26

Rule by Referees? The Curious World of Academic Judgment

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INTRODUCTION

This chapter focuses on the process of judgments that lead to decisions to publish or reject work in international studies.[1] I begin with outlining my own biases, move on to discuss the ideal standards for making evaluations about contributions to our knowledge base in journals relevant to international studies, then compare the ideal to what appears to be the actual process and its shortcomings, indicate potential sources of bias, argue for the critical role of editors over referees, and offer a few modest recommendations for improving the process.

The first principle we are taught in graduate school is that we come to our research interests with a set of biases and perspectives that color our view of the world. The key to being good researchers is to make these biases clear and explicit, to understand how they filter our views of the world, and to seek to minimize them to the extent that we are able to do so. As with all of us, I bring a set of biases and background considerations to this essay, and in the interest of transparency, I begin by trying to make them explicit.

This chapter is primarily about the operation of journals in our field and I am not a disinterested observer of how the world of academic journals operates. I have been involved with the creation of several, and spent twenty years overseeing the operation of what is now a stable of seven journals under the umbrella of the International Studies Association (ISA). Through my tenure as executive director at ISA I have learned far more than I ever wanted to know about the *business* of publishers, publishing, and the quest for journal rankings. I have also learned a great deal about the governance of journals: those that have governance, those that appear to have governance, and those that do not even create a pretense of having governance mechanisms in place over their editors.

Apart from my work with ISA, I have published in twenty-six different journals

in my field. Typically, I review manuscripts for fourteen different journals both in International Relations (IR) and in political science. I presently sit on the editorial boards of ten. Like most of us, I have lost some battles with editors, I still bristle at (and take very personally) the occasional rejection letter – even after forty years in the business – and there are journals to which I never send manuscripts. I have much admiration for people who are willing to become journal editors, respect many of them, dislike three, and had pledged a long time ago that I would rather eat glass than become one.

The argument below is a simple one. Yes, there are substantial biases in the double-blind review process we use in most of our journals. But, no, it is not simply a 'rule by referees'; the various stages of the process privilege editors at least as much as referees. Finally, the key to how much bias there likely exists depends on the quality and commitment of editors, the workload under which they labor, and the governance processes to which they are subjected.

JOURNALS: WHAT ARE WE TALKING ABOUT?

The world of academic journals is in ferment, due to challenges created by technology, the open access movement, changing resource capabilities of universities and institutional subscribers, the rapidly changing business models of publishers,[2] and demands from academia to evaluate an ever-growing number of (potential) faculty for hiring, promotion, and tenure. Through all this turmoil, though, the numbers of journals continue to proliferate. This point should come as no surprise for anyone who is a scholar and has an email address: we are constantly barraged by solicitors to submit manuscripts to a huge range of new journals none of us have ever heard anything about previously.

Focusing on the world of journals and the academic judgments they contain, we need to keep in mind the generic context: there is a tremendous variety of publications potentially available to scholars working in IR. For instance, while Wikipedia lists fifty-three 'notable academic journals on international relations,' it notes that there are actually hundreds currently being published.[3] The SCImago Journal and Country Rank portal, based on the Scopus database from Elsevier, lists no fewer than 390 different international relations journals of various utility.[4] Most, but far from all of these are peer-reviewed, while fewer likely follow a double-blind review process.[5] Further, while these compilations list some, they do not include all the additional journals that carry a discipline (political science, history, sociology, economics, law, etc.) or area studies focus that may accept work on IR. Including those publications may increase the potential pool of journals relevant to IR scholars to as many as 500 different publications.[6]

Of course, journal quality is quite different from the quantity of published journals focusing on IR, and an entirely different chapter could be written over the controversies involving measurement of quality publications. Citation indexes constitute one plausible strategy for assessing the impact of a journal, albeit fraught with substantial validity problems.[7] Alternatively, publishers can assess how often their publications are being downloaded and/or accessed online, and this approach may lie at the heart of Google Scholar rankings of journals.[8] Still another strategy is to ask members of the global academic IR community about the most valuable journals in the field (e.g., the TRIP survey). The latest available outcome from such a survey,[9] listing the top twenty journals preferred by IR scholars from thirty-two different countries, is noted in Table 26.1, and accompanied by both the Thomson citation index and the Google Scholar index for comparison.

