

Reforming the Review Process: Right Problem, Wrong Solution

Kenneth J. Meier, Editor, *American Journal of Political Science*

Professor Borer in “The Ugly Process of Journal Submission” quite rightly questions the journal review process. His situation, while perhaps rare, is clearly not unique. A common topic among political scientists at conventions is the vagaries of the review process. Virtually all scholars concede the process has a stochastic element; and most accept that sometimes the review process goes seriously wrong.

I would like to focus the discussion of reforming the review process in several ways. First, I will outline the three problems (although some of these problems occurred more than once) that generated this unfortunate case. Second, I will argue that the proposed solution, a competitive review process, might make the process less ugly for authors but will have serious negative consequences for editors, reviewers, and the profession. Third, I will propose some alternative solutions to make the review process less ugly; they will not be as radical as Professor Borer’s, but I think they will be more likely to improve the process.

Problems in the Process

1. Unresponsive Reviewers

The root cause of Professor Borer’s problems were reviewers that either did not respond to requests for reviews or did so in a superficial manner. The editor in all likelihood sent the manuscript to three reviewers when it was initially submitted. Of these, one did not respond and one did so in only a superficial way. Three quality reviews at the first submission would likely have avoided any serious problems.

A significant percentage of political scientists refuse to review manuscripts or do so only after an extended period of time. Despite my own efforts to weed such reviewers from the *AJPS* files and multiple reminders, approximately 20% of reviews do not arrive at all. The expe-

rience at other journals is likely similar. Reviewing manuscripts is a professional obligation, and I have some ideas about how to encourage it, but all editors have to learn to live with this problem. What Professor Borer’s editor should have done is requested additional reviews from at least one and possibly two more reviewers.

2. The Editor Failed to Make a Decision

Not every decision an editor makes is correct (at least in my case), but editors are paid to make decisions. Inviting an author to revise and resubmit a manuscript should mean that the likelihood of success is quite high. It should not mean “I am not sure.” If an editor is “not sure,” based on the reviews, the default option is to reject the manuscript and let the author get on with his or her effort to publish at another journal. Virtually every rejection letter sent out in political science notes that many rejected manuscripts are subsequently published in other journals. Professor Borer received two invitations to revise, followed by a rejection, then permission to revise yet again, and then a rejection. The time to reject a manuscript as “inappropriate” is on the first set of reviews, not after a long, extended process.

3. The Editor Was Inconsistent

The review process should be predictable. Authors should be able to rely on what editors tell them. If the editor tells an author that a new reviewer will be added, then a new reviewer should be added. Any revision is an investment of both the author’s time and the editor’s time. Procedures should be predictable and communicated to authors. There is simply no reason for an editor to add any additional randomness to the process.

Why Multiple Submissions Is a Bad Idea

From an author’s perspective, the lure of multiple submissions is seductive. Such an option would permit an author to get relatively quick decisions on manuscripts so that a bad review process at a single journal would have far fewer consequences. At the same time, multiple submissions would create other serious problems and might even undercut what the author wants—good quality reviews in a relatively short period of time.

Allowing multiple submissions would increase the number of manuscripts submitted to individual journals. While there is no way of determining how much, a conservative guess would be a doubling of submissions (I can envision scenarios where all authors submit to five or more journals; the submission of paper proposals at the APSA Annual Meeting is instructive on this point). A doubling of submissions would create severe problems for most editors. Every editor conscientiously tries to select reviewers who provide good quality reviews in a reasonable amount of time. We all keep track of response time, and most editors also grade reviewers in terms of quality. Every editor learns that the supply of good quality, fast reviewers is limited; and we try to select from that portion of the pool with those characteristics. Given a doubling of the number of submissions, an editor has two options.¹ One is to select more reviewers from the portion of the pool that is slower or less informed. This would slow the review process, degrade the quality of the reviews further, and generate more situations like that faced by Professor Borer. The second option is simply to ask our good reviewers to review twice as much. That is not feasible. I did an unscientific survey of members of the methods network. The average full professor responding did 17 re-

views a year; two noted that they did 50 per year. I personally know of others who also review this many manuscripts. Doubling this demand is unreasonable; it is far too much to ask of volunteers. Reviewers are the dearest thing to editors' hearts (those of us who have them); we ration them, feel guilty about requesting reviews too often, and try seriously not to abuse them.

