The Revolt of the Reviewers: Towards Fixing a Broken Publishing Process

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Abstract

The sociology review process is broken. There are too many reviews per paper and the norm that all papers go through a revise and resubmit process burns through reviewers, slows down the publication process, creates distorted papers as authors try to satisfy incompatible demands, and puts too high a premium on novelty over methodological rigor. Editors should get fewer reviews per paper and make editorial decisions. No new reviewers should be added in reviewing a revised manuscript. Peer review should not be treated as an opportunity to receive editorial assistance and mentoring and instead should be focused on ensuring that published papers are correct. Asking for large numbers of reviews will not solve the problem that sociologists do not agree on their standards. The reviewer problem points to larger questions about the structure of sociology as a field, the need for more venues for publishing solid empirical work, and the importance of replications. It also points to the structure of academic publishing in which for-profit publishers make money by selling to university libraries the products of the free labor of university professors.

Keywords: journals, peer review, replication, sociology, publishers

I hit a nerve a couple of years ago when I complained publicly on a blog[i] about receiving a reviewer's packet with five reviews in it and threatening never to review for the journal. Then I segued into a rant about the broken revise and resubmit process and a young scholar I knew who'd had a total of 10 reviewers on a paper at ASR in several rounds of R&R before it was finally rejected. The blog post got 44 comments and contributed to a discussion that flamed around other blogs and the field of sociology more broadly. Many people had horror stories about endless rounds of R&Rs and up to a dozen reviewers for one journal. This "revolt of the reviewers" has already had an impact. I personally talked to the editors of several journals about my grievances: some promised to reform in response to my complaints while others cut me off from their list of reviewers due to my criticisms of them. All of the candidates for the position as editor of ASR last year ran on a platform of cutting down on R&Rs. Virtually all the journals seem to have cut back on how many review requests they send out per article and there clearly is a lot more rapid rejection in most sociology journals rather than endless R&R cycles than a few years ago.

Behind the revolt of the reviewers are some more fundamental problems about the state of publishing and the discipline. My original blog rant addressed two correlated but distinct complaints: too many reviewers being sent manuscripts on the first round and a broken revise and resubmit process that (among other flaws) adds new reviewers at each round, a procedure that both burns through reviewers and asks authors to attempt to satisfy the shifting and incompatible demands of a large number of reviewers who do not agree with each other. Each of these complaints points to larger issues, to the structure of academic publishing and the escalating competition for jobs, and to the deep nature of sociology as a discipline. I write as someone who has been a very active sociology reviewer since the 1980s (when I was on two editorial boards at the same time) and have watched the problems get worse over the years. It is really not true that the way things are now is the way they have always been

As I reflect, I think there are three big categories of problems: (1) A review process that is wasting the time of writers and reviewers, is treating peer reviewers as advisors and editors, and is obscuring a deeper problem of disagreement about standards and methods. (2) A review process that places too much focus on novelty, innovation, and framing and places too little focus on whether results are accurate and replicable. This is tied to the problem of too little appreciation in the discipline for the need to build up a corpus of accurate and replicable findings. (3) A corporate structure of academic publishing that is making profits on the backs of the free labor of writers and reviewers while extracting rents from academic libraries. This essay is an opinionated and provocative attempt to call attention to important issues that will require thought and effort to resolve. In each case I name a problem and some possible solutions.

PROBLEM: TOO FEW REVIEWERS, TOO MUCH TO REVIEW

In math, the custom of most journals is to send a paper to ONE peer reviewer. The sociology custom used to be two reviewers but has escalated so that three is the norm, and I've often seen initial review packets with four reviews. I threw a fit when I saw five. Ignoring issues of reviewer competence and why sociologists don't trust one reviewer (discussed more below), there are other structural reasons why there are too few reviewers relative to the number of submitted articles. One big reason is the huge escalation in the number of articles being submitted and the number of journals they are being submitted to. There is a large pool of graduate students and new PhDs who know they have to publish to get jobs (and tenure) who are sending out large numbers of manuscripts to journals. There are not enough jobs for all of these students, nor enough slots in high-prestige journals for their articles, and the competitive pressure has escalated. These justly-anxious young scholars put pressure on journals for fast turn-around in the review process because their professional lives are at stake.

