming majority of referees. If the editor disagrees with your assessment, the editor can then turn to another referee without undue delay.

The Cover Letter

The cover letter should contain an assessment of the contribution of the paper along with very concise reasoning supporting your recommendation. The editor wants to know the positives as well as the negatives of a paper. The cover letter should be brief. It should *not* be a cut and paste of the referee report, which is a waste of the editor's time. In addition, your cover letter and recommendation to the editor should be consistent with your report. Otherwise you impose on the editor the task of explaining the discrepancy to the author. This will irritate both of them.

> *I assigned a paper by a well-known author to a particular referee. I had a very low prior on the paper. The referee submits the report which is amazingly positive using words like "pathbreaking", "important insight", "game-changer", "will change the way people think about a problem", etc. I do notice that the referee has checked off "reject" in the electronic system—which I assumed must have been a mistake. However, after reading the cover letter, I understand. The referee thinks the paper is a poor idea and is nowhere near the hurdle for the journal. I reject the paper and the author is furious and immediately appeals the decision quoting the very positive referee report. Such inconsistencies between recommendations and the report frustrate both authors and editors—and create a lot of extra work for the editors.*

The ideal cover letter should succinctly provide three types of information. First, it should include a statement of the broad interest and importance of the paper's contribution relative to existing work. Keep in mind that the editor may not be an expert in this subfield, and it is often hard to figure out the paper's main point or line of reasoning. Second, is the analysis convincing? Third, the cover letter should offer a frank assessment as to whether the paper is publishable as it stands, or whether the paper is likely to be publishable within one round of revision.

Be decisive. As a referee, you are being asked to make a recommendation: accept, revise, or reject. The reasons given for the recommendation are at least as important as its summary value. But it is much easier for the editor to understand which considerations the referee considers most important when a summary value is provided. For example, if you are recommending a revise-and-resubmit, you must give a sound reason, or the editor will likely reject the paper.

It is fine to mention reasons for uncertainty about the recommendation. If the paper is somewhat outside your area, you might suggest that a second opinion be sought, and you should provide names of candidate referees, and if possible, what specific issues the alternative referee can address that you felt were outside your area of expertise. But a request for a second opinion should not be used as a cover for indecisiveness or not wanting to spend a lot of time studying details.

Length of the Referee Report

A referee report is not a mind-dump about the paper. Polish your report with an eye to condensing. It is almost always possible with an extra revision by the reviewer to make the report both shorter and more incisive.

The main purpose of the report is to help the editor decide whether to publish. It can also be extremely valuable to have extra material to help the authors improve the paper, but only when presented in a way that does not interfere with the main purpose. So if comments about minor details are provided, they must be segregated into a clearly marked separate section that is easy for the editor to skip. Broad philosophical issues or rhetorical flourishes should be eliminated if they have no bearing on improving the paper or on determining whether the paper should be published. Finally, our suggested format of sharply separating comments which affect publishability from those which are just to improve the paper will be very helpful for the editor in making a decision efficiently and accurately.

> *Overly long referee reports are a burden on editors. Unless a referee needs to make extremely technical points, 2–3 pages should be sufficient. Going beyond this raises the likelihood of coercion/overburdening. As an editor, I view a 10-page referee report as punitive.*

We would not go so far as this editor to endorse a blanket principle that 2–3 pages is the right length for almost all papers, but brevity is valuable. Indeed, brief reports often contain more important content than lengthy ones. We have seen reports that are too minimal in both length and thought, but the more

common problem is lengthy reports that are not accompanied by correspondingly extensive insight.

A more mechanical point, but one that has a surprisingly large effect on the efficiency of the review process, is that comments should be numbered. A report consisting mainly of discursive undifferentiated paragraphs is a burdensome disservice to all involved in the review process. Insightful discussion is a plus, but not at the expense of a clear bottom line that includes a numbered list of suggested actions or problems. We suggest separate numbering for category 1 of key problems and category 2 of suggestions.

Ethics

A referee who has any conflict of interest with the manuscript must alert the editor—promptly, and *before* agreeing to accept the assignment. The editor might decide to find a new referee or might ask the original referee to complete the report. Conflicts that require alerting the editor include when an author of the paper is: a past (over the past five years), current, or planned coauthor; a current colleague; a former student or advisor; a close personal friend or family member; or a person with a financial relationship with the reviewer. Conflicts can also arise if you have current research that is competing with the research in the submitted paper. If the paper contradicts or corrects your research, you need to tell the editor. If there have been disputes between you and one of the authors in the past, alert the editor.