Table 26.1 Comparison of TRIP survey with Thompson citation index and Google Scholar rankings

	2014 TRIP question: What are the top four journals?	*2014 Thompson citation index, 5-year average, 86 IR journals*	*Google Scholar*
1	*International Organization*	*International Organization*	*Foreign Affairs*
2	*Foreign Affairs*	*World Politics*	*Journal of Peace Research*
3	*International Security*	*International Security*	*International Organization*
4	*International Studies Quarterly*	*Journal of Peace Research*	*Journal of Conflict Resolution*
5	*World Politics*	*Journal of Conflict Resolution*	*International Affairs*
6	*European Journal of International Relations*	*European Journal of International Relations*	*International Studies Quarterly*
7	*American Political Science Review*	*International Studies Quarterly*	*Journal of Democracy*
8	*Foreign Policy*	*Security Dialogue*	*European Journal of International Relations*
9	*Journal of Conflict Resolution*	*Foreign Affairs*	*Third World Quarterly*
10	*American Journal of Political Science*	*Review of International Political Economy*	*World Politics*
11	*Review of International Studies*	*Journal of Common Market Studies*	*Security Dialogue*
12	*International Affairs*	*Review of International Organizations*	*Review of International Studies*
13	*Millennium*	*International Theory*	*The Washington Quarterly*
14	*Security Studies*	*International Political Sociology*	*International Studies Review*
15	*Journal of Peace Research*	*Chinese Journal of International Politics*	*Journal of European Studies*
16	*Review of International Political Economy*	*Conflict Management and Peace Science*	*International Studies Perspectives*
17	*International Relations*	*British Journal of Politics and International Relations*	*The Pacific Review*
18	*International Studies Review*	*Review of World Economics*	*Millennium*
19	*Survival*	*World Economy*	*Ethics & International Affairs*
20	*Global Governance*	*International Interaction*	*Global Governance*

Note that the methodologies for inclusion differ across the three sources and not all of the journals listed meet the criteria of requiring a double-blind process for manuscript acceptance (e.g., *Foreign Policy* and *Foreign Affairs*). Nevertheless, despite the extent of theoretical and methodological conflicts spanning the scholarly IR community, and despite the difficulty in identifying what constitutes high-quality journals in the field, and given the numbers of journals being published, there is a remarkable amount of agreement across the three sources displayed in the table. Much of the variation occurs over criteria for inclusion, as the Thomson citation index here is confined to IR journals only. Were we to include political science journals with substantial IR content, many of the differences would be eliminated. Only a small handful of journals stand out as unique classifications across the very top journals (*Security Dialogue* in the Thomson index, *Third World Quarterly* and *Journal of Democracy* in Google Scholar). Primary dissension is over differences in rankings, and at the bottom of the respective ladders.

The discussion that follows is based primarily on experiences with the journals that are identified as highly ranked in the field. As I had noted above, I fully recognize that these journals do not necessarily represent the range of journals available to scholars, and that in all likelihood the list carries its own biases, privileging certain types of publications over others. Nevertheless, I focus on them for two reasons. First, both citation data and self-identification (through the TRIP surveys) indicate their salience for scholars. Second, it is conceivable that one could be familiar with all of the hundreds of journals 'out there,' but I lack that familiarity and I suspect that few do. Given the limited information we have available regarding the inner workings of journal editors and reviewers, expanding this discussion outside of the so-called top journals would only lead me into even greater guesswork than otherwise.

JOURNALS: HOW ARE MANUSCRIPTS PROCESSED AND JUDGED?

The process of exercising judgment over the value of submitted manuscripts to journals has remained relative stable over several decades, at least for the most widely cited and respected journals in IR. I describe below the ideal standard for this process, and then sketch out the less than ideal version in practice.[10] In doing so, I seek to show that it is not just referees who make the critical judgments, but that the involvement of and the directions projected by editors are at least as crucial to the evaluation and outcome for submitted manuscripts as are those of the referees.

The ideal version of manuscript reviews and the accompanying acceptance/rejection process, with minor changes, has remained much the same for nearly half a century. Depending on the size of the editorial team and the volume of manuscripts being received, the journal editor – along with associate editors and managing editors – process incoming manuscripts according to the following steps that are – in principle – designed (a) to create a critical, impartial assessment of the quality of the work, regardless of who the submitting author may be; (b) to spread the responsibility across reviewers who provide evaluation across a diverse orientation to the field or the discipline; (c) cumulatively, between reviewers and editors, to provide quality feedback to authors, whether or not the manuscript is ultimately accepted; and (d) to insure fairness, the process unfolds under a double-blind methodology, as both the reviewers and the submitting authors remain anonymous.[11]

The steps required to ensure these objectives involve:

1 A cleaning of the original manuscript to ensure that anonymity of the authors is maintained.
2 An initial editorial judgment (desk rejection) regarding whether or not the manuscript is 'appropriate' for review. Ideally, desk rejects

occur for one of three reasons. First, in cases when the journal specializes in a specific subject (e.g., regional studies, conflict processes, etc.) the manuscript simply may not address the journal's subject focus sufficiently, the author is so informed, and perhaps the editor may suggest an alternative outlet for the author. Second, an initial determination may be made that the manuscript is of such low quality that it is not worth troubling outside reviewers. Editors encounter numerous manuscripts stemming from undergraduate term papers or incoherent ramblings of angry authors, unsuited to a scholarly journal. Third, in non-specialized journals of the field the editor may make a decision (although sometimes this is left to the reviewers) as to whether or not the subject matter and the findings would be of interest to those readers who do not specialize in the area.