Senior scholars regularly receive at least four and as many as a dozen requests to review a month. People vary in how much they are willing to review, but no "name" reviewer can review everything they are asked to review. Senior reviewers will be turning down at least 50% of the review requests they receive. When I was younger, before I got so crabby, I was reviewing an article a week and, in the process, having a major impact in shaping my field, I know some people who are reviewing at that pace now. Most reviewers say that they feel two reviews a month is their maximum, or 24 a year. Some sociologists argue that they owe the system three reviews for each article they submit (on the assumption that each article gets three reviews), but this is a fallacy. Most articles are submitted by previously-unpublished young scholars who are not considered competent reviewers and will not be asked to do a lot of reviews. I believe any tenured professor has to do a minimum of at least six reviews per article submission under the usual calculus of 3 reviews per article, or 24 reviews a year for a steady producer who submits two articles a year. High-volume article-writers who submit more than that are not pulling their weight as reviewers if they cap their reviews to 24 a year. And, of course, if papers are actually receiving 4-12 reviews each, then all the calculations about who owes what need to be reworked.

Reviews take time, and senior reviewers have a lot of other demands on their time. Peer review is unpaid volunteer labor (more about this later) that tends to slip down in the “to do” queue behind preparing lectures, grading papers, advising and reading papers for one’s own students, and (hopefully) doing one’s own research and writing. Most reviewers are chronically late in submitting reviews. I used to purposely delay sending a review because it seemed that each time I sent one in, the journal mailed me another paper, although the editor denied that was really how the system worked. “Chronically late” is relative, as this can mean a couple weeks late or a year late.

Editors manage (or were managing) the dual pressure from authors to speed up the review process and the high turn-down and slow response rate from reviewers by sending papers out to extra reviewers in the hopes of increasing the chances that they would get at least two or three competent reviews back within a reasonable time frame. But this strategy makes the problem worse, as it increases the number of requests to review flooding reviewers’ inboxes and increases the rate of rejection and late response. That adaptive strategy is a death spiral.

Since my blog rant, it is my impression that at least some journals have changed strategies and now focus on getting quick responses from reviewers about whether they will review or not (I now get weekly reminders when I’ve been asked to review) and will cancel the request for a non-respondent. They also send much more frequent email reminders as the review deadline approaches. Whether this has helped the problem, I don’t know.

Overall, there simply are not enough qualified reviewers to sustain the system in its present bloated form. The normative number of reviewers per article must be scaled back. Editors should initially solicit no more than two senior reviews per article, and should solicit a third only if the first two are not conclusive or if they are trying out a new or junior reviewer. It should be acceptable, even encouraged, for a tenured professor reviewing an article to ask a graduate student to do the first draft of a review, with the professor checking the work and finalizing the review. And the practice of routinely adding new reviewers to an R&R just needs to end. Apart from the way it has distorted the publication process (more about this below), it burns through reviewers at an unacceptable rate.

PROBLEM: DISTORTED EXPECTATIONS DISTORT THE REVIEW AND WRITING PROCESSES

Editors complain that there is relative dearth of good reviewers and that many (most?) reviewers write mediocre and uninformative reviews. Editors say this is one reason they send out papers to more than two reviewers, or add reviewers after an R&R, trying to bring more competence into the review process. There is, of course, some dispute over what a good constitutes a good review or a good reviewer. Junior scholars, some of them unpublished, are asked to do reviews, because there are simply not enough published senior scholars to go around. But many of these are not particularly good reviewers because they are still learning the ropes themselves. But no matter how senior and well-vetted the scholar-reviewer, editors and authors alike are offended at short evaluative reviews: both "this is great, publish it" and "this stinks, reject" are considered uninformative. Editors say they need reasons why the article is good or bad. Fair enough. But that isn't the end of the job. "Good" reviewers are also supposed to write comments for the author that are constructive, kind, and offer detailed suggestions for revision, thus blurring the boundary between reviewer and advisor or editor. There is an expectation that there will be a revision process and it is the reviewer's job not just to provide evaluation, but to provide a detailed roadmap for revision. Doing a "good" review that provides a roadmap for revision thus entails at least two hours of work and can take eight hours or more. These reviews are typically at least two single-spaced pages long (i.e. about 1200 words) and fairly often are double or triple that length.

