Against Desk Rejects!

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A scourge is sweeping the discipline of political science (and other sciences as well). Affecting an enormous proportion of active political scientists, the name of this new disease is "Desk-Reject Affliction." Every major journal in political science is infected, with journal editors suffering, by far, the most serious cases of this disease. The malady has progressed to the point that many journals, including respected journals, are now desk rejecting[1] half or more of the manuscripts submitted to them.[2]

Some desk rejects involve a single individual—possibly the editor of the journal but perhaps a subeditor—making the decision that a manuscript is not appropriate for the journal and/or, because it is judged that the paper would not survive a normal peer-review process, is not worthy of expending precious reviewer resources.[3] Beyond the traditional practice of rejecting unequivocally "off-the-wall" manuscripts (e.g., papers without a bibliography), desk rejects are a fundamental violation of the hallowed principle of peer review. Whereas it may be true that *published* papers are subject to peer review in one form or another, those *not published* are not published due to the decision of a small number of people—perhaps only one—who may not be experts in the paper's subject matter. For a subeditor in the field of macro-level comparative politics to decide that a paper on micro-level political psychology, for example, does not warrant review by a journal is not a valid example of peer review. More generally, the person making the decision to desk reject a manuscript often has entirely different qualifications and expertise compared to a typical subject-matter peer conducting a review. Moreover, whereas peer review may not necessarily require the views of more than a single reviewer, the standard in political science has been (and is, for papers that are not desk rejected) to make decisions on the basis of multiple independent reviews (see "the dreaded third review").[4] In this sense, our political science journals now have become more like law reviews: many papers are rejected by non-peers without any substantive review and with little justification.

The reasoning behind a desk reject is nearly always opaque and rarely if ever explicated in any detail. Editors sometimes offer gratuitous advice to send the paper to a subfield journal—although the standards for what should be published in a general versus a subfield journal often are completely undetectable from the papers that are published in the journal and are virtually never explicitly articulated by the journal. On occasion, editors may have specific hidden criteria for what type of papers they seek to publish (e.g., no formal theory). It is difficult to regard the typical desk-reject "justification" as a meaningful review of a rejected manuscript.

The large literature on procedural justice teaches us that there are two aspects to any given transaction, such as submitting a paper for publication. First, there is the outcome. I suspect it is rare for authors to complain too loudly about procedural failings when the decision is to publish their paper.

However, as has been written, "legitimacy is for losers." That is, the second-most galling aspect of a desk reject is that the rejection is procedurally unfair.[5] First, there are instances of editors taking months to desk reject papers, although I suspect this is rare. Second, the failure to provide substantive reviews to justify an editorial decision renders the rejection arbitrary and illegitimate.[6] Even the notorious New York Police Department's use of "stop and frisk" left a paper trail about why the decision to stop someone was made (albeit only after a judge ordered it to do so). Judicial scholars sometimes understand judges' decisions as reflecting "what the judge ate for breakfast." Perhaps the same theory is apposite for some editors as well. To be rejected by an unfair process is the worst cut of all.

There is another unwelcomed consequence of the desk-reject system. When Pat Patterson was editor for the *American Political Science Review* (ancient history), he often spoke about the review process as a "seminar by mail." I am sure that most authors have profited from negative reviews from true peers.[7] However, papers that are desk rejected repeatedly have little or no hope of improvement because the author is never told what specifically is wrong and how the shortcomings might be corrected. I believe our science and our scientists are harmed by that.

Another implication of Desk-Reject Affliction is that editors have assumed far more power than they traditionally have been given. Few authors want an editor who is simply a calculator, doing nothing more than counting the number of positive and negative reviews and deciding accordingly. However, for editors to be able to decide without any accountability (via a paper trail) that a large proportion of papers are not worthy of publication means that editors—whose terms often are years long—have unprecedented influence over the discipline.[8] This is not a wise strategy. A discipline shaped by only a few editors is likely to be much different from one in which peers decide what does and does not get published.

Why have desk rejects become so commonplace in our discipline in such a short time? Editors (like criminal lawyers who engage in plea bargaining) inevitably claim that if every paper submitted were to consume the time of three reviewers, the editorial process would grind to a halt. There simply are not enough peer reviewers (i.e., juries) to handle the crushing load. Therefore, the problem is that too many papers are being submitted. Desk rejecting has become the solution to this problem.

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POSSIBLE SOLUTIONS TO THE PROBLEM

Is the cure worse than the disease? Perhaps there are other solutions to the problem of excessive submissions that might better maintain the integrity of the peer-review process.