3 Assuming the manuscript survives the 'desk rejection' step, the manuscript is sent out to several reviewers (typically three). Ideally, the manuscript is sent to potential reviewers who have demonstrated expertise in the subject, are willing and able to provide close scrutiny of manuscripts, and would likely provide insightful commentary regarding not only whether or not a particular work is worthy of publication, but as well the circumstances (revise and resubmit) under which it could be accepted.

4 After the external reviews are concluded, the comments are then returned to the editor, who reads them carefully, seeks to identify from the reviews the strengths and weakness of the work, and then opts on the basis of the reviews and her knowledge of the field to (a) reject the manuscript; (b) grant it a revise and resubmit (R and R) status along the lines spelled out in the judgment letter; or (c) accept the manuscript as is, subject to minor stylistics changes. In our ideal world, when all three outside reviewers recommend acceptance or rejection, the editor concurs. When there is conflict across the reviewers, as is likely to be the case in many instances, editorial discretion substantially increases: the editor carefully evaluates the nature of the conflicts, makes an estimate as to whether the issues raised can be addressed successfully by the authors, and then decides whether or not to grant the manuscript R and R status. The letter identifying the conditions for R and R becomes an implicit contract between the journal and the submitting authors, specifying hurdles that need to be met and criteria by which the changes will be evaluated. Once the revised manuscript is resubmitted, it typically will go back to one or more of the original reviewers for reconsideration. In order to maintain transparency, the external reviewers are copied in on each other's comments and the final judgment of the editor. This last point is a standard practice for both ISA's journals and for most others for which I have reviewed. I do not know, however, if all journals noted in Table 26.1 follow this procedure, but it is a critical step to insure transparency and minimize editorial bias.

5 Once manuscripts have been accepted, those utilizing quantitative, 'large-*N*' based research designs are requested/required to have all replication materials (including the data and the statistical command files) made publicly available.

JOURNAL EVALUATIONS: REALITY CONFRONTING THE IDEAL[12]

The five steps I note above constitute the gold standard for major journals in IR and political science for at least the last half-century. However, from the standpoint of both editors and authors, they have become increasingly problematic, and conformance to them varies greatly across journals. I describe below some of the challenges involved with each of these steps.

Maintaining Author Anonymity

There are at least two issues with living up to this standard. First, there actually is no anonymity between author and the editor, who is able to desk-reject or decides which reviewers will read the manuscript. We assume that editors do not cheat: they are as objective with successful scholars as they are with aspiring ones, and do not provide an easier road to colleagues and friends than contemporaries with whom they have intellectual (or personal) issues or conflicts. That is a large

assumption to make, and hopefully the process that governs the selection of editors weeds out those most likely to personalize these judgments. Neither I nor anyone else I have ever talked to about this problem has had a sliver more than distant, anecdotal evidence to indicate the size of this problem, although it appears to be concentrated in certain journals that lack broad and transparent governance mechanisms.

The second problem is more recent, and partly due to the nature of evolving technology. Many of us engage not in a single piece of research but in a larger research program. Furthermore, the manuscripts we submit have been typically part of a larger vetting process through the submission of the work to a variety of audiences (workshops, panels at conventions, invited presentations, etc.). It takes very little effort today to find the likely author of an anonymous manuscript by Googling the title or key phrases associated with the subject of the manuscript. If the reviewer is an expert in the area, it is also likely that she would have seen a prior iteration of the submitted work at a conference or workshop anyway. This problem is particularly vexing in areas where the number of scholars producing work is limited.[13] In my discussion with dozens of journal reviewers I have raised this issue repeatedly and I estimate that the reviewers were confident about the real identity of more than half of the 'anonymous' authors. Of course, what becomes more problematic to estimate is the extent to which such 'knowledge' impacts – either negatively or positive – referee judgment. Some experimental work conducted in another discipline indicates that it has a substantial impact as reviewers (and perhaps editors?) are more supportive of prestigious colleagues and those working in prestigious institutions (Peters and Ceci, 1982; Suls and Martin, 2016). What is obvious is that the fraying of one part of the double-blind process substantially increases the *opportunity* for biased evaluations on the part of the gatekeepers.

Desk Rejection

This is far from a trivial issue. Desk rejection rates range from fifteen to as high as nearly 50 percent of submitted manuscripts across journals, and even across different editorial teams of the same journal. The worry here is that individual editorial bias – toward the submitted author, or toward a particular theoretical or methodological approach – is unchecked by anonymous reviewers. Increasingly, most IR journals use not one but a team of 'editors' and they typically discuss these manuscripts among them before desk rejecting (unless the volume of incoming manuscripts is too high). Such group decision making could minimize individual bias, but that assumes that the editorial team is highly diverse both theoretically and methodologically.