Reviewers who do this much work expect to be appreciated. So now we have the institution of the "response to reviewers"—a 30+ page document in which every single comment of a reviewer, no matter how ill-considered or uninformed, is indexed and responded to in detail. This is now considered "standard practice," and many sociologists truly believe this is the only proper way to respond to an R&R. As these reviewers never agree among themselves, the author is also compelled to distort the paper to try to accommodate several different shadow coauthors who don't agree with each other. Many editors seem to have abandoned editing entirely and provide authors with almost no guidance for how to deal with conflicting or even wrong-headed suggestions by peer reviewers: all too often, the editor simply says "respond to the concerns of the reviewers." Worse, there are even tales of reviewer #2 complaining that the author failed to treat the comments of reviewer #1 with sufficient respect! This model is, to put it bluntly, a waste of many people's time. I have reviewed hundreds of articles and am generally considered to be a "good" reviewer who reads carefully, makes insightful comments, and avoids grinding theoretical or methodological or personal axes. But even a paragon of virtue can have an off day, and upon multiple occasions I have read sloppily and missed an error or saw one that wasn't there, made a suggestion that was simply wrong, or have responded grumpily or overreacted to an argument that annoyed me. It is embarrassing to see authors contort themselves trying to deal with reviewer lapses and errors.

These review response memos are bad enough, but some editors even require a "track changes" document so that changes can be identified. I find this stupefying. I have written and been a reviewer on some of the most widely-cited articles in my specialty, and I will report that the best of them have involved revisions that went way beyond "track changes" edits and instead involved authors doing wholesale rewrites or incorporating new material not suggested by the original reviewers. Really good scholars frequently have gained new insights while waiting for reviews, and it is often these refined insights that make the article stand out. Authors who slavishly do exactly and only what the reviewers ask for are like the students in your classes who anxiously ask you to tell them exactly what they need to do to get an A, and editors who follow this model seem to me to be bureaucrats, not scholars.

The expectation that a reviewer will make extensive substantive comments and suggestions for revision has become incorporated into the norms and practices of the discipline. I have talked to many young scholars who send out papers they know are too rough to get published to top journals with the goal of "getting good reviews." They think their submission fee is buying them the right to 4-8 hours of the time of several top scholars to read and comment on their work, and are disappointed if the reviews are "not helpful." That is, the norms of the discipline are conflating peer review with advising and editing. Senior scholars do this too: they know they will always be asked for revisions, so they send out rough papers figuring they will save a step and do the final polishing of the paper after seeing the reviews. On the other side of the table, there are many people who have now persuaded themselves that it is a reviewer's job to mentor and nurture young talent and, thereby, raise the level of writing in the field. That is, they have totally blurred the distinction between an advisor or mentor versus an anonymous peer reviewer whose job is to ensure the scientific integrity of what is published in a journal. This blurring is what is causing the problem of the endless R&Rs. Any good advisor reads a draft multiple times and makes several rounds of suggestions for revisions in the writing or analysis.

The only solution to this problem is to scale way back and decouple advising/mentoring from peer review. Of course senior scholars should give advice to junior scholars, but the peer review process is the wrong way to do this. Many ASA sections offer some sort of mentoring program that pairs junior scholars with senior scholars, and this is the way to improve the level of writing in the discipline. If we were not so busy writing long reviews with suggestions for revision in peer review, maybe we would have more time to take the same level of care in a professional mentoring system.

PROBLEM: CONTENT OF THE REVIEW PROCESS

A side-effect of the blurring of the roles of reviewer and mentor is that too many peer reviewers have shifted away from the core function of vetting the methodological correctness of a paper and instead write reviews focused on the way a paper is framed. While framing and shaping a paper are critical aspects of writing and something I devote a lot of attention to in my own work and in mentoring students—and, candidly, something I think I am particularly good at doing—I fear that it has become a fetish or a diversion in the review process, as getting published becomes more and more about how the article is written or played and less and less about whether the article is reporting a solid well-documented empirical finding or providing a clear test or exposition of some theory. There is a growing non-replicability scandal in many fields, including biology, economics and psychology that is due to an excessive emphasis on having something "novel" or "interesting" or "unexpected." This is a big problem.

