EDIFYING EDITING

by R. Preston McAfee*[,1]

I've spent a considerable amount of time as an editor. I've rejected about 2,500 papers, and accepted 200. No one likes a rejection and less than 1% consider it justified. Fortunately, there is some duplication across authors, so I have only made around 1,800 enemies.

The purpose of this little paper is to answer in print the questions I am frequently asked in person. These are my answers but may not apply to you.

Who makes a good editor?

When Paul Milgrom recommended me to replace him as a co-editor of the *American Economic Review*, a post I held over nine years, one of the attributes he gave as a justification for the recommendation was that I am opinionated. At the time, I considered "opinionated" to mean 'holding opinions without regard to the facts,' and indeed dictionary definitions suggest 'stubborn adherence to preconceived notions.' But there is another side to being opinionated, which means having a view. It is a management truism that having a vision based on false hypotheses is better than a lack of vision, and like all truisms it is probably false some of the time, but the same feature holds true in editing: the editor's main job is to decide what is published, and what is not. Having some basis for deciding definitely dominates the absence of a basis. Even if I don't like to think of myself as "obstinate, stubborn or bigoted," it is valuable to have an opinion about everything.

Perhaps the most important attribute of an editor is obsessive organization, processing work unrelentingly until it is done. The *AER* is a fire-hose: in my first year I handled 275 manuscripts. In my first year at *Economic Inquiry* I processed 225 manuscripts to completion. I typically write referee reports the same day they are requested, so that I keep my inbox clear. I did this even in the days before electronic inboxes. This "clear the inbox" strategy may not be a good strategy for success in life but it is a great characteristic in an editor. Otherwise, upon returning from a couple of weeks of vacation, there may be a mountain of manuscripts visible on satellite photos awaiting processing.

The third characteristic of successful editors is a lack of personal agenda. If you think papers on, say, the economics of penguins are extraordinarily important, you risk filling the journal with second-rate penguin papers. A personal agenda is a bias, and when it matters, will lead to bad decisions. As everyone has biases, this is of course relative; if your reaction is "but it isn't a bias, I'm just right" you have a strong personal agenda.

The last attribute of a good editor is a very thick skin. One well-known irate author, after a rejection, wrote me "Who are you to reject my paper?" The answer, which I didn't send, is "I'm the editor." There are authors who write over and over, asking about their paper, complaining about decisions. If you lose sleep over decisions and wring your hands in anguish, or take every disagreement as a personal affront, it is probably best to decline the offer to edit a journal. One author wrote me, with no evidence of a sense of humor, that if I rejected his paper, he would be denied tenure and his three children would go hungry. My response, which I didn't send, was "Good luck in your next career." There are papers I wish I had accepted, three of them to be exact. Not bad for 2,500 rejections.

How do I become an editor?

One of the surprises of being an *AER* co-editor was the number of people who believe the journals are controlled by the top departments for the benefit of the top departments, that is, who believe in the conspiracy theory. This has been the prevailing theory of authors in spite of the wide editorial net cast by the *AER*. Three different authors (from departments without graduate programs) thanked me after I accepted their papers for breaking the conspiracy of journal editors to favor the top ten departments.

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I'm confident that there is no conspiracy, for if there were, I wouldn't have been chosen as a co-editor. The causality actually runs the opposite direction – people who publish a lot wind up hired by top departments. *Papers and Proceedings* is run, of course, for the benefit of the *AEA* president who organizes it and thus represents a conspiracy.[2]

Anyone can become an editor by being a super referee. Referees who respond quickly with thoughtful reports are appointed as associate editors after half a dozen years or so, and from there soon become co-editors.

What editorial strategies and tricks can you share?

An acceptance from the *Journal of Economic Theory* in the past was a list of possible decisions with a check mark next to "Accept." While everyone prefers an acceptance to the alternatives, this is really a pretty hideous notification method. Consequently, I decided to write what my assistant called the gush letter, in which I explained to the author why I was enthusiastic about publishing their paper, and why they should be especially proud of the contribution. Authors like this a lot – many have told me it was the only the positive feedback they have ever received from a journal – but it serves an additional role. If as an editor you can't painlessly explain why you are excited to publish a paper, you should probably reject it. If you can painlessly explain, then do so for the good of humanity – it creates a lot of social value at very low personal cost.