A high desk rejection rate is not necessarily an indication of bias: some journals are more likely to receive manuscripts that are inappropriate than others. *Foreign Policy Analysis*, for instance, receives numerous manuscripts that are policy statements rather than academic research. Some journals specify theoretical orientation (e.g., *International Political Sociology*), subject matter focus (e.g., *Global Governance*), or both subject and methodological preference (e.g., *International Interactions*). Rejection of submitted manuscripts for failure to meet the writ of the journal does not constitute bias (as long as there are numerous other journals where the author can submit). However, roughly half of the journals on the top twenty list in Table 26.1 do not explicitly specialize in one theoretical orientation, or subject, or methodology. High desk rejection rates could be more suspect in these cases.

However, it would be patently unfair to argue editorial bias even in these cases of high desk rejection rates simply on the basis of the percentage of manuscripts turned away. The real questions they raise revolve around governance issues of journals. Is there oversight on editorial judgment and, if so, is

that oversight rigorous and itself impartial? One suggestion presently circulating among several editorial boards is to conduct systematic audits of desk rejects in order to probe for substantive, methodological, or theoretical bias.

The issue of high desk rejection rates and the subsequent biases it may produce is not a problem that will likely disappear in the near future. Particularly in the case of high-quality journals reflected in the table, the volume of manuscript submissions will continue to grow, taxing the resources of editorial teams, both with respect to available reviewers and the follow-up required by the team after the reviewers have submitted their analyses. Desk rejects become a tempting alternative to short-cut these resource problems, even if there is little explicit bias in evaluation by the editor. When such pressures also combine with our normal biases, this initial gatekeeping responsibility by editorial teams can create substantial distortions in what (and who) gets published.

Referee Reviews

In an ideal world, the editor selects three referees (or reviewers – both terms are used here and in the profession), perhaps two with specialization in the appropriate subject matter and a third who views the manuscript as an outsider to the subject, but well acquainted with the field's work and various research designs. In practice, identifying appropriate reviewers is an arduous, difficult practice, requiring creativity and enormous patience on the part of the editors. For high-quality journals, the huge number of submissions place an enormous amount of stress not only on the editor, but also on the potential pool of reviewers.[14] It is not atypical in these cases for editors to solicit up to a dozen or more potential reviewers for a single manuscript before they can find two or three willing to take up the task, and to provide reviews in a timely manner.[15] To deal with the issue of too many manuscripts and too few referees, some editors stop at two outside referees and do not look for a third unless the two to whom the manuscript is sent strongly disagree with each other.

The next time we get angry at an editor for a horrible review, consider the numbers: a high-quality journal may send out 500 to 700 manuscripts a year for review. In such circumstances, the journal may need to rely on a stable of some 1,000 reviewers annually in order to generate judgments that are both timely and constructive.[16] Making this situation far worse is the fact that there are hundreds of peer-reviewed journals, all seeking quality external reviewers, and likely competing for the same 1,000 reviewers. Good editors are on a constant hunt for increasing the stable of potential reviewers, searching for those presenting valuable papers at conferences, collaborating with editors of other journals, identifying those with successful publications in journals, finding colleagues who are willing to make referrals, etc.

The editorial team needs to search for referees who: (a) are well acquainted with the subject, the literature around the subject, and the method of evidence being used; (b) are willing to assess the manuscript and respond with commentary and critique in a timely manner;[17] and (c) are willing to entertain perspectives – both theoretical orientations and methodological approaches – with which they may not be comfortable. These are three of the most important drivers of reviewer bias: relative ignorance, laziness, and, most important, keeping an open mind regarding approaches to research that differ from one's own. Of the three, clearly the third represents the most destructive impediment to publishing creative scholarship. Every author who seeks to create innovative perspectives, or bridges across theoretical chasms or approaches to empirical evidence, runs the risk of alienating reviewers on all sides. Relying on editors to find the reviewers needed to provide a fair and thorough analysis of such works is a substantial exercise in optimism. One colleague

who recently published a piece that integrated two theoretical traditions typically at war with each other, and employed quantitative analysis in a theoretical tradition that rejects the value of such evidence, noted that it took ten years and several different journals before he found an editor who was able and willing to identify three reviewers that were both knowledgeable and sufficiently open-minded to create a fair evaluation of the manuscript.

What motivates referees to engage in high-quality reviews? I am aware of no systematic research that has been done on assessing motivations to produce quality reviews in our field,[18] so this is mostly an impressionistic assessment, based on unscientific samples and discussions with colleagues. Nevertheless, I conclude from those discussions that given reviewers' central role in the process of knowledge production, a pessimist may worry that the range of motivations appears to be dominated overwhelmingly by altruistic drives. I have found that scholars are willing to spend their time and energies doing reviews from a sense of self-esteem, responsibility to the academy, a desire to enhance knowledge in the field, and occasionally from learning through reviewing.[19] Despite their central roles in a process that ultimately creates substantial tangible consequences for others,[20] the tangible benefits they may derive from participation are minimal,[21] or negative for the profession,[22] or non-existent.