Some journals pay for reviews. Even a small amount of money often can entice people to sell their time. If the problem is that peers will not produce reviews for free, then we should determine how much it would cost to change their behavior. Some journals have experience with paying reviewers; indeed, I suspect that virtually all book publishers pay reviewers for their efforts.[9] Perhaps they would be willing to advise on the pros and cons of such policies. It seems likely that paying people for reviews would significantly enlarge the reviewer pool.[10]

Where would the money come from to pay reviewers? An obvious first answer is submission fees.[11] It seems almost certain that given the choice between not getting a substantive review for "free" and getting one for a fee, most authors would choose the latter.

Some journals charge these fees (*Journal of Politics* once charged a nominal fee). I assume some reviewers (like me) would continue to review for our main academic journals without charging a fee. If necessary, a mechanism might be established to subsidize those who claim they cannot afford the submission fee (see subsequent discussion on graduate students). Authors and reviewers might be able to allocate the monies they earn from paid reviews to offset submission fees through a type of credit system. Regarding administrative overhead, I expect that managing the money is less time-consuming than reaching out to five or six reviewers to have three or so to agree to provide their services for free. With payment for reviews, the rate of agreement to review certainly would increase. Some funding agencies allow investigators to budget for publication fees, and some journals are profit-making organizations.[12] Perhaps the profit-making journals (like some drug manufacturers) could pay the fees for those who claim they cannot afford them.

There are other ways in which revenue could be raised for our academic publishers, ranging from requiring that people who publish in the journal be a member of the association (even if only for a year) to outright publication fees (in addition to submission fees). Economists have documented the fact that there are substantial personal financial gains to be gotten from publishing papers in academic journals; it seems a small imposition to ask those whose papers we publish to provide at least some payment or remuneration to the journal.

What about graduate students? I have heard but cannot confirm that some graduate seminars require that all students submit their seminar paper for publication—just as it seems that virtually all students now apply for National Science Foundation (NSF) dissertation grants. The same might be said of so-called third-year papers. These practices, I suspect, place an unacceptable burden on our journals—although most of these are likely desk rejected in the first place—and should be prohibited. An easy solution would be to require a student's faculty adviser to sign off on a paper before it is submitted.[13] I certainly believe that some (perhaps many) graduate students produce publishable work, but many do not, and allowing unworthy graduate students' papers to muck up the review process is much too high a price for our science to pay.

Another implication of Desk-Reject Affliction is that editors have assumed far more power than they traditionally have been given.

I have published a "short paper" in one of our major journals (they used to be called "Research Notes"—I also published some of those). However, I have two observations about those notes. First, in my estimation, they are decidedly of lower quality and/or narrower interest, almost by definition.[14] Second, although they may not consume exactly the same peer-review resources as a typical paper, they do consume resources (most editors, in my experience, do not ask for a "review lite" of these papers). It is unclear to me why, when faced with a suffocating demand for reviews, a new category of easily produced papers requiring review would be implemented. Perhaps there are good reasons, but from the vantage point of preserving the endangered peer-review process, I certainly would tradeoff the short-paper submissions for fewer desk rejects of real papers.

But, to parrot a famous question: What is to be done? It may be clear by now that I think desk rejects are an existential threat to the quality and integrity of our peer-review system and our science. I have suggested ways in which the number of papers requiring review might be reduced, as well as ways in which peer reviewers might be enticed to be more willing to conduct reviews. However, there is another way by which the costs of desk rejects can be altered.

However, there is a fundamental principle of fairness here: We should not review for a journal that will not review our papers.

I have adopted a simple policy on desk rejects: I will not review for a journal that will not review my papers. I put a time limit on the "red card." I tell editors who desk reject my papers that I suspend being a reviewer for at least a year for their journal. I also report that I will try to convince my coauthors (where relevant) to adopt my policy. Moreover, it is obvious that I am writing this article, in part, to convince my colleagues to adopt my policy.

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One editor replied, in effect, that I do not do much reviewing for his journal so little is lost. Fine. But I, like most of us, do a significant amount (too much) of reviewing. For some journals, I probably review three or four or even more manuscripts per year, even on an "emergency" basis. Therefore, after a desk reject at one of these journals, it loses my services for perhaps three reviews. It could have expended three reviews on my paper, which my three reviews on other papers would have balanced out. Therefore, given its refusal to review my paper, the journal lost a net of three reviews. Not such a big deal, perhaps, in part because I do not know what policy my coauthors adopt. However, there is a fundamental principle of fairness here: *We should not review for a journal that will not review our papers.* As a corollary, if we do not like editorial policies, we should do what we can to convince editors to change them.[15]

An editor of a major journal recently told me that he receives few complaints (beyond mine) from authors on desk rejects and other issues. One lesson that all professional socialization courses teach graduate students is: Don't get into a war with editors, no matter how badly you have been treated (e.g., a year or more on a revise and resubmit accompanied by a summary rejection—true story). In general, this is good advice for authors. However, editors should not assume that because they receive few complaints that authors are happy. Many are not. Because I think this issue warrants wider discussion within our discipline, I have decided—perhaps unwisely—to ignore my own advice about keeping one's mouth shut.