Multiple submissions would also generate major inefficiencies in the review process. A rational author might well submit to five journals at once. So five editors would then ask 15 persons to review the manuscript and then make a decision. Fifteen reviewers is far more than needed to come to a judgement about a manuscript (imagine trying to please them all). At the same time, journals would invest their own resources in both the review process and the editing process. The costs of uncertainty currently borne by authors would simply be transferred to editors.

Multiple submissions would likely lower the quality of articles that are published in the journals. The iterative process of reviewing means that many rejected manuscripts are improved substantially before they are published in another journal. Good authors refine their arguments and improve their analysis in response to reviewers. At times, manuscripts that *AJPS* has rejected have appeared in other journals; and my reading of the published version often leads me to conclude that if the manuscript had been that good when *AJPS* reviewed it, that it might well have been accepted at *AJPS* initially. A second quality concern involves the editors. Without any guarantee that a submitted article would appear in his/her journal, no rational editor would invest a great deal of time in advising an author how to improve the manuscript. Some manuscripts are brilliant when they are submitted, but most go through a process where the editor, the reviewers, and the author invest additional time. With multiple submissions, editors would have neither the time nor the inclination to make such investments.

Why Doesn't the Market Deal with This Problem

Journals clearly vary in the speed and quality of their review processes. Authors have access to information both from personal experiences and from talking to colleagues; most journals release information on turnaround time. At the margins, some authors vote with their manuscripts and only submit to journals with prompt review processes. Why isn't this adequate to avoid the sort of problems experienced by Professor Borer?

First, journals do not offer an undifferentiated product. Some have more prestige, and some are more widely read by persons in the same subfield. Voting with one's manuscript, thus, is not a costless proposition. Second, while information is available, it often is not consumed; and access to that information varies greatly from person to person. Quite clearly, young scholars and those at smaller departments face problems of information asymmetry. Third, it is not the average case that is the problem, and most information is presented in regard to the average case (e.g., the median turnaround time); the problem cases are the unusual cases (at least I hope they are). So while the market works to some degree in this situations, it has some limits and costs.

Alternative Reforms

Simply criticizing Professor Borer's proposal without offering alternative suggestions would imply that I either think his problem is not serious (it is), or that I think the review process is fine the way it is (it is not). A debate on ways to improve the review process would be a valuable exercise. So here are my proposals for reform; some feasible, some probably not.

1. Improve the Information Available

Most political science journals have web sites; visit them. To facilitate the review process and to provide incentives for more prompt turnaround, editors need to report more information. The median turnaround is a fine measure, but it

hides what many want to know about, that is, what is the positive skew? I report turnaround figures for the 50th, 60th, 70th, 80th, and 90th percentile (for 1996 these were 40 days, 43 days, 48 days, 53 days and 66 days; *AJPS* counts weekends in these totals). I also report the number and percentage of manuscripts that take more than 90 days (1.7%) and 120 days (0) to review. *PS* would be a valuable place to publish this with notes on how turnaround is calculated at various journals.

2. Measure the Quality of the Process

Some entrepreneur should do a survey of authors on their experiences with political science journals. We do this with airlines, why not journals? How often do such problems occur? Are the problems random with respect to journals or do a few journals generate most of the problems? Does the quality of reviews systematically vary across journals? Much of the feedback editors get is from unrepresentative samples, and a lot is biased. I think even editors would like this information. Barring an entrepreneur to do such a survey, I suggest individuals with complaints about the review process send an e-mail message to the Center for Journal Quality Control (joestew@umn.edu).

3. Define Expectations

In my initial reading of Professor Borer's essay, I felt that his expectations of submitting a manuscript and getting an acceptance in four months was unrealistic. I propose that we define an acceptable review period as 90 days (this is from receipt of the manuscript so add some postal time). The *American Journal of Public Health* has done this by fiat; it sends manuscripts to five reviewers. At the end of 90 days the editors make a decision based on the number of reviews that are in hand (sometimes only one). Ninety days is a reasonable time period. If you get a manuscript decision in less than 90 days, tell your friends and encourage them to submit to that journal.