What is an editor to do under the circumstances? Besides working hard, cajoling, begging, and threatening, some editors take necessary shortcuts, leading to potential additional bias in the judgments about submitted manuscripts. Sometimes they choose inexperienced referees.[23] Some referees turn back reviews of the type we have all experienced: cursory reading of the manuscript that misses the point entirely, or makes incorrect judgments about the literature, the theory, or even the empirics at play. Some authors will strongly contest these reviews, and there are many editors who recognize the problem and engage in what is euphemistically referred to as 're-review.' Others, partly under the weight of their heavy workloads, simply refuse to budge and turn down the manuscript, especially if it is outside their field of expertise. The result is bias stemming from combinations of academic arrogance, intellectual sloppiness (referees), and an overwhelming workload (editors).

Editorial bias enters also at the stage when reviewers disagree over the direction the manuscript should take, with some arguing for rejection and others for acceptance, with or without revisions. Here, it is the role of the editor to carefully scrutinize the reviewers' issues, and as well her own reading of the manuscript, and make the appropriate judgment. It is also here where the process creates substantial flexibility and opportunity for bias to creep into editorial judgment, and especially at a point where the so-called 'double-blind' protections are no longer in place.

R and R

According to my reading of the annual reports of nine different IR journals, the substantial majority of submitted manuscripts that are not rejected receive conditional approval in the form of requests for revisions. Thus, the second-round' handling of R and R manuscripts has become critical to what gets published ultimately. It has been standard practice that the editor establishes clear conditions for revision, and uses those conditions to reevaluate the revised manuscript, by resending it to some but not necessarily all of the previous reviewers. Scholars who get this far *should* enjoy substantial likelihood of eventual success, even if they have to re-navigate across reviewers interested in creating closure and moving on.

Unfortunately, that may not be the case. Again, we lack systematic evidence, but it appears from scores of interviews with both authors and former editors that the rate of rejections of R and R manuscripts has been dramatically increasing over the last

few years, perhaps corresponding with the numbers of manuscripts being submitted to journals. Rejections appear to have come through one of two routes. First, the editor, even though the author has conformed to the editorial requirements, has decided that she does not like the manuscript after all. Second, the editor sends the manuscript back to the most critical reviewer, who is still not satisfied, and had not participated in delineating the conditions that the editor had sent to the author. In either case, an author who had faithfully fulfilled the editor's requirements for changes to the manuscript now finds herself having fruitlessly invested another four to six months in this process.[24]

Replication Capability

It has been a positive development that final acceptance comes with the additional requirement to provide replication materials for those engaged in quantitative research (and there is a strong movement to require the same for qualitative evidence as well). Such a requirement in principle reduces the likelihood that the empirical analysis is falsified and it also offers the academic audience opportunities to expand on extant findings in the literature. That is, if we can assume that the replication materials are in effect available. Some journals lack the capacity to store and make available these materials and only require that authors post them on their own web pages. Other journals lack the capacity for regular maintenance of these data, and the replication files may disappear. In a recent review of 450 pieces of literature across eleven journals published over the last five years, my colleagues and I found that in some 20 percent of the cases we would not have been able to reproduce findings, either because the 'do files' were incomplete or because the actual data were no longer available. Some journals, in order to deal with this problem, have replicated on their own the results contained in the manuscript as a requirement *prior* to granting final acceptance.

JOURNALS: SO HOW MUCH BIAS?

A colleague of mine, in the midst of a heated discussion about who was more or less privileged in our field, pulled a letter from his jacket pocket and urged me to read it. The colleague *is* one of the privileged: by any standard of judgment he is incredibly successful in the field, and he has been well rewarded financially. He has a vast array of publications and has one of the highest citation rates of anyone in IR. His reputation is well established and he carries power and authority among his peers. He is innovative and thoughtful. He makes any list of the top ten most influential scholars in our profession. The letter from the editor of a top twenty journal listed in Table 26.1 read (and I paraphrase to protect everyone's anonymity): 'As long as I am editor of this journal you will never publish here!' My colleague indicated that the letter was correct: despite his numerous requests to have his manuscript *reviewed*, the editor refused, disliking his theoretical orientation and approach to evidence, although I suspect that there were personal issues surrounding the editorial decision as well.

I use this case to indicate a number of issues about bias. First, it hits not just those who are junior, who challenge the established and the orthodox (after having worked with some 7,000 scholars across more than 100 countries, I no longer know what today is orthodox), or who lack influence or reputation in the profession. If this kind of response can occur to a colleague at the top of the profession, then one should justifiably worry about those without influence, reputation, or who are challenging 'orthodoxy.' This is likely to be a far greater problem in the social sciences where there is substantial contestation over theory and method than in the natural

sciences; such differences are reflected in the far higher rejection rates of submitted manuscripts in the social sciences. In IR and political science, a high rejection rate for a journal is typically used as a measure of the journal's salience as a research outlet. Yet, the number of papers even in the more established fields that have been initially rejected, only to become major contributions to a discipline's knowledge base, are substantial (Campanario, 1995). Second, while I recognize that a single case is very far from constituting systematic evidence, it is instructive precisely because we lack such systematic evidence. I have no doubt that more than half of the journals listed in Table 26.1 exercise meticulous editorial judgment and many editors work extremely hard to solicit reviewers who would make professional assessments of submitted works, while some of the others less so. Yet, and given the importance of the process to both the field and the individuals who work in it, this judgment on my part is no more than an impressionistic assessment, lacking evidence. Ironically, the so-called double-blind process prevents such systematic evidence from being developed. So, we are thrown back to mentioning individual cases and peer discussions by submitters, discussions that may be heavily influenced by rejection, which in turn may or may not have been deserved.