The peer review process needs to focus first on the methodological adequacy of the work (is it correct?) and then secondarily on its importance as an intervention into ongoing scholarly debate. A reviewer's job should be assessing the accuracy of a paper, not being an editor or shadow coauthor. To speed the review process, authors should be encouraged to include methodological appendices with first submissions with answers to reviewers' likely questions, including the results of sensitivity tests or alternate specifications of models or details on data collection that are too lengthy for an article format as well as extended explanations of why they are not including particular literatures or perspectives in the article's framing even though someone might think they are relevant.

Reviewers should focus the main thrust of their reviews on the methodological question of whether the results are correct and trustworthy. Questions of how to frame an article for theoretical impact should be addressed only if the reviewer has signed off on the quality of the results and such discussions should be appropriate to the journal. Reviewers can be helpful and make formative suggestions about how an article could be improved, but they should recommend outright rejection if their suggestions would require major changes in the nature of the paper.

Editors should not use the R&R process to avoid or delay making decisions. We have ample evidence from decades of peer review that sociologists do not all agree with each other about whether an article is good or about how results should be interpreted. As an example, my good friend and colleague Myra Marx Ferree and I once learned that we had been the dueling reviewers on a paper that we both thought had important results, but we disagreed about the best theoretical interpretation of those results and asked for conflicting changes in several rounds of review. Fortunately, this was long enough ago that editors still edited, and both Myra and I were good enough reviewers to pick up on what was happening and both backed off enough to allow the author to publish. Editors who mindlessly tell authors to please all reviewers and reviewers who are less willing to recognize and respond gracefully to inter-reviewer conflict would have ended up killing that paper.

Editors are senior scholars who should be trusted to make decisions and should make them. R&Rs should never be given because the first-round reviews are inadequate or inconclusive: editors should read the MS themselves, pass the article to trusted in-house reviewers or a deputy editor or make a special request to a trusted reviewer. Under no circumstance should any editor ever send an R&R to an author telling the author to respond to disagreeing reviewers without offering specific guidance about what to do. Revised manuscripts should be sent only to the original reviewers. If the editor has some specific concern that requires another reviewer, the editor should inform the author in the R&R letter what the concern is and what type of reviewer will be solicited for the next round. This new reviewer should be given explicit instructions about their role in the review process.

An R&R should be given only for checking the accuracy of results or for one revision of the exposition for results that are sound. Editors should never give an R&R if the nature of the necessary changes is a different research project (i.e. different sample, different variables, new data, or wholly new analytic strategy.) An R&R may only request changes in how the article is written (i.e. framing or literature cited) or in improving the analysis of data already in hand. Editors who want to foster a project that needs extensive further work may reject and invite resubmission, perhaps offering to waive the submission fee for a revised paper.

Editors should stop asking authors to write lengthy revision memos that are longer than the original article. Editors should also not ask for documents with “track changes” turned on. Editors’ letters to authors should clearly state the points that need to be addressed, and authors’ responses should be brief bullet points noting what has been done regarding the key points summarized by the editors.

Editors should tell the author who is asked to perform methodological checks on the stability of results that if those checks reveal that the original result is wrong, they should consider the article rejected. Authors who are in this situation should be permitted to send a methodological report to the editors and the initial reviewers for a response before undertaking a full revision.

