An essay on the art of reviewing: some thoughts for new ASR reviewers
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Thanks to Lisa Brush and Alexis Walker whose reviewer suggestions for Gender & Society and Journal of Marriage and the Family respectively have been extremely valuable for me and for many of my students. Many of these good ideas are theirs (and more detail can be found on these journals' websites), but the responsibility for the bad ones must be laid at my door alone. Thanks also to Lisa Brush, Charles Camic, Paula England, Jerry Jacobs, Judith Lorber and Franklin Wilson for their "good reviews" of this essay.

Every author who submits an article hopes for "good reviews." Of course, this desire includes wishing for reviews that lead to a positive decision, but even more importantly a "good review" is one that helps a paper develop to its full potential and make its contributions as clear and as powerful as they can be, both theoretically and empirically. If one is going to take the time and energy to do a review, one should try to make it be a "good review" in this sense. This essay aims to help build a wide pool of good reviewers for ASR – and for the many other journals who call upon this vital resource.

Reviewing provides two equally valuable services: it winnows the wheat from the chaff of what is submitted to the journal and it cultivates better writing and thinking in the journal's writers and readers. A "good reviewer" makes better papers, and editors rely on good reviewers to turn submissions into material that makes a great journal, not only by selecting but by cultivating the best possible papers. To focus only on the winnowing or gate-keeping function of reviewing is to see only half the contribution it makes to scholarship. Thus, a "good review" addresses both the editor and the author, helps both to see clearly what the paper does and what it still needs to do to reach its potential, and moves the paper and the scholarship to which it contributes further along the path toward achieving it.

Sometimes writing a "good review" in this sense is impossible. If one finds oneself so out of sympathy with the overall interests of the author – believing that the total approach is wrongheaded and that a paper with this sort of argument should in principle never see the light of day – then the reviewer has an ethical obligation to make that fundamental disagreement clear to both the author of the paper and the editor of the journal. Such a core disagreement on philosophy or theory should not be expressed in a disagreeable tone, whether hostile or condescending, but should be frank and fair in addressing the heart of matter, explaining the issues at stake and what the points of disagreement are. While not a review in the sense of a careful examination of all aspects of the paper, this sort of open opposition is valuable feedback to the editor, and may be grounds for a "comment" in the journal. To attempt to deliver a "stealth attack" by hunting for ways to undermine the paper or holding it to some higher standard of evidence than one would use for other papers is unfair to both the author and the journal. But assuming the reviewer is willing to take on the challenge of writing a good review (one that is helpful and constructive even if ultimately negative), how is this done?

While every "good review" is different, just as every paper is, reviews no less than papers have a structure recognized as effective and widely useful. While form and content are related, the reviewer's primary obligation – like the author's – is to adapt the conventional form to the specificity of communicating the relevant argument effectively to a particular audience, which in the case of a review is both the author of the manuscript and the editor of the journal. While each audience has specific needs and sensitivities, the good review helps both. The conventional form of a review has three major parts.

First, the review provides the reviewer's <u>brief</u> statement of what the primary arguments and contributions of the paper are. This provides the author with reassurance that the reviewer has indeed grasped the central point of the paper – or makes it obvious that this point was not communicated! This latter outcome is of course frustrating to the author, whose responsibility it is to make this meeting of minds happen, but whose first impulse is to blame the reviewer for failing to grasp what the author knew – but apparently did not say. It is nonetheless a highly informative even if not directly evaluative part of the review, for editor and author alike (the latter perhaps only after passion has cooled and reason returns). When the reviewers differ about what constitutes this central point, the editor knows that the paper is confusing and unfocused, even if the separate reviewers see merit in what they are reading into the indistinct presentation. When the reviewers concur, but the point is minor, this may be grounds for the editor to divert the paper to another journal, even if the way that the paper makes this point is effective and appropriate. An excessively long and detailed summary raises the editor's suspicion that there is no central point.

Second, the review provides an overview of what the major strengths and weaknesses of the paper are. Both strengths and weaknesses need discussion. The discussion of strengths is an opportunity for the reviewer to make clear just how major or minor the paper's points are in relation to what the reviewer already knows. Since the reviewer, like the author, is a specialist in an area that the paper addresses, it is a service to the editor (who may not have this specific expertise) if the review makes clear just what the value of the contributions of the paper are and why the reviewer thinks so. The reviewer should also point out whether the author is overstating these contributions, or less often, but not infrequently, understating them in relation to the bodies of knowledge --named and unnamed in the paper itself -- to which they apply. Just to say that a paper is great or makes a valuable contribution is never as helpful to either the editor or the author as when the reviewer takes the trouble to spell out what that contribution is, and why this sort of contribution does or does not belong in ASR.

In general, ASR is looking for papers that have a broad sociological relevance beyond any one specialized area of concentration, so it is perfectly appropriate for a reviewer to indicate that there is a valuable contribution being made by a paper but that it is too narrow in scope to fit into ASR, and suggest a more appropriate specialty journal. Remember that the editor may come to that conclusion even if the reviewers do not! If in the reviewer's judgment there are wider implications latent in the argument that are not yet being drawn out successfully, the reviewer should suggest how the conceptualization could be broadened to speak to more potential readers. If the paper is broad but reviewers do not let the editor know when and how the paper speaks to their (diverse) expertise and interests, it makes it more difficult for the editor to see the reach of the paper, and makes a negative decision more likely.