Manipulation by referees of recommendations and reports for the purpose of advancing or holding back publication with the goal of advancing their own work is unethical. One such manipulation is for a referee to focus on adding citations to the referee's own work.

> *I received a paper that I had a low prior on that cited the work of a potential reviewer. I assigned this person as the referee and the reviewer enthusiastically recommended a revise and resubmit. The report made reference to an extraordinary six extra citations of the reviewer's work. Essentially, the reviewer wanted to increase the cite count from two to eight in the paper. After carefully reading the paper, I rejected the manuscript.*

In addition, it is not acceptable to hold up the reviewing process with the goal of buying time for your own related research agenda.

> *I assigned a paper to a referee on a hot current topic. I sent many reminders to the referee because the report was very late. I then notice the referee posts a competing paper on SSRN. The competing paper cites the paper in review (which was also public on SSRN). I withdraw the referee request but the referee has successfully delayed the review process for the original paper. While it seems like the referee was successful, that was not the case. I alerted the Editors of the other top journals. I never saw the referee's paper in print.*

Of course, it is inevitable that a referee will sometimes get an idea based on reading a submitted paper for how to write another paper on the same broad topic. If the author has not distributed the paper publicly, and if you have already agreed to review a paper, you cannot write your own paper on the topic, as you are not in a position to reference the submitter's prior work appropriately.

In contrast, if a paper is publicly distributed, you are free to work on the same topic (just as everyone is), as long as you clearly cite the relevant paper as prior existing work. If you decide that you will be working on the same topic before completing your review, you *must* inform the editor immediately. The editor can then decide whether to remove you as referee; and whether to invite an additional, potentially more objective referee. To be ethical, do not advance your new paper in your work queue for the purpose of pre-empting the submitter's existing paper.

Do not seek to game the system by magnifying a paper's drawbacks or contributions. In addition to being unethical, such gaming can hurt your reputation. Editors often catch unethical reviewer behavior.

Finally, if you are aware that the authors have behaved unethically with respect to the submission—such as submitting essentially the same paper to multiple journals, or they have committed plagiarism—notify the editor immediately. Focus on the facts, not on expressions of contempt or outrage.

Courtesy

Be courteous in the report and focus on substance. Avoid ascribing bad intent to authors ("The authors were trying for a cheap publication," "The authors were trying to brush past literature/conflicting findings under the rug …") and focus on the substance of the paper. Do not insult the authors, or use overly emotional or accusatory language.

> *Reports that are totally negative and nasty in language undercut the critical points made [in the report]. And one can be critical and tough and still be polite in the write up. If one really wants to be nasty to the authors, do it in the cover letter to the editor and not in the report itself.*

If there are indications of intellectual dishonesty, state the facts rather than speculating on intent. If an accusation of such dishonesty is made, leave it for the cover letter to the editor, who can then decide whether it should be passed along to the authors.

Moreover, be mindful that referees are subject to the same behavioral biases as everyone else. It is a mistake to approach a paper looking for evidence that confirms your pre-existing viewpoint and discount evidence that does not. Try to avoid forming unduly favorable assessments of work that appear to be written by well-known authors, members of the referee's personal social network, or papers that cite the reviewer.

Conclusion

The peer review process that occurs under the auspices of academic journals is crucial for the advancement of research. It is a central mechanism by which the profession determines collectively, through a decentralized process, both the standards for economic research and what constitutes progress in such research. However, we believe that fundamental change in how researchers review each other's journal submissions is needed to improve the integrity, quality, and efficiency of the review process. We believe that such change in refereeing culture is possible, and that when this is widely recognized, it will happen. Such change will improve how new research is developed and communicated, and will allow scholars to reallocate time from navigating the publication process to developing innovative research.

Refereeing is a hard job. Unfortunately, just like others in the profession, the three of us are all guilty of making many of the mistakes highlighted in this article. We hope that by discussing guidelines for referees, and by pointing out some dysfunctional features of current refereeing practices, we can improve our own refereeing, and play some small part in changing the culture of the review process in economics. It may be too ambitious to aim for a world in which nobody makes the mistakes that we underscore. But to improve, we need to be cognizant of our failings. A general awareness that certain refereeing practices are barriers to the advancement of knowledge would be a very large step forward.

■ *We thank the editors—Mark Gertler, Gordon Hanson, Enrico Moretti, and Timothy Taylor—for extremely helpful comments.*

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