Third, perhaps the most striking point about the 'letter' example is that there are editors of significant journals who think nothing of putting into print their arrogance and bias (just as there are many other editors who shudder at the thought of committing these sins). This point raises another about the governance of journals (discussed below) and how much of it is appropriate for the field.

So, how much bias? What we can gleam from the discussion of the process through which we publish in journals is four-fold. First, there are ample *opportunities* for bias across the steps involved from submission to judgment, stemming from either editorial direction or the nature of the reviewers being utilized. Second, some bias is likely intentional but it just as likely to occur as an unintended consequence of the structure of the workload and academic training, both for editors and for reviewers.[25] Third, given the consequences of editorial decisions for both the academic enterprise and those who labor in it, we know amazingly little about how much distortion there is in the process and about the governance mechanisms used to insure quality and fairness. Fourth, the changing nature of publications, especially from print/hard copy to electronic formatting of journals,[26] *should* be reducing the 'tyranny of limited journal space' and allowing for more diversity of published materials. Whether or not that is the case is worthy of additional inquiry.

CONCLUSION

In one form or another, the peer review process has been utilized since the seventeenth and eighteenth centuries (Lee et al., 2013). Presumably, its critics date from around the same time. The numerous objections raised about the modern process are not easy to dismiss, prompting one observer to suggest that peer review is more of a process of *faith* rather than a scientific procedure through which we find evidence of quality (Linkov et al., 2006). As with all else in life, there is no single silver bullet to address all the issues raised by such critiques. It is also the case that there are other alternatives to the double-blind peer review model available, but they are fraught with a difficult range of problems that can exacerbate bias even further and/or are made impractical by requiring the involvement of a far larger number of scholars than those volunteering now to assist in the evaluation process (Ware, 2008; Suls and Martin, 2009; Lee et al., 2013).

With all that in mind, my modest proposal is that the present system of journal judgment through double-blind reviews can be

strengthened by stronger and more transparent governance, a governance that makes explicit the potential biases that such a process can accommodate and, through oversight and the public display of evidence, shine enough light on the process to minimize bias. Journals can be accountable to one or both governance mechanisms: an editorial board and, in the case of associations such as ISA, a publications committee and a governing council that provides additional oversight on the activities of the journal(s). What I propose below is plausible to achieve, whether the governance mechanism is an editorial board or an association, although an association with substantially more resources can pursue these steps more vigorously.

To some extent collective governance and transparency over a double-blind peer review process is a bit of an oxymoron as it runs into two problems: editorial autonomy and anonymity of submitters and reviewers. Yet, by shining a clear and consistent light on general practices as opposed to specific judgments, it can make a substantial difference in minimizing bias.

How? Oversight mechanisms (such as editorial boards and associational governance mechanisms) need to highlight specific areas where bias may enter, and generic conditions under which bias may lurk. I suggest the following:

1 Desk rejections: where desk rejection rates are high (e.g., some threshold in excess of 20 to 25 percent) editorial boards should periodically conduct random sample audits to determine if there is potential bias in removing certain types of manuscripts from reviewer scrutiny, based either on theoretical or methodological orientation.
2 Referee selection: editors need to provide annual, systematic evidence of the types and numbers of referees solicited, including evidence on potential bias in their selection: lack of gender, theoretical, methodological, geographical, or nation-state diversity.
3 Rejection/acceptance types: reviewers and editors could favor senior scholars over junior scholars, men over women, scholars from prestigious institutions over those from less prestigious ones, those from North America and Europe versus the Global South, those with certain types of theoretical or methodological orientations, or those who challenge the extant literature. In each of these cases, a systematic accounting of aggregate numbers regarding submissions versus acceptances can shine a light on potential biases, either at the editorial or reviewer level. None of these data are actual indications of bias, but they are suggestive of patterns that should be more carefully monitored by both editors and governors (members of governance bodies).
4 The disposal of R and R manuscripts: a systematic accounting of the percentages of R and R manuscripts finally rejected, along with the reasons for those rejections, can create sufficient transparency to allow governors to assess whether or not such commitments by editors and authors are being fairly processed on the journal's side.
5 Outreach functions: where disparities exist in the categories indicated above, it becomes incumbent on governors to request editors to take affirmative steps to seek to remedy substantial imbalances. For instance, if there is a gender imbalance regarding reviewers or submitters, editors should be asked to engage in outreach efforts to recruit more authors and manuscript reviewers through their editorial boards and through direct contacts at conferences.