A great efficiency gain is to look at reviews as they arrive. About half the time I feel comfortable rejecting on the basis of a single negative review. Since the expected waiting time for the second review is usually two or three months, this procedure cuts the waiting time substantially.

When I am having trouble making a decision on a paper, one strategy is to talk it over at lunch. I provide a description of the issue and see where the conversation goes, peppering the discussion with the author's contribution. Whether a group of economists find the results intriguing is useful data on whether the paper will be well-received.

In his study of refereeing, Dan Hamermesh (1994) discovered that, conditional on not receiving a report in 3 months, the expected waiting time was a year. Economists often make promises that they don't deliver, which is a grim fact of editing. One author wrote chastising me for making him wait four months for a response to his submission; I politely responded that I had been waiting over five months for a referee's report from him! As a result, I often request more than the standard two reports. At the *AER*, more than two-thirds agree to review manuscripts, while at *Economic Inquiry*, that number is below half. To get two, I now need to request four. Finding referees used to be much more challenging and I would assiduously keep track of fields of expertise of everyone I encountered at conferences (making me quite unpopular), but SSRN makes finding reviewers much more straightforward since it is now easy to identify people with recent working papers on any topic.

I reject 10–15% of papers without refereeing, a so-called "desk rejection." This prompts some complaints – "I paid for those reviews with my submission fee" – but in fact when appropriate a desk rejection is the kind thing to do. If, on reading a paper, I find that there is no chance I am going to publish a paper, why should I waste the referees' time and make the author wait? Not all authors agree, of course, but in my view, we are in the business of evaluating papers, not improving papers. If you want to improve your paper, ask your colleagues for advice. When you know what you want to say and how to say it, submit it to a journal.

As noted above, some authors are irate about desk rejections on the principle that their submission fee pays for refereeing, or that they deserve refereeing. But in fact the editor, not referees, make decisions and I generally spend a significant amount of time making a desk rejection. I think of a desk rejection as a circumstance where the editor doesn't feel refereeing advice is warranted.

There are authors who attempt to annoy the editor. I'm not sure why they consider this to be a good strategy. I attempt to be unfailingly professional in my journal dealings, as this is what I seek in editors handling my work. Back when I had a journal assistant (everything is electronic now), I asked her to impose a "24 hour cooling off period" whenever I seemed to write something emotional or unprofessional. I still write and delay sending even now, if I feel at all peevish or irritated.

Authors, in their attempt to irritate the editor, will ask "Have you even read my paper?" This is a more subtle question than it first appears, for there is an elastic meaning of the word ‘read.’ The amount of time necessary to establish beyond a reasonable doubt that a paper is not suitable for a journal ranges from a few minutes – the paper’s own summary of its findings are incomprehensible or not ambitious – to many hours. One of the effects of experience as an editor is that the amount of time spent on the bottom half of the papers goes to about zero (except for the desk rejections, which get a bit more), and most of the time is devoted to those papers that are close to the acceptable versus unacceptable line.

Gans and Shepherd (1994)’s article created among editors what I think of as the fear of rejecting the “Market for Lemons,” based on the fact that Akerlof’s 1970 “Market for Lemons” paper was rejected by three prominent journals, including the *AER*. No one wants to go down in history as the editor who rejected a paper that subsequently contributed greatly to a person’s winning a Nobel prize. However, I eventually came to the conclusion that the fear is overblown. There are type 1 and type 2 errors and any procedure that never rejects the “Market for Lemons” produces a low average quality. One lesson, indeed, is to be open to the new and different. I use a higher bar for ‘booming’ topics that generate a lot of current excitement and hence may be a fad. (At the time of this writing, behavioral economics is such a topic.) A second lesson from Akerlof’s experience is to be careful in crafting rejection letters; the letters Akerlof received, with their smug acceptance of general equilibrium as the end state of economics, look pathetic today. Finally, Akerlof’s experience was unusual in that his rejection wasn’t perpetrated by Lord Keynes. Absent Keynes, who I think suffered mightily from the personal agenda problem discussed above, there are not so many great rejected papers.