PROBLEMS ABOUT THE COMPETENCE OF REVIEWERS

Underneath the complaints about the content of reviews is an even more insidious problem that is rarely openly acknowledged. The pool of reviewers is essentially the same as the pool of published authors. The vast majority of authors' publications are viewed as inadequate and are rejected, at least at the time of initial submission. Therefore, it should not be surprising that most article reviews are viewed as inadequate by the editors, not to mention by the authors and their friends. A frequent topic of discussion at most sociology gatherings is the uninformed or misguided commentary of article reviewers. These stories range from complaints about reviewers that are grinding some theoretical or political axe to complaints about reviewers who appear not to understand the methods they are reviewing. Although most of these complaints are about more elaborate statistical methods or the standards for qualitative research, I've also seen complaints about reviewers who appear not to know that the mean of a binary 0-1 variable is the same as the proportion of the sample coded 1 on the variable, or at least are convinced that it is wrong to list a proportion or percentage in a table of means. The journal rejection rates and review complaint stories lay bare either the profound incompetence of a large fraction of sociologists, or a profound lack of agreement about the methodological and expository standards in sociology.

The proliferation of reviews and the scandal of articles that are read by as many as 12 different people while under review by one journal is a disturbing symptom of a huge problem in the discipline: we don't trust each other's ability to assess methodological correctness or the quality of argumentation. We don't even agree on what the standards are! Even our discipline's longstanding custom of having three reviewers per paper, not just one, is itself a sign of the problem. By contrast, mathematics articles are typically reviewed by only one reviewer, who checks the math and offers an opinion about the article's importance.[ii] Editors collect multiple reviews, learn that they don't agree, and then collect more reviews, as if that would solve the problem. It won't. If you really cannot trust one or two reviewers on a paper, then the whole peer review system should be tossed out and replaced with something else. There have been serious proposals to shift to crowd sourcing for peer review: post papers on the web and let dozens of commentators make comments and vote on the paper's quality. Any editor who feels the need to ask 10-12 reviewers to read a paper may as well give up entirely and shift to crowd sourcing.

DEEPER ISSUES ABOUT THE PURPOSE OF PUBLISHING AND PEER REVIEW

What is the purpose of publication in the first place? Is its primary purpose to establish a prestige order among scholars? Or is it to contribute to an accumulation of knowledge about how the world works? If the purpose is to advance knowledge, not only the journal-review process, but the entire conception of what makes a journal article "good" or publishable needs an overhaul and there needs to be a recognition of the importance of different kinds of "good" articles. Advancing knowledge is a collective enterprise. Science needs both the occasional brilliant theoretical breakthrough that reorients a field and the careful accumulation of relatively mundane empirical research or theoretical refinement. Neither is meaningful without the other. Further, the same empirical finding can be relevant to multiple theories.

Brilliant writing or thinking may serve the function of prestige ordering of academics, but to be useful for understanding the world, this thinking has to be based on or relevant to empirical data. Although there are rare exceptions, a single quantitative analysis or qualitative case study usually does not contribute all that much in isolation, but the accumulation of research that replicates patterns across samples and settings can add up to solid knowledge. But this won't happen if publication of every article requires making some novel theoretical point. Too many publications are based on the over-interpretation and over-theorization of what proves to be a non-replicable interaction effect. The scandal of non-replicable results is not confined to sociology, but has been well-documented in psychology, economics, biology and medical research, as well.

There is a great deal of tiptoeing around the question of whether the standards for publication should be lower or different for specialty journals or the less-prestigious general journals and, if so, what that different standard should be. The editors of lower-tier and specialty journals typically send their articles out for review to the same pool of reviewers as the top general journals and often take offense at any reviewer's suggestion that they have adjusted their standards according to journal prestige. Contrary to the idea that high rejection rates demonstrate quality, my own view is that any well-done empirical study should be published, and the only question is where. Sociology as an empirical field can advance only by accumulating a large body of empirical research that replicates and re-replicates its core findings, so that we are able to know for sure which facts are well-established. There needs to be more appreciation for the value of solid well-documented and reliable descriptive information, both quantitative and qualitative, and places to publish such work. Theories about social processes have to build on a solid base of facts. Replications of the same basic finding using different models in the same data and different data are crucial to having a solid empirical base. This implies the need for a system that publishes these core empirical results and makes them accessible to the community of scholars.