There are several additional steps that can be added to this list. I use the five points above as examples of the more general notion of seeking to identify potential bias, and then requiring editors to provide systematic information, which in turn can allow governors to oversee the progress of the journal. I have no illusion either that such steps will eliminate bias in journal manuscript evaluation or that the perception of bias will be dramatically eliminated. Nor do I have any illusions that this proposal will not further burden editors and governors; both groups have more than enough work to do. However, the light of transparency is a strong one; it allows governors, if they exist and care to pursue their roles, to question and prod without interfering in either editorial autonomy or the anonymity of the double-blind process.

Ultimately, though, the most important oversight is at the beginning: choosing wisely

and carefully in the selection of an editor and the editorial team. We need editors who (a) exercise high standards of scholarship and seek reviewers with similar standards; (b) are willing to entertain theoretical and methodological approaches different from their own and seek reviewers with a similar outlook; (c) are sensitive to the range of biases that may enter the review process, seek reviewers who are sensitive to them, and are willing to assertively monitor such possible biases; and (d) are willing to make the substantial personal and professional sacrifices through their editorial tenure that are required to accomplish these steps.

The judgment about the lead editor is critical to this process. However, we should perhaps recognize that the field has grown so complex, and so diverse, that no single editor can create the needed oversight to the broad range of subjects and methods being submitted. The lead editor needs a strong, diverse, and highly competent team of associate editors who can complement her skills and collectively provide the diversity of knowledge and familiarity with the field that perhaps no single editor can. The tough, critical assessment in evaluating a potential editor by governors should be complemented by the same critical assessment of the editorial team that the lead editor offers to bring to the process.

My experience with ISA suggests two issues in particular about this problem. First, too often the editorial 'team' (below the lead editor) promised for the journal changes once the team has actually been awarded the journal and especially across the team's tenure cycle. Second, the initial evaluative spotlight still tends to focus primarily on the lead editor and not sufficiently on the entire team.

I recognize that it is difficult for governors to make these judgments about potential editors and their team during the selection process. What does help is to make these criteria explicit and to invite those applying to clearly address how they will pursue each of these components. Then, in combination with an annual oversight function, it is hoped that the issues raised in this essay can be minimized.

Finally, we need to pay some attention to the reward structure (or its absence) pertaining to both editors and reviewers. We need to insure that editors who take up these tasks are recognized fully for their work by their home institutions through editorial boards or associations making overt efforts with the editors' home institutions to recognize their contributions to the scholarly community, the time and effort it takes to do the job well, and the sacrifices the editors make regarding their own research agendas. We also need to provide editors with sufficient resources so they are not overwhelmed. Their resources are generated typically from one or more of three sources: the home institution, the publisher, and/or the association/editorial board. Ironically, all three sources are simultaneously interested in seeing the journal succeed, and investing as limited a set of resources in its operation as possible.

Likewise, we need to create special recognition, both in the field and in their home institutions, to reviewers who do their jobs well. There ought to be a large 'all-star team' of reviewers in international studies: people who spend much time and energy engaged in excellent reviews, and do so as untainted by bias as possible. Editors across the huge numbers of IR journals could call on these reviewers more so than others, and in turn highlight them as a group in their publications and to their home institutions. Such recognition may not pay the bills, but most of us did not decide to work in this field for its financial rewards.

Notes

1 The title of this chapter was chosen by the *Handbook* editors, who took inspiration from Michèle Lamont's *How Professors Think: Inside the Curious Mind of Academic Judgment* (published in 2010 by Harvard University Press), which offers an in-depth sociological analysis of peer-reviewing as it is practiced in the context of funding institutions.