What are some common problems with manuscripts?

Around 25% of the submissions to the *AER*, in my experience, are rejected due to poor execution. That is, the paper represented a good start on an article-worthy topic, but provided too little for the audience.

Most of my experience is editing general interest journals, and as a result my number one reason for rejection is that the paper is too specialized for the audience. When the interest in the paper is limited to a specific field, the paper belongs in a field journal, not in the *AER* or even *Economic Inquiry*. I expect submissions to make the case that the paper is of interest beyond the specific field and often ask “Why should a labor or public finance economist want to read this paper?” A good strategy is to identify the audience and then submit to a journal that reaches that audience.

A surprising number of papers provide no meaningful conclusion. I consider these papers to be fatally incomplete. I have seen one that had a heading “Conclusion” with only one sentence: “See the introduction.” Opinions vary but I consider a serious conclusion section to be essential. After going through the body of the paper – usually very hard work – it is time to get a payoff, which is delivered in the conclusion. The difference between an introduction – in which one motivates a problem and summarizes the findings – and a conclusion is that the reader has actually gone through the body of the paper at the point where they encounter the conclusion. Thus, the kinds of points you can make are different. If, after finishing the body of the paper, you really have nothing more to say, it is not clear why anyone wants to read the paper. The conclusion should be more than just a summary of the paper.

Paul Milgrom is fond of saying that theory papers can be evaluated based on generality and simplicity and it is important to remember that both are goods. I think Milgrom’s insight is similar to what is sometimes known as the “bang for the buck” evaluation; how much work do I have to do and time do I have to spend for the amount of insight I receive? Being clear about the contribution and relating it accurately to other papers makes the paper simpler to understand and more likely to be accepted.

Do you have any amusing anecdotes to share with us?

There is a lot of heartbreak in journal editing since most of the job is rejecting papers. If you are looking for amusing anecdotes, subscribe to Readers’ Digest.

The job of theory editor at the *AER* is unique in one way. There are thousands of people who believe they have a Great Economic Idea that economists desperately need to know. Let us agree to call these people "kooks" for want of a better term. Pretty much 100% of kooks are theorists; you won't meet a, say, physicist or physician with a Great Economic Idea that involved running regressions or doing lab experiments, although occasionally there is a table illustrating a correlation between some economic variable like lawyers or fluoridated water and per capita GDP.

An illustration of the Great Economic Idea is the value of time. A paper was submitted pointing out that the order of consumption of goods may matter; one may want to consume Alka Seltzer after a large meal, not before. The paper proceeds to compute the number of orders one can consume a given number of goods. Why the number of orders is interesting is not explained. It is an inessential and unsurprising detail that the author has never heard of multinomials and manages to get the formula slightly wrong. The important thing is that he submitted two papers, the second identical to the first, except that the term consumption has been replaced with production. Both papers have no references but have a helpful statement that the paper is so novel that there are no appropriate references. I received these prior to instituting desk rejections and sent both papers to one referee. To counter the author's assertion that economists have never considered the timing of consumption, the referee wrote a one sentence report: "Arrow-Debreu commodities are time-dated." The referee also provided two references and wrote in the letter to me that "the AER refereeing fee is just enough to buy a bottle of scotch, which helps me forget these miserable papers."

Another paper began with the memorable sentence "An economic system is like an electric power plant." The paper proceeded to analyze electric power generation in great detail. There were diagrams of power plants and discussion of Kirchoff's laws and other essential ingredients of electrical engineering. What was not present, however, was anything vaguely recognizable as economics, like prices, demand or even cost. There was no attempt to explain in what way a power plant was like an economic system. Not surprisingly, I rejected this paper, which prompted a boundless series of irate complaints including a claim that von Neumann worked on and was unable to solve the problem that the author had solved. No reference was given to demonstrate von Neumann's interest in the problem; the generous interpretation is that von Neumann only published when he actually solved the problem. After more than a dozen letters I eventually informed him that I would no longer open his letters. They kept coming for months.