The core of a review should address the strength and validity of the logical links between the theory and the question, the question and the analysis, and the analysis and the conclusions. It is these logical links that are the backbone of any paper. Does answering the concrete question actually contribute to some theoretical debate or testing some theory? Does the analysis actually answer the question? Do the conclusions actually flow from the analysis? In which of these areas do significant weaknesses appear?

In outlining the weaknesses of a paper, a good review makes its priorities clear from the outset and starts with the most major problems. If the serious problems are so large that the paper is not likely to be able to resolve them, the reviewer should make clear why and how this is the case, and provide concrete and convincing examples of how these problems are manifested (page numbers referring back to the manuscript help both editor and authors follow this exposition). If the problems are significant but potentially fixable, the reviewer should be as specific as possible in outlining what it would take to fix them. If there are alternative ways of fixing a problem that occur to the reviewer, it is very helpful for both editor and author to see these alternatives spelled out. Not only does presenting alternatives make clear that the reviewer is not trying to write a different paper than the one that the author set out to write, but it makes the nature of problem itself clearer. For example, an inadequately defined theoretical argument is easier to recognize as such when contrasted to several ways of defining what the missing theory is than when just one other way of seeing the problem is offered. Likewise, offering more than a single alternative data analysis strategy will not suggest that this is the reviewer's own favorite "hammer" for every problem but indicate the inferential gaps left by the existing analysis that could be filled in these ways.

Sorting out problems not only in terms of seriousness but also in relation to what sort of work is needed to fix them is useful to both authors and editors. The types of work called for might include (among other things) rethinking the problem to widen or narrow or shift its focus, changing the data analysis to better reflect the way the problem is framed or to fix problems of inference and reliability, or rewriting to prune away unnecessary digressions or verbiage, fill in the gaps of exposition, or both. A review that ties a clear description of the nature of problem to an equally clear statement of what would fix it will inevitably come across as more constructive than carping in tone, and it also provides a more realistic take on how likely it is that the author will be willing and able to do the work being called for.

When there are serious gaps in exposition or analysis, the reviewer should make clear to both the editor and the author when even filling in these gaps expertly might still produce a paper that is too narrow theoretically or limited empirically to merit inclusion in ASR. Sometimes, a revision ends up fixing the stated problems without in the process becoming the contribution that the reviewer and editor hoped to see, a situation that is extremely frustrating to the author. Reviewers can help editors avoid such problematic revise and resubmit (R&R) decisions by not merely identifying the errors and gaps in the present version but also trying to imagine what the paper really needs to do to be good enough to publish. Sometimes this leads to a rejection that does a favor to the author, having saved a fruitless round of resubmission back to ASR and offered a solid view of fixable problems that (when corrected) will make the article easily acceptable in a more specialized venue. Sometimes, this leads to a revise and resubmit decision that contains more encouraging suggestions for what needs to be developed, clarified, presented and emphasized as well as identifying gaps to be filled, problems to be fixed and excesses to be pruned away. But these are still "good reviews" for the author as well as for the editor.

The third, and most optional, part of a review is the "small stuff" that should be fixed but that does not significantly detract from the merits of the overall argument. In this category, the reviewer is entitled to complain about misspellings (whether or words or names), muddy labels or poor quality graphics, poorly presented statistics (such foolishnesses as $p < .000$, for example), insufficient subheads, excessive jargon, run-on sentences or passive voice, and any other flaws that make the article less than ideal reading. Reviewers are often very deeply influenced by such presentational matters, as indeed any journal reader would ultimately be. The disproportionate impact such problems have on the credibility of the author and the argument is why such problems – if they exist -- absolutely should be noted and ultimately fixed, but the reviewer should fight the strong temptation to jump directly into expressing outrage that a paper with these problems would even be submitted.

Authors would be well advised to do more than they now do to avoid creating such provocations for reviewers, but even when the author is deficient in exercising care, a good review does not make more of these minor issues than they deserve. A paper that makes a major contribution but contains only such immediately resolvable problems is one that can be accepted forthwith, but all these minor problems should be noted by the reviewer in order to be fixed by the author. If a paper is already sunk because of the major problems it has, it serves only to muddy the waters to also belabor these relatively superficial flaws. Still, it can be a service to the author to note when and how more diligence should be exercised in preparing the manuscript to be submitted elsewhere. A good review should always be aiming to make a better paper, whether or not that paper is destined for ASR.

Alas, it takes more lines to describe a good review than to actually write one. A good review is brief and to the point. A reviewer who re-reads his or her review (always a good idea) and sees generalities without examples, criticisms without constructive suggestions, superficial problems leading discussion of more substantial flaws, or a one-sided focus only on weaknesses or strengths should consider a second draft! The first draft may help clarify the issues in the reviewer's own mind and allow them to be communicated much more effectively to the editor and author, but don't let perfectionism delay you. The *sine qua non* of good reviewing is promptness – authors suffer emotional and sometimes practical costs when reviews are late, and chasing after late reviews is a major drain on editors' time and patience. Just because it should never be longer than a month on your desk doesn’t mean it should sit here so long if it can be avoided. Quick reviewers do an enormous service to authors and editors alike.

In the end, enjoy the process of reviewing for what it is – a chance to see what the newest work in an area has to offer, an opportunity to cultivate better work in an area and raise the overall level of scholarship, and a form of teaching that reaches your colleagues, not only your students. Gate-keeping work is part of all three of these aspects. Yet when a focus on selecting the best leads to ignoring the rewards and challenges of making sociology better, reviewing is less enjoyable for reviewers as well as less helpful to journals and authors alike. Reviewing is itself an important contribution to the health of the discipline and to the creativity and flexibility of your own sociological imagination. Do it early and often!