2 When ISA sent out a request for proposals to publishers for its stable of journals, I reviewed sixteen different proposals containing sixteen different approaches to the future of their world. There appeared to be no agreement about that future and we received wildly different approaches to publishing journals.
3 https://en.wikipedia.org/wiki/List_of_international_relations_journals. The selection process for 'notable academic' journals is not revealed. It does include as a main IR journal the *Michigan Journal of Political Science*, which publishes undergraduate papers.
4 Retrieved January 5, 2016 at www.scimagojr.com/journalrank.php?category=3320
5 Excluded from the discussion are online journals that require scholars to pay for the evaluation and publication of their manuscripts. None of those publications are listed as having a high citation rate, nor are any listed among those considered to be of high quality.
6 Excluded are law school journals which may entertain a variety of manuscripts on topics in IR, but the structure of these law journals (typically staffed by law students) differ from the journals discussed below.
7 One common strategy for editors is to ask, beg, or request authors to cite previous publications from their journals. Another involves media blitz strategies by some publishers.
8 Retrieved January 4, 2016 at https://scholar.google.com/citations?view_op=top_venues&hl=en&vq=soc_diplomacyinternationalrelations
9 Retrieved January 5, 2016 at https://trip.wm.edu along with additional data from TRIPS, courtesy of Michael Tierney. The survey's respondents identified no fewer than 228 journals of high interest.
10 The 'ideal' version of manuscript reviewing and judgment I sketch out below is a compilation based on (a) my summary of the standards publicly identified by journal editors in the field; (b) my experience on ten different editorial boards routinely briefed by editors; and (c) my role in helping to create oversight for seven different ISA journals, including extensive discussion with leading publishers (both in North America and Europe, but restricted to publishers on those continents) about the criteria they wish to have editors use.
11 I have no evidence regarding when the double-blind review process became the gold standard in our field. However, both international relations journals (e.g., *International Studies Quarterly*) and political science journals publishing international relations materials (e.g., *American Journal of Political Science*) have practiced a double-blind review process for at least the last half-century.
12 I am grateful to two former editors from one of our top ten journals who provided very helpful feedback for this discussion.
13 For instance, I work on status issues in international politics. There are roughly ten to fifteen people who are presently producing scholarship in this area. Unless they are brand new to the subject, when I read an 'anonymous' manuscript on status, I can easily identify the author from the writing style and research approach.
14 Two proposals that have been discussed by editors in order to increase the pool of reviewers include: (a) creating a common pool across journals of reviewers whose history is consistent with providing quality reviews and responses in a timely manner; and (b) creating a 'wall of shame' that would list people who publish in a journal but refuse to review manuscripts, or agree to do so but fail to fulfill the commitment. A third, that is, providing a token payment for reviews, has also been tried but does not seem to work outside of the discipline of economics.
15 On one particular manuscript, focused on a mainstream IR subject, I was the thirteenth person asked by the editor. For a discussion of 'reviewer fatigue' and other effects on reviewers, see Breuning et al. (2015). In their survey of American Political Science Review (APSR) reviewers, 58 percent of IR scholars asked to review manuscripts agreed to do so; however, among those that actually agreed to review, nearly 23 percent failed to complete their reviews, suggesting that if APSR reviewers are typical, substantially less than half of IR scholars approached produce actual reviews.
16 The latest annual report for the *International Studies Review* (2015) indicates that it approached 1,221 potential reviewers in order to generate 521 positive responses for evaluating 190 submissions that were not desk rejected. For the same time frame, the *International Studies Quarterly* (2015) called on 1,680 reviewers, and generated 1,022 for 351 manuscripts that survived desk rejection. (Source: annual journal reports, available at www.isanet.org/Publications)
17 Scholars need feedback on their work in a timely manner. For instance, the International Studies Association asks its seven editorial teams to generate turnaround times for submitted manuscripts at around eight weeks or less.
18 For a recent survey of reviewers in political science, see Djupe (2015).
19 A high-quality review of a complex manuscript, resulting in several pages of commentary to the editor and the submitting author, may well take up to 15–20 hours of one's time, time that could have been spent on one's additional research,

on teaching responsibilities, public outreach, or consulting that results in actual additional remuneration.

20 Publishers are financially invested in their packages of journals, especially through institutional subscriptions and downloads; for young scholars, publications determine their access to the job market; promotion and tenure decisions are critically impacted by numbers as well as quality of publications; editors' reputations may revolve around success stemming from the journal's rankings.

21 For instance, recognition by the journal in a long list of reviewers, and placement of participation on one's resume. Some publishers provide 'discounts' to their journals or books for reviewers, but typically both the journals and the books are available through their university affiliations.

22 Referees may veto manuscripts critical of their work or insist that authors quote or cite that work as a condition of acceptance (e.g., see Glass, 2000, as comedic relief on this point).

23 Several major journals rely on reviewer pools that include all those who have had manuscripts accepted at the journal. I am aware of two different cases when graduate students, who were second or third authors on an accepted manuscript, were asked to review submissions that obviously came from leading scholars in the field. While there is no reason why graduate students could not review manuscripts, in these two cases both students felt that to 'make their reputations' they had to demonstrate toughness by finding reasons to reject the manuscript.

24 Although I am aware of several cases, including one of my own, when an editor had changed the rules after the R and R manuscript was resubmitted. Just as problematic: when the R and R manuscript is resubmitted but the editorial team has changed and the new editor does not like the manuscript.

25 A classic warning that runs through much of the literature on peer reviews, as well as discussions between submitting authors, revolves around the propensity for our colleagues (at least those of us who have been trained in North America, but I suspect that this problem is far more geographically widespread) to generate more self-worth by deprecation and negative critiques than positive assessments. That is, after all, how most of us have been trained: to be professional critics.

26 For instance, over 90 percent of ISA's members opt for electronic copies of journals. Most libraries are now also opting for electronic versions of journals. Previously, the costs of hard-copy production, including shipping costs, required limiting journal space; while there are still costs today to having 'extra pages,' they are primarily due to copy editing costs, but increasingly those are now also borne by editorial teams and authors.

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