The essential mystery of editing is why the reports I receive as an editor are so much better than the reports I receive as an author. Reading thousands of referees' reports has changed my perspective on reports. We may wait a long time for reports but they are generally serious, thoughtful and insightful. Authors who complain about referees usually focus on inessential details rather than the main substance of the review. By and large, reviewers understand papers well enough to evaluate them; when they don't, it is usually because the author failed to communicate very well. Moreover, referees offer good advice about how to improve the paper and take the research to the next level. It is worth remembering that the referee's task is to give advice to the editor, not to give advice to the author.

Many people write me saying that they have already refereed a manuscript for another journal and want to give the author a new chance. I see this response as wildly inefficient. First, the referee has a very good idea what the author has accomplished and can quickly review the current draft. Second, if the author has ignored serious issues pointed out previously, that is very important information about the quality of scholarship and I really want to know about it. Third, the fact that another editor selected the same person is a confirmation that we have selected well; papers should pass muster with experts in the field. The only circumstance where I don't want to hear from a repeat referee is when the referee recommended rejection for personal, unprofessional reasons, which is precisely the set of the circumstances where they won't tell me they reviewed the paper for another journal.

I overheard an author tell another economist at a conference what an idiotic referee he had for an *AER* submission. He went into some detail about all the stupid things the referee said and the economist listening to the story commiserated and wholeheartedly agreed with the author. You have probably already figured out that the commiserator was the referee in question. This referee had actually written a very thoughtful and serious report on a paper of a friend; as is sadly common, the author didn't appreciate the insight available in the report.

As a final anecdote, I received a report from a respected economist, who said in the letter to me: 'I have written a gentle report, because the author is obviously inexperienced and very junior, and I don't want to discourage him. But make no mistake: this paper makes no contribution and you should not encourage a revision.' The author of that paper, which I rejected, had already won a Nobel prize in economics.

What's up with Economic Inquiry?

I strongly recommend Ellison's 2002 paper on journal publishing. This paper definitely changed my perspective on problems with economics journal editing, so much so that I took action in 2007. Ellison finds that the profession has slowed down, doubling the "submission to print" time at major journals. What was unexpected for me was the finding that most of the slowdown is the number of revisions, not the 'within round cycle time.' I hadn't realized that the interminable wait for a response was common twenty-five years ago. What has changed, Ellison shows, is that we have about doubled the number of rounds. I had thought it was merely deficiencies in my own papers that caused me to revise three, four, even five times. But no, it is a profession-wide phenomenon.

Like most economists, I am personally obsessed with efficiency, and wasted resources offend me in an irrational way. The way economists operate journals is perhaps the most inefficient operation I encounter on a regular basis. It is a fabulous irony that a profession obsessed with efficiency operates its core business in such an inefficient manner. How long do you spend refereeing a paper? Many hours are devoted to reviewing papers. This would be socially efficient if the paper improved in a way commensurate with the time spent, but in fact revising papers using blind referees often makes papers worse. Referees offer specific advice that push papers away from the author's intent. It is one thing for a referee to say "I do not find this paper compelling because of X" and another thing entirely to say that the referee would rather see a different paper on the same general topic and try to get the author to write it. The latter is all too common. Gradually, like a lobster in a pot slowly warming to a boil, we have transformed the business of refereeing from the evaluation of contributions with a little grammatical help into an elaborate system of glacier-paced anonymous co-authorship. This system, of course, encourages authors to submit papers crafted not for publication but to survive the revision process. Why fix an issue when referees are going to force a rewrite of a paper anyway?[3] My sense is that the first revision of papers generally improves them and it is downhill from there.

The 'anonymous co-authorship' problem has an insidious aspect: having encouraged a revision, referees often feel obliged to recommend acceptance even if the paper has gotten worse. Referees become psychologically tied to the outcome because they caused it. I once directed an author to roll-back a paper to an earlier state, because a referee encouraged the author to make a mess of what had been a clean, insightful analysis.