There no doubt are some half-baked ideas in this missive; others might have better solutions to the desk-reject problem. The fundamental point I want to make is simply that Desk-Reject Affliction has engulfed our discipline, with little public discussion, and with substantial consequences. The practice typically is justified by the problem of too many submissions. Perhaps that problem can be solved in other less-damaging ways if we—those who submit our papers to *our* journals—put our mind to it.

Most important, this non-peer-review process has slowly and silently crept into our discipline and now has become the "new normal." It has done so to the displeasure of many authors (who also are reviewers) and without widespread debate and discussion. There undoubtedly are some manuscripts that do not deserve peer review, but if they are half or more of all papers submitted, then something may be amiss. My hope is that this article will inform editors of the widespread ($N \geq 1$) unhappiness of their authors and will initiate a much broader and open discussion of ways in which the peer-review process can be saved.

Journals at which this article has been desk rejected: none, so far.[16]

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NOTES

1. It is with some trepidation that I use "desk reject" as a verb. I do so, however, because it clearly has entered the active lexicon of unhappy political scientists (as in: "I just got desk rejected"). I use the shorthand term "desk rejects" to refer to editorial decisions to not allow a paper to proceed to peer review.
2. An article published in this journal (Peress 2019) reports that no political scientist has published more papers in our discipline's most prestigious journals than I have. What that article does not report, however, is that there is a fair chance that I am also among the most rejected of all political scientists. In this article, I resist the temptation to personalize my complaints against the desk-reject system that has become so prevalent in political science. Instead, I am trying to raise issues and concerns that apply generally to many editors and authors.
3. My purpose is to discuss alternatives to desk rejecting manuscripts. Therefore, my characterization of processes by which this currently happens may not be exactly true of every journal in all instances (e.g., an editor might have to "sign off" on a subeditor's decision). I am certain (from experiences that I can document) that my description of the process pertains to at least one of our journals.
4. There is another important distinction between desk rejects and the traditional peer-review process. Whereas reviews of NSF proposals may be thought of as "1.0" blind (i.e., the investigator is known, the reviewer is not) and peer reviews are said to be "2.0" blind (i.e., double-blind, although the reality is that these are frequently closer to "1.5" blind), a desk reject often is "0.0" blind. That is, often (but not always) the editor making the decision to desk reject a paper knows the identity of the author(s) and the identity of the deciding editor or subeditor also often is known to the author. Again, this process bears some resemblance to law reviews, in which authors often are asked to submit their vita along with the paper that they are seeking to publish. That junior scholars serving as editors or subeditors are willing to allow being publicly assigned the responsibility for a desk reject is brave and perhaps a little surprising.
5. On arbitrariness in decision making (and its connection to domination), see Lovett (2012).
6. A little experiment might be helpful. The next time a student comes into your office to complain about a grade, tell the student that you stand by the grading decision but you will not tell the student why you are "desk rejecting" the appeal. Or, when grading a term paper, assign a grade but provide no comments on the reasoning behind the grade.
7. On this and many of the empirical issues I address in this article (e.g., the benefits of manuscript reviews, term limits, and the financial benefits of publications), there is scientific evidence that could be cited and consulted.
8. Perhaps this also is a call for strict term limits for editors.
9. This also is beginning to happen with various types of letters of recommendation.
10. This has worked for MTurk.
11. I do not profess to know much about the financial aspects of journals, but at least some seem to be profit-making and others are sponsored by wealthy organizations such as the American Political Science Association.
12. It always has puzzled me about why political scientists would so freely subsidize these businesses.
13. This process has been used in other contexts (e.g., graduate student submissions to present conference papers). Perhaps something can be learned from these experiences.
14. I mean this only in the sense that these papers are designed to address specific—often empirical—issues, typically without comprehensive hypothesis testing.
15. Perhaps a more satisfactory solution would be for editors to decide that when they desk reject an author's paper, they will not ask that author for future reviews for a specific period of time.
16. Perhaps it would be beneficial to the discipline if authors of papers that ultimately are published were to list in their acknowledgments the journals at which they were unable to get a peer review. This might shed more light on the decisions of editors to refuse reviews.

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