This is not to say that all publication outlets should be the same. There are the "top" journals that should be publishing major findings of large, novel projects that break important new theoretical ground and are of broad interest to all sociologists; there are lower-tier general journals that publish more modest contributions that add to the accumulation of knowledge with articles that are less definitive or more replicative than original; there are specialty journals that accumulate knowledge and theorizing within a smaller, more focused community of scholars. While these different types of journals should all have high standards of methodological competence and correctness of results, they differ in the scope or impact of the articles they publish. The impact of articles in "top" journals tends to become a self-fulfilling prophecy, as an article's placement in a top journal enhances its visibility. At the same time, it has to be recognized that some of the important novel theoretical contributions in many fields were initially rejected by "top" journals and ultimately published in "lower tier" journals.

Even as it is clear that sociology as science benefits from the publication of all worthy research and that on-line publication is the most cost-effective way to make this happen, two problems remain. First is the prestige ordering problem. How can some people get good jobs and big raises and other people be relegated to marginal employment at low wages if we cannot rank scholars and say who is better? High rejection rates and the narrow gate into the small number of top journals is more about sorting and sifting scholars than it is about sorting and sifting ideas. But if we apply our sociological training and recognize this dual function of peer review, perhaps we can get to a system that does a better job both of accumulating knowledge and of ranking people.

The second problem is the practical one of how to organize the system. A true appreciation for the importance of replicating and verifying empirical results would provide publication outlets for all good research. Both financial costs and environmental costs limit the space in paper journals. However, technology permits on-line publications and archives to serve the purpose of providing a publication outlet for all competent research.

In thinking about how to do this effectively, a distinction needs to be made between research involving secondary analysis of existing data and research involving new data collection. A replication involves performing the same study on a new sample and possibly modifying the procedures slightly. For primary analysis, this involves collecting new data, for example conducting a similar ethnography in a different site, or a similar historical analysis on a different case, or a new experiment, or collecting new survey data. For secondary analysis of existing data, a replication involves testing for the same relationship in a different data set.

Relationships that can hold up across sample and across variations in procedures are a much more solid basis for theorizing that relationships that are supported by only one study on only one sample. This is true whether the data are quantitative or qualitative. There should be a place in the publication stream for well-conducted studies that use the same or similar methods as previous researchers on new samples, and authors should be rewarded with publication both for demonstrating that they obtained similar results as previous researchers, or that they did not. These replication studies in general probably do not warrant publication in "top" journals as breakthroughs, but they should be published and there should be systematic attempts to record whether key findings replicate across samples and variations in methods.

For secondary analysis, there are two other types of replication. The first may be called duplication or reproduction: another scholar obtains the same data, runs the same models, and obtains the same results. This matters, because sometimes published results contain mistakes. Quite a few graduate programs require students to duplicate the published results of some paper as the final project in a graduate statistics class, and this should probably be a standard requirement. An on-line registry could report: these results have been successfully duplicated. The second is a robustness check: using the same data, the analyst makes slightly different decisions about model specification and reports whether this changes the core finding. Often the author of the original publication has already conducted multiple robustness checks. These robustness checks are also good training exercises for students and probably do not merit publication on their own, but there is certainly value in providing a repository for original authors and others to post the results of such robustness checks, and there is knowledge to be gained for identifying just how robust a finding is. An example of the value of this kind of collective attention to robustness can be seen in a 33-author project in which 33 teams analyzed the same data, each making slightly different decisions about how to handle the data and which covariates to include, and obtaining a range of coefficients for the effect of central interest. The project yields a range of coefficients and what factors affect it among all 33 replications and provides substantially more solid information than picking one "best" model[iii].

Sociology needs to adopt the custom of other fields and use arXiv or other similar platform as a place to publish and share basic findings. Posting an article in this forum would count as priority or primacy for a finding, as needed, and checks and replications of others' publications would be welcomed. Posting to arXiv would not "count" as much as final publication in a peer-reviewed journal, but it would "count" as having put one's work out for public comment and reaction. This mechanism could also help some recent scandals in which papers delayed in the R&R process were "scooped" by publications going through a less rigorous peer-review process.