When I was asked to recommend an editor for *Economic Inquiry*, it occurred to me that *EI* was ideally positioned for an experiment. It isn't sensible to experiment with extremely successful journals like the *AER* or *Journal of Political Economy,* because of the large potential downside. It also isn't very useful to experiment with a brand-new journal. New journals aren't on anyone's radar screen and it is extremely challenging to attract high quality papers to a new journal. As a result, successful new journals tend to be run in an autocratic way by a committed and talented editor; policies play a small role in the operation. As a result, the ideal experiment is a journal like *EI*, which has a decent, but not stellar, history.

I offered to serve as editor, provided I was given a free hand to experiment with policies, including the "no revisions" option. The *no revisions* option is a commitment by the journal to say "yes or no" to a submission, hence preventing the endless rounds of revision common at other journals and at *EI* itself. *No revisions* is an option for the author, not a requirement. I implemented *no revisions* when I assumed editorship in July 2007. About 35% of the papers are now submitted under this option.

At the time I started, Steve Levitt mentioned *no revisions* in his immensely popular Freakonomics blog and I was very surprised by the comments he received. Most anonymous commentators were negative. They (1) didn't think it necessary, (2) didn't think I could commit to it, or (3) ignored the fact that it was optional and considered whether it would be socially optimal for all journals to impose it.

No revisions is and should remain optional. Inexperienced authors are ill-advised to choose it; perhaps more importantly, authors with a very novel, difficult thesis will often need a conversation with referees to convince them. *No revisions* works best with experienced authors who know what they want to say and how to say it, and just want a forum to broadcast that to the profession. The option removes the journal from the business of rewriting papers and escalates the business of evaluating them. Consequently, the entire discussion based on what would happen if all journals forced all papers through the *no revisions* process is misguided; it is like saying that *Taco Bell* should not exist because it would be a bad thing if *Taco Bell* were the only restaurant.

Commentators who think *EI* can't commit aren't thinking clearly. The argument is the "thin edge of the wedge," which is to say, papers will be submitted that deserve revision but are too flawed to publish as is. But this is not a problem at all unless the journal is desperate for manuscripts – there are lots of other journals to take the author's revised paper. There have been at least a dozen manuscripts rejected that would have been clear revise and resubmits absent *no revisions*. That is a risk the authors take when they choose the option. There have also been half a dozen that would have received revise and resubmits but instead were accepted.

Finally, is the *no revisions* policy socially useful? The beauty of the option is that no one is required to use it; that about 35% of the submissions come in this form suggests some authors think it is a useful experiment. Only one journal has copied the policy to date, but the sensible thing is to wait and see if *Economic Inquiry* improves.

No revisions does not prohibit an author from benefitting from advice. In fact, at this time 100% of the authors who received acceptances under *no revisions* actually revised their manuscript in light of referees' comments. The difference is that these revisions were voluntary, not coerced. That is, the referees and editor say 'this paper meets our standards as is, but would be even better if . . .' and the author is then free to improve the paper.

I've spent a lot of time thinking about the co-editor process. At the *Journal of Economic Theory,* associate editors are *de facto* co-editors in the sense that they send papers to referees for review and make recommended decisions which almost always stick. There are about 40 associate editors, which insures there are always a couple of bad ones. Bad co-editors pollute journals, preventing the journal from having consistent standards and responses. The more co-editors, the more likely the problem of conflicting standards and expectations arises. To be specific, there were auction papers published by *JET* while I was an associate editor that were not as good as papers I rejected, a very frustrating event for an associate editor and more so for the rejected author. However, employing few co-editors makes the job larger than most would accept. So what is the right organizational form?