By defining such an expectation, I think editors will work toward get-

ting the overwhelming percentage of manuscripts done in that time period. One hundred percent attainment is not feasible (that has been my goal for three years, but there are always a percentage of troublesome manuscripts). This also implies some expectations on the part of authors. Do not contact editors before the 90 days are up unless you have not received notice that the manuscript has arrived. Always contact editors by e-mail or snail mail; phone calls are the worst. We deal with 50–100 manuscripts at a time. We don't remember them all, and any reasonable response to a question requires us to go get the file and read what is in it before answering a question.

4. Rethink How We Select Editors

The process of editor selection varies from journal to journal, but my impression is that the prime criterion for selection is the individual's scholarship. I would propose the heretical notion that good scholars are not necessarily good editors and vice versa. Perhaps 80% of an editor's job is managerial—setting up systems, selecting people to operate those systems, and monitoring how they work. This applies to both the review process and the production process. Very little of what we do is based on our own research expertise because that expertise is highly specialized and virtually all of our decisions affect areas where we have done no research. I propose that two equally important criterion for selecting editors are breadth of research interests and management skills. Selection committees ask editor candidates for their qualifications; these should be high on the list.

5. Let's Invest Some Time in Training Editors

Editing is a different enterprise for most of us. There are indeed associations of editors where one can go to learn about the process. Every association should build this into their budgets. The time to do this is before the editor takes over. Any editor spends his or her first few months in a subtle balance between

chaos and insanity, swamped with manuscripts, overwhelmed by the technicalities, and in quest of the perfect reviewer for a manuscript on Catholic social thought as it relates to a formal model of the democratization process in the third world. By then, it is too late. The learning curve in editing is exceptionally steep. A head start would be helpful.

6. Perhaps We Should Consider Whether General Political Science Journals Are a Good Thing

Other disciplines, such as psychology, have given up the notion of a general journal covering the entire discipline. By specializing the journals, they simplify the task of selecting editors. Good scholars have an advantage in editing in such circumstances. At the present time, subfields in political science are like religions, the further one is from one's own, the more homogeneous it appears. Just as all followers of Islam look alike to an Episcopalian, all students of public administration look alike to a political theorist. I wonder about a process that permits me to make key decisions about the direction of research in international relations. Giving up general journals, however, would also impose some additional costs. Little communication is done across subfields at the present time; restructuring the journals would make it virtually nonexistent. I have no opinion on whether specialist journals are a good thing or not; I do think it is something that should be discussed.

7. Let's Find a Way to Reward Reviewers

The review process can never be better than the reviewers who participate. That it works as well as it does, based solely on professional obligation, is often an amazement to me. Some scholars consistently refuse to review manuscripts, becoming free riders in the process. My infeasible way to solve the problem is this. Every person entering political science will be given 21 reviewer chips. The cost of submitting a manuscript to a journal is three chips. Those chips are then given to the three persons who review the

manuscript. In this way, people who review more manuscripts have the opportunity to submit more to journals; those who do not review, cannot submit. Those who review a great deal should be allowed to give additional chips to graduate students or junior colleagues. Faculty salaries might reflect such contributions. I would even support a market for such chips as long as journal articles noted the source of the author's reviewer chips. While there is no chance such a system would ever be adopted, any system relying on the kindness of strangers should think seriously about how to reward altruistic behavior.

Conclusion

In ending, let me compliment Professor Borer. It took guts for an author to start this discussion. There are real problems in the review process. There may well be additional ones that we have not covered. The political science profession is poorly served by a review process that takes too long and generates erratic results. I am not convinced that my proposals are the solution. I see this as the beginning of a discussion to trigger an on-going effort to make the review process more prompt, more rational, and more helpful.

Notes

1. I should also note that journals in political science are run on very tight budgets. My assessment is that most of them are running at or above capacity now given the number of staff. Most of us could not handle a doubling of manuscripts without a doubling of our budgets which would have a commensurate impact on the cost of journals.

About the Author

Kenneth J. Meier is professor of political science at the University of Wisconsin-Milwaukee and editor of the *American Journal of Political Science*. When not editing or acting as a gadfly to the profession, he fondly remembers what it was like to be an author. When his term as editor expires in December 1997, he looks forward to returning to his research agendas: a new theory of public policy, a normative theory of bureaucracy, and introducing new methods to public administration.