THE PROBLEM OF CAPITALISM AND ACADEMIC PUBLISHING

When I got annoyed about reviewing, I started thinking about all the free labor I provide as a reviewer. Writers also provide free content to journals. Even the academic editors of journals typically receive little or no compensation from the journal itself; if the editor is lucky, their host institution will provide a little teaching relief. So journals are subsidized by the labor of academics, and sold back to our academic libraries. Once upon a time, this made sense: academic journals were volunteer activities subsidized by free labor and membership dues. But these days, the academic journals are increasingly owned or leased by for-profit publishing houses which make substantial profits through selling their journals to a captive audience (also known as a "cash cow")—university libraries. These publishers have rarely invested in the start-up costs of building a journal and its prestige; instead they buy them or buy the rights to them from the founders. Some of the publishing houses, Elsevier most notoriously, have acquired through purchase the top journals in some fields and then have used this ownership to bundle their own in-house lesser journals with them and sell the bundle to libraries at enormous cost. In fact, Elsevier has been caught creating "fake" journals that print self-serving pseudo-science by entities such as pharmaceutical companies and including their fake journals in academic bundles. This plus their predatory pricing has made me decide to join the "boycott Elsevier" movement, and cut down my advising burden a bit as I now refuse to review for Elsevier journals. This is, of course, only possible because Elsevier does not own the top sociology journals.

My library at a top public university reports that journal subscription fees have been increasing at $400,000 per year. These journals are making big profits, and they make enough money to pay professional associations significant sums in exchange for being the marketer of an association journal. The publishers, not the authors, hold the copyrights to the intellectual property produced by the authors, and they restrict access to this property. They actually charge the producers of the content money if the authors want to make their work available to the public.

It is well worth asking whether these publishers are providing a service that is worthy of the rent they are extracting. I cannot help but feel "taken" when I'm providing a review that takes 4-5 hours of my time for free to help generate a profit for a capitalist business. The question is whether printing and publishing and extorting money from libraries provides a service to science, or whether academic scientists should instead collaborate with university libraries to re-take control of our intellectual property. We are not going to get paid for what we write, and it is generally in our interest to share our work freely among other scientists. And it is in the interest of science as a collectivity to have some form of peer review. Further, this big money these days probably is not in journals themselves, but in the electronic aggregations of journals like EBSCO and ProQuest.

At a minimum, sociologists need to be informed about the financial arrangements surrounding the journals we publish in. Are they for-profit or non-profit? What revenues are they generating? Are they generating profits? Who is getting the profits? How are the top managers compensated? Which journals are essentially desktop or volunteer organizations, and which ones are money-makers that are possibly exploiting writers and reviewers? Are some journals subsidizing other journals?

It seems to me that for-profit journal publishers should pay their editors for editing either directly as consultants or indirectly by way of compensating the editors' universities for teaching relief to make time for editing. No college or university should ever be asked to provide "free" teaching relief to support the editor of a journal published by a for-profit publisher. No one should agree to edit for a profit-making journal without being compensated either directly in the form of pay as a consultant or indirectly in the form of teaching relief paid to the host institution by the journal. This argument ws made by Hugh Gusterson in a 2012 article in the *Chronicle of Higher Education*.[iv]

CONCLUSION

It is urgent that sociology as a discipline take ownership of its peer review process. We are all doing way too much work and producing worse science in the process. We have to stop burning through reviewers, we have to stop contorting and distorting articles through a torturous review process, we have to stop prioritizing quirky novel results and interesting theoretical twists over solidly documented empirical findings. We have face the fact that we don't all agree with each other about what are good methods or good theories and figure out what to do about that. We have to take a good look at the prestige hierarchy of journals and the financial realities of the publishing and decide what to do about giving more prestige to online and open access journals. We have to face the fact that our university budgets are being treated as cash cows by capitalist journal publishers and cooperate with other disciplines that are pushing back against for-profit publishers and creating alternate modes of communicating research.

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[i] https://scatter.wordpress.com/2013/07/23/too-many-reviewers/

[ii] However, there is a "scandal" in math reviewing that suggests that half of all reviewers do not actually check the math.

[iii] http://home.uchicago.edu/~npope/crowdsourcing_paper.pdf

[iv] http://chronicle.com/article/Want-to-Change-Academic/134546/