Empirically, the top journals run four to six co-editors. They are distinguished by field. However, being an editor at this rarefied level is strongly rewarded by the profession; at lesser journals, the professional benefits are much smaller. Consequently it will be much more difficult to find people willing to take a quarter of *EI* than, say, a sixth of the *AER,* even though a sixth of the *AER* represents handling more manuscripts per year. Moreover, the top journals require "jack of all trades" who can handle papers in a very diverse set of areas. As an *AER* co-editor, I had to handle theory papers on trade, finance and environmental economics, fields in which I had never read a paper when I started. The "broad general co-editor" is very hard to find, even for the top journals.

The strategy I have adopted is a hybrid scheme. Like the top journals, *EI* has general co-editors for applied microeconomic theory, empirical microeconomics, and macroeconomics. In addition, we have specialized co-editors for two kinds of subfields. First, in subfields where we receive a reasonable flow (more than ten per year), like sports, defense, experimental, and health, we have specialized co-editors who handle all the papers. Second, in fields where I would like to send a signal of interest, like neuroeconomics or algorithmic game theory, because I think the field is likely to boom in future years, I also have specialized co-editors. Thus, unlike *JET,* responsibility among the specialized co-editors is pretty clear. This hybrid scheme is an experiment, to see if it makes evaluating manuscripts more efficient.

I want to call out one of these specialized co-editors: Yoram Bauman (www.standupeconomist.com) for Miscellany. The *JPE* has a history of publishing entertaining articles under the column of the same name, a tradition that began to lapse with Stigler's death. As the publisher of Leijonhufvud's classic 1973 humor article (before *EI* changed its name from the more descriptive *Western Economic Journal*; we remain a journal of the *Western Economic Association*), we also have a venerable history in this area. I think the profession needs an outlet for this kind of thing, and I am gratified to see that two of the forthcoming papers for Miscellany are by Nobel laureates.

It is too early to tell whether these experiments have made the journal sustainably better, but the rate of submissions has more than doubled.

Do you have anything else to say or are you finally done?

There is a great deal of effort devoted to trying to scope out what editors are interested in, and bend papers toward specific editor's interests. There is similar effort devoted to figuring out what topics journals seeks. I don't think journals really have favorites and patterns are more a consequence of the pattern of submissions. Editors do have favorites – it is unavoidable – but the papers accepted are not strong evidence of what the favorites are. When I accepted a paper for the *AER,* I would usually raise the bar a bit for papers on the same topic. I didn't want a single area to dominate the journal. I didn't raise the bar a lot, but in a close decision it could matter. So topics in the journal, for me, were actually slightly negatively correlated with the likelihood of acceptance, although such a correlation was weak.

I use higher standards in my own research area than in other areas, because it is harder to impress me. In areas with which I am unfamiliar, a paper benefits from educating me about basic insights available in other papers. This is also a small effect since such benefits won't be experienced by the referees, who have substantial expertise, but only in my reading. Nevertheless, in a close decision, it could make a difference. Overall, I think submitting a paper where the editor has deep expertise usually produces a higher bar but less variance in the evaluation.

Being an editor hasn't made me a more effective author, or at least much less so than I anticipated. It has made me much more critical of my own work and much more effective at providing advice to colleagues. I can reference a broader literature. Being an editor at a major journal is a great way to keep abreast of new developments, because even if a particular paper isn't submitted to the journal one edits, it is usually discussed in some submission to the journal. But overall, it probably isn't a good strategy to be an editor for the sake of being a more effective author.

Mostly I've talked about the challenging aspects of being an editor. But the great thing about editing a journal is reading terrific manuscripts one wouldn't have otherwise encountered. This happens just often enough to make me glad to serve, and keep me gushing.

Notes

1. I thank Kristin McAfee, Dan Hamermesh and Glenn Ellison for very useful comments.
2. Laband and Piette (1994) argue that the journal conspiracies are efficient.
3. I'm not going to comment here on two other major inefficiencies. First, once we publish the paper, which was freely provided, as a profession we lose general access to it because of monopoly pricing by journals. Monopoly pricing of economics journals represents also an appalling state of affairs or a delicious irony, depending on your perspective. See Bergstrom (2001). Second, there are a huge number of papers being refereed many times, a dramatic cost of not coordinating across journals.

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